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PREDICTING DIFFERENCES IN THE RETENTION OF PROSE MATERIALS ON THE BASIS OF SUBJECT PRIOR KNOWLEDGE

A Thesis Presented

By

CLIFFORD E. KONOLD

Submitted to the Graduate School of the
University of Massachusetts in partial fulfillment
of the requirements for the degree of

MASTER OF SCIENCE

September 1979

Psychology

PREDICTING DIFFERENCES IN THE RETENTION OF PROSE MATERIALS

ON THE BASIS OF SUBJECT PRIOR KNOWLEDGE

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CLIFFORD E. KONOLD

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James M. Royer, Chairperson of Committee

Harry Schumer, Member

Charles E. Clifton, Jr., Member

Bonnie R. Strickland, Department
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Psychology

To my parents, Ross and Myrle, who remain my major source of encouragement.

ACKNOWLEGEMENTS:

EGGNOG ON ANOTHER MASTER OF SCIENCE

It is unlikely that the content of this thesis will ever be of much interest or importance to anyone; it is now of little to me. However, the processes and events which led to its completion are part of my history and, while of perhaps little interest to others, of particular interest to me. It is for this reason that the imaginary audience of this acknowledgement section is myself, some years from now; its purpose to formally recognize the chronology of public and private events which, for me, are this thesis, and which I would not want to forget.

During my first semester of graduate school (Fall, 1975) I met every Monday morning with my advisor, Mike Royer. I would usually have read some articles or book chapters, and we would discuss them. These were uncomfortable encounters for me. I wanted to go into every meeting with some profound insight or question but, once in the meeting, would go blank, become less articulate than usual, and let lose a sigh of relief when the meeting would sputter awkwardly to a conclusion.

I soon learned that Mike had a self-acknowledged reputation for unwittingly rendering students helpless in his presence. Consequently, I labored even harder for clever insights as a shield against my ignorance in the weekly undoings. After all, this was my destiny: to earn through feats of cerebral valor the glorious title, Master of Science. So I was particularly delighted one Monday to have drawn what I thought

was an insightful implication from a paper Mike had recently completed. He gave me a lot of encouragement at this point, and it was while attending my ailing '63 Renault in the basement of the Campus Garage that the idea of using automotive students to test the hypothesis in Experiment 1 came to me. (Later it would come to haunt me.)

Again, Mike encouraged me and suggested that I do my Master's on the topic. I was elated to have a topic so soon, having become aware that many students a year or more along still had no idea of what to do for a thesis. He turned over to me a folder burgeoning with reprints that he had accumulated for the purpose of writing a review paper on the topic of the relation between speed of learning and retention. He suggested that I do the paper which would serve both to fulfill a course requirement, and as a review of the literature for my thesis.

I started on the review paper in November of 1975 and didn't finish it until sometime in the spring of 1976. It was the most difficult thing I had ever attempted to write: I spent days on single sentences which Mike would later suggest I revise or omit; I started to walk a different way to my office in the morning (down the center corridor where the rats were housed) to avoid encountering Mike who, without speaking a word, somehow communicated his disappointment in the fact that the paper was still a promise. It was such a relief when I had finished, not only because I could again look Mike squarely in the eyes, but also because the smell of rat dung every morning was growing increasingly unbearable.

The review paper, with too few revisions, is included as Appendix A in the thesis. I look back now with fond recollection to some of the disputes Mike and I engaged inover wording. As a tribute to my insolent persistence, it still contains a phrase that caused Mike to groan painfully -- "investigatory cul-de-sac." He tried to get me to edit it several times. As a matter of fact, I considered using it as a subtitle for Experiment 1.

Designing the first experiment was an activity I dabbled in for the best part of another year. I found it difficult to think of an automotive concept that students in an automechanics course would have never encountered, but could be related to concepts they had learned. It was Brain Stagner who finally gave me the idea of using the planetary transmission as my target concept. I went to the library and checked out a dusty book which carefully described the transmission, complete with several diagrams. It took me 3 or 4 days of continual study before I understood how the transmission worked, and another week to coherently (I had thought) write it down. In the first experiment, I asked students to try to comprehend this enigma after about 30 minutes of study. I thank those who, under these conditions, read the passage and did nothing more injurious than to snicker at me when I asked them 2 weeks later to write down everything they could remember about it. "Something about a tranny", was all one budding mechanic could retrieve, but he let me go my way in spite of it.

With the most difficult experimental passage written, it wasn't long before I finished a proposal and then, after showing it to Mike, a second proposal which was finally approved by my committee in February of 1977. (In addition to Mike, the committee included Harry Schumer and Chuck Clifton.)

All I needed now was to find some willing (I would eventually settle for unwilling) vocational students to serve as subjects. The school year was over before I found such a group. In the fall of 1977 however, and with the help of Jim Franklin, a friend whose uncle taught an automotive course, I secured the permission of both the principal and the course instructor, Karl Dilhman, to run the study at Franklin County Technical School. They were both very helpful and somewhat excited about the project.

I ran the first group of students in November and finished a month later. No part of the project went as quick nor was as dirty. The students were initally excited to be doing something different in class. However, when they discovered that I was asking them to do the same kind of "junk" that was required in more traditional educational settings, their interest turned to quiet resignment in most cases, and defiant refusal in a few. I helplessly watched while a student of the later variety smugly drew an arbitrary path down the true/false options on the retention test; he scored higher than most of his classmates. It would have caused me professional embarrassment to have recorded in the results section most of what occurred in these sessions: mud dug out of winter boots being flung about the room, catcalls, laughter, boistrous talking, visual consultations of neighbor's work. Feats of cerebral valor? A Master of Science? I couldn't control a bunch of beardless boys! I hoped in vain that when I had "cleaned" the data by coding and running it through a computer these indignities would not matter. I'm grateful to Harry Schumer who, having tried to warn me of this outcome, has only reminded me of it twice since.

At this point I was fed up with the project; it had been dragging me along for 2 years, and now it seemed that no interpretable data would result; I tried to get into another vocational school, but they wouldn't have me; I was running college students in the other experimental condition, but having difficulty recruiting them. Mike had been away on sabbatical since September, and I was more or less on my own as far as deciding what to do. Writing a thesis with the major conclusion being that a pilot study should have been conducted to insure the appropriateness of the materials was not an inviting alternative.

It was in this depressed mood and while still running the college students for Experiment 1 that the idea for Experiment 2 occurred to me. I was quite excited about the design, mostly because I felt that, if nothing else, it would provide data which could be interpreted -- I would have something other to say than "oops." I quickly geared up to start running it and informed a somewhat less than enthusiastic Mike Royer over the phone of my plans.

During the summer of 1978 there just weren't enough students around who would indulge me as subjects, and so it wasn't until early in the fall semester that I finally finished collecting data. Mike had returned during the summer and, even though I had relocated my office and could forgo detours down rat alley, his proximity again provided an ever-present impetus.

Having analyzed the data, all that remained was to write the majority of the thesis. Karen, Merinda and I were leaving in December to spend a month with our families in San Diego, so I decided to have my oral defense before leaving. (The thought of facing my parents with the confession that I still had not completed my Master's had absolutely nothing whatsoever at all to do with this decision.) I began writing (churning out) sections and handing them to Mike for suggested revisions.

This process continued rather smoothly until it was time to write Chapters 7 and 8. Mike and I quickly reached an impasse on what could validly be concluded from Experiment 2. He felt that the design was inappropriate for testing the hypothesis and I, rather arrogantly, disagreed. For 3 weeks we could not remain pleasant in each others company. Friendly luncheon encounters at the Blue Wall became grim scenes of debate. It finally reached the point where an arbitrator became necessary, but the argument was not well enough defined for this to help. Our major disagreement was, in fact, over what the disagreement was about! Mike emphasized what he saw as problems in my muddled design; I argued that the problem was one of his statistical naivete.

These arguments remind me now of similar disputes my father and I still have about the evils of card playing. I think both my father and I have come to enjoy these perennial go arounds mostly because we come away from them even more assured of our respective positions. And somehow they're endearing. The ultimate argumentative experience will occur when Mike, my Dad and I meet, and while defending my design to Mike, I can try to justify to my dad having invented yet another card game and instructed nearly a hundred youth on how to play it.

The issue was finally resolved when Mike decided to let me venture onto the enfeebled limb of my design, and promised, against my protestations, not to shake it in the presence of the vulturous committee.

The faint aroma of rats drifted in from down the hall as the procession of committee members filed into the small room. I felt a bit awkward to have in plain view the tea rings and eggnog Karen had prepared the night before in hopeful anticipation of a celebration feast. Nevertheless, with all the composure I could muster I defended my thesis. And in spite of my cocky invitations and hard-to-pass-up opportunities in the December chill of my defense, Mike faithfully kept the promise he had made. When it was over I was excused from the room so that the committee, in secrecy, could decide whether I should be permitted the sought-after title. I was readmitted almost too quickly, then knighted with hearty pats on the back. I had hoped to succeed; I was happy to survive.

"Drink up. The eggnog's on me."

April, 1979

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CHAPTER I

INTRODUCTION: EXPERIMENT 1

Regardless of how simple or complex the learning task, individual performance on the task will vary considerably. An obvious question is whether such differences in learning are related to differences in retention. The question of whether a relation exists between speed of learning and amount retained has actively engaged psychological interest since the early work of Ebbinghaus. Based on an extensive review of the literature, and with the added information provided by her own studies, Gillette (1936) concluded that fast learners retain information better than slow learners. This conclusion was generally accepted until Underwood (1954) presented the argument that the techniques employed by Gillette and others to equate initial performance of fast and slow learners had not succeeded. He argued that fast learners had appeared to possess superior retentive abilities only because they were at a higher level of learning at the onset of the retention interval. Using a technique designed to equate levels of original learning, Underwood (1954, 1964) provided evidence that no difference exists in the amount retained by fast and slow learners. This conclusion has been supported by several other investigators (e.g., Schoer, 1962; Shuell & Keppel, 1970; for a comprehensive review of the literature concerning retention vis-a-vis speed of learning, see Appendix A).

It should be pointed out, however, that as research in this area progressed from Ebbinghaus to Underwood, and as the problem of equating initial performance became more apparent, investigators began to rely more and more exclusively on the use of simple list materials, typically paired associates. The question of whether Underwood's conclusion can thus be generalized to more complex meaningful materials was not directly raised until recently. In an article by Royer, Hambleton & Cadorette (1978) a number of issues were raised which pose a challenge to the conclusion reached by Underwood and others. Those issues were not raised, as in the past, strictly in the form of methodological criticism. Rather, they arose from an approach that began with a desire to test the findings of Underwood using more complex materials, but which quickly grew into a theoretical conceptualization of the problem of equating learning.

The Question of Generalizability

Royer et al. questioned whether the conclusion of no retention differences derived from list research could be generalized to more complex materials. This question was posed in the context of a fairly recent conceptualization of memory (referred to as "Constructive Theory" by Royer et al.) which suggests that the nature of a memory trace is jointly determined by three distinct factors: 1) The stimulus event, 2) the context in which the stimulus is presented, and 3) any prior knowledge that is relevant to the stimulus and context. Royer et al. point out that the extent to which these factors contribute to differences in learning in a given experiment is dependent on the particular task, procedure and subjects employed. Consider first the context in which a stimulus is presented. In a natural setting, a given stimulus is seldom perceived from the same angle, immediately preceeded by the same sequence of events, or attended to for the same reasons by all observers. However, in the typical laboratory experiment, the attempt is made to control for these potential sources of variation so that the stimulus context is as similar for all subjects as possible. Indeed, it has been demonstrated that the comprehensibility of a stimulus can be dramatically affected by manipulating the context in which it is presented (Bransford & McCarrell, 1974; Bransford & Johnson, 1972).

The influence of prior knowledge can be minimized or maximized by choosing subjects with differing prior knowledge or by manipulating the experimental materials. One crucial dimension discussed by Royer et al. is the meaningfulness of the materials. The influence of prior knowledge is presumably maximized when the materials to be learned can be easily related to existing knowledge structure. A subject simply relates the new material to what is already known. With relatively non-meaningful materials, such as a paired-associate list of trigrams, the extent to which prior knowledge affects learning is greatly reduced. (It is for this very reason that trigrams are employed -- to reduce the effect of prior experience.) Similiarly, the influence of prior knowledge can be varied by selecting subjects who either do or do not possess knowledge that is relevant to a certain task.

According to this perspective, the amount of variance attributable to prior knowledge covaries with the meaningfulness of experimental materials and differences in prior knowledge possessed by the subject population. The studies from which the conclusion of no retention differences has been drawn are thus viewed as a special case, in that they involved materials which limited the possible contributions of prior knowledge. Given this perspective it can be asked whether using experimental materials which are more amenable to integration into existing knowledge structures will result in retention differences between fast and slow learners.

As a test of this hypothesis, Royer et al. conducted two studies. The first involved learning a categorizable free-recall list, and the second a programmed instruction unit on "The Structure of Matter". When retention was assessed 35 and 30 days later respectively, high-ability students proved, in both cases, the better retainers (for a more complete review, see Appendix A).

From the perspective of Constructive Theory, one interpretation of the Royer et al. findings is that fast-learning (or high-ability) subjects retain the information better because it has been integrated into an existing knowledge structure for which multiple retrieval paths have been established. Subjects who have learned the same information but who are unable to relate it to existing knowledge, or whose existing knowledge structure is not as rich, have fewer retrieval paths available. This relevant-knowledge deficit results in a reduced probability of retrieving, and thus retaining, learned information. The authors go on to suggest that rate of learning may also be influenced by relevant knowledge. That is, the reason that high-ability subjects learn more quickly may be that they have available a knowledge structure into which the new information can readily be integrated, while the low-ability subject must spend more time searching for relevant connections between what is known and what is being presented, strengthening those connections where they are weak, and "rote memorizing" or using some other strategy to store information for which connections with prior knowledge cannot be economically made.

The Problem of Equating Learning: Some Assumptions

While the two studies reported by Royer et al. seem to contradict previous (or at least the more recent) conclusions, they can be criticized on the grounds that the level of initial learning of high- and low-ability subjects was not equated. Aware of this possibility, Royer et al. cite some evidence that their results were not a function of different levels of learning (see Appendix A).

More significant, however, are the authors efforts to explicate the assumptions involved in attempts to equate two subjects with respect to level of learning. They present the argument that Underwood, in cautioning that performance measures can be deceptive as to the actual level of underlying learning, makes the assumption that the level of learning on a specific task, and not just performance, can be equated for two individuals. From this assumption a corollary is derived -- if it is possible to equate the learning of two individuals, then it must be true that, while the memorial representations of a specific stimulus can vary in strength between individuals, they must be identical in nature. If this corollary is not accepted, it is difficult to conceive, according to Royer et al., how learning, as opposed to performance, could ever be equated.

If one accepts the premise from Constructive Theory that the memory trace is a function of prior knowledge, the environmental context and the experienced stimulus event, the conclusion must be that the nature of the memory unit associated with a particular stimulus event necessarily differs between individuals. Equating learning, in a literal sense, is thus viewed as impossible. Rather, one must always expect that two individuals with equal performance measures differ in some respect as to the nature of the underlying memory representation.

From the perspective of Constructive Theory then, the task of developing a methodological technique that is capable of producing and demonstrating equivalent levels of learning so as to allow comparisons of group retention differences becomes impossible. Even if it were the case that two individuals could be equated with respect to learning, the techniques developed by Underwood for equating performance on simple list materials cannot be used for more complex materials.

Given the difficulties with finding a satisfactory procedure for equating initial performance levels with complex meaningful materials, Royer et al. suggested an alternative research strategy. This involves an experimental design which varies subject ability and prior knowledge in such a way that it can be predicted that low-ability subjects will be the better retainers on a task for which they possess the more relevant knowledge. Actually, it is not necessary to vary subject ability, for while it may be true that knowledge structure is a factor in determining acquisition rate, this has only been suggested by Royer and his associates as a possibility and is not critical to the test of the hypothesis that prior knowledge is an important variable in retention. What is required is that two groups of subjects be identified, one which possesses knowledge relevant to a certain task, the other which does not. The prediction, based on Constructive Theory, would be that the former group would retain information better than the latter group.

Predictions

The purpose of the present study was twofold -- first, to determine whether retention differences would result when subjects differing in relevant prior knowledge learn complex materials, and second to provide evidence for the role of knowledge structure in the retention of the materials. If retention differences can be predicted on the basis of relevance of prior knowledge, the Constructive Theory of the nature of the memory trace will receive support. But irrespective of the validity of Constructive Theory, the simple question of whether retention is superior when new information can be related to and integrated with previously stored information holds important implications for educational practice.

The study involved presenting two passages (one which concerned the workings of the Model T planetary transmission (PT), the other which described the rules of a ficticious card game (CG)) to two groups of subjects. The subjects, college and vocational students, were expected to differ with respect to prior knowledge relevant to the two passages, the vocational students being more familiar with automechanics, the college students with card games.

It was predicted on the basis of Constructive Theory that the vocational students would demonstrate superior retention of PT and inferior retention of CG relative to the college students. The null hypothesis, that the groups would retain information from both passages equally well, would argue against the hypothesis that prior knowledge is related to retention (the knowledge-structure hypothesis) and would support Underwood's conclusions.

CHAPTER II

METHOD

Design and Subjects

The experimental design was a 2(student type: college or vocational) X 2(passage type: card game or planetary transmission) X 2(retention interval: immediate or delayed) mixed design with retention interval being the within-subject variable.

A total of 64 males participated in the study. Half of the subjects were students (grades 10-12) at the Franklin County Technical School located in Miller's Falls Massachusetts. They participated in the study, with the permission of their instructors, during their regularly scheduled class period. The remainder of the subjects were undergraduate psychology students at the University of Massachusetts who volunteered to earn extra course credit. Three of the vocational students and four of the college students did not show up for the second session of the experiment and were therefore not included in the analysis. In addition, two vocational students were dropped due to failure to follow the experimental instructions. Three additional subjects were randomly dropped to equate cell sizes. Thus, a total of 52 subjects were included in the analysis, with 13 subjects in each of the 4 student- X passage-type conditions.

Materials

Two passages were prepared. One dealt with the rules and terminology of a fictitious card game called Anchor Rummy (CG). The other passage described the components and functioning of the Ford planetary transmission (PT) which was used in the Model T. The passages were approximately 900 and 770 words in length, respectively. Several sentences in each passage (72 and 141 words in the CG and PT passages respectively) were included specifically to help subjects relate information in the passage to knowledge they already possessed. These sentences were set apart from the rest of the passage by placing a solid line (box) around them.

It was feared that some of the subjects, particularly the vocational students, might read through the passage too quickly and that as a result their level of learning might not be at a high enough level to allow meaningful measurement of delayed recall. Therefore, ten study questions requiring short-answer responses were constructed for each passage. The questions were designed so that they could be answered by simply referring to information in the passage and were not intended to require inferential abilities. It was hoped that requiring subjects to answer these study questions would lessen the liklihood of very low learning levels.

The dependent variable of major interest was the free recall of information contained in the passages. However, the possibility existed that the vocational students might be able to correctly recognize information they had studied, but not be able to coherently write it down. This concern was supported by the course instructors who provided samples of the students' written work. With the possibility in mind that the vocational students might perform very poorly on the free-recall task, a true/false recognition task was included as an additional dependent variable. Forty-four true/false items were constructed for each passage to test for simple recognition of information contained in the passages. The questions were constructed in pairs so that two questions tested for knowledge of the same general idea. Members of each pair were then randomly assigned to one of two forms with the restriction that the number of true and false items on each form be equal. Thus, there were two parallel true/false forms for each passage, each containing 22 items.

A 36-item vocabulary test (French, Ekstrom & Price, 1963) was employed both to impede rehearsal during an interval before immediate recall and also to provide an estimate of verbal ability for use as a covariate. The test, however, proved too difficult for the vocational students. An additional 36-item test was therefore developed by the author specifically to discriminate among the vocational students.

In an attempt to determine the degree of familiarity with card games and automechanics, a questionnaire was constructed which contained the names of 20 card games and 20 automotive repairs. Five of the card games were fictitious, and five of the automotive repairs were either never or rarely performed (e.g., "rebuild regulator") or, according to the course instructors, had never been performed by the vocational students (e.g., "bore out engine block"). These were included in the questionnaire as a method of determining when a subject might not be responding truthfully.

A complete set of the experiemtnal materials is included in

Appendix B.

Procedure

Subjects were run in groups which ranged in size from 1 to 26. They were assigned to the passage condition by handing out randomly-arranged envelopes containing the experimental materials as they arrived for the experiment.

Subjects were first instructed to read a cover page which explained that the experiment was designed "to help us better understand how what a person already knows affects the learning of new information". The cover page also instructed them that they were to read the passage on the next two pages through twice, the first time through quickly, and the second time more slowly. They were instructed to read it so that when asked to do so, they could recall as much of the information as possible. They were informed, too, that they would not be asked to recall information which had been surrounded by a box, that this information had been included to help them relate the new information to what they already knew.

Subjects were given as much time as they wanted to read the passage. After reading the passage, they were instructed to complete the series of ten study questions by trying first to answer them without referring to the passage, and then by checking their answers with information in the passage.

After completing the study questions and replacing them along with the passage in the envelope, subjects were given 8 minutes to complete the first 36-item vocabulary test.

After the 8 minutes had passed, subjects were instructed to write down everything they could remember from the passage they had read. They were told that neither order nor exact wording were important, but that they should try in their own words to "reconstruct the important points and details of the passage". When they had written down everything they could remember, they were instructed to put their protocols in the envelope and were then administered one of the forms of the 22-item true/false test. They were instructed to make the best choice they could if they had no idea whether a given statement was true or false. There was no time limit on either the recall or recognition task. The sequence of the true/false tests were counterbalanced so that the two forms were equally represented on each test occasion.

The second session of the experiment was conducted 2 weeks later. Upon returning, subjects first were administered the second 36-item vocabulary test. When the vocabulary test had been completed, or when 8 minutes had elapsed, subjects were tested for retention of the passage. They were first asked to write down everything they could remember from the passage. Having completed this task, they were administered the alternate form of the true/false test. Subjects were given as much time on both tasks as they desired. When subjects had completed the recall tasks they were administered one of the forms of the true/false test covering the passage they had not read. The topic of the passage was provided, and the subjects were told that they were not expected to know many of the answers to the questions. They were instructed, however, to make the best guess they could on each item. This task was included to determine whether, indeed, the information covered in the passages was new to both the college and vocational students. There was no time limit on this task.

The questionnaire was administered last. Subjects were asked, regardless of which passage they had read, to put a mark by every card game which they had played before and by every automotive repair which they personally had performed. Having completed this task, the materials were collected, and subjects were debriefed as to the purpose of the experiment.

Scoring

In order to score the free-recall protocols, the passages were broken down into "idea units" (cf., Royer & Cable, 1975). The CG passage yielded 61 idea units and the PT passage 55. In addition, there were 8 and 7 key terms in the passages, respectively, which were scored. The score of a subject's protocol was the number of idea units it contained plus the number of key terms mentioned. The maximum score on the passages were 69 (CG) and 62 (PT). (See Appendix C for the breakdown of the passages into idea units and key terms.)

To estimate the reliability of scoring the free-recall protocols using this procedure, six randomly-chosen protocols from the CG condition were scored independently by the author and three undergraduate volunteers. Training included having the volunteers read the passage through twice, write down everything they could remember, and score their own protocols. The author then discussed with the group, problems which had been encountered in scoring. When the group had no further questions about the procedure, they were given the six protocols to score. They were instructed to indicate each acceptable idea unit by enclosing it in brackets and by writing the number associated with the appropriate idea unit immediately above. They were not allowed to return to a protocol once they had finished scoring it, and the order of scoring was varied across scorers. The proportion of the number of agreements to the total number of agreements and disagreements among the scores was .86.

The true/false and vocabulary tests were scored for total number right, and the individual scores on the two vocabulary tests were summed together.

From the questionnaire, separate scores for card-game and automotive experience were obtained by summing the number of valid games and repairs checked. A subject's score on the questionnaire was not included in the analysis if more than one of the five fictitious items were checked.

CHAPTER III

RESULTS

The first step in the analysis was to demonstrate that vocational and college students differ with respect to prior knowledge of card games and automotive repairs. The mean number of card games checked by the vocational and college students was 2.32 and 7.35 respectively, the difference being statistically significant, $t(50) = 3.30$, $p < .01$. The corresponding means for the automotive repairs were 8.65 and 3.81, $t(50) = 2.87$, $p < .01$. Based on these results it seems justified to conclude that the two groups differ with respect to prior knowledge relevant to the two passages.

Also, it was necessary to demonstrate that the passages contained information that was novel to both groups. Chance performance on the true/false test covering the passage which was not read would provide such evidence. Mean performance for the college students on the CG and PT passages was 9.85 and 11.08. The corresponding means for the vocational students were 12.00 and 10.85. Individual t-tests were performed and revealed that none of these values differ significantly from the chance performance score of 11.

Initially, free recall and true/false performance were analyzed separately, each in a 2(student type: college or vocational) X 2(passage type: CG or PT) X 2(retention interval: immediate or delayed) analysis of covariance design, with vocabulary score as the covariate and the retention interval as the within-subject variable.

Free Recall

Vocabulary score proved to be a significant covariate in the case of free-recall performance, accounting for 17% of the between-group variance. Table 1 includes a summary of the analysis. The adjusted cell and marginal means for free recall are provided in Table 2. As was expected, recall after the 2-week interval was significantly lower than initial recall performance. Also, the PT was more difficult than the CG passage for both groups, and, relative to performance on CG, more difficult for the college than for the vocational students, as evidenced by the significant group by passage interaction.

Based on the knowledge-structure hypothesis, it was predicted that the college students would demonstrate superior delayed recall of CG relative to the vocational students, and that the reverse would occur in the case of the PT passage. Confirmation of these predictions would be found in the analysis of the first-order interactions between group and recall interval at each passage, and in a significant second-order interaction between all three variables.

The predicted second-order interaction is not significant. The overall group by recall-interval interaction, however, is significant and is plotted separately for each passage in Figure 1. These interactions at each passage are also significant, with $F(1,48) = 5.49$, $p < .025$ for CG and $F(1,48) = 7.32$, $p < .01$ for PT. Inspection of Figure 1 reveals that while the nature of the interaction at PT is what might be expected given the knowledge-structure hypothesis, the pattern of the means at CG are not what would be predicted, the college students

Source	Sum of Squares	df	Mean Square	F	Prob.
Between Group					
Group (G)	399.42	1	399.42	3.00	
Passage (P)	2148.55	1	2148.55	16.15	.001
GP	932.87	1	932.87	7.01	.025
Covariate	899.86	1	899.86	6.76	.025
Error	6252.73	47	133.04		
Within Group					
Recall (R)	2318.09	1	2318.09	93.78	.001
RG	315.01	1	315.01	12.74	.001
RP	57.01	1	57.01	2.31	
RGP	1.62	1	1.62	.07	
Error	1186.52	48	24.72		

Table 1. Summary of analysis of covariance: Free recall.

Retention Interval	Passage Type CG		PT		Recall Means
	College	Vocational	College	Vocational	
Immediate	33.17	15.82	16.57	10.71	19.07
Delayed	19.02	8.13	4.88	6.48	9.63
Group Means Within Passage	26.10	11.98	10.73	8.60	
Passage Means	19.04		9.67		

Table 2. Adjusted free-recall means.

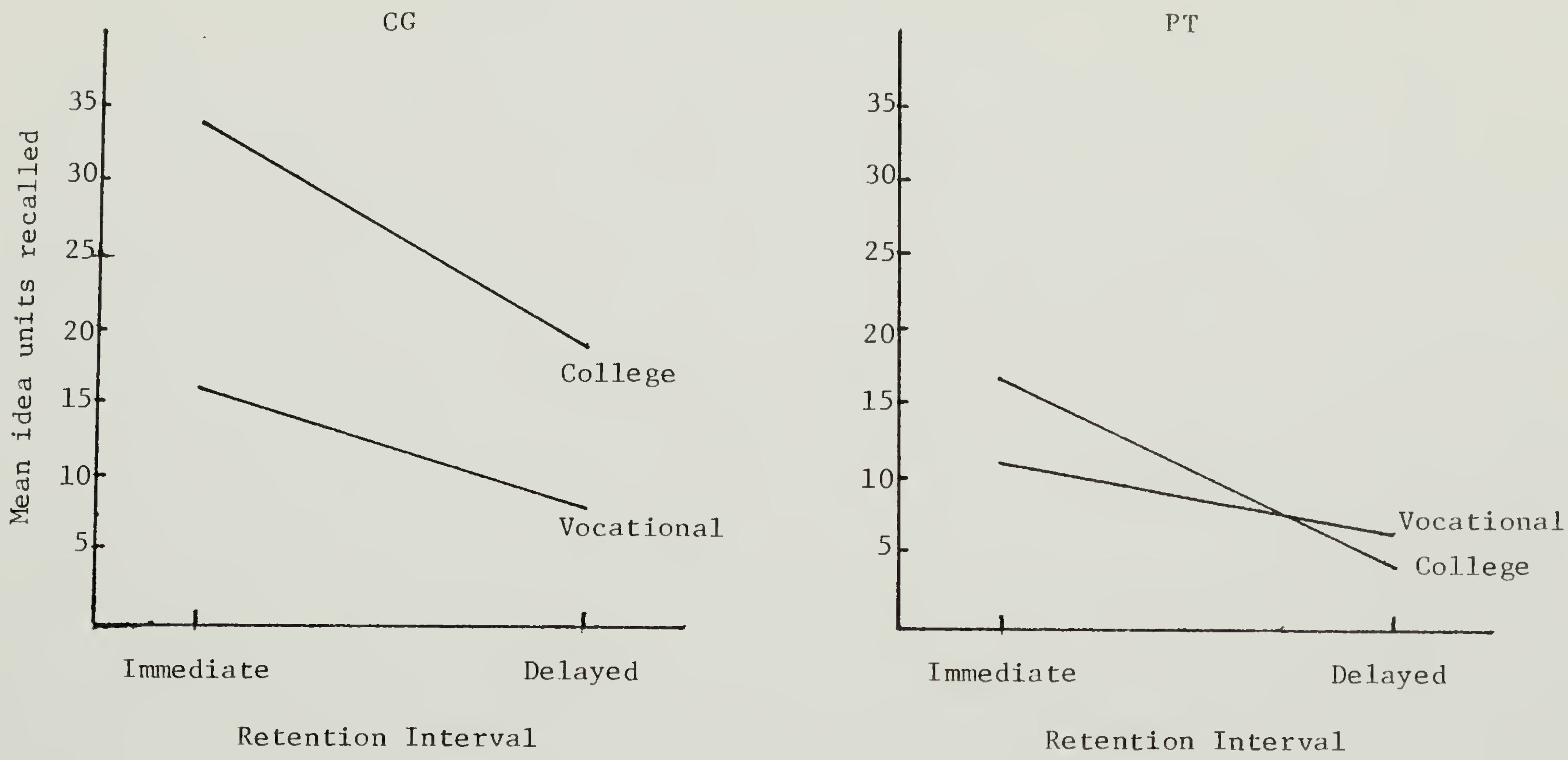

Figure 1. Group by retention-interval interaction at CG and PT.

recalling less (relative to immediate recall) than the vocational students after the 2-week interval.

Since there was evidence that the covariate was differentially effective both within groups and passages (see Table 3), it was decided to repeat the analysis, omitting the covariate. The only different outcome was a significant group effect, $F(1,48) = 61.72$ $p < .001$, such that, overall, the college students performed better than the vocational students (means = 23.75 and 4.95, respectively).

Group		Recall Interval	
		Immediate	Delayed
College	CG	.553	.035
	PT	.338	.177
Vocational	CG	.078	.181
	PT	.680	.771

Table 3. Correlations between vocabulary and recall scores for each group by passage condition.

True/False

The covariate proved nonsignificant in the case of true/false performance and, therefore, was not used in the analysis. The cell and marginal means are displayed in Table 4. The summary of the analysis of variance is included in Table 5. The group, passage and retention interval main effects are all significant, replicating the results with the free-recall data. None of the other effects, however, are significant.

Retention Interval	Passage Type: CG College	CG Vocational	PT College	PT Vocational	Recall Means
Immediate	19.15	15.23	17.54	13.38	16.33
Delayed	17.31	11.92	14.92	11.31	13.87
Group Means Within Passage	18.23	13.57	16.23	12.35	
Passage Means	15.90		14.29		

Table 4. Unadjusted true/false means.

Source	Sum of Squares	df	Mean Square	F	Prob.
Between Group					
Group (G)	473.88	1	473.88	31.62	.001
Passage (P)	67.85	1	67.86	4.53	.05
GP	3.85	1	3.85	.26	
Error	719.46	48	14.99		
Within Group					
Recall (R)	157.54	1	157.54	27.38	.001
RG	1.38	1	1.38	.24	
RP	.35	1	.35	.06	
RGP	6.50	1	6.50	1.13	
Error	276.23	48	5.75		

Table 5. Summary of analysis of variance: True/false.

CHAPTER IV

DISCUSSION

The only result which could be interpreted as supportive of the knowledge-structure hypothesis is the group by retention-interval interaction at PT, where the vocational students, as predicted, displayed superior delayed recall relative to the college students. An interpretation of this result based on the knowledge-structure hypothesis is that the vocational students, having a richer knowledge base concerning automotive concepts, were able to better integrate and thus better retain the new information about the planetary transmission. The corresponding interaction at CG, where the college students were predicted to have demonstrated superior delayed recall, is not consistent with the knowledge-structure hypothesis, the vocational students again demonstrating the better retention relative to initial performance.

One reasonable explanation of these contradictory results is found in the relatively low scores for initial performance by the vocational students on both passages. The unadjusted cell means for immediate free recall on CG (9.62) and PT (6.23) and the corresponding means for true/false performance (15.23 and 13.38) are indicative of a "floor effect". If, indeed, there was a floor effect, it would have biased against the knowledge-structure predictions in the case of CG, and for the predictions in the case of PT. Thus, the results cannot be interpreted as either supportive or nonsupportive of the knowledge-structure hypothesis.

Additional insight relevant to interpreting the particular interactions reported here, as well as those obtained in any similar test of the interactive hypothesis proposed by Royer et al. (1978), is provided in a recent article by Bogartz (1976). Bogartz points out that a correct interpretation of a statistical interaction must be based on an understanding of the relation between the specific research design employed and a theoretical model of the behavior under study.

To illustrate, a simple model of delayed recall under the conditions of this study can be formulated by specifying three hypotheses:

Hypothesis 1. Any idea unit is recalled after a delay interval with the probability p.

Hypothesis 2. Under free-recall instructions, the probability of correctly guessing an idea unit not retrieved from memory is zero.

Hypothesis 3. Under the null hypothesis, the probability in the case of the relevant-knowledge group of recalling an idea unit (p^k) is equal to the corresponding probability in the knowledge-deficient group ($p^{\bar{k}}$). Under the alternative, knowledge-structure hypothesis, $p^k > p^{\bar{k}}$.

With the additional assumption that initial recall for the knowledge-relevant group (x) will be greater than for the knowledge-deficient group (y), the theoretical results of the design when $p^k = p^{\bar{k}}$, can be specified as below.

Group	Recall Interval	
	Immediate	Delayed
Knowledge-relevant	x	px
Knowledge-deficient	y	py

The interaction between recall interval and group is $(x-y)-p(x-y)$. Thus, under the null hypothesis when there are no differences between groups in the probability of recalling a particular idea unit, a positive interaction[1] is obtained when $x > y$ and $p < 1$. This occurs since, according to Hypothesis 1, recall is specified as a proportional function of amount originally learned. Therefore, those who learn the most are predicted, under the null hypothesis, to also forget the most, the proportion forgot being equal.

Under the alternative hypothesis, either a positive, zero, or negative interaction can occur, depending on the specific values of p^k and $\bar{p}^k$. For example, if $x = 100$, $y = 80$ and $p^k = .50$, an interaction term of zero will result when $\bar{p}^k = .375$. When $\bar{p}^k$ exceeds .375, a positive interaction will result, a negative interaction occurring when it is less than that value. It is clear that given the hypotheses described above, when using the analysis of variance design employed in this study, only zero and negative interactions can be interpreted, since a positive interaction is possible under both the null and alternative hypotheses.

Having made explicit an hypothesized relation between the experimental design and recall performance, the biases introduced in this study by the floor effect might be more clearly specified. First considering the group by retention-interval interaction at CG, either a zero or negative interaction would have led to the rejection of the null hypothesis that both groups retained the information equally well. The

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[1]When $(x-y) > p(x-y)$, the interaction is positive. When $(x-y) < p(x-y)$, the interaction is negative.

floor effect, however, biased against a negative and toward a zero or positive interaction, the latter having been observed. Since it is not known whether a positive interaction would have obtained in the absence of a floor effect, no conclusions can be drawn regarding the knowledge-structure hypothesis.

In the case of the interaction at PT, it is clear that, even in the absence of a floor effect, ambiguous results would have been obtained under the alternative hypothesis. Since the college students had recalled more information than the vocational students immediately after reading the passage, a positive interaction would have been predicted on the basis of the knowledge-structure hypothesis. A positive interaction, in this case, is the only possible outcome under the null hypothesis. Here, the floor effect biased in favor of a predicted positive interaction. However, since such an interaction is also the predicted outcome under the null hypothesis, no conclusions can be reached.

One conclusion that can be reached on the basis of this study is that researchers who employ an analysis of variance design to determine whether groups that differ in amount learned also differ in retentive abilities need to consider the effect of hypotheses held regarding learning and retention on the interactions to be predicted. Failure in the present study to do so led to the inclusion of a condition (PT) in which it would have been impossible, even in the absence of a floor effect, to decide between null and alternative hypotheses.

CHAPTER V

INTRODUCTION: EXPERIMENT 2

In the analysis of variance design used in the first study, evidence of a relation between retention and prior knowledge was sought in the analysis of specific interactions between one or more stimulus passages and groups of subjects which differ with respect to prior knowledge relevant to the passages. As pointed out, however, to interpret the results from this design some specific assumptions must be made, the most important of which concerns whether amount retained (delayed recall) is a proportional function of amount learned (immediate recall). Because groups will usually differ in amount learned, different assumptions about the nature of the learning-retention function will result in different interaction predictions.

Choosing between these assumptions is not necessary within a regression framework which requires only that learning and retention be linearly related. For this reason, Experiment 2 was conducted within a regression framework with immediate and delayed recall, and measures of prior knowledge as variables. One option was to specify delayed recall as the criterion variable, and determine whether, after entering immediate recall as a predictor in a regression equation, additional predictive power could be obtained by entering measures of prior knowledge. To find that the addition of prior knowledge allows better predictions of delayed recall would support the Royer et al. studies.

A related technique, which is particularly suited to testing hypothesized causal relations among variables is path analysis, (Blalock, 1962; Kerlinger & Pedhazur, 1973). The question of whether prior knowledge is directly related to amount retained or only indirectly related through amount learned can be represented in a path diagram which specifies causal relations. For example, suppose that, as Underwood (1954, 1964) has concluded, retention is a function only of amount originally learned. Assume too (though Underwood may never have suggested it), that prior knowledge is related to amount learned, but it is not directly related to retention. The hypothesis which results from combining these two assumptions is represented by the path diagram in Figure 2.

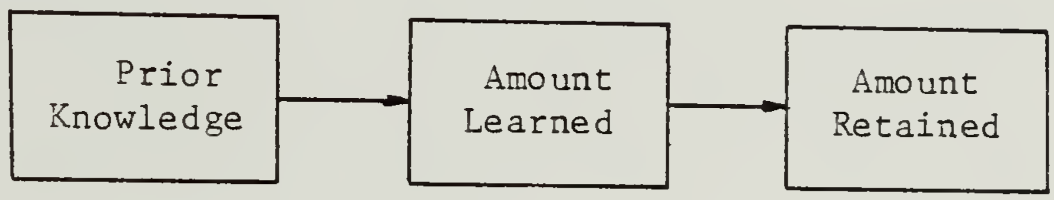

Figure 2. Path diagram with prior knowledge indirectly related to amount retained.

According to this model, any correlation between prior knowledge and retention could be explained by the intermediate variable, amount learned. Thus, if amount learned was controlled for (partialled out), the correlation between prior knowledge and amount retained would be zero.

Contrast this with the model suggested by the knowledge-structure hypothesis in Figure 3.

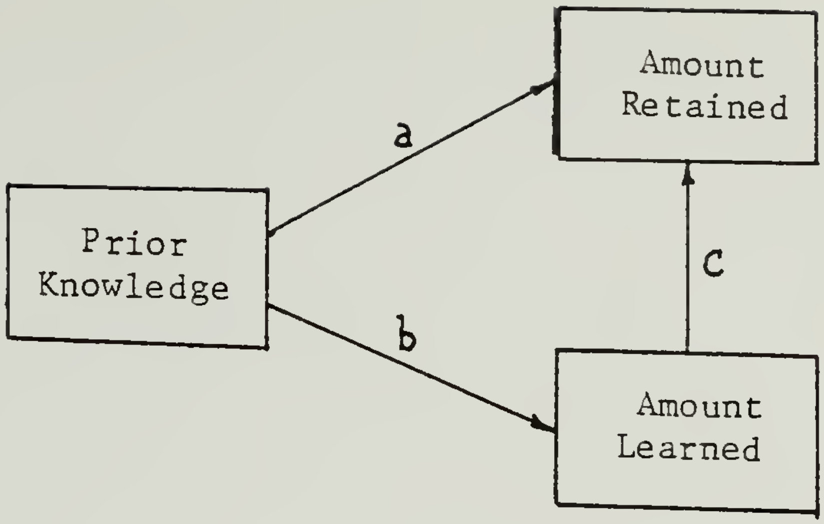

Figure 3. Path diagram with prior knowledge directly related to amount retained.

In this case, prior knowledge is not only indirectly related to amount retained through amount learned (paths b and c), but also is directly related (path a). This model suggests that the partial correlation between prior knowledge and amount retained, controlling for amount learned, is nonzero.

In a path analysis, path coefficients (standardized regression coefficients) between variables are computed. Hypothesized causal links between variables can then be evaluated as to their necessity in the model by testing whether the associated path coefficients significantly differ from zero. Thus, one test of the two alternative models depicted above would consist of determining whether the observed value of path a in the second model is significantly different from zero.

In Experiment 2, subjects studied a passage concerning a fictitious card game and were tested for immediate and delayed recall. Two types of prior knowledge were assessed: prior experience with card games and verbal ability as determined by a vocabulary test. The path model (Model 1) consistent with the knowledge-structure hypothesis is depicted in Figure 4.

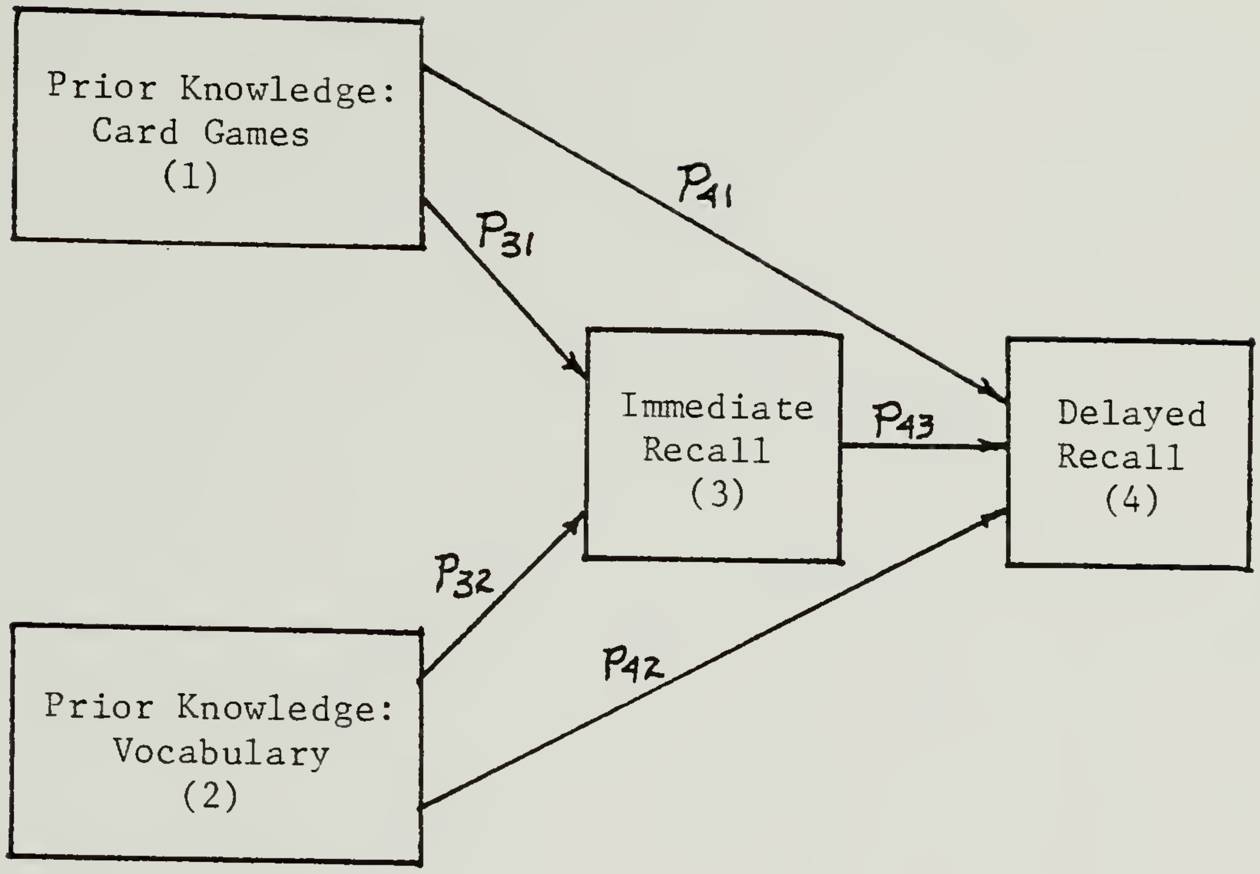

Figure 4. Path diagram predicted by the knowledge-structure hypothesis.

In Model 1, the two prior knowledge variables (1 and 2) are not expected to be related, as indicated by the fact that no path directly connects them. They are exogenous variables, variables whose causes lie outside the causal model. Both recall variables (3 and 4) are endogenous in that some of the variability in each variable is explained by variables which precede it in the model. According to the knowledge-structure hypothesis, p_{41} and p_{42}, the path coefficients relating prior knowledge to delayed recall, should be nonzero, as should the other coefficients in the model. A more parsimonious model (Model 2) is predicted under the hypothesis that delayed recall is related only to immediate recall. In Model 2, $\underline{p}_{41}$ and $\underline{p}_{42}$ can be eliminated, leaving only $\underline{p}_{31}$, $\underline{p}_{32}$, and $\underline{p}_{43}$ as nonzero. The purpose of this experiment was to determine which model best represents the data and, therefore, which hypothesis concerning the relation between prior knowledge and retention can be supported.

CHAPTER VI

METHOD

Subjects

Forty-four female undergraduates recruited from psychology courses at the University of Massachusetts received experimental credit for their participation in the study. The data from seven subjects were not included in the analysis. One failed to follow instructions; one, a foreign student, had difficulty reading the experimental materials; five failed to return for a second session. Thus, a total of 37 subjects were included in the analysis.

Materials

The passage describing the fictitious card game, the accompanying study questions, and the two vocabulary tests, as described in the first experiment, were employed in Experiment 2.

Two additional tasks were designed as indicators of prior experience with card games. A rating task consisted of 24 lined spaces in which the names of previously played card games could be written. These spaces were followed by the numbers 1 through 4, which could be circled to indicate that a subject was currently (1) not familiar, (2) slightly familiar, (3) moderately familiar, or (4) very familiar with the associated card game.

A 30 item, multiple-choice achievement test was also constructed. The test consisted of 7 questions of a general nature and 23 which dealt with the rules and strategies of 11 different card games. All questions were based on Hoyle's Rules of Games (Morehead & Mott-Smith, 1963). Copies of both the rating task and achievement test are included in Appendex D.

Procedure

As in the first experiment, the study was conducted over two sessions. Subjects were run in groups which ranged in size from 3 to 12.

The procedure for the first session followed that used in Experiment 1, except that a true/false test was not administered after the free-recall task. Subjects read the instructions, studied the passage and completed the ten study questions, were given 8 minutes to complete a 36-item vocabulary test, and then wrote down everything they could remember from the passage.

The second session was also conducted much as it was in Experiment 1. Subjects first were administered another 36-item vocabulary test and then were asked to recall as much information as they could from the passage they had read 2 weeks previous. Upon finishing the recall task, they were given as much time as they needed to complete the rating task. A paragraph at the top of the page instructed them to write down the names of all the card games they had played, and to rate their current familiarity with each game according to a key which was provided. Their final task was to complete the card-game achievement test. An introductory paragraph informed the subjects that the test was designed to tap their knowledge of the "rules, terms and strategies associated with various card games." They were further instructed to choose one of the four options which best completed the statement or answered the question, and to guess when they had no idea of the correct answer. There was no time limit for this task, and when subjects had completed it, they brought all the materials to the experimenter and were given a sheet which described the purpose and rationale of the study.

Scoring

The free-recall protocols and vocabulary tests were scored according to the same procedure outlined in Experiment 1. The achievement test was scored for number right. Two scores were initially obtained from the rating task. The total number of card games listed comprised one score. In counting games, no attempt was made to determine whether a game was valid since many games are known by several, and often esoteric, names. Also, if a subject listed several varieties of a principal game they were counted as separate games. Thus, "Five Card Draw," "Low Ball" and "Mexican Stud" were scored as separate games, even though they are all types of Poker. The other score based on the rating task consisted of the sum of the familiarity ratings.

Rather than use three separate scores for prior knowledge of card games, it was decided to combine them into one score. Since the two scores on the rating task were highly correlated ($r = .931$) the sum of the familiarity rating was omitted. The other two scores, the number of games listed and score on the achievement test, were converted to z-scores, averaged for each subject, and then converted to a T-score.

CHAPTER VII

RESULTS AND DISCUSSION

The correlations among the four variables are shown in Table 6. The means and standard deviations are included at the bottom of the correlation matrix.

The path diagram complete with the values of the path coefficients is shown in Figure 5. According to both Model 1 and 2, prior knowledge

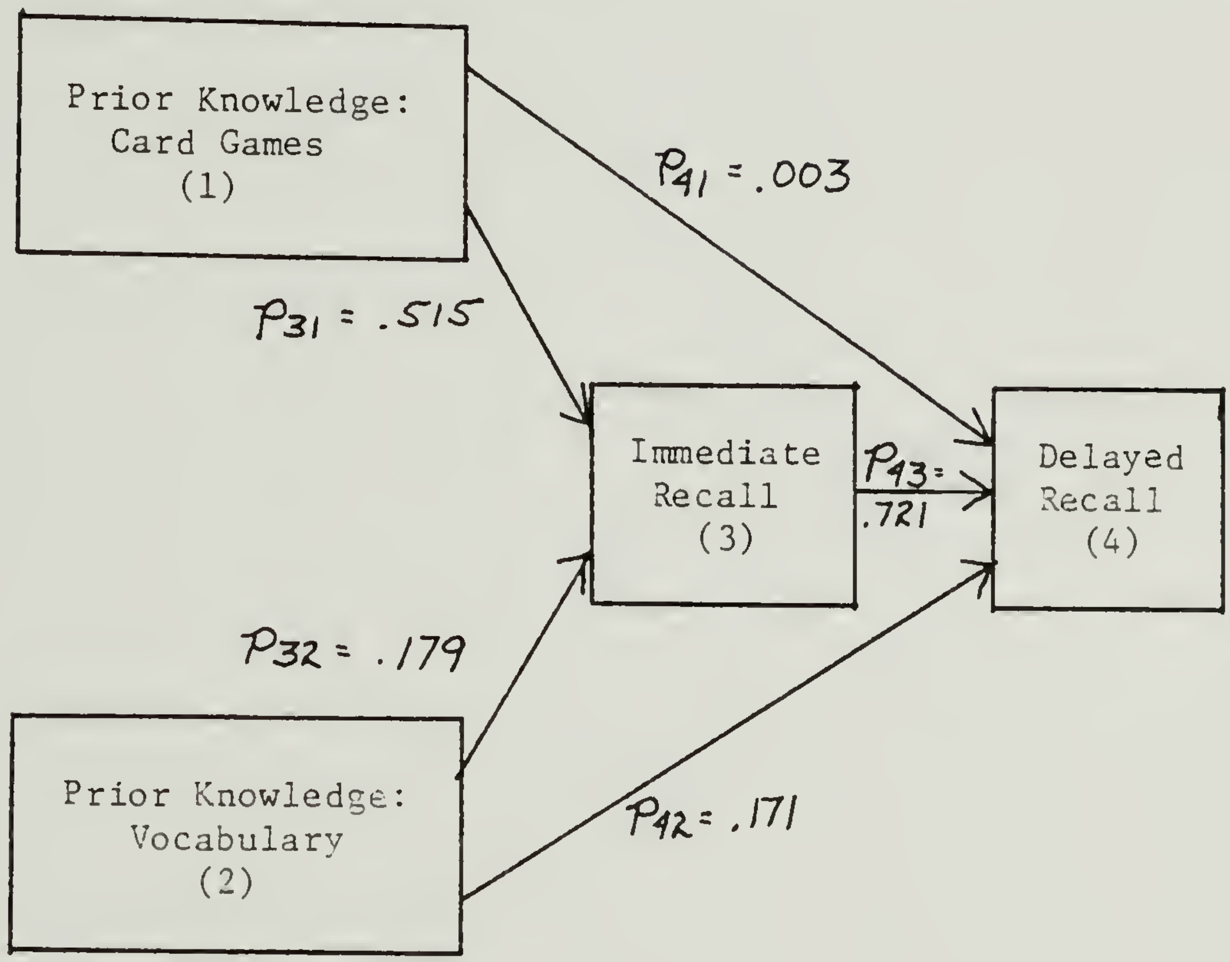

Figure 5. Path diagram with observed values of the path coefficients.

concerning both card games and vocabulary are associated with the amount of information recalled immediately after reading the passage. The relevant path coefficients, $\underline{p}_{31}$ and $\underline{p}_{32}$, are simply the corresponding correlation coefficients. The coefficient $\underline{p}_{31}$ is significant at the .001

	1	2	3	4
Card Games (1)	1	-.113	.515**	.355*
Vocabulary (2)		1	.179	.300*
Immediate Recall (3)			1	.753***
Delayed Recall (4)				1
$\bar{X}$	49.99	48.84	39.54	23.05
SD	8.74	8.42	10.07	11.23

* $p < .05$

** $p < .01$

*** $p < .001$

Table 6. Correlation matrix including means and standard deviations.

level; however, p_{32} is not significantly different from zero.[2] This suggests that, in this particular case, prior knowledge of card games enhanced immediate recall performance, while vocabulary was not related. An alternative explanation is that those with more experience with card games were more interested in the passage and spent more time studying it. According to this interpretation, study time, and not prior knowledge of card games, is the critical variable in determining immediate recall. The correlation between study time and immediate recall is not significant ($r = -.239$); neither is the correlation between vocabulary score and study time ($r = -.183$). However, prior knowledge of card games is negatively related to study time ($r(35) = -.330$, $p < .05$) such that those with the most prior knowledge spent less time studying the passage. This argues against the hypothesis that study time, rather than prior knowledge, is the important causal variable vis-a-vis immediate recall. Indeed, when prior knowledge is controlled for, the correlation between study time and immediate recall is very near zero (-.016).

The remaining path coefficients were obtained by regressing variable 4 on variables 1, 2 and 3. The coefficient p_{43} (.721) is significant, $F(1,35) = 12.63$, $p < .001$. By squaring the path coefficient, it is seen that approximately 52% of the variance in delayed

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[2]Since all path coefficients, according to either the model based on the knowledge-structure hypothesis or the Underwood findings, were predicted to be either positive or zero, one-tailed tests of significance were employed.

recall is accounted for by immediate recall. However, as argued in the introduction, the paths which are critical to the rejection of one of the two proposed models are those linking variable 4 with 1 and 2. Neither of these path coefficients is significantly different from zero.

These data suggest that the two paths directly relating prior knowledge to delayed recall are not necessary and that, therefore, the knowledge-structure hypothesis represented by Model 1 can be rejected. The 1-3-4 triad is particularly nonsupportive of the knowledge-structure hypothesis: Those with the greater prior knowledge learned more information in a shorter time, yet, as indicated by the near zero value for $\underline{p}_{41}$, had no retention advantage over a 2-week interval.

The 2-3-4 triad is less helpful in terms of providing data relevant to the two competing hypotheses, since vocabulary score seems not to have resulted in initial differences in amount recalled. A portion of the data collected in Experiment 1 provides a replication of this triad. Fourteen[3] male college students in Experiment 1 studied the card-game passage and recalled it both immediately and after a 2-week interval under nearly an identical procedure.

The means and standard deviations in Experiment 1 for vocabulary (51.43; 9.03), immediate recall (37.96; 9.60) and delayed recall (23.43; 12.52) are very similar to the corresponding values reported in

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[3]One of the subjects who had been randomly dropped to equate cell size was included in this re-analysis.

Table 6 ($\underline{t}(49) < 1.0$ for all three corresponding means). The pertinent data from these 14 subjects were added to the data from the 37 in Experiment 2, and the paths linking variables 2 with 3 and 4 were calculated. The path diagram involving these three variables is shown in Figure 6.

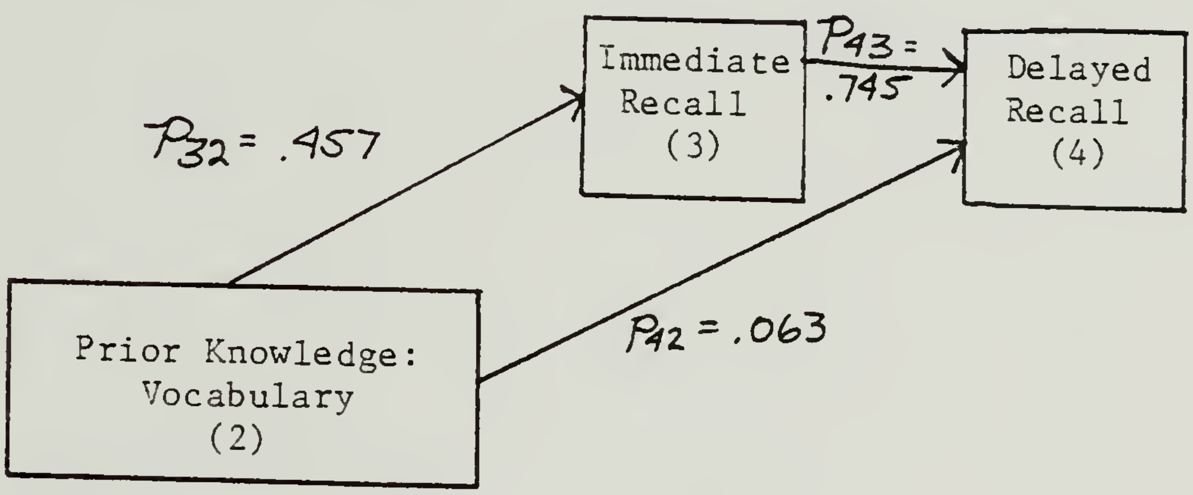

Figure 6. Path diagram of triad 2-3-4 adding subjects from Experiment 1.

The values of the path coefficients are in close agreement with the corresponding values linking card-game prior knowledge to immediate and delayed recall, $\underline{p}_{32}$ being significant at the .001 level and $\underline{p}_{42}$ near zero. These findings further argue for the rejection of the knowledge-structure hypothesis represented in Model 1, and are in agreement with the hypothesis represented in Model 2.

CHAPTER VIII

GENERAL DISCUSSION

The problem of how to collect and interpret data concerning the relations among prior knowledge, initial recall performance and retention when initial performance levels are unequal has by no means been solved. The two studies reported here each employed a different approach in an attempt to determine whether retention of prose materials is related only to amount originally learned or, in addition, to prior knowledge relevant to the materials. The results of Experiment 1, which employed an analysis of variance design, were not interpretable. Both low levels of initial performance for the vocational students and an attempt to predict interactions in the absence of assumptions concerning the nature of the relation between amount learned and amount retained contributed to the problem of interpretation. If it can be determined whether amount retained is proportionally related to amount learned, the approach of interpreting interactions amoung recall interval, group-, and passage-types seems a promising method of investigating the problem. However, in order to determine the nature of the function between amount learned and amount retained, independent of prior knowledge effects, the relation between prior knowledge and amount retained must first be specified. Unless a way out of this circular problem is found, unambiguous interpretation of interactions will remain difficult.

The regression design employed in Experiment 2 provided, in this case, the most interpretable information. The results of that experiment support the suggestion of Royer et al. (1978) that those who possess prior knowledge relevant to a given task learn the task quicker than those with less relevant knowledge: Those with more experience with card games learned more information about a new card game, in approximately the same amount of time, than those with less experience. However, the hypothesis advanced by Royer et al. that those with the more appropriate prior knowledge will retain information better was not supported. The path coefficients between prior knowledge and delayed recall were essentially zero. This finding is in basic agreement with Underwood's (1954, 1964) conclusion that level of initial learning is the primary determinant of retention.

Contrary findings in this area of research have often been dealt with by pointing to methodological problems in the conflicting studies. It would be relatively easy, taking the point of view that amount retained is related only to amount learned, to account for the Royer et al. findings by arguing and citing evidence, post hoc, that the levels of performance of the ability groups in the second study were not equal at the onset of the retention interval. Conversely, the results of Experiment 2 in this study could be questioned on the grounds that the groups were statistically equated and that the same results may not have been obtained had initial performance been equated empirically. While this type of critiquing can be challenging and often helpful, it is questionable whether, in this case, the exercise would help resolve the question at issue.

More helpful at this point would be efforts to clearly define and justify the question being asked. What is the primary interest in answering the question concerning the relation between learning and retention? Is it primarily of theoretical interest? If so, the underlying theoretical positions must be more precisely stated. For example, to find no relation between prior knowledge and retention does not constitute a challenge to Constructive Theory as described by Royer et al. (1978). The fact that stimulus context and prior knowledge have been shown, both in this and previous studies, to be related to learning, supports the theory's basic premise concerning the nature of the memory trace. What these findings do call into question is the hypothesis tenatively advanced by Royer et al. that retention is enhanced when information is integrated into a well established knowledge structure since more retrieval paths are available. It seems just as reasonable to propose that new information is harder to later disassociate from prior knowledge it has been integrated with, and that this effect diminishes any advantage associated with the multiplicity of retrieval paths. At this point, the learning/ retention question has not been demonstrated to be critical to the solution of any ongoing theoretical debate concerning memory.

Theoretical issues aside, Royer et al. mention two educational issues to which an understanding of the learning/retention relation are potentially relevant: (a) the assumption implicit in objectives-based instruction that in bringing all students to a similar level of mastery, present and future performance has also been equated, and (b) efforts to discover the nature of the deficits and, thus, possible remedies in the case of the "educationally disadvantaged." However, before further research is conducted with the intention of providing information relevant to these or other practical issues, some thought must be given to the type of findings that could be validly generalizable to the question(s) of interest. Both Bronfenbrenner (1976) and Snow (1974) have recently made the point that much of educational research is conducted in settings, with materials, and over time intervals that severely restrict generalizability. So, while it could be theoretically important to show that, over a 2-week interval, a fast-learning or knowledge-relevant group better retained a paired-associate list or a passage about a card game, it may be of little importance from an educational perspective. Rather, it would be important to demonstrate the effect with materials similar to those found in the classroom, using operational definitions of learning and retention which are more educational relevant -- to compare performance at Time 1 on some knowledge domain to performance at Time 2, where very little or no instruction intervened and where the interval consisted of months rather than days or weeks.

While very small retention differences between groups can be important from a theoretical perspective, small differences with complex materials over long retention intervals could be meaningless from an educational perspective. It is hard to specify here the size of an effect that would be important, but the percent of variance accounted for will be much more important a gauge than statistical significance, particularly when large samples are employed.

The nature of the task demands need to be considered, not only in the case of educationally relevant findings, but also if theoretical interpretations are to be made. Would it be important, for example, to demonstrate that subjects instructed to read a passage so as to comprehend it were able to recall more of the passage 2 months later than those instructed to memorize it? Moreover, if retention differences are demonstrated with a given task, is it due to the fact that some individuals adopted a nonfunctional learning approach (such as trying to memorize a long passage), or because they were lacking related prior knowledge?

It is obvious that given a practical orientation, interpretable research becomes even more difficult to design, going beyond the methodological problem of demonstrating equivalency of initial performance. But if we are to argue that the results of our research could have significant educational implications, these are difficulties which we must be prepared to engage.

The path analytic design employed in Experiment 2 seems a promising approach to the study of the learning/retention relation in educational settings. Accepting the impossibility of equating initial performance between individuals or groups in more ecological settings, this method can be used to study the relation between two or more performance measures, including relevant variables such as prior achievement, aptitude or other measures of prior knowledge. In fact, a search would likely turn up a considerable body of extant data which could be analyzed in this fashion and provide insight into the question of whether performance at Time n is related only to performance at Time n-1, where no relevant instruction has intervened or, in addition, to certain types of prior knowledge.

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APPENDIX A: IN SEARCH FOR THE RELATION BETWEEN SPEED OF LEARNING AND RETENTION: A REVIEW OF THE LITERATURE

IN SEARCH FOR THE RELATION BETWEEN SPEED OF LEARNING AND RETENTION: A REVIEW OF THE LITERATURE

The question seems straightforward and simple: Who are the better retainers? Are they those who learn information quickly, or those who require a longer time in learning? Early psychologists were not the first to offer an answer to the question. "Easy come, easy go" had long been used as an admonition to students to proceed in their studies with diligence, not hurring themselves lest information have insufficient time to properly distill in the brain. Based on early experimental findings of Ebbinghaus and others who followed, educators were soon propounding just the opposite -- "a lesson that is learned quickly because it is clearly understood is better retained than one which is imperfectly understood and therefore slowly learned" (Woodworth, 1921, p. 353). It has since been argued (e.g., Underwood, 1954) that there is no difference between the retentive abilities of fast and slow learners. The answer to the question has come full circle and still, according to some, is yet unsettled.

The difficulties in providing a definite answer center around the methodological issues involved. An early issue, which will be dealt with in the first section of this paper, was the establishment of an appropriate measure of retention. But the problem which has been most responsible for the frustration of investigatory attempts is that of achieving and demonstrating equivalent levels of learning for fast and slow learners. In the second section of this paper the various methodologies which have beem employed in an attempt to equate learning will be described and the relevant literature associated with each methodology will be reviewed. The third section will be concerned with theories that have been offered in explaining individual differences in learning. In the final section the attempt will be made to relate the findings in this area to educational theory and practice.

Measures of Retention

After reviewing the relevant literature, Lyon (1916) concluded that while findings had, for the most part, suggested a retention advanage for fast learners, a final answer could not be given since different methods of measuring retention had led to contradictory results. A distinction was to be made, according to Lyon, between ability to retain and ability to reproduce. He suggested that a measure involving units recalled fails to account for the associative strength of items below threshold and, therefore, is most appropriately referred to as a measure of reproductiveness. This deficiency is avoided with the use of relearning, the notion being that all existing associate strength of a list of paired associates or a segment of prose will be demonstrated by a time savings in relearning. Relearning, being measured in units of time, had the additional advantage, according to Lyon, of providing a more reliable measure than units recalled. Particularly with prose materials, measures of recall often involved the use of elaborate and rather subjective criteria in making judgements as to the correctness of those recalled units which approximated rather than duplicated the original response. Lyon, therefore, favored the use of relearning, arguing that it provided both additional information and a more reliable measure of retention. It should also be noted that more recent research using relearning as a measure of memory (e.g., Nelson, 1978) has concluded that it is by far the most sensitive measurement technique.

Northwithstanding Lyon's conclusions, the use of recall in this particular area of research gradually superseded measures of relearning. The growing preference for units recalled may have been initially due to the ability to detect retention differences measured over short intervals. But a more compelling reason was presented by Gates (1918) and later again by Gillette (1936): A subject who is a fast learner is likely to relearn more rapidly than a slow learner just because he is a fast learner and not necessarily because of any retention differences. While Lyon did not mention this as one of the disadvantages of relearning, it is likely that other researchers had already perceived the problem and had consequently adopted units recalled as an alternative.

Equating Learning

When asking whether fast and slow learners differ in retentive abilities, it would be ideal to start out with the two learners equated on original learning of the material. In her review of the literature, Gillette (1936) made the observation that two factors were involved in the measurement of learning: (a) time to learn the material, and

(b) amount of material learned. In their attempts to equate learning, most experimenters have held one or the other of these constant while allowing the other, of necessity, to vary. Thus, two experimental paradigms have typically been used in investigating the question of retention differences.

The method of equal amount learned. According to this method fast- and slow-learning subjects are run to a common performance criterion. So, for example, if the experimental task involved the initial acquisition of a ten-item paired-associate list, both the fast- and slow-learning subjects would practice the list until a common criterion, such as one perfect recitation of the list, had been reached. The basic problem with this procedure is that differential overlearning between the two groups is almost certain to occur. By definition, the fast learners are going to reach the common criterion faster than the slow learners. As a result, the slow-learning group will be more practiced on certain items in the list than the fast-learning group. As Gillette pointed out, this situation could produce a retention advantage for the slow-learning group.

The majority of studies reviewed by Lyon (1916) employed the method of equal amount learned and used time to relearn as the measure of retention. These included studies by Müller and Schuman (1887), Whitehead (1896), Ogden (1904), Busemann (1911), and Pyle (1911). Müller and Schuman (1887) found that fast learners forgot more but were able to relearn the material in less time than the slow learners. This is not surprising given the liklihood of overlearning for slow learners and the bias in favor of fast learners brought about through the use of relearning scores. Based on their studies, both Ogden (1904) and Busemann (1911) concluded that fast learners retain proportionally more than slow learners. While Pyle (1911) obtained similar results, his conclusion was stated in a more conservative fashion, suggesting that fast learners were at no disadvantage in retention. Whitehead (1896) obtained results which, according to his analysis, demonstrated that slow learners both relearn in a shorter period of time and retain a larger amount. It was pointed out by Pyle (1911), however, that if one subject's score were eliminated due to the fact that the subject showed a longer relearning than initial learning time, the data would support the opposite conclusion. In summing up the findings of previous studies, Lyon noted that most had obtained results which suggested a retention advantage for fast learners.

Lyon went on to report the results of his own research which was primarily concerned with comparing three methods of measuring retention: recall, recall after one additional presentation, and relearning. Subjects were tested on five different types of material which varied in meaningfulness. Subjects were instructed, for example, to study a list of words until they were certain that they could repeat them without error. Study time was used as an indicator of fast and slow learning. After a retention interval of 1 week subjects were tested, in immediate succession, according to the procedure of each retention measure. In relearning the material, subjects were again allowed as much time as they felt necessary to master the material. Basing his conclusions primarily on results obtained from the relearning scores, Lyon posited a retenion advantage for fast learners in the case of meaningful material, that advantage shifting in favor of slow learners for non-meaningful material. Not only are Lyon's results hard to interpret because of weaknesses inherent in the basic methodology (as already discussed), but additional complications arise when subjects are allowed to study the material as long as they desire. Compulsiveness becomes a possible factor in determing study time for both initial learning and relearning; and it becomes impossible to determine, for example, if a slow-learning time is reflective of a slow learner or a compulsive fast learner.

As part of her attempt to provide an answer to the speed-of learning/retention question, Gillette (1936) conducted an experiment according to the method of equal amount learned. Subjects were tested for memory of paired associates over an interval of 5 days. Both recall and trials to relearn were used as measures of retention. Gillette found that, in spite of the fact that slow learners had probably overlearned some of the original items, fast learners retained a larger percentage and relearned the material more quickly.

Gregory and Bunch (1959) conducted a later study using the method of equal amount learned. Their subjects were tested after 24 hours for retention of ten paired associates learned to the criterion of one perfect trial. A control group was included in which subjects were tested for immediate recall following the criterion trial. The investigators were interested in whether or not the additional exposure of items to slow-learning subjects, which necessarily resulted from the experimental procedure used, was predictive of superior recall when compared to fast-learning subjects. In comparing overall retention it was found that differences in favor of fast learners for both the 24-hour and immediate-recall groups were not significant. The authors suggested that the fact that slow-learning subjects required more trials to learn the material while demonstrating equal or inferior retention when compared to fast learners, lends no support to the hypothesis that slow learners should have a retention advantage because of additional practice during learning.

The average number of correct anticipations for each item during learning for both fast and slow learners was then compared to the average number of correct anticipations for each item during recall. The former was not predictive of the latter in the case of the 24-hour group. For instance, for a particular item, slow learners might have acrued, in comparison to fast learners, three times the number of correct anticipations in reaching criterion and still have fewer correct anticipations in the delayed retention task. Conversely, slow learners might have demonstrated superior recall for an item that had only been correctly anticipated twice as many times as the fast-learning group. These confusing results were interpreted as offering no support for "the assertion that the method of learning to the same criterion of mastery, as applied to the problem as a whole, results in greater overlearning on the part of the slow learner..."(p.181).

The Gregory and Bunch study actually provides little additional information vis-a-vis the overlearning hypothesis. The fact that slow learners retain no better than fast learners when brought to a common criterion was observed by, and constituted the findings of, earlier research; the fact that it requires more reinforcements for slow learners to acquire the same associative strength as a fast learner for any particular item had been demonstrated by Underwood (1954). As strong a case that slow learners do not overlearn items relative to fast learners can be deduced from the Underwood study. If the Gregory and Bunch (1959) study is considered independently of the findings of Underwood, their conclusion is invalid. To suggest that a demonstration of equal or inferior recall of a particular item by slow learners is evidence that those items were not overlearned is to accept that there are no retention differences between fast and slow learners. If fast learners are better retainers, an item could be overlearned by slow learners and still recalled less frequently in a delayed retention task. Gregory and Bunch, in fact, report a "slight, but consistent, superiority in retention" for fast learners (p. 181).

An additional weakness inherent in the method of equal amount has been pointed out by Underwood (1964). Since fast and slow learners approach criterion performance at different rates, failure to account for the learning associated with the last trial (which is used to infer equivalent levels of learning) results in a slightly higher level of learning for the fast learners. The greater the differences between groups in learning rates the greater are the differences in the amount of learning associated with the last trial.

Another possible bias involved with learning to a common criterion was suggested by Stroud and Schoer (1959). If, with a given set of materials, slow learners require a reinforcement ratio of 5 to 1 in order to acquire an item at the same associative strength as a fast learner, there will be items which will not have been reinforced at that ratio -- items which have only been correctly anticipated two or three times before criterion is reached. Stroud and Schoer offered some empirical evidence which suggests that this situation leads to poorer recall for those items by slow learners. This bias in conjunction with the inequality brought about through differential learning rates as suggested by Underwood (1964), could account for significant retention differences observed between fast and slow learners in studies employing the method of equal amount.

If it had been demonstrated that the method of equal amount was biased in favor of the slow learners via overlearning of some of the associated pairs, then the conclusion that fast learners retain better than slow learners would be justifiable given the results of the studies reviewed here. Due to the fact that the overlearning hypothesis has received no support, and that biases in favor of fast learners seem highly probable, a conclusion based on these studies is not possible.

The method of equal opportunity to learn. The method of equal opportunity involves holding the number of learning trials constant for both the fast- and slow-learning groups. Again, if a ten-item paired-associate list is the to-be-learned material, both groups are given the same amount of time (e.g., five learning trials) in which to learn the list. While this procedure guards against the overlearning of materials by slow-learning subjects, it results in different levels of acquisition. By definition, the fast learners will have learned more items in a given amount of time than slow learners. This situation also leads to difficulties in interpretation. If, for example, the fast-learning group recalls a mean of six out of ten originally learned items, and the slow-learning group recalls a mean of four out of six items, on what basis is a decision made as to which group retained the most? If we compare absolute number recalled the conclusion would be that the fast-learning group demonstrated better retention. We would arrive at the opposite conclusion, however, if we looked at the proportion of items recalled. Both interpretations are confounded with the fact that the fast learners had acquired more items to begin with. If we look instead at items forgotten, either absolute or proportional, interpretation is still unclear. Is it a demonstration of better retention to forget two items out of six originally learned, or to forget four items of ten originally learned? The fact that retention is being measured from different original learning levels makes it difficult to defend any data-based conclusion.

The Thorndike (1908) and Norsworthy (1912) studies were included by Gillette under the rubric of equal opportunity to learn. In actuality, it is difficult to assign them to either paradigm thus far discussed; but since both studies made an attempt to control learning time they are reported here.

Thorndike found 22 subjects who were willing to learn the English equivalents of 1,200 German words according to a distributed practice method. The original procedure allowed approximately 60 minutes for the study of 100 German-English paired associates. Immediately following this study period, subjects were to test themselves on their ability to write the English equivalent given the German stimulus word. A list consisting of German words only was provided for this purpose. This procedure was to continue until all 1,200 words had been studied through a total of five times, over a period of several weeks. During the course of the experiment the procedure was modified so that subjects could study anywhere from 100 to 600 words during the hour and could terminate the study phase of the experiment when they felt confident that they knew most of the 1,200 words.

A rank difference correlation of .40 was calculated between performance on the first round of study (once through the 1,200 words), and a performance measure based on scores obtained after retention intervals of approximately (a) 24 hours and (b) 30 days. Inspection of the data summaries reveal, however, that values of the independent variables varied considerably across subjects. For example, total learning time ranged from 10 to 47 hours; the interval between the first and second round of study varied between 4 and 14 days; the length of the "24-hour" retention interval ranged between zero and 216 hours; and the 30 day interval from 28 to 55 days. Thus, while Thorndike managed to conduct a comparatively naturalistic experiment, allowing subjects to study other than in the carefully constructed atmosphere of an experimental laboratory, the varying conditions make the results impossible to interpret even before considering the basic methodological problems associated with the paradigm he employed.

Using the vocabulary material prepared by Thorndike, Norsworthy (1912) tested 83 students enrolled in an educational psychology class according to a slightly different procedure. Subjects studied at least 40 words a day for 20 minutes on 5 consecutive days. Each study session was followed by the self-administered test used by Thorndike.

This procedure was repeated a total of three times with a 2- day rest period separating each 5 days of study. After 19 days, each subject had studied a minimum of 200 words, three times through. A test was administered on the first class meeting following the completion of the study periods. Subjects were asked to write the English equivalents of 50 German words which had been chosen from among the 200 words. Four weeks later subjects were again tested in the same manner. Correlation coefficients were calculated between (a), total words learned and (b) average number correct on the first test (r = .41), between (a) and (c) average number correct on the second test (r = .50), and between (b) and (c) (r = .60).

A cursory reading of the study would lead one to conclude, as Norsworthy did, that time remains constant while the amount learned varies. This would be true if subjects received test items sampled from among the total population of the words they had learned. But all subjects were tested on items chosen from among the first 200 words. This means that those who learned fewer words had spent more time in studying the 200 words from which the test sampled. This procedure is little different from the method of equal amount learned except that fast learners go on to learn additional words with the time they save. The design does not, therefore, control for the possible overlearning bias in favor of the slow-learning subjects. Norsworthy's procedure also results in measuring subjects over unequal retention intervals. For example, a subject who studied 700 words the last week of the study period would have completed the review of the 200 words from which the test words were taken early in the week, while a subject who studied only 300 words would have finished the review of the 200 words later in the week. Thus the first retention test might be measuring a retention interval of 6 days for a fast learner and only 2 days for the slow learner. All of the problems mentioned would seem to produce bias in favor of the slow learners while the results, again, indicate a retention advantage for fast learners. This was part of the rationale used by Norsworthy in concluding that fast learners retain better.

As part of a later study, Thorndike (1910) calculated a correlation of .55 between immediate and delayed recall of single syllable words. Subjects were read 5 lists of 12 words each at a presentation rate of approximately one word per second. Subjects wrote down as many of the 12 words as they could after a single hearing, proceeding then to the next list until they had heard a total of 60 words. Delayed recall was measured over a 24-hour retention interval. Thorndike estimated the true correlation to be .80 after allowing for attenuation due to mixture of the sexes and other inaccuracies. In comparison to his previous study, this was a fairly well controlled experiment, suffering only from the limitations of the method of equal opportunity. That is, the fact that subjects who learned more words also recalled a greater number of words does not answer the question of whether or not they are better retainers relative to what they originally learned.

Additional studies reviewed by Gillette (1936) as examples of the use of the method of equal opportunity included the reports of Gates (1918), Gordon (1925) and Peterson (1925).

Gates (1918), after providing a brief review of previous research in the area, concluded that the control of learning time was the preferable methodological approach. His experiment involved 299 students ranging from grades three through eight. Stimulus materials were studied 9 minutes for either 5 (in the case of non-sense syllables) or 6 (for biographical paragraphs) consecutive days. Gates reported a slightly higher correlation for meaningful material and a smaller, but still significantly positive, correlation when the datawere converted to percentage-of-retention scores (the ratio of amount learned to amount retained). Stating his conclusion in the null hypothesis form, Gates suggested that the data offered no support to the rapid learning, rapid forgetting notion.

Gordon (1925) designed an experiment to test the relative merits of spaced and unspaced memorizing. Subjects were read the Athenian Oath either three or six times and with or without lengthy intervals between readings. Coefficients of correlation were calculated between immediate and delayed (3 or 4 weeks) recall, with r = .42 and .52 in the case of unspaced presentation and r = .70 and .71 for spaced presentation. While the question of speed of learning and amount retained was not of primary concern, the positive correlations again are suggestive of a fast-learner retention advantage.

Peterson (1925) reported the results of two experiments both of which involved the learning of meaningful materials. In the first experiment subjects studied a selection consisting of 250 words for 2½ minutes. Subjects were asked to reproduce the selection immediately following the reading and again a week later. The second experiment involved the learning of a lengthier selection by the same subjects. On this occasion, however, subjects were instructed to determine their own learning time, with the knowledge that the amount of time taken would, in some way, affect their final score. Scores were then converted to amount learned and amount retained per minute reading time. While this procedure resembles the method of equal opportunity, it introduces the factor of compulsivity -- when subjects are allowed to establish their own learning time it is difficult to determine the relative contributions of being a fast or slow learner, per se, and being a compulsive or non-compulsive learner. However, the fact that those who scored highest on immediate recall also had the lowest study times argues against the compulsivity interpretation.

Results from both experiments were analyzed in the same fashion. Subjects were divided into quartiles according to performance on the immediate-recall task. Comparison across quartiles revealed no differences in average percentage retained. In fact, there was a trend in favor of the lower-ranked subjects. These results are not consistent with previous findings, particularly with those of Gates (1918). However, Peterson concluded that "inasmuch as the quartile which learned the most retained about two and one-half times as much as the quartile which learned the least, the advantage is clearly and decidely with the more rapid learners" (p. 248). It would seem from Peterson's statement that <u>efficiency</u> of learning was the important question to answer -- who is best off in the long run in terms of total amount retained, the fast or slow learner? Peterson is one of the few investigators who have raised the efficiency question. Since rapid learners are able to learn and recall more given the same amount of study time, it seems clear that they are the more efficient learners.

As in the case of the method of equal amount, Gillette (1936) conducted an experiment in which learning time was controlled. Fourth through sixth grade children were presented 5 types of paired associates, 20 pairs of each type. Four seconds were allowed for each pair, and subjects were tested for immediate memory following the repeated presentation (either three or four times) of each 20 -pair group. Delayed memory was tested 48 hours later. Analysis of the data revealed (a) a positive correlation between amount learned and amount retained, suggesting a retention advantage for fast learners; (b) a positive correlation between amount learned and absolute amount lost after 48 hours, suggesting that fast learners lose more than slow learners; and (c) very little or no difference between fast and slow learners when comparing proportion lost after 48 hours.

Gillette concluded that a definitive statement could not be made based on the results of experiments employing the method of equal opportunity or equal amount. While results had, for the most part, suggested a retention advantage for fast learners, in spite of a bias in favor of the slow learners in the case of the method of equal amount, both methods were inappropriate, producing bias which distorted any true differences that may have existed.

The method of adjusted learning. The method of adjusted learning was adapted by Gillette from a procedure used by Woodworth (1914) in a within-subject design. Woodworth was interested in the question of whether associations learned quickly by a subject were better retained than those that were learned more slowly. Accordingly, he presented to subjects a list of 20 Italian-English paired associates. Subjects were tested immediately after each presentation of the list, the alternating learning and study trials continuing until each correct response had been given once. Overlearning was avoided by dropping an item from the list as soon as it was correctly responded to. Woodworth's results, which showed better retention for those items learned early in the study trials, can be accounted for not only in terms of item difficulty, but also as an artifact of short-term memory. As the list in the last study trial becomes smaller, say two or three items, performance on the subsequent test trial is likely to involve the retrieval of the appropriate response from short-term memory. It can be seen that associations thus recalled will make up a larger proportion of items learned in later study trials (summing over subjects); since items so learned are less likely to be recalled in a delayed-memory task, the overall effect will be to reduce the percentage recall of slowly learned associations.

Adapting Woodworth's procedure to a between-subject design, Gillette's modification consisted of having subjects learn a given number of items rather than the entire list. Using a list of 20 paired associates, subjects proceeded through study and test trials until correct responses had been given for approximately ten items. (Correct recall for exactly ten items was impossible since an indeterminable number of correct responses would occur after each study trial). Since items were responded to correctly by both fast and slow learners an equal number of times (once), and equivalent levels of performance were achieved (ten items for both groups), Gillette argued that the method of adjusted learning overcame the difficulties associated with previous methodologies -- uniquivalent levels of performance and possible overlearning on the part of slow-learning subjects. Based on her results, Gillette concluded that fast learners were the better retainers: They retained a greater number of items, a greater proportion of items and required fewer trials to relearn.

Gillette's conclusions were generally accepted and her methodology was not questioned until Underwood (1954) pointed out that it too suffered from a faulty assumption -- that a correct response, or reinforcement, results in equivalent associative strengths for fast and slow learners. If this assumption is incorrect then the criticism levied against the method of equal opportunity, that it results in unequivalent levels of learning between groups, can likewise be applied to the method of adjusted learning. Underwood went on to demonstrate that, in fact, plotting the probability of a correct anticipation on the next trial given the number of previous reinforcements, resulted in different curves for fast and slow learners. Thus, controlling for the number of correct responses does not guarantee equivalent learning levels. When differences are then measured over a retention interval it is not known whether the observed differences are due to differential rates of forgetting for fast and slow learners, to unequivalent levels of original learning, or to both. This possible confounding is not only a concern with studies involving fast and slow learners but, as Underwood (1964) pointed out, is of concern when the effect of any independent variable on retention is under investigation. If level of learning is not equated before the onset of the retention interval, no clear statement can be made regarding the effect of an independent variable on retention, since retention differences will also be a function of level of learning.

Later Methods of Equating Learning

In trying to equate associative strengths, Underwood (1954, 1964) encountered an additional problem. It was inappropriate to establish as the base measure of learning the number of items correctly responded to by a subject on the last anticipation or test trial. Such a measure does not include the additional learning which occurred on the last trial. When two subjects who are learning to a common criterion approach the last trial at different rates, it cannot be assumed, on the basis of equal performance on the preceding trial, that their performance on an additional trial would be equal. Underwood (1964) argued that, while the mean score of a control group which had been run for the extra trial could be used to estimate immediate recall for the experimental group, alternative techniques which avoided the need for extra time and subjects were preferable. These he called the single- and multiple-entry projection techniques.

Single-entry projection technique. The single-entry projection technique is appropriately employed when subjects are being run for a constant number of trials. Suppose that the experimental task involves the presentation of a paired-associate list over five trials. Data for all subjects is pooled and items which have been correctly anticipated an equal number of times in the five trials are considered together. A growth curve is constructed using the percentage correct on the last trial of items correctly anticipated zero through four times on previous trials. For example, suppose of four items correctly anticipated by subjects four times, three were correct on the fifth trial. The probability of an item being correct on the fifth trial, having previously been correct on four previous trials would equal .75. This same procedure is used to calculate the percent correct on the fifth trial of items correctly anticipated zero through three times. The curve thus constructed is then extrapolated to predict the percentage correct on the imaginary sixth trial of items correctly anticipated on five previous occasions. Assuming that the percentage of all other categories (zero through five) are the same for the sixth trial, the curve is then used to predict, for each category, performance on the sixth trial. Not only can this technique be used to estimate performance of a group, but each individual subject on a subsequent trial. Underwood (1964) tested the accuracy of this technique and found that while it tended to slightly overestimate the score actual obtained on a subsequent trial, the bias was of little consequence since it affected both groups equally. Thus the difference between retention scores of two groups is unaffected by this bias.

Underwood suggested that this method could be adapted to the problem of comparing the retention of fast and slow learners by running a pilot study from which the number of trials necessary to equate the two groups would be determined. Thus it might be established that, for a given set of materials, slow learners require five trials to arrive at the same level of learning achieved by the fast learners after two trials. Each experimental group would then be run through the appropriate number of trials and retention scores compared after a given interval.

Multiple-entry projection technique. The multiple-entry projection technique is employed when subjects are learning material to a common criterion, such as one perfect trial. Again, all the data is pooled. The history of each item for each subject is tabulated. Once an item is correctly anticipated, performance on the next trial is noted. A record is similarly made of performance on the following trial of items correctly anticipated twice, three times, and so on. This procedure results in multiple entries since an item correctly recalled three times will also be figured in the analysis of items correctly recalled once and twice. The growth curve thus constructed will give the probability of a correct response on the next trial for items that have been correctly anticipated x number of times on previous trials. The probability of an item being correctly anticipated on the additional, imaginary trial may then be obtained from the smoothed curve. It is not difficult to see why the single-entry technique is inappropriate when subjects are run to a common criterion. If all the items are correct on the last trial (which is the case when subjects are run to the criterion of one perfect trial), the single-entry technique would predict an expected probability of 1.0 for all items on an additional trial, not taking into account the number of previous occasions on which each item was correctly given.

Underwood (1964) tested the accuracy of the multiple-entry technique using three sets of data and found no particular bias involved in making predictions concerning group mean scores but found the method inappropriate for the prediction of individual scores. Since the technique involves the average probability of all subjects for each category of number correct, the prediction for fast learners tends to be too low while those for slow learners are too high.

To compare retention score for fast and slow learners using the multiple-entry technique separate growth curves are plotted for each group according to the above procedure and juxtaposed where probabilities are equivalent. Retention comparisons are then made between items of equal associative strength. Using this technique Underwood consistently found no difference between fast and slow learners in ability to retain paired-associate items over an interval of 24 hours.

<u>The method of differential rates of presentation</u>. Shuell and Keppel (1970) extended the technique of equating learning to free-recall tasks by empirically determining differential rates of word presentation for fast and slow learners. The method requires a pilot study from which the appropriate rates are determined. Shuell and Keppel presented 30 nouns at the rates of either 1, 2, or 5 seconds per word. From the performance on an immediate-recall test it was determined that, for that particular list, slow learners acquired approximately as many words at a presentation rate of 5 seconds as were acquired by fast learners at the rate of 1 second per word. This equating method, however, does not take into account the learning which results from the immediate-recall task. As Underwood has pointed out, failure to do so might possibly result in

unequivalent levels of learning at the onset of a retention interval. Therefore, a control group is included in which subjects are given two successive recall tests following the presentation of the list. Shuell and Keppel used the results from the control group to show that there existed no significant difference in words recalled between fast and slow learners after an additional recall test. (A more direct approach would be to run the pilot study according to the control-group procedure. Thus, the appropriate presentation rates for fast and slow learners would be determined by observing what rates lead to approximately equal recall on the second immediate-recall test. Groups would then be equated for learning up to and including the immediate-recall test. A control group would then not be required in the actual experiment.)

Shuell and Keppel reported the results of two experiments in which fast- and slow-learning fifth graders received one presentation of a 30-item list at the rate of 1 and 5 seconds per word respectively. Results for both experiments, which differed only in terms of the length of the retention interval, indicated no difference in retentive ability over intervals of 24 and 48 hours. In interpreting the results, the authors suggested that there might exist individual differences in short-term memory which result in different acquisition rates but apparently no individual differences in long-term memory. Thus, once the short-term memory deficit is compensated for by allowing extra time for the slow-learning subject, future recall performance is comparable with the fast-learning subject.

If we accept the assumption that differences in short-term memory is the primary factor (or a factor) in determining rate of acquisition, the findings of Shuell and Keppel can be interpreted to suggest differences in long-term memory as well. Their experimental procedure does not insure that items are not being recalled from short-term memory. Items near the end of a free-recall list will very likely be recalled from short-term memory in an immediate-recall test if a thought-diverting task is not interposed (cf., Glanzer & Cunitz, 1966). If there were no consistent differences between groups in short-term memory ability, failure to insure recall from long-term memory would pose no difficulty in terms of the validity of the experiment. But this very difference is offered as a possible explanation of acquisition rate differences. If fast learners are able to hold more items in short-term memory than slow learners, then, in a free-recall task they would be expected to recall more items from short-term memory. The method used to equate the level of learning would equate the subjects for items both in long- and short-term memory. Since the fast learners would have been recalling more items from short-term memory, the slow learners would be expected to recall more items after a retention interval given no group differences in long-term retentive ability, because they would have had a greater number of items in long-term memory after studying the list. The fact that slow learners did not recall more items than fast learners is suggestive of either (a) a retention superiority for fast learners, since they recalled as many items as slow learners while having stored fewer in long-term memory, or (b) no difference between groups in ability to store in and recall items from short-term memory. In interpreting the Shuell and Keppel experiment, if we reject (b) we must accept (a).

Accounting for Individual Differences in Acquisition Rate

As is seen in the above discussion, investigators who have attempted to discover a relation between speed of learning and retention have found themselves trying also to account for individual differences in learning. Specifically, what is it that causes one individual to learn at a different rate than another? While the answer to this question might seem unrelated to the relation between those differences and retention, different hypothesis concerning the cause of learning-rate differences provide different interpretations of the retention data. Three possible explanations which have emerged from the literature dealing with the rate-of-learning-amount-retained problem are discussed below.

Differential susceptability to interference. According to one explanation, slow learners are more adversely affected by the interfering quality of intra-list items. The associative strength of an item which has been correctly responded to in a paired-associate task, for example, is the same for fast and slow learners at the moment of response occurence, but interference created by subsequent items differentially reduces the likelihood of a correct response on the next trial according to the retroactive inhibition model of forgetting. Thus, according to this explanation, the reason slow learners require more time to acquire an item at a given associative strength is because of their increased susceptibility to retroactive inhibition.

Stroud and Carter (1961) were first to investigate the possibility of differential interference effects. They reasoned that if slow learners are more susceptible to intralist interference then increasing the number of items on a paired-associate list should more adversely affect the slow learners in terms of trials-to-criterion.

Fast- and slow-learning subjects learned both a 12 and 24 item paired-adjective list to the criterion of two correct, but not necessarily consecutive, anticipations for each item. An item was dropped from the list on the occasion of the second correct response. Mean number of trials to learn was analyzed according to Ability (fast or slow learner) and Length of list. Means for the fast learners were 9.16 and 36.38 on the short and long list respectively; for the slow learners the corresponding values were 36.00 and 114.88. Stroud and Carter, on the basis of these values, concluded that slow learners were more susceptible to intralist interference. Indeed, it can easily be seen that doubling the list length results in a greater increase of trials-to-learn for the slow learners. However, if proportion of increase is used as the dependent measure the slow learners appear equally, if notless, affected by additional interference associated with the longer list (3.9 for the fast learners and 3.2 for the slow learners). Stroud and Carter's assumption that the effect should be additive is not justified and, in some respects, counter intuitive. The additive assumption predicts that there must be a constant number of additional trials for slow learners to reach criterion regardless of the number of additional items. An extreme example will serve to illustrate. Using the means reported by Stroud and Carter for the trials-to-learn on the short list, suppose we wanted to predict performance of slow learners when the length of the second list was increased so that it required 1000 trials for fast learners to reach criterion. According to the assumption of additivity, in order for there to be no Ability X Length interaction the slow learners would require 1026 learning trials to reach criterion. The prediction seems unlikely.

An additional dependent variable in the Stroud and Carter study was the number of occasions on which the first correct response was followed by a second correct response on the next trial. One would expect that an increase in interference would be reflected by a decrease in the frequency of this occurence. If slow learners are more vulnerable to interference it follows that an interaction should obtain between Ability and Length. The results, while demonstrating significant main effects, were not supportive of the differential interference hypothesis. If the data, reported in frequency fashion by Stroud and Carter, are converted to probability scores it becomes evident that the dependent measure was not reflective of an inhibitory effect for either group. The probability that the first correct response was followed by a correct response on the next trial for the fast learners was .76 on the short list and .81 on the long list. For the slow learners the corresponding values were .65 and .68. Thus, the interfering effects as evidenced by disproportional trials-to-learn for both groups (doubling the length of the list resulted in more than triple the number of trials-to-learn) did not result from a decrease in the probability of following the first correct response with another correct response on the next trial.

A third technique was employed in the study. Three warm-up items were learned to the criterion of five errorless trials just prior to the experiment proper. These pairs were then included with the to-be-learned material. The number of unsuccessful anticipations of warm-up items during the learning of the test items was assumed to reflect the degree to which the test items inhibited their recall. The results of the analysis suggested that the retroactive interference created in the above manner was greater for the slow-learning subjects. However, the procedure used to equate the learning of the warm-up items (the method of equal amount) is not likely to have resulted in equivalent levels of learning and, as has been previously explained, probably resulted in a higher level for the fast learners. It is impossible, therefore, to determine how much of the difference in correct anticipation of warm-up items between fast and slow learners was due to the different levels of learning and how much, if any, to differential effects of interference.

A follow-up study was conducted by Schoer (1962) which differed from the Stroud and Carter design in the following significant ways: (a) The entire list of words (7 or 14) was learned to the criterion of two consecutive perfect trials; (b) the probability of a correct response after one, two and three correct anticipations was calculated; (c) rather than using warm-up items to test for retroactive interference, an interpolated learning condition was employed in which subjects learned a similiar nine-item list just prior to the recall of the original list.

Results paralleled the findings of Stroud and Carter. Increasing the list length resulted in a greater absolute increase of trials-to-learn for the slow learners. But, again, the proportion of increase was near equal (2.4 and 2.2 times for fast and slow learners respectively). Schoer went on the question the use of the trials-to-learn criterion because of this ambiguity in interpretation.

The learning of the interpolated list resulted in a decrement in recall for both ability groups but did not affect one group more than the other. If an interaction had obtained it would have raised some difficult questions about investigator's findings of no retention differences between fast and slow learners. The interpolated task was designed to interfere with the recall of items stored in long-term memory. Differential effects upon fast and slow learners would have demonstrated differences in long-term memory susceptibility to interference as a function of aquisition rate, making it difficult to explain why, in general, fast and slow learners do not show retention differences.

Some support for differential interference of intralist items was obtained in the Schoer study with the finding of a significant interaction between length of list and ability level when comparing groups on probability of recall after two and three correct anticipations. Slow learners were more adversely affected by the increased list length. This finding represents the only compelling support of the interference hypothesis.

Individual differences in short-term memory. Not entirely independent of the differential interference hypothesis is the possibility that fast and slow learners differ in basic capacities associated with short-term memory performance. While this possibility had been suggested in research previously reviewed (Stroud & Schoer, 1959; Shuell & Keppel, 1970), direct support has only recently been offered (Hunt, Frost & Lunneborg, 1973; Hunt, Earl, Lunneborg & Lewis, 1975).

Hunt and his associates have demonstrated a positive relation between verbal ability (as defined by performance on the Washington Pre-College Test) and performance on several tasks associated with short-term memory ability. In summarizing their results, Hunt et al. (1975) describe three specific abilities as distinguishing characteristics of high and low verbals.

1. High verbals can more rapidly access highly overlearned information form long-term memory and thus more rapidly provide conceptual meaning for incoming stimulus information. This conclusion was most directly supported by an experiment conducted according to the Posner et al. (1969) paradigm in which subjects were asked to identify as either "same" or "different" two characters chosen from amoung the possible combinations of (AaBb), where a character could either appear with itself or any of the other three characters. In one condition the judgement was made according to physical similarity, in which case (Aa) would be different. In the other condition name identity was the appropriate criterion, in which case (Aa) would be the same. Hunt et al. found that the difference in reaction time between high and low verbals in the name-identity condition was greater than in the physical-identity condition, suggesting that low-verbal subjects require more time to retrive the name associated with a particular character.

2. High verbals are better able to maintain information concerning

the order of stimulus presentation in short-term memory. This conclusion was based on the results from two experiments, one of which employed a variation of the Peterson and Peterson (1959) paradigm. Subjects were shown four letters in sequence after which they were required to repeat a variable number of digits as they were presented to them. Performance on this task has been shown to be highly dependent on preservation of order information (Estes, 1972). High-verbal subjects proved superior in recalling the letters in correct order, making fewer interpositions of either presented or non-presented letters.

3. High verbals show a greater facility for manipulating information held in short-term memory. This was demonstrated by superior performance in two tasks. One was a variation of the Clark and Chase (1972) design in which subjects determined whether or not a picture was valid according to a previous assertion. The other design, the "Sunday + Tuesday Task", required the manipulation of data in both short- and long-term memory to arrive at arithmetic solutions.

Before the findings of Hunt and his associates can be applied to the particular problem being discussed in this paper, two questions must be addressed. The first, and obvious question, is whether or not we can safely compare high and low verbals with fast and slow learners. Several studies would seem to justify such a comparison (e.g., Ducanson, 1964; Stake, 1961; Stevenson & Odom, 1965; Stevenson, Hale, & Miller, 1968), reporting a relationship, usually from r = .30 to .60, between speed of learning and aptitude-test performance.

The second and more difficult question concerns the extent to which the specific abilities identified by Hunt and his associates apply to learning paired-associate or free-recall lists, since these are the materials from which the recent conclusions concerning rate of learning and retention have been drawn. The only ability that, without considerable interpolative effort, seems applicable is the ability to access long-term memory and provide a conceptual representation for incoming stimulus information -- each word or character must be translated from the visual code to the conceptual representation in memory. The other two abilities seem further removed from paired-associate and free-recall performance. How they might apply is a matter of conjecture in the absence of empirical data.

However, if we accept that differences in short-term memory ability is responsible, in part, for acquisition rate differences, then Underwood's technique of probability matching (or at least the specific procedure he used) could conceivably result in unequal levels of learning. In reaching his conclusion, Underwood must either assume (a) that subjects are always recalling items from long-term memory or (b) if some items are being recalled from short-term memory, there are no differences between fast and slow learners in short-term memory ability -- in the probability of recalling an item from short-term memory. Doubts about the validity of the second assumption are raised by the findings of Hunt and his associates. The assumption that all items are being recalled from long-term memory is not likely given the length of the lists used in Underwood's studies (from six to ten items) and the fact that no task was included to interfere with short-term memory recall of the most recently presented items. Given that both assumptions were invalid a probable result would be that fast learners would recall more items from short-term memory than slow learners. Equalizing learning based on this performance would put the fast learners at a retention disadvantage since the items recalled from short-term memory would drop out very quickly. The slow learners would then be at a higher level of learning at the onset of the retention interval. The lack of retention difference would thus be an artifact of differential short-term memory capacity, the fast learners actually being the better retainers. This possibility could easily be tested using Underwood's procedure with the addition of an inter-trial task to insure that items are being recalled from long-term memory.

Differences in prior knowledge. Prior to the findings of Hunt and his associates, Shuell (1972) had concluded as a result of two studies he reported that individual differences in short-term memory were apparently not responsible for individual differences in learning ability. As an alternative Shuell suggested that differences in learning might be "associated with individual differences in what the individual has already learned or in his ability to apply previously learned information to the learning task in which he is currently engaged" (p.36). Shuell went on to report the results of several studies in which the attempt was made to demonstrate the existence of such differences. Primarily, the studies involved testing for group differences in transfer of learning, learning-to-learn, and organizational ability. The studies were not conclusive, generally revealing no differences between fast and slow learners in these abilities.

A more global approach which has as its core the essence of Shuell's conjecture -- that individual differences in learning are a function of the individual's prior experience -- has been most explicitly presented in a recent article by Royer, Hambelton and Cadorette (1978). In addition, Royer and his associates have raised a number of issues which pose a serious challenge to the conclusions arrived at by recent investigators in the area of individual differences in learning and memory. Those issues have been raised not in the form of methodological criticism but rather in the form of theoretical elucidation. The authors point out, in fact, that theirs represents the first explicit theoretical approach to the problem.

Royer et al. contrast two theories of the nature of the memory trace -- one which assumes that the representation in memory is a copy of the perceived stimulus (Reappearance Theory), the other which suggests that the memory representation is a function not only of the particular stimulus event, but also of the context in which it is perceived and the current knowledge structure of the perceiver (Constructive Theory). The authors argue that if the assumptions inherent in Reappearance Theory are accepted then one can seriously talk about bringing two subjects to the same learning level. This because it is assumed that while the strength of the memory unit can be experimentally varied, the nature of the memory unit is fixed and, in fact, identical for all subjects. However, if one accepts Constructive Theory the possibility of equating learning is essentially forfeited since individual representations in memory of a particular stimulus event are viewed as differing not only in strength but also in nature.

Both Reappearance and Constructive Theory prove adequate, according to Royer et al., in accounting for the result obtained in the area of individual differences in learning and retention when studies have been limited to paired-associate and free-recall materials. Contradictory predictions result, however, when more meaningful materials are considered. Reappearance Theory still predicts no retention differences between fast and slow learners, while Constructive Theory suggests a retention advantage for the fast learners. Thus, different outcomes are predicted from Constructive Theory depending on the experimental task. It is predicted that a fast-learner retention advantage will result when meaningful materials are employed since these can be integrated into existing knowledge and since fast learners are assumed to possess "richer" knowledge structures. No retention differences are predicted with less meaningful materials (e.g., paired associates) since the role of knowledge structure is, in this way, minimized, the fast learners advantage being effectively reduced.

Royer et al. report the results of two studies which were designed to test the hypothesis that there will be systematic retention differences between fast and slow learners when the learning materials employed are amenable to integration into existing knowledge.

In the first experiment two groups of sixth graders, defined as high and low ability on the basis of IQ scores, learned a 16 item free-recall list to the criterion of 14 correct. Items consisted of four words each from four different conceptual categories. According to the notion of the effects of prior knowledge, the fact that the list was categorizable would give the fast learners a retention advantage.

The procedure developed by Shuell and Keppel (1970) of using differential rates of presentation to equate initial performance of fast and slow learners was employed in the study with one and five seconds exposure per word respectively. Each study trial was followed by a 30 second thought-diverting task after which subjects had 90 seconds to recall as many words as they could. Subjects were tested 35 days later for memory of the entire list. (The design was more complex than reported here, involving an additional independent variable and several other dependent measures.) The mean number of words recalled for the high-ability subjects was 7.75 while the low-ability group recalled a mean of 5.6 words, a difference which was statistically significant. This finding, if valid, contradicts the no-difference conclusion reached by Underwood and others and can be interpreted as support for the hypothesis that prior knowledge plays a vital role in learning and memory. It suggests that while performance might be equal for fast and slow learners at the end of a learning task, fast learners will better retain (retrieve) the information since it has been integrated into a well established knowledge structure. One challenge to the interpretation of the study is the possibility that the procedure used to equate fast and slow learners was not successful, that the fast learners were at a higher performance level at the beginning of the retention interval. However, a probability analysis based on number of previous correct recalls revealed no differences between groups.

As a further test of the role of prior knowledge in the learning and retention of meaningful materials, a second experiment was run in which high, medium and low IQ ninth grade students studied, over a period of several days, a programmed instruction unit on "The Structure of Matter". Amount learned was determined via a criterion-referenced test covering 12 specified objectives which was administered the day following completion of the study phase. Retention was measured 30 days later with an alternate form of the test. The probability that an objective was mastered on the delayed post-test given that it had been mastered on the immediate post-test but not mastered on a pretest was calculated for each ability group. High-ability students proved superior in the retention task with a mean proportion of .73 as compared with a value of .25 for the low-ability students. The medium-ability students were almost midway between (.51).

The authors discuss their findings vis-a-vis both theoretical approaches. In interpreting the data, proponents of Reappearance Theory would seriously question whether the level of learning between groups was equated initially. The vulnerability to such a challenge is most obvious in the case of the second experiment. Royer et al. speak to the problem and do cite some evidence that their results were not a reflection of different levels of initial learning. But ultimately they feel that from the perspective of Constructive Theory the problem of equating level of learning with meaningful materials is insolvable. They do, however, suggest at least one alternative by which the problem may be circumvented. This alternative is discussed in the last section of this paper.

According to Constructive Theory, the high-ability or fast-learning subject retains the information better since it has been integrated into an existing knowledge structure for which multiple retrieval paths have been established. A subject who has learned the same information but who has not been able to relate it to existing knowledge, or whose existing knowledge structure is not as rich, has fewer retrieval paths available. This relevant-knowledge deficit is manifest in a reduced probability of recall. Royer et al. go on to suggest that rate of learning may also be a function of relevant knowledge structure. That is, the reason that high-ability subjects learn more quickly may be that they have available a knowledge structure into which the new information can readily be integrated, while the slow learner must spend more time, presumably searching for relevant connections between what he knows and what is being presented, strengthening those connections where they are weak, and "rote memorizing" or using some other strategy to store information for which connections with prior knowledge cannot be economically made. It is obvious that this explanation does not apply to differential rates of paired-associate learning since it has been argued that the reason retention differences do not obtain between fast and slow learners is due to the fact that such material minimizes the role of prior knowledge. In the case of less meaningful material, therefore, the authors turn to the theory posited by Hunt and his associates which suggests that differential rates of learning are accounted for by individual differences in short-term memory ability. Implicit in this two-step explanation offered by Royer et al. is a proposed positive relation between short-term memory capacity and richness of knowledge structure. In fact, such a relationship has been posited by Hunt et al. (1975), who suggested that short-term memory ability exercises a controlling function over the amount of information stored in long-term memory -- the greater amount of information that can be effectively dealt with in short-term memory, the more information that can be transferred to long-term memory which, in turn, has an effect upon the productivity of future learning occasions. The parsimony of such an interpretation is luring but has not been empirically established.

Overview and Projection

After investing over 70 years of research in answering what seemed, on the surface, a simple question concerning the relation between rate of learning and retention, it perhaps would be of value to review progress and determine the current status.

Mention was made in the introduction to the probable origin of the question which has been addressed. It was first posed not by experimental psychologists but, instead, by educators who were operating from an applied rather than a theoretical perspective. Early researchers, while making use of paired-associate and other simple tasks, as a rule also included more meaningful stimuli such as prose and biographical paragraphs in their repertoire of experimental materials. The use of these more meaningful materials, however, became less frequent with the growing methodological exigency of equating learning and with the growing behavioral Zeitgeist. But limiting experimental inquiry to list materials subtly changed the nature of the question. The motivation for providing an answer was no longer rooted in the desire to gather data that could be of value to educational practice, nor was it to test the validity of a theoretical perspective. Effecting a methodological solution had assumed paramount importance. Thus, Underwood (1964) does not discuss educational implications of his findings, but only the important implications they have for memory research.

We have not, then, answered the question that could be of importance to educational practice -- given meaningful stimuli typical of materials encountered in the classroom, will retention difference exist between fast and slow learners? It has not been established that such a question is unanswerable. The important implications that this question has for mastery learning, compensatory education, dealing with individual differences, to name a few, seem compelling enough to attract scientific interest; the potential foothold that could be achieved with insight into the basis of individual differences in learning should be tempting enough to engage scientific endeavor.

But how do we proceed? The findings of Underwood represent an investigatory cul-de-sac for researchers interested in using materials that even approximate those confronted in the classroom, for how is learning to be equated? The techniques developed by Underwood (1954; 1964) or Shuell and Keppel (1970) are impractical with materials which are more complex than those for which they were specifically designed. A search for some other methodological technique for equating learning using more meaningful materials holds little promise given the great difficulty encountered in developing such techniques for simplier materials, and in view of the fact that the theoretical perspective which suggests that learning can be equated (Reappearance Theory) may not be correct.

Royer and his associates (1978) have provided what may be an answer to the dilemma. In so doing they have not denied the problem of interpreting group retention differences when learning has not been equated initially. They have suggested, however, that it may be impossible to equate learning and have offered at least one alternative which seems to circumvent the problem altogether. The solution depends on the viability of the theoretical perspective they have advocated (Constructive Theory). It assumes that rate of learning is a function of knowledge structure and thus predicts that an individual who in learning one task may be slow because he is lacking the appropriate knowledge structure, may be a rapid learner when confronted with another set of materials for which a relevant knowledge base has been established. The prediction is also made that retention will be better for information which has been integrated into an existing knowledge structure than for information which has not been so integrated. The possibility exists then of testing the theoretical perspective, and, at the same time, testing for retention difference between fast and slow learners. This is accomplished by finding a task for which low-ability (or slow-learning) subjects possess relevant knowledge and, therefore, would be expected to demonstrate better retention of learned materials than high-ability subjects who do not possess the appropriate knowledge. The resultant crossover effect of such an experiment would disallow alternative explanations involing initial learning inequalities.

The identification of appropriate tasks that will permit the testing of the hypotheses generated from Constructive Theory hold some promise for answering the speed-of-learning/amount-retained question in a form that carries important implications for educational theory and practice.

APPENDIX B. MATERIALS FOR EXPERIMENT 1

Passage Instruction:

Please do not turn the page until instructed to do so.

The purpose of this experiment is to help us better understand how what a person already knows affects the learning of new information.

On the next two pages is a passage of approximately 800 words which you will be asked to study. Most of the information in the passage will probably be new to you. You should study the passage in such a way that when you are asked to do so, you can recall as much of the information as possible. Do not attempt to memorize the passage. Rather, try to gain an understanding of the important points mentioned in the passage. Also, you will not be asked to recall any information which has been set off from the rest of the passage--sentences surrounded by a box. This information has been included because it may be of some help in relating the new information in the passage to knowledge you already have. Thus, information contained in a box should be read, but perhaps not studied as carefully as the rest of the passage which you will be quizzed on.

It is suggested that in studying the passage you read it through quickly the first time to get a general idea of what it is about. Then read through it a second time more slowly, paying more attention to the details of the passage.

When you have finished reading the passage through a second time, raise your hand and an assistant will give you further instructions.

If you have any questions during any part of the experiment, raise your hand for assistance.

Passage CG: Anchor Rummy

Rummy is a card game of Spanish origin which consists of two ideas: (1) scoring by combining specified cards and (2) "going out" (getting rid of all cards in the hand).

There are many types of Rummy. One of the most recent variations is called Anchor Rummy. The rules of play and scoring for Anchor Rummy are described below.

Cards. A pack of 48 cards is used. From a regular pack of 52 cards, discard all of the tens. Thus, the Anchor Rummy deck consists of the A,K,Q,J,9,8,7,6,5,4,3,2 of each suit.

The Deal. If only two are playing, each player is dealt ten cards; if three are playing, each is dealt eight cards; and if four are playing, seven cards are dealt to each player. As a rule, no more than four should play.

Stock and Discard Pile. The undealt remainder of the pack is placed face down in the center of the table, forming the stock. Its top card is turned face up and placed beside it; this card is the beginning of the discard pile.

In Anchor Rummy the discard pile is referred to as the chain, and the stock is referred to as the hole.

Building Crews. The object of play is to form the hand into crews. A crew is formed by building an odd or even sequence of similar-colored cards, headed by a Jack of the same color, in the case of odd-numbered cards, or headed by an Ace of the same color in the case of even-numbered cards. A few examples of acceptable crews are shown below.

Example #1 - J 3 5 7 9

#2 - J 3 5

Example #3 - A 2 4 6

#4 - A 2

Notice that a crew must include, at a minimum, two cards, and can be as long as five cards. Odd-numbered cards (e.g., 3,5,7,9) must be anchored by a Jack of the same color. Even-numbered cards (e.g., 2,4,6,8) must be anchored by an Ace of the same color. Thus, Aces and Jacks are called anchor cards. Twos and threes are referred to as first mates. The remainder of the numbered cards are called deck hands. The smallest acceptable crew therefore consists of an anchor and its similar-colored first mate. Notice too that a proper crew cannot have a gap. A,2,6 would not be an acceptable crew since the 4 is missing. Only the A,2 could, in this case, be laid down as a crew.

The Play. The player to the left of dealer begins play. Play passes continuously to the left (clockwise). In turn, each player must adhere to the following order.

(a) Draw. He must begin by drawing one card: the top card of the hole (stock), or the top card of the chain. This option exists only when the top card of the chain is not a King or Queen. A player may not pick up a King or a Queen from the chain and, therefore, is forced to draw from the hole.

(b) Lay-Down. He may then, if he pleases, place any number of cards from his hand face up on the table in front of him, provided that they form a proper crew. He may also "lay-off" cards from his hand which add to or complete crews previously laid-down by either himself or an opponent. For example, if a player laid-down a black A,2,4, that same player or an opponent could later lay-down a black 6, or a black 6 and black 8.

(c) Discard. He must end his turn by placing one card from his hand face up on the top of the chain, except that he need not discard if he has laid down all his remaining cards. In addition, if a player holds a King or Queen, he may place it over his discard. This prevents an opponent from picking up the discard, for neither Kings nor Queens may be retrieved from the top of the chain.

Setting Sail. When any player has no cards left in his hand he is said to "set sail". Play then stops and the deal is scored. If no player sets sail by the time the hole is exhausted, play continues until either no one can pick a card off the top of the chain (until a King or Queen is placed on the top), or the same card has been discarded twice.

Scoring. After play has terminated, each player adds up the point values of cards which he has laid-down in the form of crews or lay-offs, and subtracts from that the point values of any cards left in his hand. Aces are valued at 14, Jacks at 10, and all other cards at their face value. Kings and Queens remaining in the hand are not scored. For example, suppose a player sets sail and has laid down A,2,4,6 and A,2 and J,3,5. His score would be 60 (14 for each A, 10 for the J, and 22 for the numbered cards). If his opponent had previously laid down only a J,3,5,7 and had left in his hand A,4,6,J,7,K, his score would be 25 - 41 = -16.

Reaching Port. After the scores have been recorded, another hand is dealt and played. Play continues until one of the players reaches or exceeds 150 points, at which time he "reaches port". The game has then been won.

Passage PT: Ford Planetary Transmission

The function of a transmission is to permit different speed ratios between a car's engine and its drive wheels. When starting, for example, the engine must run comparatively fast and the wheels must turn slow. When the car gets underway, the relative speed of the wheels and the engine must change in order to achieve maximum efficiency.

Several different types of transmissions have been designed. One of the most interesting designs was used in the Model T and is called the Ford planetary transmission. Unlike a standard transmission, planetary gearing does not require gears to be shifted into or out of mesh. The gears, in fact, are always in mesh.

The planetary transmission consists of 3 main components: (1) the triple gears, (2) the central gears, and (3) the revolving drums.

Triple gears. The flywheel has 3 studs (protruding pins), each of which carries 3 gears of different sizes which are joined solidly together to form what is called a "triple gear".

Central gears. The triple gears mesh with 3 gears of different sizes in line with the drive shaft. These 3 gears are called the central gears. The gear closest to the flywheel face is fastened to the drive shaft and, therefore, is called the drive gear. The other two central gears are not fastened to put float on the drive shaft. The gear next to the drive gear is the slow-speed gear. It is smaller in diameter than the drive gear. Farthest from the flywheel and largest in diameter is the reverse gear.

Revolving drums. The slow-speed and reverse gears each are connecting to a separate cylindrical drum which revolves with the corresponding gear. By depressing a foot pedal, either drum, and consequently the gear which is connected to it, can be prevented from rotating. This action of preventing one of the floating gears from rotating is what allows the gears to be shifted.

While most transmissions have at least 3 forward speeds, the Ford planetary transmission is capable of only 2 forward speeds and reverse.

High gear. In a standard transmission high gear is accomplished basically by having a large diametered gear turn a small diametered gear. This effectively increases the speed of the drive shaft since the smaller gear turns more quickly than the larger one.

High gear in the planetary transmission involves no changes of gear ratios. Rather, all of the components of the transmission revolve together as a single mass with the flywheel. The drive shaft and drive gear, the triple gears, the floating gears and drums all rotate at the same revolutions per minute.

Low gear. Low gear in a standard transmission again involves the changing of gear ratios. This time a small diametered gear turns a large diametered gear which thus slows down the speed of the drive shaft relative to the engine or flywheel.

Planetary gearing accomplishes the same result in a different way. When the slow-speed revolving drum is gripped after the appropriate foot pedal has been depressed, the slow-speed gear is held stationary, no longer turning. This causes the triple gears to rotate on their studs as the flywheel revolves. (It was with this action in mind that the Ford design was named the planetary transmission. When one of the floating gears is prevented from turning, not only do the triple gears rotate with the flywheel, they revolve around their studs as well, much as the planets revolve on their axes as they rotate around the sun.) The triple gears are attached to each other and, therefore, must all make one revolution in the same amount of time. But since the slow-speed gear is smaller in diameter than the drive gear, the triple gears have a shorter distance to travel in revolving around it. To make up the difference, as the triple gears revolve on their studs they "push" the drive gear slowly forward. Thus, the drive shaft and rear wheels revolve slowly, powering the car in the forward direction.

Reverse. When the reverse pedal in the planetary transmission is depressed, the reverse drum and gear are prevented from turning. Again, the triple gears are forced to rotate on their studs as they revolve with the flywheel. But since the reverse gear is larger in diameter, the triple gears have a greater distance to travel in revolving around it than they have to travel in revolving around the smaller diametered drive gear. To make up the difference, the drive gear is "pulled" slowly backward as the triple gears revolve around it. Thus, the drive shaft turns in the opposite direction and the car is powered backward.

Study Questions: Anchor Rummy

Below are ten study questions concerning the passage you just read. Read the first study question and try to answer it in your mind without looking at the passage. After you have tried to answer the question in your mind, refer back to the passage for the answer. Having looked again at the passage and arrived at the correct answer, write the correct answer below the study question. Then, go on to the second study question and go through the same process: First, try to arrive at the correct answer; then refer to the passage for the correct answer; and finally, write the correct answer below the study question.

After you have written correct answers below each study question, raise your hand for further instructions.

1. How many cards are in the Anchor Rummy deck?

2. How many cards are dealt to each player when four are playing?

3. List an acceptable 4-card crew that is not given as an example in the passage.

4. List an unacceptable 4-card crew that is not given as an example in the passage.

5. Which cards are referred to as "anchor cards"?

6. When can a player not pick up a card off the top of the chain?

7. When does a player not have to discard at the end of a turn?

8. What would be your score if play terminated and you were holding in your hand the A,2,6,K,J,3?

9. What would be your score if when play terminated you were holding in your hand the K,J,5,A,3, but had laid down the J,3,5,7 and the A,2?

10. What is the difference between "setting sail" and "reaching port"?

Study Questions: Ford Planetary Transmission

Below are ten study questions concerning the passage you just read. Read the first study question and try to answer it in your mind without looking at the passage. After you have tried to answer the question in your mind, refer back to the passage for the answer. Having looked again at the passage and arrived at the correct answer, write the correct answer below the study question. Then, go on to the second study question and go through the same process: First, try to arrive at the correct answer; then refer to the passage for the correct answer; and finally, write the correct answer below the study question.

After you have written correct answers below each study question, raise your hand for further instructions.

1. What are the three main components of the planetary transmission?

2. List, in order of size, the three central gears.

3. List, in order of increasing distance from the flywheel, the three central gears.

4. Which of the central gears are connected to a revolving drum?

5. Which of the central gears are rotating when the transmission is in high gear?

6. Which of the central gears are rotating when the transmission is in low gear?

7. (a) How many studs are there on the flywheel?

   (b) Thus, how many sets of triple gears are connected to the flywheel?

8. On which two occasions do the triple gears rotate around their studs as well as revolve with the flywheel?

9. Explain how the drive gear is "pushed" slowly forward when the reverse pedal is depressed.

10. Explain how the drive gear is "pulled" backwards when the reverse pedal is depressed.

True/False Test (Form A): Anchor Rummy

Below are 22 statements about Anchor Rummy. If a statement is true, circle the "T" next to the item number; if the statement is false, circle the "F". If you have no idea whether a statement is true or false, make the best choice you can. There is no time limit on this test.

T F 1. There are no 10's in the Anchor Rummy deck.

T F 2. The game has been won when a player reaches or exceeds 100 points.

T F 3. A player always begins his turn by drawing either off the top of the chain, or from the hole.

T F 4. A Queen is valued at 0 points.

T F 5. The game has been won when a player "reaches port".

T F 6. An Ace or a Jack may never be picked up from the top of the chain.

T F 7. J,3 would be an acceptable crew provided they were the same color.

T F 8. If a player has not set sail by the time the hole is exhausted, the chain is turned face down (without shuffling) to form a new hole.

T F 9. Cards may only be "laid-off" against an opponent.

T F 10. In Anchor Rummy, Aces and Jacks are also referred to as "first mates".

T F 11. A player need not discard if he has laid down all his remaining cards.

T F 12. Anchor Rummy is a relatively new variation of Rummy.

T F 13. The Anchor Rummy deck has 52 cards.

T F 14. When two are playing, each player is dealt ten cards.

T F 15. A player's hand is scored by totaling the point values of cards he has laid down and subtracting from that the point values of cards left in his hand.

T F 16. A Jack is valued at 10 points.

T F 17. An 8 is valued at 1 point.

T F 18. J,3,5,9 would be an acceptable crew provided they were the same color.

T F 19. After a player sets sail, his opponents each have one turn to lay down any cards they can.

T F 20. An Ace is valued at 10 points.

T F 21. When three are playing, each player is dealt seven cards.

T F 22. A turn may be ended by placing two cards on the top of the chain.

True/False Test (Form B): Anchor Rummy

Below are 22 statements about Anchor Rummy. If a statement is true, circle the "T" next to the item number; if the statement is false, circle the "F". If you have no idea whether a statement is true or false, make the best choice you can. There is no time limit on this test.

T F 1. Anchor Rummy is a relatively old variation of Rummy.

T F 2. A Jack is valued at 14 points.

T F 3. A player cannot lay down cards from his hand until he has a crew containing five cards.

T F 4. In Anchor Rummy, Kings and Queens are also referred to as "deck hands".

T F 5. When four are playing, each player is dealt seven cards.

T F 6. When the hole has been exhausted, play may be terminated by discarding the same card twice.

T F 7. Cards may be "laid-off" against one's self or against an opponent.

T F 8. A 7 is valued at 7 points.

T F 9. Ace,2,3 would be an acceptable crew provided that they were the same color.

T F 10. A Queen may never be picked up from the top of the chain.

T F 11. When the hole has been exhaused, play may be terminated by discarding an Ace.

T F 12. The game has been won when a player reaches or exceeds 150 points.

T F 13. The points earned by the player who sets sail are the total point values of the cards he has laid down plus the point values of the cards remaining in the hands of his opponents.

T F 14. An Ace is valued at 14 points.

T F 15. J,2,4,6,8 would be an acceptable crew provided they were the same color.

T F 16. When two are playing, each player is dealt eight cards.

T F 17. Play is always ended by placing one card on the top of the chain.

T F 18. If a player is holding a King or Queen in his hand, he must begin his turn by drawing from the hole.

T F 19. The Anchor Rummy deck has 48 cards.

T F 20. After a player sets sail, his opponents may not lay down any more cards.

T F 21. In Anchor Rummy, 2's and 3's are also referred to as "first mates".

T F 22. A King is valued at 13 points.

True/False Test (Form A): Ford Planetary Transmission

Below are 22 statements about the Ford planetary transmission. If a statement is true, circle the "T" next to the item number; if the statement is false, circle the "F". If you have no idea whether a statement is true or false, make the best choice you can. There is no time limit on this test.

T F 1. The reverse gear is larger in diameter than the drive gear.

T F 2. In reverse, the drive gear is pulled slowly backward as the triple gears revolve around it.

T F 3. The drive gear is not attached to, but freely floats on the drive shaft.

T F 4. The Ford planetary transmission was used in the Model T.

T F 5. The slow-speed gear is connected to a revolving drum.

T F 6. The reverse gear is smaller in diameter than the slow-speed gear.

T F 7. In low gear, the drive gear remains stationary as the slow-speed gear revolves slowly forward.

T F 8. Stepping on a foot pedal stops both of the revolving drums from rotating.

T F 9. The drive gear is not connected to a revolving drum.

T F 10. The slow-speed gear is farther from the flywheel than the reverse gear.

T F 11. The triple gears mesh with all three of the central gears.

T F 12. The gears of the planetary transmission are always in mesh.

T F 13. The flywheel is one of the three main components of the planetary transmission.

T F 14. If the drive shaft was turned by hand in the reverse direction, and no foot pedals were being depressed, only the reverse gear would rotate.

T F 15. In high gear all of the gears revolve together at the same RPM.

T F 16. In reverse, the reverse gear is rotating with the drive shaft.

T F 17. In low gear, the triple gears are rotating around their studs.

T F 18. The triple gears are joined solidly together.

T F 19. If the reverse gear was the same diameter as the slow-speed gear, when the reverse pedal was depressed the car would remain stationary.

T F 20. In the planetary transmission, gears are shifted by preventing the triple gears from rotating on their studs.

T F 21. The central gears are in line with the drive shaft.

T F 22. The triple gears are attached to the drive shaft.

True/False Test (Form B): Ford Planetary Transmission

Below are 22 statements about the Ford planetary transmission. If a statement is true, circle the "T" next to the item number; if the statement is false, circle the "F". If you have no idea whether a statement is true or false, make the best choice you can. There is no time limit on this test.

T F 1. The gears of the planetary transmission are never in direct contact with one another.

T F 2. In reverse, the triple gears are rotating around their studs.

T F 3. The reverse gear is not connected to a revolving drum.

T F 4. The slow-speed gear is larger in diameter than the drive gear.

T F 5. The triple gears are in line with the drive shaft.

T F 6. In the planetary transmission, gears are shifted by preventing one of the floating gears from rotating.

T F 7. Stepping on a foot pedal stops one of the revolving drums from rotating.

T F 8. The triple gears are connected to the flywheel.

T F 9. In low gear, the drive gear is pushed slowly forward as the triple gears revolve around it.

T F 10. The slow-speed gear is closer to the flywheel than the drive gear.

T F 11. High gear in the planetary transmission involves changes in gear ratios.

T F 12. In reverse, the drive gear remains stationary as the reverse gear revolves slowly backwards.

T F 13. When the transmission is in low gear, the slow-speed gear is not turning.

T F 14. In high gear, the triple gears are rotating around their studs.

T F 15. The central gears are joined solidly together.

T F 16. If the slow-speed gear was the same diameter as the drive gear, when the slow-speed pedal was depressed the car would remain stationary.

T F 17. The triple gears mesh only with the drive gear.

T F 18. The slow-speed and reverse gears float on the drive shaft.

T F 19. The revolving drums are one of the three main components of the planetary transmission.

T F 20. If the drive shaft was turned by hand in the forward direction and no foot pedals were being depressed, all of the central gears would rotate.

T F 21. The drive gear is closer to the flywheel than the reverse gear.

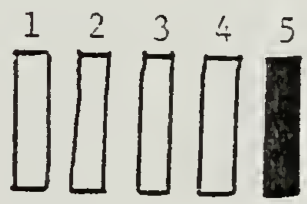
T F 22. The planetary transmission was used in the Model A.

Vocabulary Test: Instructions

This is a test of your knowledge of word meanings. Look at the sample below. One of the five numbered words has the same meaning or nearly the same meaning as the word above the numbered words. Indicate your answer by making a heavy line with your pencil in the box on your answer sheet which is numbered the same as the correct answer.

jovial

1-refreshing
2-scarce
3-thickset
4-wise
5-jolly



The answer to the sample item is number 5; therefore, the box under number 5 on your answer sheet should be filled in.

Your score will be the number marked correctly. Therefore, it will be to your advantage to guess if you do not know the correct answer to a particular item.

You will have <u>8 minutes</u> to complete this test. An experimenter will let you know when 8 minutes have passed. If you finish early, remain seated quitely at your desk until you are given further instructions.

Vocabulary Test: Form A

1. mumble

   1-speak indistinctly
   2-complain
   3-handle awkwardly
   4-fall over something
   5-tear apart

2. perspire

   1-struggle
   2-sweat
   3-happen
   4-penetrate
   5-submit

3. gush

   1-giggle
   2-spout
   3-sprinkle
   4-hurry
   5-cry

4. massive

   1-strong and muscular
   2-thickly populated
   3-ugly and awkward
   4-huge and solid
   5-everlasting

5. feign

   1-pretend
   2-prefer
   3-wear
   4-be cautious
   5-surrender

6. unwary

   1-unusual
   2-deserted
   3-incautious
   4-sudden
   5-tireless

7. veer

   1-change direction
   2-hesitate
   3-catch sight of
   4-cover with a thin layer
   5-slide

8. orthodox

   1-conventional
   2-straight
   3-surgical
   4-right-angled
   5-religious

9. stripling

   1-stream
   2-narrow path
   3-engraving
   4-lad
   5-beginner

10. salubrious

   1-mirthful
   2-indecent
   3-salty
   4-mournful
   5-healthful

11. limpid

   1-lazy
   2-crippled
   3-clear
   4-hot
   5-slippery

12. procreate

   1-sketch
   2-inhabit
   3-imitate
   4-beget
   5-encourage

13. replete

1-full
2-elderly
3-resentful
4-discredited
5-restful

14. frieze

1-fringe of curls
on the forehead
2-statue
3-ornamental band
4-embroidery
5-sherbet

15. treacle

1-sewing machine
2-framework
3-leak
4-apple butter
5-molasses

16. ignominious

1-inflammable
2-elflike
3-unintelligent
4-disgraceful
5-mysterious

17. abjure

1-make certain
2-arrest
3-renounce
4-abuse
5-lose

18. duress

1-period of time
2-distaste
3-courage
4-hardness
5-compulsion

19. bayonet

1-small tent
2-basket
3-helmet
4-sharp weapon
5-short gun

20. astound

1-scold severely
2-make angry
3-surprise greatly
4-drive out
5-ascertain

21. contamination

1-contradiction
2-contempt
3-warning
4-pollution
5-continuation

22. amplify

1-electrify
2-expand
3-cut off
4-signify
5-supply

23. mural
<u>pertaining to</u>

1-growth
2-manners
3-the eyes
4-war
5-a wall

24. hale

1-glad
2-fortunate
3-tall
4-robust
5-ready

25. meander

1-marvel
2-predict
3-slope
4-forget
5-wind

26. burnish

1-polish
2-wave
3-dye
4-heat
5-consume

27. duplicity

1-extent
2-double-dealing
3-agreement
4-cleverness
5-overlapping

28. mundane

1-worldly
2-obstinate
3-deafening
4-servile
5-penniless

29. deleterious

1-injurious
2-hysterical
3-critical
4-slow
5-thinned out

30. nascent

1-colorful
2-broad
3-unpleasant
4-floating
5-beginning

31. prolific

1-freely reproductive
2-prehistoric
3-talented
4-highly temperamental
5-frivolous

32. paroxysm

1-bleach
2-disaster
3-storm
4-fit
5-revolution

33. antipodal

1-outmoded
2-slanted
3-medodious
4-opposite
5-four-footed

34. acrimony

1-promptness
2-boredom
3-divorce
4-stupidity
5-bitterness

35. lissome

1-lonely
2-young
3-dreamy
4-supple
5-dainty

36. succinct

1-sudden
2-concise
3-prosperous
4-literary
5-cunning

Vocabulary Test: Form B

1. attain

   1-excel
   2-witness
   3-achieve
   4-prohibit
   5-try

2. intact

   1-whole
   2-corrupt
   3-polite
   4-sharp
   5-quiet

3. debate

   1-differ
   2-deceive
   3-lecture
   4-injure
   5-argue

4. boast

   1-belittle
   2-brag
   3-raise
   4-push
   5-cook

5. remedy

   1-cure
   2-intensify
   3-recall
   4-report
   5-charge

6. appeal

   1-accuse
   2-petition
   3-uncover
   4-equip
   5-excuse

7. bias

   1-ignorance
   2-unity
   3-justness
   4-prejudice
   5-duality

8. categorize

   1-list
   2-blend
   3-name
   4-brand
   5-classify

9. comprehend

   1-describe
   2-determine
   3-construct
   4-arrest
   5-understand

10. fragile

    1-severed
    2-sprightly
    3-tattered
    4-brittle
    5-prudent

11. modify

    1-recondition
    2-add to
    3-alter
    4-dissemble
    5-partition

12. bizarre

    1-strange
    2-frightening
    3-subdued
    4-delightful
    5-sour

13. omit

1-allow
2-throw away
3-leave out
4-seize
5-release

14. cactus

1-percussion instrument
2-desert plant
3-synthetic wool
4-mufti
5-geometric

15. client

1-cadet
2-employer
3-unit of measure
4-customer
5-cadence

16. domain

1-territory
2-completeness
3-an established law
4-natural
5-a principal part

17. rage

1-anger
2-ruler
3-acceleration
4-fear
5-argument

18. plausible

1-menial
2-surly
3-ornate
4-believable
5-complex

19. profile

1-design
2-prefer
3-categorize
4-outline
5-detect

20. pierce

1-detest
2-strike
3-stab
4-decide
5-cut

21. recede

1-march
2-withdraw
3-follow
4-rest
5-tumble

22. merit

1-degrade
2-measure
3-predict
4-fabricate
5-earn

23. emerge

1-exclude
2-construct
3-clothe
4-appear
5-drag

24. remote

1-shabby
2-religious
3-distant
4-automatic
5-motionless

25. dictate

1-disagree
2-impose
3-govern
4-weaken
5-deject

26. singe

1-decorate
2-destroy
3-burn
4-steal
5-praise

27. intuition

1-insight
2-fee
3-honesty
4-suspicion
5-mandate

28. legion

1-palace
2-meeting
3-incision
4-fairy tale
5-multitude

29. sanction

1-single
2-function
3-repeal
4-imprison
5-approve

30. renovate

1-restore
2-reinforce
3-burn down
4-nullify
5-devour

31. shrewd

1-conspiring
2-tricky
3-vicious
4-dangerous
5-sinful

32. arrogant

1-yielding
2-proud
3-fragrant
4-distorted
5-vocal

33. intimidate

1-introduce
2-question
3-interrupt
4-invite
5-bully

34. epistle

1-problem
2-saint
3-plant
4-letter
5-religion

35. caper

1-inconvenience
2-feat
3-seizure
4-prank
5-mishap

36. lewd

1-ample
2-damp
3-indecent
4-prideful
5-dross

Questionnaire

Name: ______________________________

Age: __________ Year in school: ________________

Below is a list of card games. Put a check by every card game which you have played before.

__ Bridge	__ Gin Rummy
__ Blackjack	__ Handicap Poker
__ Canasta	__ Hearts
__ Crazy Eight	__ Oklahoma Gin
__ Cribbage	__ Pinochle
__ Diamond Roulette	__ Reach
__ Draw Poker	__ Red Dog
__ Euchre	__ Solitaire
__ Fish	__ South Dakota Draw
__ Foolem	__ Spit in the Ocean

Below is a list of automotive repairs. Put a check by every repair which you personally have made.

__ change points	__ re-core radiator
__ set timing	__ replace wheel bearings
__ replace generator	__ replace starter motor
__ grind valves	__ replace piston rings
__ turn piston	__ rebuild automatic transmission
__ lube job	__ replace clutch
__ replace U-joint	__ rebuild standard transmission
__ rebuild regulator	__ bore out engine block
__ adjust brakes	__ front end alignment
__ change oil	__ rebuild carburetor

APPENDIX C: PASSAGE IDEA UNITS

Idea Units: Passage CG

A. Score one point for the first occurrence of the following terms.

1. Anchor Rummy
2. chain
3. set sail (setting sail)
4. reach port (reaching port)
5. deck hands
6. first mates
7. hole
8. crew

B. Score one point for the first occurrence of the following ideas.

1. There are many types of Rummy
2. Anchor Rummy is a recent variation
3. A pack of 48 cards is used
4. All the ten's are discarded from a deck of 52 (throw away the 10's)
5. If two are playing, each is dealt 10 cards
6. If three are playing, each is dealt 8 cards
7. If four are playing, each is dealt 7 cards
8. No more than four should play
9. The discard pile is called the chain
10. The stock is called the hole.
11. The object of play is to build crews (you must make crews or sequences)
12. A crew is an odd or even sequence of cards (cards in order)
13. A crew must be composed of similar-colored cards
14. Even-numbered cards are headed by an Ace
15. Odd-numbered cards are headed by a Jack
16. (Give 1 point for the first occurrence of an even-numbered crew)
17. (Give 1 point for the first occurrence of an odd-numbered crew)
18. The smallest possible crew is 2 cards
19. The largest possible crew is 5 cards
20. Aces and Jacks are called anchor cards (anchors)
21. 2's and 3's are called first mates
22. The remainder of numbered cards are called deck hands
23. The smallest crew is composed of an anchor card and 1st mate (if credit for 23, also give credit for 18)
24. A proper crew cannot contain a gap
25. (Give 1 point for the first example of #24)
26. Player to the left of dealer begins play
27. Play passes to the left (clockwise)
28. A player begins his turn by drawing one card
29. May draw from the top of the chain (discard pile)

30. May draw from the hole (stock)
31. The option of drawing from either the hole or chain exists only when K or Q is not on top of the chain (if credit for #31, also give credit for #29 and #30)
32. May not pick up K or Q from the top of the chain (If only K or Q mentioned, give ½ credit)
33. After drawing a card may lay down cards
34. Cards are laid down face up on the table
35. In order to lay down cards, they must form a proper crew
36. May lay-off cards which add to or complete previously laid-down cards
37. (Give 1 point for the first example of #36)
38. May lay off cards against one's self or an opponent
39. A turn is ended by discarding
40. In discarding, a card is placed on the top of the chain
41. A player is not required to discard if he has laid down all of his remaining cards
42. A K or Q may be place over the discard
43. Placing a K or Q over the discard prevents an opponent from picking up the discard (if credit for #43, give credit for 42)
44. One sets sail when he has no cards remaining in his hand
45. Play stops when a player sets sail
46. After a player sets sail, the deal is scored
47. If the hole is exhausted before a player sets sail, play continues
48. Given #48, play is ended when a card may not be picked up from the top of the chain (when K or Q on top)
49. Given #48, play is ended when the same card is discarded twice
50. In scoring, the cards a player has laid on the table are added up
51. (In reference to #50) These include both crews and lay-offs
52. Cards left in the hand are subtracted from score
53. An Ace is valued at 14 points
54. A Jack is valued at 10 points
55. Numbered cards are scored according to their face value
56. K and Q in the hand are not scored (or equal zero)
57. (Give 1 point for the first example of scoring a hand)
58. After scoring, another hand is dealt
59. Play continues until a player reaches 150 points
60. When a player reaches 150 points, he reaches port
61. The game is won when a player reaches port

Idea Units: Passage PT

A. Score one point for the first occurrence of the following terms.

1. Planetary transmission
2. rotating drums (revolving drums)
3. central gears
4. triple gears
5. drive gear
6. slow-speed gear (slow gear or low-speed gear)
7. floating gear

B. Score one point for the first occurrence of the following ideas.

1. Several types of transmissions have been designed
2. F.P.T. used in the Model T
3. F.P.T. one of the most interesting designs
4. Gears are not shifted into or out of mesh
5. Gears are always in mesh
6. F.P.T. consists of 3 main components
7. Triple gears are a member of #6
8. Central gears are a member of #6
9. Revolving drums are a member of #6
10. Flywheel has 3 studs (pins) connected to it
11. Each stud carries 3 gears
12. Gears in #11 are called triple gears
13. Gears of triple gears are of different sizes
14. Triple gears joined solidly together
15. Triple gears mesh with central gears
16. There are 3 central gears
17. Drive gear is a member of #16
18. Slow-speed gear is a member of #16
19. Reverse gear is a member of #16
20. Central gears are of different sizes
21. Central gears are in line with the drive shaft
22. Gear closest to the flywheel is the drive gear
23. Drive gear is fastened to the drive shaft
24. Because of #23 it is called the drive gear
25. Gear next to drive gear is the slow-speed gear
26. Gear furthest from flywheel (or next to slow-speed gear) is the reverse gear
27. Slow-speed gear floats on (not fastened to) drive shaft
28. Reverse gear floats on drive shaft
29. Slow-speed gear smaller than drive gear (or smallest)*
30. Reverse gear is largest in diameter*
    *(If credit for #29 or 30 give credit for #20)
31. Slow-speed gear is connected to a revolving drum
32. Reverse gear is connected to a revolving drum
33. Revolving drums revolve with the floating gears

34. Preventing the floating gears from rotating is what allows the gears to be shifted
35. F.P.T. has 2 forwards speeds (gears)
36. F.P.T. has reverse
37. High gear involves no changes in gear ratios
38. In high gear, all components rotate at the same RPM (or revolve together)
39. Planetary gearing accomplishes low gear in a way different than in a standard transmission
40. When slow-speed pedal depressed, revolving drum is gripped
41. When reverse pedal is depressed, revolving drum is gripped
42. When a foot pedal is depressed, a revolving drum is gripped* *(if credit for #40 or 41, no credit for #42)
43. When revolving drum is prevented from turning, corresponding gear also does not turn
44. When a floating gear (either could be mentioned) is prevented from turning, the triple gears rotate around their studs
45. The above action is similar to planets revolving around the sun
46. (1 point for each of the specific comparisons in this analogy) Triple gears = earth, studs = axis
47. Triple gears must make one revolution in the same amount of time
48. Triple gears have shorter distance to travel around the slow-speed gear, when it is held stationary, than around the drive gear
49. When slow-speed gear is stationary (or s-s pedal depressed) the drive gear is pushed forward
50. (Given #49) by the triple gears
51. (Given #49) the drive shaft and rear wheels revolve slowly (or car powered in forward direction)
52. Since the reverse gear is larger than drive gear, when reverse gear stopped, triple gears have further to travel around reverse gear than the drive gear
53. To make up the difference, when reverse gear is stationary, the drive gear is pulled backward
54. (Given 53) by triple gears
55. (Given 53) drive shaft turned in opposite direction (or car powered backward)

APPENDIX D: ADDITIONAL MATERIALS FOR EXPERIMENT 2

Rating Task

In the spaces provided below, write down the names of all the card games you have played before. Beside each game that you list, indicate on the scale provided how familiar you currently are with the rules and strategies of the particular game. Do this by circling the appropriate number according to the following key:

1 = not familiar
2 = slightly familiar
3 = moderately familiar
4 = very familiar

______________________	1	2	3	4
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Achievement Test

This is a short test designed to tap your knowledge of the rules, terms, and strategies associated with various card games.

Items 1-10 consist of a definition with four terms listed below. Choose the term which best fits each definition. If you have no idea as to the correct answer, make the best guess you can.

1. A bet or contribution to the pot before the deal

    1. check
    2. opening
    3. ante
    4. bete

2. An attempt to evade having a card captured by an opponent's higher-ranking card

    1. finesse
    2. en passant
    3. chouette
    4. squeeze

3. A card with which a player can obtain the lead

    1. entry
    2. slam
    3. bower
    4. face

4. The nine of trumps when it is the lowest of the suit

    1. bug
    2. pip
    3. dummy
    4. dix

5. The winning of two games out of three

    1. sweep
    2. capot
    3. rubber
    4. box

6. A face-down card in STUD POKER

    1. hole card
    2. stock card
    3. duck card
    4. low card

7. A POKER hand containing three of a kind and a pair

    1. flush
    2. natural
    3. straight
    4. full house

8. Turn down one's face-up cards to signify dropping

    1. pass
    2. laps
    3. fold
    4. renege

9. In POKER, to meet the previous bet

    1. raise
    2. call
    3. force bid
    4. stand

10. One card from the hand of each player

    1. round
    2. trick
    3. book
    4. sequence

For items 11-30 choose the word or phrase which best answers the question or completes the statement. If you have no idea as to the correct answer, make the best guess you can.

11. What is the basic objective of FISH?

    1. to acquire all the cards of one suit
    2. to meet or exceed a contracted number of tricks
    3. to be the first to get rid of all your cards
    4. to lay down the most groups consisting of four cards of the same rank

12. What, in general, is the objective of HEARTS?

    1. to win the most tricks which contain hearts
    2. to win the fewest tricks which contain hearts
    3. to win the queen of hearts
    4. to not win the queen of hearts

13. In the game of HEARTS, which diamond sequence would be the worst holding?

    1. 2-3-5-K-A
    2. 6-8-9-10-J
    3. 2-3-5-6-7
    4. K-A

14. In BLACKJACK (or TWENTY-ONE) the Ace is valued at either 1 or

    1. 10.
    2. 11.
    3. 12.
    4. 13.

15. In BLACKJACK the dealer must take additional cards as long as his total does not exceed

    1. 12.
    2. 14.
    3. 16.
    4. 18.

16. What is the objective of CRAZY EIGHTS?

    1. to meet or exceed a contracted number of tricks
    2. to lay down the most groups consisting of four cards of the same rank
    3. to be the first to get rid of all your cards
    4. to accumulate the most 8's or eight-card straights

17. In CRAZY EIGHTS which card cannot be played on the 6 of hearts?

    1. 5 of hearts
    2. 6 of clubs
    3. 7 of diamonds
    4. 8 of spades

18. In OH HELL a player accumulates points by winning

    1. the exact number of tricks that were bid
    2. at least as many tricks as were bid
    3. tricks which contain no face cards
    4. tricks which contain only face cards

19. Which is an acceptable meld or set in RUMMY?

    1. K-Q of diamonds
    2. J of hearts, Q of diamonds, and K of spades
    3. all four of the 9's
    4. 3-2-A-K of diamonds

20. What does the term "go rummy" refer to?

    1. going out when none of the opponents have laid down any cards
    2. picking up all the cards from the discard pile
    3. getting rid of all remaining cards in one's hand
    4. melding an entire hand in one turn, having made no previous melds

21. In RUMMY what happens when the stock is exhausted before one of the players has "gone out"?

    1. cards must be drawn from the discard pile
    2. the hand is scored and another dealt
    3. the discard pile is placed face down to form another stock
    4. play continues without a stock until a player goes out

22. In GIN RUMMY a player may challenge an opponent's hand when the total of his deadwood is 10 points or less. Which term describes this showdown?

    1. knocking
    2. gin
    3. freeze out
    4. stand-off

23. Which of the following games is <u>most</u> similar to RUMMY?

    1. CRIBBAGE
    2. CANASTA
    3. PINOCHLE
    4. HEARTS

24. Which of the following POKER hands is the highest ranked?

    1. three of a kind
    2. straight
    3. two pair
    4. flush

25. If in DRAW POKER you are dealt the K-2 of hearts and the A-K-Q of diamonds, which cards should you hold if you want to maximize your chances of improving your hand?

    1. K-K
    2. K-K-A
    3. A-K-Q of diamonds
    4. K-K-A-Q

26. In BRIDGE which is the lowest-ranking suit?

    1. hearts
    2. diamonds
    3. spades
    4. clubs

27. Which BRIDGE hand should probably be opened at a bid of one no-trump?

    1. a point count of 17, evenly distributed in all suits
    2. a point count of 21, evenly distributed in all suits
    3. a point count of 14, with eight hearts
    4. a point count of 19, with six hearts

28. To which group of cards does the term PINOCHLE specifically apply?

    1. K-Q of trump
    2. K-Q of a suit other than trump
    3. J of spades and 9 of clubs
    4. Q of spades and J of diamonds

29. In CANASTA which cards are designated wild cards?

    1. Jokers
    2. Jokers and 2's
    3. 3's
    4. Jacks and 3's

30. In CANASTA which cards are designated stop cards?

    1. Jokers
    2. black 2's
    3. black 3's
    4. black Jacks