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INDIVIDUAL DIFFERENCES IN ASSOCIATIVE LEARNING AND FORGETTING

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We conducted a study to examine individual differences in retention, with particular emphasis on its relationship with item-specific learning speed, general learning speed, and general cognitive factors. Young adults (N = 710) were taught 13 name-number pairs to varying successive success criteria using an item dropout procedure to ensure equal learning. Subjects were then taught re-pairings to varying criteria, in an A-B, A-Br design, so as to produce a range of forgetting conditions. Subjects then were asked to recall and relearn the original pairs. Item-specific learning speed predicted retention and reacquisition speed: The fastest learners, despite having the fewest number of study opportunities, remembered more and relearned faster. This relationship held over all forgetting conditions, and after statistically equating for within-list interference. General learning speed, as indicated by performance on an independent set of items administered in a variety of formats, also predicted retention and relearning. To examine the effects of other cognitive factors on learning and retention, various latent-variable path models were fit to the data. The best model (by parsimony and goodness-of-fit criteria) specified that retention is determined by item-specific learning speed (r^2 = .14), which in turn is determined by

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general learning speed ($\underline{r}^2$ = .16). General learning speed itself is reasonably well predicted by other cognitive factors ($\underline{r}^2$ = .62), especially general knowledge. Our results are contrasted with those of other investigators who, by employing different procedures for equating degree of learning, have concluded that there are no individual differences in retention. Implications for the discrepancy in conclusions are discussed.

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SUMMARY

An important question to address in developing a general model of learning ability is whether retention can be predicted from speed of initial learning. Some research has suggested that while individuals differ greatly as to how fast they learn in the first place (by approximately 6:1), once material is learned, it is forgotten at the same rate by both fast and slow learners. If this is the case, there should be no relationship between how quickly one learns and how much one remembers later, if adjustments are made for how well the material is learned originally.

We put these ideas to a test by administering a learning task requiring that subjects first learn a set of 13 name-number pairs (e.g., Jones-13, Barnes-48), then learn a mixed-up set (e.g., Jones-48, Barnes-13), and then to try to recall the original set. Our main finding was that speed of learning predicted amount retained. That is, faster learners forgot less. By a simple statistical procedure, we were able to show that this finding was not due to the fact that the slower learners had to wait longer before they were asked to recall the original list.

Our main conclusion is that the reason previous research has found no differences in forgetting rate is that equating learners in terms of how well they know something in the first place is a rather tricky issue. We equated learners by controlling for the number of successive mastery experiences they had. Others, who find different results, have equated learners on the basis of how much they remember on an immediate retention test.

PREFACE

Development of this paper was supported by the Air Force Learning Abilities Measurement Program (LAMP), a multi-year basic research program conducted at the Air Force Human Resources Laboratory (AFHRL) and sponsored by the Air Force Office of Scientific Research (AFOSR). The goals of the program are to specify the basic parameters of learning ability, to develop techniques for the assessment of individuals' knowledge and skill levels, and to explore the feasibility of a model-based system of psychological assessment.

The work documented here was begun by Dr Kyllonen during his earlier employment at AFHRL. Support was subsequently provided by AFHRL and AFOSR through Universal Energy Systems, Inc., under contract number F41689-84-D-0002/58420360, and subcontract number S-744-031-001 to the University of Georgia Research Foundation, Inc., with Dr Kyllonen as principal investigator.

The authors wish to thank Major Hector Acosta and his crew at Lackland Air Force Base for their data collection efforts, as well as Janice Hereford, Frank Rilling, Rich Walker, and other members of the OAO Corporation for their technical assistance on all phases of the study.

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Individual Differences in Associative Learning and Forgetting

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We conducted a study to examine individual differences in retention, with particular emphasis on its relationship with item-specific learning speed, general learning speed, and general cognitive factors. Young adults ($N = 710$) were taught 13 name–number pairs to varying successive success criteria using an item dropout procedure to ensure equal learning. Subjects were then taught re-pairings to varying criteria, in an A–B, A–Br design, so as to produce a range of forgetting conditions. Subjects then were asked to recall and relearn the original pairs. Item-specific learning speed predicted retention and reacquisition speed: The fastest learners, despite having the fewest number of study opportunities, remembered more and relearned faster. This relationship held over all forgetting conditions, and after statistically equating for within-list interference. General learning speed, as indicated by performance on an independent set of items administered in a variety of formats, also predicted retention and relearning. To examine the effects of other cognitive factors on learning and retention, various latent-variable path models were fit to the data. The best model (by parsimony and goodness-of-fit criteria) specified that retention is determined by item-specific learning speed ($r^2 = .14$), which in turn is determined by general learning speed ($r^2 = .16$). General learning speed itself is reasonably well predicted by other cognitive factors ($r^2 = .62$), especially general knowledge. Our results are contrasted with those of other investigators who, by employing different procedures for equating degree of learning, have concluded that there are no individual differences in retention. Implications for the discrepancy in conclusions are discussed.

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Individuals differ widely in how fast they learn and in how much they retain, and a major challenge to psychology has been to understand and predict such differences. Numerous studies have investigated the relationship between cognitive variables and learning outcomes, but we still lack an adequate working understanding of the relative importance of general cognitive skills, learning ability, and task variables in determining how much a particular individual will remember from an instructional session. A goal of our work is to develop a general model of learning ability, which should lead to a system for measuring learning skills and predicting learning outcomes in various instructional contexts.

One approach to developing a model of learning ability, and the approach that we adopt in this study, is to address the question of the dimensionality of learning. Content dimensions to learning ability have been demonstrated. For example, quantitative, verbal, and spatial learning can be shown to be somewhat distinct (Snow, Kyllonen, & Marshalek, 1984). However, in this paper we focus attention on certain *process* dimensions of learning. We are concerned with three fundamental questions. First, can retention of a set of items be predicted from speed of initial learning of those items? Second, is the ability to retain a set of items a narrow, item-specific skill, dependent on idiosyncratic relations that a subject can develop for those particular items, or is it related to more general associative learning skill, as indicated by performance on other items, measured in different ways? Third, are learning speed and retention abilities predictable from other cognitive factors?

RETENTION AND LEARNING SPEED

Perhaps surprisingly, there is considerable evidence that if learners' memories for items are equated for strength of association, then the rate at which those items are forgotten is constant. Although it seems counterintuitive that fast and slow learners forget at the same rate, this appears to be a fairly reliable finding (Shuell, 1972). The key insight is that fast learners appear to remember better because they learned better in the first place. If fast and slow learners can somehow be equated for degree of original learning, then they will forget at the same rate (Underwood, 1954). This demonstration obviously depends on the way in which degree of original learning, or associative strength, is defined, and on how two items are thus said to be of equal strength.

Under the most popular definition of degree of learning, two items are said to be of equivalent associative strength if the probability of getting the items correct on the next trial is the same. Underwood (1954) used this operational definition as a way of statistically equating fast and slow learners for degree of learning, using a method known as *successive probability analysis*. For each group (divided at the median) he computed the conditional probabilities of correctly responding to a paired-associate item as a function of the number of times the item had previously been correctly responded to, collapsing over items and subjects within group. He showed, for example, that it took slow-learning subjects 19 success trials to reach a next-trial probability criterion (of about .90, it appears from Underwood's Figure 1) that the fast learner reached in only 9 trials. Thus fast learners apparently benefitted more from each successful trial than did slow learners. But once these next-trial probabilities (i.e., associative strengths) were equated, the probability of recalling an item after a 24-hr interval did not vary over groups.

Other investigators (Gentile, Monaco, Iheozor-Egiofor, Ndu, & Ogbonaya, 1982; Shuell & Keppel, 1970) have varied presentation rate in a study-test paradigm to achieve the same kind of equating of degree of learning. For example, Shuell and Keppel showed that slow subjects required 5 s per word exposure to achieve the same free recall score (about 34%) that the fast subjects achieved with only 1 s per word exposure. But once again, the two groups, having been brought to this criterion, recalled the same after a 24-hr interval.

Because fast and slow learners did not differ in how well they retained information, these studies appear to indicate that acquisition and retention abilities are independent factors. Thus a model of learning ability capable of predicting learning outcomes would have to include both factors. But there may be conceptual and pragmatic problems with this conclusion.

A pragmatic problem is that strength of association, as defined in the successive (next-trial) probability method, might not be a functionally useful construct because of its private nature. In applied contexts, the instructor does not have access to the individual's next-trial probabilities. At best, the instructor only knows the experiential history of the learner. Strength, in the Underwood and Shuell–Keppel studies, is operationally defined in terms of a future event (viz., probability of passing the item on the next trial).

The fact that the latent construct—strength—is operationalized in terms of a future event points to a fundamental conceptual problem with the constant forgetting conclusion. Presumably, item strength decays as soon as that item is removed. Thus, the expected next trial probability of passing the item, which is taken as an uncontaminated measure of strength, is determined not just by the strength of the item immediately after study, but also by an individual's forgetting rate. Equating next-trial passing probabilities is only a completely justifiable method for strength-equating if the conclusion—constant forgetting—is assumed. Indeed, this possibility was pointed out by Underwood (1954) when he wrote that

> the results of the above analyses lead to a strong temptation to assert that the critical difference between fast and slow Ss is that the associative strength resulting from a reinforcement is less for slow than for fast learners. . . . Nevertheless, it is possible that a reinforcement for a fast and a slow S adds the same associative strength, but that in the context of learning a group of items (as contrasted with the relative rest over 24 hr.) more forgetting might take place for an item for slow Ss than for fast Ss

> over these short [maximum 1.5 min] intervals between trials. Thus it must not inevitably follow that reinforcements add different associative strengths at the time they occur for fast and slow Ss; we believe they do add or produce different amounts of associative strength but our data are not conclusive on this matter. We can conclusively say only that when probabilities of response for successive trials during learning are equated for fast and slow Ss no differences in forgetting occurred. (p. 281)

The problem is that next-trial probabilities are only indicators of strength, that is, operational realizations of the underlying latent construct. And although Underwood treated as unlikely the possibility of differential forgetting within the first minute or so, it could be that a sizeable proportion of the total forgetting occurring over 24 hr occurs in the first 2 min. Proposals of exponential or power-law forgetting (e.g., Wickelgren, 1974) indeed are claims that most forgetting occurs soon after an item is removed. If most forgetting occurs relatively soon, it is not unfeasible to speculate that individual differences in forgetting are revealed primarily in the first few minutes after the item is removed.

Because of these problems with next-trial probabilities as indicators of strength, it is useful to consider other indicators of strength. One is *cumulative successes*. An instructor can count the number of successful experiences the student has had with a particular fact, and use that information to make predictions about whether the student will remember the fact at some later time. This conception of strength has been employed in some current memory theories (e.g., Anderson, 1983). However, as Postman (1971, p. 1124) has pointed out, a problem with this measure is that criterion attainment may reflect a "chance peak" in performance. That is, varying strength levels can result in a correct response. If fast-learning subjects accrue more strength per exposure, a successful response will on the average indicate greater strength for them than for slow learners. Further, it is already well established that fast learners retain more when learners are equated by cumulative successes (Underwood, 1954).

This disadvantage of the cumulative-success criterion may at least partly be overcome through the use of *successive success experiences* as a criterion, which we employed in the present study. Under this method, an item responded to correctly on *n* successive trials is removed from the list, and a subject continues study until all items are thus removed. The rationale is that requiring successive as opposed to cumulative successes is likely to assure less variance over individuals in strength of association at the end of acquisition. At the very least, when compared to the situation of using the cumulative success method, slow learners have an advantage in that they are likely to require more cumulative successes than fast learners before attaining the successive success criterion. Further, the answer to the question of the relationship between acquisition speed and retention using this criterion is a genuine contribution to the literature: As far as we know, no study of individual differences in retention has yet employed this method.

In any event, an advantage to any success experience operationalization, successive or not, over a successive probability operationalization, is that questions concerning the indicator itself, because it is observable, have applied significance. By centering an investigation around successive mastery experiences as a strength measure, we will be able simultaneously to address the practical question of whether those who are observed to master material quickly remember that material better

INDIVIDUAL DIFFERENCES IN RETENTION

Even if learning speed is found to be unrelated to retention, it is a separate question as to whether there are any reliable individual differences in retention. Some past investigations (e.g., Shuell & Keppel, 1970, p. 64) have inappropriately concluded that there are no individual differences in retention on the basis of their failure to find a relationship between learning speed and retention.

The existence of individual differences on a measure is normally determined by the reliability of a measure. But classical reliability theory assumes independence of trials and is thus inappropriate for determining individual differences in learning speed or retention tasks where trial independence is almost certainly violated. An alternative possibility for determining individual differences in retention is to consider whether any cognitive factor is related to retention. In this study, in addition to considering the relationship between learning speed on a set of items and retention of those items, we also consider whether retention is related to learning speed measured on an independent set of items (across a variety of associative learning formats), general reasoning proficiency, general knowledge, and memory span. The inclusion of these additional cognitive measures allows us to test a variety of process models relating cognitive factors to both retention and learning speed.

To summarize, despite the existence of an extensive literature on both the relationship between acquisition and retention, and individual differences in learning, we still lack answers to fundamental questions of how learning outcomes are related to subject and task characteristics, and we thus are hindered in the development of a practical system for predicting learning outcomes in instruction. The purpose of the present study was to examine the relationship between item-specific learning skill, general learning skill, and other cognitive factors in predicting retention on a paired-associates learning task. Subjects were taught name–number associations to various criteria (from one to three successive successes), using an item drop-out procedure, then were tested on the original associations. Between acquisition and the time of the retention test, subjects were administered an interfering learning task, in an A–B, A–Br design. We varied criteria on the A–Br task from zero to three successive successes. The purpose of the interference task was to permit an examination of the generality of any acquisition-retention relationship found. The first question we addressed in the analysis was, does learning speed, as indicated by performance on a set of items, predict retention of those items? A second question addressed was, does learning speed, as indicated by performance on a set of unrelated items, administered in a variety of formats, predict retention of the first set of items? A final question addressed had to do with whether retention can be predicted by other cognitive factors, such as general knowledge, reasoning proficiency, and memory span.

METHOD

Subjects

Subjects were 710 military recruits on their 6th day of basic training at Lackland Air Force Base, Texas. The sample was 78% male, 78% Caucasian (15% Black, 3% Hispanic), 99.2% high school graduates, and 28% with at least some college. The mean Armed Forces Qualification Test percentile score for the sample was 64.4 with standard deviation of 17.0. The AFQT has been calibrated on a national probability sample of 16- to 23-year-old American youths (OASD/MRA&L, 1982).

Testing Stations

The testing facility consisted of 30 testing stations in a large room. Each station was a TERAK 8510a microcomputer system with disk drives for test item and response data storage, a standard keyboard for response entry, and a medium resolution (320 × 240) black and white video monitor for timed presentation. Millisecond timing for stimulus presentation and response latency recording was achieved with an algorithm developed by Armstrong (1984). All test materials, including items, response scoring and recording procedures, and feedback presentation procedure, were written in PLATS, a high-level cognitive task authoring system (Walker, 1985). Data compilation at the end of a session was accomplished by a network system tying the 30 TERAKs to a PDP 11/34 minicomputer for transfer from the floppy disks to standard magnetic reel-to-reel tape.

Experimental Tasks

Subjects were administered a paired-associates learning test and a battery of computerized reference tests, which consisted of three short-term memory tests (Memory for Digit Order, Digit Span, Missing Digits), three verbal learning tests (Paired-associates, Memory for Classes, Free Recall), and three reasoning tests (Three-term Series, Letter Series, Number Sets). A description of these tests follows. From previous military entrance testing, we also had available subjects' Armed Services Vocational Aptitude Battery (ASVAB) subtest scores. In the analysis, we employed three of those scores, from the Word Knowledge, Paragraph Comprehension, and General Science subtests, as indicators of general knowledge.

Experimental Criterion Task: Paired Associates

Task Description. The task consisted of three sets of trials, A–B, A–Br, and A–B (retest) of 13 name–number pairs (e.g., Jones–13), administered in a modified anticipation method. Pairs were formed by randomly matching common surnames to two-digit numbers for the A–B pairs, then randomly rearranging the pairings of the same names and numbers for the A–Br pairs (e.g., Jones–97). In an initial study phase, pairs were presented successively for 4 s each. In an immediately following test phase, names were presented (e.g., Jones-?), and the subject responded by typing in the associated number. Regardless of whether the subject answered correctly, the computer displayed the correct associations for 1 s (e.g., "The correct answer is Jones–13"). Pairs were randomly reordered after each cycle through the list. Pairs were dropped from the list if the subject correctly responded, or if the subject correctly responded on two or three successive cycles through the list, depending on the experimental condition. Subjects were tested until all 13 of the list pairs were dropped. This procedure was followed first for the A–B set, then for the A–Br set. After subjects completed the A–Br set, they were retested on the original A–B pairs to a criterion of two successive correct recitations.

Design. The two major between-subjects factors were degree-of-learning on the A–B and A–Br lists, manipulated independently. On the A–B list, the criterion for eliminating items was 1, 2, or 3 successive correct recitations. On the A–Br list, a 0 condition was added to these three. Those in the 0 condition on the A–Br list took a break for either 6, 12, or 18 minutes. Thus there were a total of 18 cells. For each of the three blocks, A–B (initial), A–Br, and A–B (retest), two scores were computed: percent correct (PC) and trials-to-criterion (TTC). Percent correct was computed over the first 13 items immediately following study, to produce a score comparable over cells. Because of the way in which items were cycled (all 13 items were tested before any pair was tested a second time), this reflects the amount learned during the first study phase, for both the A–B and A–Br block (there was no additional study phase before the A–B retest block). Trials-to-criterion cannot be easily interpreted over cells because different cells had different criteria. In an initial inspection of the data we saw no effect for break duration (for those who did not learn the A–Br list) on the A–B retest, for either percent correct, $F\ (2,220) < 1$, or trials to criterion, $F\ (2,220) = 1.7$, $p > .10$. Thus the three levels of this factor were collapsed over for the analyses.

Verbal Learning Tasks

Memory for Classes (MC). This task consisted of 9 trials of 5-word sets presented in the study-test method, and requiring recognition judgments. Word sets were formed with words sharing a common semantic property (e.g., names of alcoholic beverages: gin, beer, whiskey, wine, rum). Subjects studied the word set for 10 s. In the test phase, which followed the presentation of all sets, subjects were asked to determine whether singly presented words (a) had actually been presented (verbatim recognition), (b) were class members that did not actually appear (implied class member recognition), or (c) were not a member of any class presented (correct discrimination). The task was modeled after reference tests for Brown, Guilford, and Hoepfner's (1968) Memory-for-Semantic Classes factor. Subjects received 3% correct scores, corresponding to their accuracy with respect to each of the three kinds of judgments required.

Paired-Associates (PA). This paired-associates learning task consisted of two 14-pair lists (28 items) of consonant–vowel–consonant (CVC) trigrams paired with simple English words administered in a study-test method. During study, pairs were successively presented for 3.5 s each. During the immediately following test, subjects were shown a stimulus term and five alternatives from which they were to select the synonym of the correct associate. Synonyms were used to increase the likelihood that retrieval rather than simply recognition processes were tapped by the test. Distractors were synonyms of other same-list associates. Within each list, half the pairs were designed to be highly mnemonic (e.g., DUP–COPY), and subjects were encouraged to use a semantic elaboration strategy to memorize these pairs. Subject's score was percent correct.

Free Recall (FR). The task consisted of a 70-item categorized word list, with 3 high-, 1 medium-, and 3 low-frequency instances per each of 10 categories selected from the Battig–Montague (1969) norms. Word order varied across subjects. Words were successively presented for 2 s each. Following the last word, subjects were given 5 min to write all the words they could recall on response sheets with numbered blanks. Subject's score was the number of words correctly recalled.

Memory Span Tests

Memory for Digit Order (MO). Subjects were presented a string of 7 digits simultaneously for 3 s. After a 1-s pause, a probe string was displayed and subjects were to indicate whether the probe matched the study string. Mismatches (half the probes) were created by randomly transposing adjacent digits. There were 80 trials total.

Digit Span (DS). Subjects were presented 5, 7, or 9 digits successively for 1 s each with no inter-stimulus–Interval (ISI). Digits were displayed left-to-right, 2 characters apart on the display screen. Immediately after presentation, probe digits were presented from either the beginning or end of the list. The subject's task was to indicate (by pressing either the L or D key) whether the probe matched the presented digit. After the subject responded, a probe for the next to last digit was presented and subjects responded in the same fashion. On half the items, probes were from the end of the list, and on the other half, from the beginning of the list. From 2 to 5 probes were presented for each list. Subjects were given item credit only if they correctly discriminated all probes on the item. Items were blocked by digit string length and were presented in order (5-digit items first, followed by 7- then 9-digit items); there were 10 items per block and 3 blocks, for a total of 30 items.

Missing Digits (MD). Subjects were presented 9 digits successively for 0.5 s each with a 0.5 s ISI. Digits were displayed at the same spatial location on the screen. After a 2-s pause, 8 digits and a blank representing the 9th digit were displayed in a format spatially consistent with temporal presentation order. Subject's task was to key-in the missing digit. Two blocks of 27 items, for a total of 54 items were presented.

Reasoning Tests

Number Sets (NS). On each trial, subjects were presented 4 sets of 3 digits in which the task was to select the set whose elements did not obey the rule characterizing the relationship among elements in the other 3 sets. Typical relationships were identity and succession. Subjects selected the odd set by keying-in the number corresponding to the spatial position of the set (1, 2, 3, or 4). There were 20 trials.

Letter Series (LS). Subjects were presented letter sequences which required extrapolating a pattern to predict the final letter in the series. An example item is R S R T R U R V R __. The sequence was displayed until the subject hit the space bar on the keyboard, which blanked the screen. The subject then keyed-in the predicted letter. There were 20 letter sequences.

Three-term Series (3T). Subjects were presented linear syllogism problems with a multiple-choice response, such as: Dick is better than Pete, John is worse than Pete, Who's best? (1) Dick, (2) John, or (3) Pete. The task was to key-in the number of the correct alternative. Two kinds of relations (goodness, tallness) were used with the same three terms, and problems varied on voice, negation, adjective markedness, and congruence as in other studies (e.g., Huttenlocher, 1968). There were 64 problems.

Procedure

A proctor briefed subjects on the purposes of the testing, then assigned them testing stations. A computer program provided subjects practice in using the keyboard. Subjects were first administered the paired-associates task according to cell assignment. Subjects were then administered the 9-task reference battery in either the following order or its reverse: Memory for Digit Order, Paired-associates, Number Sets, Digit Span, Memory for Classes, Letter Series, Missing Digits, Free Recall, Three-term Series. Two 5-min breaks were inserted at various points during the session (in addition to the break for those in the A–Br = 0 condition). Sessions lasted from 2-3 hr. After the session, subjects left their testing stations and were accompanied to another room where they waited for all subjects to finish.

RESULTS

Of the 710 subjects administered the lists, 25 failed to complete all three lists in the required time, and were thus dropped from the analysis (subjects eliminated this way appeared from informal inspection to be evenly distributed over the design cells). Table 1 presents descriptive statistics of the list learning variables by design cell for the remaining N = 685 subjects.

TABLE 1
Descriptive Statistics for Criterion Task by Cell

Criterion List			List 1 ($A-B_1$)		List 2 (A–Br)		List 3 ($A-B_2$)	
$A-B_1$	A–Br	N	PC	TTC	PC	TTC	PC	TTC
1	...	241	23.7	70.4	29.2	***	21.5	129.0
	0	81	22.1	77.8			32.9	117.9
	1	54	24.2	68.4	30.9	47.2	23.2	137.7
	2	53	23.4	68.0	28.3	142.2	15.4	131.0
	3	53	25.0	67.5	28.4	169.2	14.3	129.5
2	...	236	26.9	136.0	32.1	***	38.2	79.7
	0	81	25.3	143.2			58.4	63.0
	1	54	28.2	128.9	35.2	47.1	38.5	73.7
	2	47	28.8	128.2	32.5	108.4	31.4	88.8
	3	54	25.2	143.7	28.5	153.2	24.5	93.3
3	...	232	26.3	172.1	34.1	***	47.6	68.0
	0	70	22.3	179.8			67.8	47.6
	1	54	23.9	165.6	30.2	57.5	44.7	68.6
	2	54	26.6	193.9	32.8	115.4	37.4	84.7
	3	54	32.5	148.9	39.3	141.2	40.6	71.2
...	...	709	25.6	***	31.8	***	35.6	92.6
...	0	232	23.2	***			53.1	76.2
...	1	162	25.6	***	32.1	50.6	35.5	93.3
...	2	154	26.3	***	31.2	122.0	28.1	101.5
...	3	161	27.6	***	32.1	154.5	26.5	98.0

Note. PC = Percent correct; TTC = Trials-to-criterion. Triple dots indicate harmonic means over cells in the collapsed-over condition; asterisked entries appear for cases in which collapsing is over trials-to-criterion where criteria vary; nonexistent cells are left blank.

Analysis 1: Learning Speed, Item Strength, and Retention

The central question addressed in the first analysis can be expressed as follows: Do those who learn a set of items quickest remember those items best? According to the constant forgetting model, there should be no difference between fast and slow learners in retention, once the two groups are equated for strength of association. An alternative is that learning speed itself is at least partly determined by resistance to forgetting, and thus fast learners forget more slowly, and hence should show better retention.

To address this question, we constructed two measures of remembering on List 3, number correct (NC_3) on the first pass (0–13), which measures retention, and trials-to-criterion (TTC_3), which measures relearning. We also constructed two indicators of learning speed: number correct (NC_1) and trials-to-criterion (TTC_1) on List 1. Number correct was computed over the first pass through List 1 (i.e., on the first 13 A–B pairs) immediately following study, and thus is comparable over design cells. Trials-to-criterion is not comparable over design cells. For those in the Criterion 1 condition the minimum TTC_1 score is 13; for those in the Criterion 3 condition the minimum score is $3 \times 13 = 39$. To produce a TTC_1 score comparable over cells, we divided subjects evenly into 15 categories ordered by TTC_1 within each of the three List-1 criterion conditions. Thus the 6.5% fastest subjects in the Criterion-1 group were assigned a TTC_1 score of 1, as were the 6.5% fastest subjects in the Criterion-2 group, and the 6.5% fastest subjects in the Criterion-3 group. The 6.5% slowest subjects in the Criterion-1 group were assigned a TTC_1 score of 15, as were the 6.5% slowest subjects in the Criterion-2 group, and so forth. TTC_1 scores were then normalized with zero mean and unit variance.[1]

An inspection of the distributions of the learning variables showed that NC_1 and NC_3 were close to being normally distributed (skewness = 0.94, 0.57, kurtosis = 0.80, −0.33, respectively, where the standard error for these two statistics with $N = 685$ is approximately 0.20 [skewness] and 0.10 [kurtosis]). However, TTC_3 was highly positively skewed (skewness = 2.32; kurtosis = 8.21). Log TTC_3 was more normally distributed (skewness = 0.37; kurtosis = −0.27), and thus was substituted for TTC_3 (subsequent references to TTC_3 refer to log TTC_3).

The full model for retention (as represented by TTC_3) is

$$\begin{aligned} TTC_3 = {} & \text{constant} + b_1(TTC_1) + b_2(NC_1) + b_3(A1) + B_4(A2) + b_5(B1) + \\ & b_6(B2) + b_7(B3) + b_8(A1B1) + b_9(A1B2) + b_{10}(A1B3) + \\ & b_{11}(A2B1) + b_{12}(A2B2) + b_{13}(A2B3) + e_1 \end{aligned} \tag{1}$$

[1]Normalization was accomplished with Jöreskog and Sörbom's (1986) *Prelis* program.

where the bs are the regression weights for each of the variables, A1 and A2 are dummy variables that code List-1 criterion (i.e., strength), B1–B3 code strength of the interfering (A–Br) association, and A1B1–A2B3 code the List 1 by List 2 interaction. A restriction on this model is to let $b_1 = b_2 = 0$ (Model 2). Comparing these two models ($SS_{error}1 = 16.484, ISS_{error}2 = 14.444, SS_{tot} = 44.614$)[2] gives $F(2,671) = 294.0$, which suggests that learning speed affected retention even after controlling for strength. An identical analysis for NC_3($SS_{error}1 = 2356.5, ISS_{error}2 = 1618.1, SS_{tot} = 7081.7$) yielded $F(2,671) = 230.4$. (Note that these analyses were not empirically independent in that $r\ [NC_3, TTC_3] = -.80$.)

We also noted in conducting this analysis that setting the restriction $b_2 = 0$ on Model 1 did not affect model fit for TTC_3, $t(1,671) = -0.3$, although it did slightly for NC_3, $t(1,671) = 3.05$. That is, number correct on List 1 added slightly to the prediction of retention (NC_3), but not at all to relearning.[3] Insofar as TTC scores are determined by a greater sample of behavior, TTC is probably the more reliable score.[4] TTC can also be justified a priori as the more sensitive indicator of learning speed. Thus to simplify all further analyses, as well as their interpretation, we dropped NC_1 as a learning speed measure, and used only TTC_1 (i.e., we defined a Model 2 as Model 1 with the restriction that $b_2 = 0$). Nevertheless, it is worth noting that although the two learning speed measures (TTC_1, NC_1) are highly correlated ($r = -.71$), the fact that number correct uniquely contributes to the prediction of retention indicates that it measures a slightly different aspect of learning speed than is reflected in trials to criterion, a difference not accounted for in terms of the differential reliability of the two measures.

Accepting that there is an effect of learning speed on retention, a question is whether the effect is constant over variations in item strength. A weak version of the constant forgetting model might predict that at low strength (e.g., Criterion 1 condition) fast learners could forget less because the criterion manipulation itself is not a sufficient guarantee of equivalent associative strength (i.e., true strength is highly variable at low criteria). The prediction then would be that only at high strength (e.g., Criterion 3) there should be no relationship between learning

[2]We report ISS_{error} which is the increment in SS_{error} over the full model when the restrictions are imposed. Thus SS_{error} for the restricted model is SS_{error} for the full model plus ISS_{error}. Full model R^2 is of course $(SS_{tot} - SS_{error})/SS_{tot}$; increment in R^2 is ISS_{error}/SS_{tot}.

[3]We note in passing that an optimal weighting of TTC_1 and NC_1 in predicting retention was approximately 4 (TTC_1) to 1 (NC_1).

[4]Actually estimating the reliability of trials to criterion is conceptually difficult. Classical reliability theory assumes statistical independence of trials, which certainly does not hold in learning data. Bush and Lovejoy (1965, reported in Cronbach & Snow, 1977) claimed that TTC was an unreliable variable, highly sensitive to chance, but this conclusion derived from the particular procedure they used to generate learning data (viz., they assumed a strong theoretical model). We made no attempt to compute a reliability estimate for TTC.

speed and retention. More generally, there should be an interaction between strength and the learning speed–retention relationship.

This hypothesis can be evaluated by redefining the full model (Model 2) to include additional terms coding the 11 interactions between learning speed (TTC_1) and all $k - 1$ cell variables (A1–A2B3). Call this Model 3, which is defined for TTC_3 as

$$TTC_3 = \text{constant} + b_1(TTC_1) + b_2(A1) + b_3(A2) + b_4(B1) + b_5(B2) + b_6(B3) + b_7(A1B1) + b_8(A1B2) + b_9(A1B3) + b_{10}(A2B1) + b_{11}(A2B2) + b_{12}(A2B3) + b_{13}(TCC_1{*}A1) + b_{14}(TTC_1{*}A2) + b_{15}(TTC_1{*}B1) + b_{16}(TTC_1{*}B2) + b_{17}(TTC_1{*}B3) + b_{18}(TTC_1{*}A1B1) + \ldots + b_{23}(TTC_1{*}A2B3) + e_2 \quad (2)$$

Comparing the fit of Model 3 to Model 2 is then a test of the hypothesis of a constant retention-on-learning speed regression slope over design cells. A comparison for TTC_3($SS_{error}3$ = 15.998, $ISS_{error}1$ = .489) yielded $F(11,661) = 1.84$, $p = .045$, and for NC_3 ($SS_{error}3$ = 2241.4, $ISS_{error}1$ = 147.8), $F(11,661) = 3.96$. The fact that these two effects are small (R^2 change = .011 [TTC_3], .021 [NC_3]) suggests that it is reasonable to assume constant retention on learning speed slopes. Still, it could be informative to track down what effects may exist.

First, because the three-way interactions are difficult to interpret anyway, we compared Model 3 with a Model 4 that sets the regression weights for all the three-way interactions to zero ($b_{18} = b_{19} = \ldots = b_{23} = 0$). Comparison of these two models showed no loss due to this assumption, for either TTC_3 ($ISS_{error}4$ = 0.249), $F(6,661) = 1.72$, $p = .11$, or NC_3 ($ISS_{error}4$ = 23.72), $F(6,661) = 1.17$, $p = .32$. Thus we redefined the full model to be Model 4 (with $df = 667$, and $SS_{error}4$ = 16.247 [TTC_3], 2265.1 [NC_3]). A Model 5 then can be formulated, that assumes that the retention on learning speed slope is constant over changes in item strength ($b_{13} = b_{14} = 0$). This assumption appears safe for TTC_3, $ISS_{error}5$ = 0.0005, $F(2,667) < 1$, but probably not for NC_3, $ISS_{error}5$ = 30.14, $F(2,667) = 4.44$, $p = .012$. A Model 6 can be formulated that assumes that retention on learning speed is constant over changes in strength of the interfering item ($b_{15} = b_{16} = b_{17} = 0$). A test of this model versus Model 4 indicated changing regression slopes according to degree of interference, whether retention was measured as TTC_3 ($ISS_{error}6$ = 0.227, $F(3,667) = 3.10$, $p = .026$, or NC_3, $ISS_{error}6$ = 99.77, $F(3,667) = 9.79$.

To get a sense for what this means, consider Figure 1 which plots expected (a) NC_3, and (b) TTC_3, as a function of learning speed, trace strength (List 1 criterion), and interference (or competing trace strength, i.e., List 2 criterion). The curves for fast, medium, and slow learners were obtained by setting learning speed (TTC_1) to +1, 0, and −1, respectively, using Model 4 as the plot-

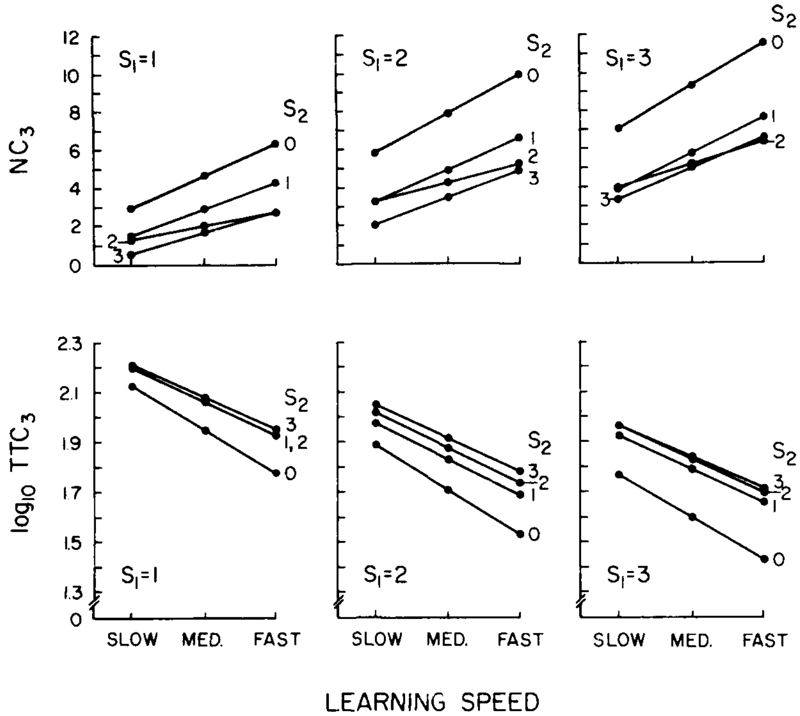

FIG. 1 (a) Retention (NC_3) and (b) relearning (TTC_3) as a function of learning speed, separately for initial ($A-B_1$) strength levels 1 (leftmost) to 3 (rightmost); different lines within a graph represent different strength levels for the interfering (A–Br) list.

generating equation (recall that TTC_1 is normalized and thus these values represent learners 1 *SD* above, at, and 1 *SD* below the learning speed mean). Note that the ordinate values for TTC_3 in Figure 1b are $\log_{10}$ TTC_3.

In all conditions it can be seen that learning speed affected both retention and trials to relearning; in no condition was the retention on learning speed slope flat. Further, learning speed was particularly important when trace strength was maximized (S_1 = 3) and there was no interference (S_2 = 0). These results are inconsistent with the weak version of the constant forgetting model.

Also note in Figure 1a the source of the learning speed by interference level interaction: The retention on learning-speed slope is slightly less steep with moderate amounts of interference (i.e., in the criterion = 2 condition for List 2) across all strength levels. This interaction may at least partly be understood as follows. Assume that an intermediate degree of strength of an interfering trace produces maximal interference (i.e., one's knowledge of Columbus–1492, presumably a very strong trace, does not interfere with new name–number associations). Call this value of strength m. For fast learners, strength $= m$ for interfering traces that had been taken to Criterion 2. For slow learners, strength only approaches m for traces that had been taken to Criterion 3. Assuming that the criterion manipulation equates for strength initially, the differences in criterion needed to reach strength $= m$ at retention time then simply reflect differences between fast and slow learners in forgetting rate.

The evidence thus far suggests that fast learners forgot less regardless of degree of learning or degree of interference. However, there is a potential confound: Slower learners may have been subject to more retroactive interference independent of that associated with the A–Br list. Specifically, more A–B_1 items may have intervened on average between drop-out (final study) and test for a particular item for slow subjects. And in fact, although there is no necessary relationship between the learning speed score (TTC_1) and the average number of trials between drop-out and test, we did find a high correlation between these two variables in our data ($r = .78$).[5] This is retroactive interference due to the item drop-out procedure we employed.

To test the importance of any retroactive interference (RI) effects that resulted from our drop-out procedure, we constructed two RI scores, one for A–B_1 and one for A–Br, to allow for differential interference effects for the two lists. RI (A–B_1) was the average number of items intervening between drop-out for each of the 13 items and the trial at which the final item dropped out. RI (A–Br) was simply trials to criterion for A–Br. Because both these variables were highly positively skewed we took their respective logarithms for the analysis (with skewness = 0.20, 0.02; kurtosis = 0.20, −1.47 for List 1 and List 2 RI, respectively).

The question we addressed was as follows: Does learning speed predict retention after taking out all effects due to retroactive interference? We defined a new full model (call this Model 7) as Model 1 (i.e., constant retention on learning speed slopes over conditions), with the two additional RI variables (for TTC_3, $SS_{error}1 = 14.98$, $SS_{tot} = 44.614$; for NC_3, $SS_{error}1 = 2311.40$, $SS_{tot} = 7081.71$). For TTC_3, comparing this model with one that did not include the RI effects ($ISS_{error} = 1.51$), gave $F(2,670) = 33.66$, showing that there was a retroactive interference effect on retention due to the drop-out procedure. The same result obtained with NC_3 as the dependent measures ($ISS_{error} = 77.79$), $F(2,670) = 11.27$. But more importantly, we found that even after accounting for all retroactive interference effects, learning speed (TTC_1) still predicted retention whether measured as TTC_3 ($ISS_{error} = 77.79$), $F(2,670) = 11.27$, or NC_3 ($ISS_{error} =$

[5]This is actually the correlation between TTC_1 and the logarithm of the average number of intervening trials between dropout and finishing the first list. The correlation is attenuated considerably (to $r = .18$) when considering A–Br trials due to the fact that the latter value is largely due to cell assignment.

108.49), $F(1,670) = 31.45$. Thus our finding of a relationship between learning speed and retention cannot be attributable to differential amounts of retroactive interference for fast and slow learners.

Analysis 2: Item-specific or General Associative Learning Effects?

Our first set of analyses demonstrated that those who reached criterion on a set of items quickest remembered those items best. We would like to assume that speed of criterion attainment reflected learning speed. But it is possible that our criterion attainment measure did not merely reflect the integrity of the associative learning mechanism per se: It may also have reflected pre-experimental associations. This seems fairly unlikely to be the case. We attempted to minimize any large prior learning component by using arbitrary surname–number pairs. But it is still possible that idiosyncratic factors might have made List 1 learning easier for some subjects. Then this accidental advantage could have been responsible for these subjects' enhanced recall and relearning on that same list.[6] Even if pre-experimental associations per se seem implausible as a grounds for explaining our results, one might still argue that some kind of idiosyncratic advantage, whether due to item or, more generally, method factors (e.g., the type of test, the use of name–number pairs) could have played a role.

The purpose of the second analysis was to address this ambiguity in our findings. We repeated the main analysis (Model 1 vs. Model 2 test), only this time, rather than taking trials-to-criterion on the list used in the retention test as the learning speed measure, we estimated learning speed with different items, administered in a variety of formats—free recall, cued recall, and recognition. What is common over the three tests used is that they all reflect the integrity of the associative learning mechanism. Thus the second analysis addressed the question of whether those who in general learn fastest retained more.

Table 2 displays descriptive statistics for the three associative learning measures used in this analysis (as well as the other cognitive measures used in Analysis 3), separately for subjects administered the two task orders. There is some evidence for a general fatigue effect (all cognitive tasks were administered after the three-list learning task), but the only case in which the effect appears to have substantially affected scores was on the Free Recall task. An analysis of the Free Recall protocols revealed a number of intrusion errors for those subjects who had taken the Memory for Classes test prior to this one (i.e., Order 1 subjects). To adjust for this order effect, because the standard deviations for the two groups were approximately the same, we simply added the Order 2–Order 1 difference (= 4.77) to the Free Recall scores of the Order 1 subjects.

Table 3 presents distribution statistics for these same tasks collapsing over

[6]This possibility was pointed out to us by Norman Slamecka, who reviewed a previous draft of this report.

TABLE 2
Order Effects on Cognitive Tasks

Test		Order 1 N	Order 1 M	Order 1 SD	Order 2 N	Order 2 M	Order 2 SD	Difference M	Difference F
MS:	Memory-Digit Order	412	.706	.086	183	.686	.085	.020	6.90
MA:	Paired Associates	412	.657	.144	197	.656	.156	.001	0.00
R:	Number Sets	407	.613	.213	204	.650	.199	−.037	4.20
MS:	Digit Span	406	.754	.091	206	.745	.110	.009	1.21
MA:	Memory for Classes	403	.552	.113	211	540	.118	.012	1.50
R:	Letter Series	401	.626	.171	214	.667	.155	−.041	8.71
MS:	Missing Digits	394	.626	.162	215	.658	.132	−.032	6.29
MA:	Free Recall	342	11.09	7.23	207	15.86	7.44	-4.77	54.97*
R:	3-term Series	334	.847	.150	215	.818	.137	.029	5.11

Notes. Subjects were administered cognitive tasks in one of two orders: Order 1 is as listed (Memory for Order . . . 3-term Series); Order 2 is its reverse (3-term Series . . . Memory for Order). Discrepencies in N per order are due to chance. Discrepencies in N within order are due to session time limits. MS = Memory Span; MA = Associative Memory; R = Reasoning.

*$p < .01$, adjusting for 9 comparisons.

order. For three of the tasks (Free Recall, Digit Span, and Three-term Series), distributions of transformed scores were more normal than those of the raw scores; transformed scores were used in all subsequent analyses. Note that for all tasks, $N < 685$; this is due to some subjects not being able to complete all tasks in the session time. For the present analysis, only the data from the $N = 530$ who completed all three associative memory tasks as well as the criterion task were analyzed.[7]

The model for retention was identical to Model 1, except that scores on the three associative learning measures were substituted for scores on the TTC_t measure (thus, the model consisted of $k - 1 = 11$ design variables and 3 learning speed variables). For the retention measure (NC_3), $SS_{tot} = 5500.5$, $SS_{error} = 2807.2$, $ISS_{error} = 209.4$, $F(3,515) = 12.80$, $p = .0000$. For the relearning measure (TTC_3), $SS_{tot} = 31.28$, $SS_{error} = 18.95$, $ISS_{error} = 1.89$, $F(3,515) = 17.10$, $p = .0000$. Thus we can reject the idea that our finding of a relationship between learning speed and retention was due to idiosyncratic factors associated with the acquisition of the list on which retention scores were computed. Learning speed measured on one set of items predicted retention on a different set.[8]

[7]Note that this exclusion works against us in establishing any relationship between the learning speed and retention measures in that we are restricting the range of individual differences on (presumably) both measures by eliminating slow subjects, that is, those who did not work fast enough to finish all the tasks in the session time.

[8]With Model 6 as the full model, however, the associative memory variables did not add to the prediction, $F[2,513] = 2.13$, $p = .0955$, for TTC_3, and $F[2,513] = 1.22$, $p = .3008$, for NC_3. This is due to the high correlation between the associative memory measures and the retroactive interference variables. An argument can be made that this is not a fair test of the hypothesis of the learning

TABLE 3
Descriptive Statistics for Cognitive Tasks

	N	*M*	*SD*	Skewness	Kurtosis
Associative Memory Tests (MA)					
Memory for Classes (MC)	614	.548	.115	−.14	.11
Paired Associates (PA)	609	.657	.148	−.34	−.15
Free Recall (FR)	549	15.9	7.3	.72*	.25
(square root [FR + 1])				.21	.36
Memory Span Tests (MS)					
Memory-Digit Order (MO)	595	.699	.086	.06	.49
Digit Span (DS)	612	.751	.098	−.97*	2.59*
(log [errors + 1])				.36	.19
Missing Digits (MD)	609	.638	.153	.31	.15
Reasoning Tests (R)					
Number Sets (NS)	611	.625	.209	.48*	.66
Letter Series (LS)	615	.640	.167	.30	.11
3-term Series (3T)	549	.836	.145	1.21*	1.29*
(log [errors + 1])		1.034	.503	.90*	.00
General Knowledge Tests (GK)					
ASVAB Word Knowledge (WK)	572	28.867	4.349	.611	.211
ASVAB Paragraph Comprehension (PC)	572	12.397	1.779	.741	.606
ASVAB General Science (GS)	572	18.157	3.555	.208	.550
Criterion Task					
NC_1	627	3.324	2.389	.955	.867
TTC_1	627	.001	.993	.015	.382
NC_1 (residualized)	627	.000	1.700	.039	.381
TTC_1 (residualized)	627	.000	.151	.388	1.233

Notes. Scores are proportion correct except Free Recall (number recalled), ASVAB raw scores, and the criterion task scores. Transformed scores for Free Recall, Digit Span, and Three-term Series were used in the analysis, but scores from the latter two tests were reflected first.

*Skewness *Se* or kurtosis *Se* > 4 (*Se* [skewness] = .10, *Se* [kurtosis] = .20, for samples of this size).

Analysis 3: Learning Effects or General Cognitive Proficiency?

The purpose of the third set of analyses was to explore further the causes of the apparent relationship between learning speed and retention. We were interested in two re'ated questions. First, does the relationship between learning speed and retention reflect the importance of learning speed per se, or is the relationship due to some third mediating variable. In particular, are other, more general

speed–retention relationship. Learning speed measured by the general associative learning tests (LS) predicts the amount of retroactive interference (RI_1) experienced ($r = .235$), and this portion of the learning speed variables is partialled out of the prediction of retention. Still, this result complicates our interpretation.

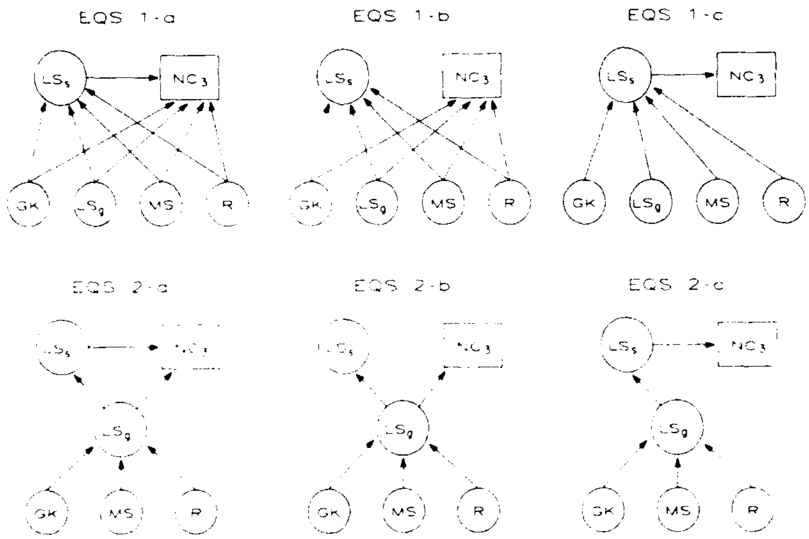

FIG. 2. Path diagrams of the structural equation portion of six latent variable path models. EQS-1 and EQS-2 models differ in whether LS_g mediates the relationship between the cognitive factors and the criterion task scores; the a, b, and c model variants differ in whether retention (NC_3) is determined by item-specific learning speed, the general cognitive factors, or both.

cognitive variables, such as general knowledge, memory span, or general reasoning proficiency responsible for the learning speed–retention relationship, which then arises only because both learning speed and retention are correlated with the more general cognitive variables? A second question, especially important if the answer to the first question is negative (which would enhance learning speed's role as an explanatory construct), is, what are the cognitive psychological determinants of learning speed? Is associative learning proficiency a unique factor (i.e., an independent skill), or is it related to memory capacity, general reasoning skill, or the breadth of factual knowledge available to an individual?

The interconnectedness of these questions indicates a path analytic approach, which we adopted by specifying various models and testing those models against the data using the linear structural equation modeling method. Figure 2 illustrates the structural equation portions of the models we tested, in the form of standard path diagrams. Rectangles represent observed variables, circles represent latent factors. In all models, the goal was to predict the observed retention score, NC_3.[9] (We did not model TTC_3 since it reflects both learning speed per se, and retention.) The factors in the models were initial item-specific learning speed

[9]However, to simplify the analysis we first extracted the treatment effects (i.e., List 1 and List 2 criterion). The retention scores in this analysis are unstandardized residuals.

(LS_s), indicated by NC_1 and TTC_1, and general learning speed (LS_g), memory span (MS), reasoning (R), and general knowledge (GK), each of which was indicated by scores from the associated tests listed in Table 3.

Two classes of models (EQS-1 and EQS-2) and three variants in each class (a, b, and c) are shown in Figure 2. In the EQS-1 models, the four cognitive factors, LS_g, MS, R, and GK, exert a direct influence on item-specific learning speed (LS_s) and retention. In the EQS-2 models, only LS_g exerts a direct influence on learning speed and retention; the other cognitive factors (MS, R, and GK) affect learning speed and retention only through the mediating influence of general learning speed (LS_g). The EQS-2 models are thus more parsimonious descriptions of the process by which performance in the specific learning context is determined by general cognitive skills.

The a-model in each class specifies that retention is due to both item-specific learning speed and a general cognitive factor (or factors). The b-model imposes the restriction that there is no link between item-specific learning speed and retention. Thus a comparison between the a- and b-models addresses the question of whether a third underlying variable accounts for the relationship between initial learning speed and retention.

The c-model imposes an alternative restriction on the a-model: that the general cognitive factors exert only an indirect rather than a direct influence on retention. Thus a comparison between the a- and c-models addresses the question of whether the general factors have any direct influence on retention after the influence due to item-specific learning speed is accounted for.

We fit these models to the variance–covariance matrix of all cognitive task scores using Bentler's (1985) EQS computer program. (The correlation matrix for these scores is given in the Appendix.)[10] Note that this covariance matrix only reflects scores from the $N = 437$ subjects who completed all tasks, however (we used list-wise deletion). Table 4 presents the results of the model comparisons. Shown are (a) the degreees of freedom for each model (the number of elements in each covariance matrix minus the number of free parameters specified by the model); (b) the chi-square goodness-of-fit statistic, which reflects the difference between the input covariance matrix and the recovered matrix; (c) the Bentler–Bonett (1980) normed fit index (NFI) and non-normed fit index (NNFI), which compare the goodness-of-fit of the model with the goodness-of-fit for a null model specifying complete independence among variables (neither index depends on sample size); and the (d) mean standardized residual and (e) maximum standardized residual in the recovered matrix (each variable and factor is standardized to unit variance, putting residuals in the correlation metric).

Consider first the within-class comparisons. Comparing a- and b-models

[10]Raw data and the variance–covariance matrices used in this or any of the analyses are available from the first author as ASCII files on 5¼-in. 360KB or 1.2MB diskettes formatted under MS/PC-DOS. Hard-copy listings also could be made available.

TABLE 4
Statistics from EQS Model Comparisons

Model	*df*	χ^2 (ML)	χ^2 (RLS)	NFI[a]	NNFI	Standardized Residuals *M*	Standardized Residuals Max
Model 0 (null model)	105	1816.505					
Model EQS-1a	76	158.423	151.106	.913	.933	.0290	.114
Model EQS-1b	77	171.927	163.280	.905	.924	.0306	.113
Model EQS-1c	80	165.057	157.744	.909	.935	.0304	.115
Model EQS-2a	82	179.271	171.643	.901	.927	.0349	−.114
Model EQS-2b	83	220.999	209.555	.878	.898	.0394	.286
Model EQS-2c	83	179.589	172.034	.901	.929	.0353	−.113

Notes. ML = Maximum Likelihood estimates; RLS = Reweighted Least Squares estimates; NFI = Bentler–Bonett (1980) Normed Fit Index; NNFI = Bentler–Bonett (1980) Non-Normed Fit Index, which takes into account degrees of freedom of the model.

[a]Bentler (1985) suggests that NFI, NNFI > .90 is "desirable."

shows that the additional 1 *df* restriction of the b-model results in a much poorer fit to the data, in both the Class 1 and Class 2 cases. This is reflected in a large *increase* in χ^2, a drop in the NFI and NNFI, and an increase in the mean size of the residuals. And in the Class 2 case, the maximum residual, which happens to be the learning speed–retention correlation, is quite high. On this basis we can reject the hypothesis that the learning speed–retention relationship is solely due to a third common factor.

Comparing c- and a-models shows that the additional restrictions of the c-models do not lead to a greatly decreased fit. In both the Class 1 and Class 2 cases, at least one of the fit indexes, the NNFI, improves when the restriction is imposed. Further, the chi-square difference test shows that the decreased fit is not high compared to the gain in degrees of freedom in either the Class 1 case, χ^2 (4) = 6.634, $p > .10$, or the Class 2 case χ^2 (1) = 0.318, $p > .10$. In the latter case (Model EQS-1c), a test of the NC_3-on-LSg regression weight ($b = -.055$; se = .098) also shows it not to be substantially different from zero, $z = -0.564$, $p < .001$. On this basis we can reject the hypothesis that general cognitive factors have an effect on retention beyond that due to item-specific learning speed. The c-model in each case appears to be the best of the three descriptions of the learning-retention process.

It is more difficult to select between EQS-1c and EQS-2c. By the Table 4 criteria, the chi-square difference test, χ^2 (3) = 14.532, $p = .007$, and the NNFI and NFI goodness-of-fit indices, EQS-1c appears to fit the data slightly better than EQS-2c. However, an inspection of the loadings in EQS-1c suggests that its comparably good fit may be due to capitalization on uninterpretable factors in the data. In particular, EQS-1c included a strong suppressor effect ($\beta = -.34$) in the

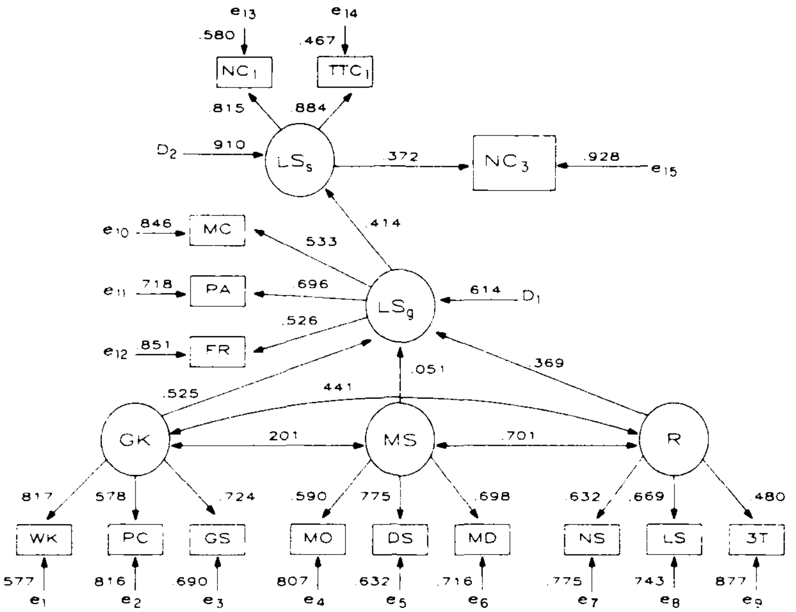

FIG. 3. Model EQS 2-c with parameter estimates from the standardized maximum likelihood solution.

LS_s-on-GK regression, whereas there were no suppressor effects in EQS-2c. Thus we prefer the EQS-2c model because it fits the data almost as well as EQS-1c, and EQS-2c yields the more parsimonious and interpretable solution.

It is useful to inspect the parameter values, presented in Figure 3, to get a sense for the relative magnitude of the various relationships in EQS-2c. With respect to the performance on the name–number paired-associates task, the relationships were not large. While 14% of the variance in retention was explained by item-specific learning speed, the remaining 86% of the variance was left unexplained. Similarly, while 16% of the variance in item-specific learning speed was explained by general learning speed, the remaining 84% of the variance in LS_s was due to unique factors. Note also that LS_s's loading on the LS_g factor was slightly lower than was the loadings of the other LS_g tests on the LS_g factor. This may at least partly have been due to the fact that our LS_g test was the only one involving numbers (as opposed to more mnemonic associates) as responses. Note that this worked against us in establishing the link between LS_g and retention on another set of items (Analysis 2), but by the same token the relative uniqueness of the LS_s test permits a stronger generalization on the relationship between learning speed and retention.

In contrast to the specific case, general learning speed seems to have been reasonably well explained (i.e., 62% of its variance) by the other cognitive factors. This was due mainly to the contribution of General Knowledge, and somewhat less to Reasoning proficiency. Memory Span did not at all predict LS_g, $z = 0.450$, $p > .30$.

Finally, it should be noted that these loadings are probably underestimates with respect to the subject population in that only those subjects who finished the entire battery were included in this analysis. Under pairwise deletion of cases, many of these relationships were somewhat larger.

DISCUSSION

Although past research has concluded that learning speed and retention are independent factors (Shuell & Keppel, 1970; Underwood, 1954), we have shown that learning speed predicts retention. In this study, those who learned a set of items quickly, retained those items better. This relationship held regardless of the degree of interfering learning experienced between acquisition and retention, both extra-list interference (A–Br) and within-list interference (RI_1). This relationship also was not due to subjects coming to the task with pre-experimental associations or any other idiosyncratic advantage. When we measured learning speed on items different in both content and format from those for which retention scores were computed, we found a substantial relationship between learning speed and retention. These results ought, at the very least, to be considered as providing boundary conditions on the assertion that there are no individual differences in retention.

Before discussing the reasons for the discrepancy, it is necessary to address one apparent inconsistency in our results. We argued that the finding, in Analysis 2, that learning speed measured on one set of items predicted retention (NC_3 and TTC_3) measured on a different set, ruled out the possibility that item-specific idiosyncratic factors were responsible for both fast learning and high retention. But in Analysis 3, we showed that general learning speed (i.e., learning speed measured on the second set of items) did not predict retention after item-specific learning speed was taken into account. A question is whether this invalidates the conclusions of Analysis 2. The answer is no, for two reasons. First, Analysis 2 stands on its own merits and provides an unambiguous answer to the question of whether learning measured on one set of items predicts retention measured on a different set. Second, the relationship between learning speed measured on List 1 and retention of List 1 includes not only idiosyncratic factors, but also the general associative learning speed component. This was indicated by the fact that general learning speed (LS_g) was found to correlate with learning speed on List 1 (LS_s). If the LS_s score was comprised only of the idiosyncratic component, it would have been found to be unrelated to LS_g. That LS_g did not contribute uniquely to the prediction of List 1 retention now can be understood as being due to LS_s reflecting both the item-specific idiosyncratic factor and the general learning component.

Returning to the question of why our results seem to be at odds with previous studies of individual differences in retention, it seems clear that a key difference lies in the way in which acquisition and retention stages of learning have been separated. It is probably widely, if not universally, agreed that separating acquisition from retention is an important step in studies investigating factors that influence retention (Slamecka & McElree, 1983). The quibbling begins over determining the best way to accomplish this.

The standard approach, at least in the individual-differences-in-retention literature, has been to equate learners on an immediate retention test, then to examine differences in performance between the immediate retention test and a delayed retention test given, say, 24 hours later. Any differences in the delayed retention test then would be taken as an indication of individual differences in retention. In successive probability analysis, there is no actual immediate retention test, but rather a projection or estimate (which has been shown to be fairly accurate; Underwood, 1964) of an immediate retention test score. In the Shuell–Keppel (1970) method, there is an immediate retention test. Whether actual or hypothetical, both kinds of studies have shown that the amount of memory loss suffered from Time 1, a minute or so after study, to Time 2, 24 hr later, is not significantly greater for slow versus fast learners, and therefore, despite the *prevalence of individual differences in acquisition, forgetting is constant.*

It should be acknowledged, however, that there is a certain amount of arbitrariness in disentangling acquision from retention. Slamecka and McElree (1983) have attempted to sort out some of the confusion by distinguishing *retention,* defined by performance on a single memory test, from *forgetting,* defined as the difference (i.e., the slope) in performance between two retention tests (to this point in this paper, we have used these terms interchangeably). In keeping with this terminology, the slope of interest in previous individual differences studies is that between retention tests given at Time 1 and Time 2 (defined above). But certainly whatever is happening to produce differential retention at Time 1 may be considered forgetting with respect to Time 0 (at encoding, where 100% retention may be assumed). Thus the question would be, does early forgetting (Time 0 − Time 1) predict later forgetting (Time 1 − Time 2)? If it does, as our study appears to show, then statistically or experimentally equating subjects for early forgetting may be seen as the reason individual differences in later forgetting have not been found.

The safest summary of past research and the current study may be this. If fast and slow learners are equated by performance on one (e.g., an immediate) retention test, then there should be no difference in performance between the two groups on a later retention test. But if learners are equated by the number of successes experienced with the learning material or, as in this study, by the number of successive successes experienced, then fast learners will retain more.

In the end, which of these two equating procedures to use may rest on pragmatic considerations. But it is also the case that past conclusions drawn from the literature on the topic, such as the following from Gentile et al. (1982) need to be tempered:

> If a "slow" learner can be brought to the same standard of performance (learning criterion) as a "faster" learner, there is no reason to expect the former to have a different forgetting curve than the latter. Clearly, further work should be done on this phenomenon to find the extent to which "fast" and "slow" learners can be equated. . . . More immediately, teachers may be happy to know that the time and effort required to teach "slow" learners to a high standard reaps the benefit of retention that is at least no worse than that of "faster" learners attaining the same standard. (p. 137)

This conclusion can now be seen as depending on the equating method used: What one means by "standard of performance" is a critical determinant of whether the conclusion holds.

On one point, however, the current study is in agreement with past research. That is, regardless of whether forgetting is constant or whether it is dependent on learning speed, we suggest that future research on individual differences in learning might most fruitfully be focused on determinants of immediate retention. This is because either there are no individual differences in forgetting, or, as this study seems to show, what reliable individual differences there are are accounted for by differences in immediate retention.

In this study, we made a start by showing that general associative learning proficiency is moderately well predicted by general knowledge and by reasoning proficiency. Memory span did not affect learning scores. The failure to find a relationship with memory span is consistent with past research (Underwood, Boruch, & Malmi, 1978; see also references therein). However, our finding of a relationship with general knowledge surprisingly has not been duplicated in all previous work. For example, Underwood et al. (1978), taking learning tasks such as those used in this study as measures of *episodic memory*, and vocabulary, spelling, and the Scholastic Aptitude Test-Verbal test scores as measures of *semantic memory*, concluded that their "episodic memory tasks and the semantic memory tasks represent two different worlds" (p. 409). An inspection of the raw correlation matrices for the two studies shows that there is not that much difference in the magnitude of the relationships of individual tests. The main difference is that by specifying latent variable models, we focused attention on the relationship between the general knowledge (i.e., semantic memory) and associative memory *factors*, as opposed to the raw test intercorrelations, which are attenuated by considerable test-specific variability. There are good reasons for believing that general knowledge should be an important factor in mediating associative learning success: Continued research in this area should explore this relationship more thoroughly.

Finally, it is useful to consider the generality of our findings, as they pertain to acquisition and retention phenomena. That is, it is important to consider whether our model is likely to prove applicable to associative learning generally, or whether it is likely to tell us something only about retention of name–number pairs administered in the standard paired-associates format.

It is certainly a limitation of our study that we investigated only the retention of name–number pairs. It is also a limitation that we administered only three learning tasks as measures of a general associative learning factor (LS_g). But there is at least some evidence that we likely would have found basically the same relationships had we used different measures of retention and acquisition. The Underwood et al. (1978) study was probably the most ambitious attempt to date to study the interrelationships among measures of associative memory. In that study, although they extracted five factors, the first of these, a paired-associates task factor, accounted for 58% of the common variance; the first two of the five factors, the second factor reflecting scores on free recall tasks, accounted for 73% of the common variance. This would appear to indicate that associative learning tasks involve at least two relatively independent factors. But in a follow-up study (Malmi, Underwood, & Carroll, 1979), most of the variance in a matrix of associative learning tasks was accounted for by a single associative learning factor: Even when two factors were forced, the two factors correlated .85. In both studies the authors conceded that general individual differences in associative learning were dominant, pointing to the theoretical importance of the mechanism governing the rate at which associations are formed, and relegating to minor status any other individual differences factor in learning. This suggests that other learning tasks would not have yielded dramatically different results from those we reported here.

But perhaps a more severe criticism to our present findings would be that the learning tasks administered, not just by us, but in the Underwood et al. (1978) study, and even in the other studies of individual differences in retention, are inherently artificial laboratory exercises. A detailed counter to this criticism is certainly beyond the scope of this paper, but there are some points worth considering. First, our finding of a fairly strong relationship between a general knowledge factor, measured by performance on tests administered outside our laboratory (at recruitment test centers), and the associative learning factor provides some support for the "ecological validity" of the learning measures. Second, studies that have used more meaningful material, such as poems (Gentile et al., 1982) and sentences (Slamecka & McElree, 1983), in addition to the more arbitrary associative material, have drawn identical conclusions regarding the effects of various factors on retention with the different kinds of material. Thus, although there is certainly a large idiosyncratic component involved in memorizing arbitrary relations, there is no compelling evidence that learning other kinds of materials in other contexts involves qualitatively different learning. At most, one might speculate that the more naturalistic learning context, compared to the one we investigated, is more dependent on the pre-experimental store of semantic knowledge a subject brings to the experimental situation.

In any event, learning ability of the kind we investigated here appears to be a fairly general ability, representing the individual's capacity for accreting and retaining new knowledge. An important next step in our work is to investigate more systematically the underlying determinants of the success of this accretive process. Our analyses of learning determinants in this study was exploratory. We did find that various measures of cognitive skill were predictive of the process, a conclusion that is at odds with the view popularized by Woodrow (1946) that learning ability and intelligence are independent abilities. But our findings are consistent with some of the more recent analyses of the learning–cognitive ability relationship (Cronbach & Snow, 1977; Snow et al., 1984).

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APPENDIX

Correlation Matrix of All Measures (Input for EQS Analysis)

	WK	PC	GS	MC	PA	FR	MO	DS	MD	NS	LS	3T	NC1	TTC1	NC3	TTC3
Word Knowledge (WK)	1.000															
Para. Comprehen. (PC)	.489	1.000														
General Science (GS)	.592	.389	1.000													
Memory for Classes (MC)	.303	.239	.283	1.000												
Paried Associates (PA)	.458	.259	.394	.340	1.000											
Free Recall (FR)	.248	.215	.247	.332	.346	1.000										
Memory-Digit Order (MO)	.160	.175	.116	.172	.223	.172	1.000									
Digit Span (DS)	.092	.065	.085	.170	.196	.124	.437	1.000								
Missing Digits (MD)	.136	.119	.058	.159	.090	252	.372	.572	1.000							
Number Sets (NS)	.174	.235	.228	.234	.292	.203	.358	.298	.271	1.000						
Letter Series (LS)	.182	.173	.268	.234	.276	.218	.331	.346	.322	.445	1.000					
3-term Series (3T)	.192	.212	.185	.147	.218	.089	.314	.280	.236	.280	.306	1.000				
NC1	.081	.105	.087	.108	.254	.211	.148	.152	.135	.148	.092	.047	1.000			
TTC1	.118	.086	.140	.196	.262	.289	.193	.191	.190	.241	.224	.178	.720	1.000		
NC3-residualized	.048	.151	.051	.047	.098	.116	.064	.047	.019	−.029	.014	.034	.341	.308	1.000	
TTC3-residualized	−.037	.058	−.064	−.090	−.068	−.069	−.088	−.131	−.164	−.152	−.162	−.099	−.070	−218	.698	1.000
STANDARD DEVIATIONS	4.336	1.722	3.621	1.137	1.449	.867	.846	1.711	1.427	2.036	1.592	4.977	2.488	.944	1.631	1.321

Note. Some standard deviations were multiplied by .01 to minimize the range (desirable for parameter estimation in EQS analysis).