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Historical Review and Appraisal of Research on the Learning, Retention, and Transfer of Human Motor Skills

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This review of human motor skills is historical and critical, and starts about 100 years ago. Three historical periods are identified. The main topics are knowledge of results, distribution of practice, transfer of training, retention, and individual differences in motor learning. Basic research is emphasized, but applied research is included also. The article concludes with projections for the future that are based on past research and the present research climate.

Why review nearly a century of research on motor skills? A good experiment can result from bouncing off the last one or a small subset of experiments in the literature, so why bother with the panorama?

Any good scientific question always seems to have a long story. Perceptive investigators see the key variables and issues of a scientific topic early, and the generations that follow persist in efforts to understand those variables and issues. The first answer to my question, then, is that a sense of history helps an investigator lock onto important themes. Experiments enriched by history could contribute to the science rather than only brightening an inconsequential corner. Second, a sense of history tends to shunt an investigator away from the fads and fashions of his or her field. Fads and fashions are those inconsequential corners that are temporarily magnified out of proportion and that draw the energy of investigators who either have not seen the worth of persisting themes or who allow themselves to be turned from them. Third, the canons of scholarship are based on history because they require that (a) the origins of ideas be known so that one's own ideas are in perspective and, (b) earlier experiments be known so that the knowledge increment in one's own empirical findings is clear. My aim is to organize variables and issues for learning, retention, and transfer that have regularly attracted investigators of human motor skills, and to discuss the experiments that have been done to understand them.

The last historical review of human motor skills was by Irion (1966). My review differs from Irion's in that I cover more material, have a different perspective, and update developments in the field. This article begins with a definition of the domain, and then divides the historical coverage into three historical periods: the Early Period, 1880–1940; the Middle Period, 1940–1970; and the Present Period, 1970 to the present. The periods are delineated by surges in research activity, although I cannot always be strict about the dividing line. The surge associated with World War II began around 1940. The late 1960s were a time of reduced activity that ended with a surge about 1970.

Allowing for the variation that occurs in any historical period, the main topics covered are knowledge of results, distribution of practice, transfer of training, retention, and individual differences in motor learning. There are three omissions that the reader might regret, but they were rationalized on the grounds that they have had other purposes and have contributed little to the cumulative account of learning, transfer, and retention of skills. One is research that tilts toward the engineering side, mostly tracking. Only tracking research with an emphasis on designing a man-machine system that can be effectively controlled by a human operator is omitted, not all tracking research. The second is research on the physiology of movement; my review is unashamedly behavioral. Third, analysts of motor behavior consider motor control to be distinct from motor learning, and so it is omitted. Motor control is a child of physiology and of information processing in psychology, neither of which have had much interest in learning. Thus, information-processing adherents in psychology have used motor behavior to exemplify their interest in processes like attention, the preparation and programming of movement, and speed–accuracy trade-offs (see Magill, 1983; Stelmach, 1976, 1978; Stelmach & Requin, 1980).

Definition of Skill

It would be useful if this historical enterprise could begin with a definition of skill that defines a set of events for examination, as a historian might define presidential elections in the United States as a set of events for review. In some areas of psychology the outline of the set is clear, such as vision being defined by the visual system and events that impinge on it, or Pavlovian conditioning being defined by a paradigm. Skill is not favored with a clear outline, however. Analysts have been struggling with its definition for decades.

Pear, a British psychologist, was invited by an American journal of industrial psychology to give his views on skill as they might apply to the skills of workers in American industry. What he said about the definition of skill (Pear, 1927) left its mark because the definitions by others that followed usually included something Pear had said. Pear said that skill has an explicit ref-

Reviews by Gavan Lintern, Amir M. Mané, Karl M. Newell, and Richard A. Schmidt improved this article.

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erence to the quantity and the quality of output. A skill is learned, and it is distinguished from capacity and ability because an individual may have the capacity and ability to perform a skill but cannot do it because it has not been learned. After noting that the term *skill* is reserved for higher grades of performance, Pear gave his definition: "The concept of skill which is proposed is that of integration of well-adjusted performances, rather than a tying together of mere habits. In man, at least, skill is acquired and fused with natural aptitude" (pp. 480–481). Pear ranged widely in his examples of skilled behavior, which his definition allowed him to do. Anything that has well-learned behavioral complexity, in which the partial actions are integrated into a behavioral whole, qualifies as a skill. J. A. McGeoch (1927), in his review of the acquisition of skill, accepted Pear's definition and included everything from ball tossing and typewriting to walking a tightrope as skills. Commenting on Pear's definition, McGeoch said that skills "may vary in the complexity of the integration from cancellation, perhaps, at one end to typewriting or language at the other" (p. 437). J. A. McGeoch (1929), however, also wrote that the research being done was so heterogeneous that Pear's definition was not providing useful guidelines for investigators.

In the manner of Pear (1927), the British pursued the definition of skill more ardently than others. Bartlett's (1948a, 1948b) papers on the measurement of skill devoted considerable space to its definition. Bartlett said that the beginnings of skill are to be found in the graded response in which the amount, direction, and duration of the response corresponds to variations in the regulating stimuli. Several receptor and effector functions are linked in the efforts to attain a goal. A response is not merely activated by stimuli, as with reaction time, but is guided and determined by them; the stimuli are interpreted by perceptual and cognitive processes. Timing and anticipation are important. Variety in behavioral routes to a goal occurs. Skills are both mental and motor, and they are learned. Here, too, the definition has wide boundaries in which Bartlett was easily able to fit a physician making a clinical decision, a mathematician, an aircraft pilot, and a cricket player.

The wide boundaries, which were implicit in his 1927 definition, were no longer acceptable to Pear when he reacted to Bartlett's papers. Pear (1948) said that the definition of skill should not be so broad as to include a physician making a clinical decision or the skill of a mathematician, and he added "muscular" to his 1927 definition, so that it now read, "Skill is the integration of well-adjusted muscular performances" (p. 92). Pear's revised position—that skill should be concerned with movement and its determinants—left its mark. Fitts (1964, pp. 244–245), for example, in a definition mostly by examples, has exemplars that all require a great deal of motor behavior.

Given these attempts by others, what is the definition of skill that can guide the present review? Maybe the term *skill* has lost its value and there is no longer any justification for it, as J. A. McGeoch (1929) said, but rejecting it is not an option for a reviewer who needs his field marked off from others with rules of inclusion and exclusion. Rules, if properly chosen, will correspond to the scientific interests of readers. Skills, therefore, have three defining characteristics that set the boundaries for this review.

1. Skill is a wide behavioral domain. From the beginning, skill has meant a wide variety of behaviors to analysts, and the behaviors have almost always been complex.

2. Skill is learned. That skills can lack proficiency, and acquire it gradually with training, conflicts with the dictionary definition of skill, popular usage, and how a number of investigators have used the term. A dictionary and a layman will define skill as the ability to do something well, and psychologists have often said the same thing. Pear (1927) said it also. Bartlett (1948a, 1948b) implied it. Guthrie (1952, p. 136) said that "skill consists in the ability to bring about some end result with maximum certainty and minimum outlay of energy, or of time and energy." Welford (1968, pp. 12–13) said that "skill is acquired after long training, and consists of competent, expert, rapid and accurate performance." No investigator, however, should have more than a passing interest in behavior at its asymptote; a scientific understanding of skill must be concerned with all grades of it.

3. Goal attainment is importantly dependent on motor behavior. Any behavior that has been called skilled involves combinations of cognitive, perceptual, and motor processes with different weights. Mathematicians have cognition heavily weighted in the description of their behavior, with virtually no weight for perception or the motor response with which they write the answer to a problem. On the other hand, the behavior of tennis players could not be meaningfully described without including the motor responses stemming from their perceptual evaluation of the situation and the cognition in their decision making. I agree with Pear's (1948) effort to limit his own definition by saying that skilled performance is "muscular." My concern in this article is more with the processes of the tennis player than the mathematician.

More often than not, investigators of skills have accepted these three defining characteristics, and they have variously labeled the behavior that has them as "skills," "motor skills," "perceptual-motor skills," or "skilled performance." I use these terms interchangeably too.

Early Period

The Early Period finds the topics of this article struggling for direction and, with the exception of the first topic—the form of learning curves—all developed further in the later periods. The first topic, nevertheless, was the effective beginning of systematic research on skills, and it stimulated the first theory of motor learning.

Characteristics of Learning Curves

All branches of experimental psychology have experimented on comparatively simple tasks, with the rationale that variables are more easily identified, measured, and controlled. Nevertheless there has always been research on complex tasks, partly because they abound in the everyday world, and partly because scientists are restless to push beyond conventional domains and identify new phenomena. Telegraphy and typewriting are complex tasks, and investigations of them go back to the beginning of experimental work on skills. Complex tasks were important in the Early Period because of what learning them had to say about the characteristics of learning curves. The shapes of learning curves, and their determinants, commanded attention in the Early Period (e.g., Hunter, 1929; J. A. McGeoch, 1942, chap. 2; Woodworth, 1938, chap. 7).

Bryan and Harter (1897, 1899) fathered research on complex skills. They studied the learning of sending and receiving Morse code, and what intrigued them was that learning curves for receiving (not sending) had plateaus in which periods of gain were followed by periods of no gain (the plateau), and then periods of gain again. Their explanation was that a complex skill like the processing of Morse code involves a hierarchy of habits, in which letters must be learned first, followed by sequences of letters that form syllables and words, and then by phrases and sentences. A plateau is a point of transition, when the lower order habits are not sufficiently learned for them to advance to the next echelon of habits in the hierarchy, and so progress is slowed until the learning is completed; the lower order habits must first be automated.

Book (1925) studied the acquisition of typing. He found flat periods of both short and long duration in the learning curves, and he gave them different names and explanations. The short ones, lasting 6–8 daily sessions, were called *breathing places,* and the longer ones, lasting 17–33 daily sessions, were called *plateaus.* Book took introspective reports from his subjects, in addition to performance measures, and so could make inferences about levels of interest and attention. He said that breathing places are caused by lapses in attention and effort. Plateaus also reflect failures of attention, but for reasons that essentially came from Bryan and Harter. In complex tasks like typing, in which simple habits are continually being learned and organized into higher order habits, plateaus are the critical stages when transitions to higher order habits are occurring. The simple habits are being perfected before advance to the higher order habits, and their rate of learning is slow during a plateau. The slow progress is frustrating, attention and effort wane, performance slows more, and the result is a plateau.

What can be said about plateaus? Many psychologists today probably believe in them, because discussions of Bryan and Harter regularly turn up in textbooks. There are several good reasons to doubt the existence of plateaus, however. Foremost is that Bryan and Harter's (1897, 1899) findings cannot be replicated. The most notable failure to replicate is an unpublished doctoral dissertation by R. Tulloss (cited in Taylor, 1943; Keller, 1958). In his investigations of the learning of Morse code, Tulloss found little difference between receiving sentences, unrelated words, nonsense material, and random letters, which is unexpected from a hypothesis of a hierarchy of habits. In addition, there were no plateaus. Keller (1958) reported other failures to replicate Bryan and Harter's results. Second, it was not clear whether plateaus are perceptual or motor. Bryan and Harter found them for receiving code (perceptual) but not for sending it (motor). Book, on the other hand, found them for typing, which is motor. If Bryan and Harter cannot be replicated on the perceptual side, and Bryan and Harter do not agree with Book on the motor side, then the validity of plateaus is uncertain. Finally, few of the motor learning curves reported in the psychological literature since Book's research have had convincing plateaus (see J. A. McGeoch, 1927, 1929, 1931, 1942). If the plateau is not a "phantom," as Keller (1958) called it, then there are several likely reasons why it has not appeared in the literature: (a) possible uncertainty about the identification and measurement of a plateau (a methodological problem that did not trouble Bryan and Harter, or Book), (b) the tasks commonly used in motor skills research are too simple to require the hierarchy of habits that plateaus are said to need for their appearance, (c) investigators seldom administer the many weeks of practice required for plateaus to be revealed, and (d) the use of an average curve for a group could obscure plateaus occurring at different places for the subjects (Estes, 1956; Hayes, 1953). Even if the conditions of plateaus could be reliably identified, their explanation would be a distance away. As Hunter (1929) said, a plateau is a period of no progress and any variable that retards learning can produce it.

Knowledge of Results

Edward L. Thorndike (1874–1949) was a giant in the psychology of learning. His distinguished career began with a famous doctoral dissertation in 1898, and his ideas remained influential even after his death. Learning without awareness, for example, was a lively topic in the 1960s (see, e.g., Bower & Hilgard, 1981, pp. 44–46), and its origin was Thorndike's (1935, p. 62) position that learning was an automatic process without the intervention of conscious awareness. Thorndike was the father of instrumental learning, and motor learning theorists have had an active, continuing interest in knowledge of results, usually error information, as an event after a response that affects the probability of response occurrence. Tolman (1938), a leading learning psychologist from the 1920s to the 1950s, felt that the theory of animal learning (which was Tolman's interest), and human learning also, was a matter of agreeing or disagreeing with Thorndike or trying to improve on him in minor ways.

Thorndike's contribution to the history of skilled performance is best understood within the context of his ideas and research during his lifetime. His doctoral dissertation from Columbia University is remembered (Thorndike, 1898/1970). Objective, quantitative studies were reported on instrumental learning in cats, chickens, and dogs, as a reaction to the practice in psychology of explaining animal behavior anthropomorphically. Thorndike's explanation, developed more fully in subsequent publications (1907, 1913, 1932), was selective learning and the Law of Effect. A subject has many responses available in his or her repertoire, some of which occur in the learning situation. Eventually one of the responses leads to reward and is strengthened, which increases the probability of its occurring again and being strengthened again. With enough rewards, the response occurs reliably and is selected out of all of those in the repertoire, and it is said to be learned. Correspondingly, Thorndike said punishment weakened a response, although later he came to doubt the weakening effect of punishment. Learning did not need the intervention of conscious thought processes such as ideas or reasoning, but was the automatic, unconscious, and direct strengthening of a habit connection between a stimulus and a response; Thorndike was not a cognitive theorist. Rather early in his career he moved to Teachers College at Columbia University and transplanted his thinking about instrumental learning to human learning and education. Most of his experiments after this move were on the instrumental learning of verbal responses, in hopes of finding implications for the quite verbal classroom; however, a few of the experiments were on motor learning.

Thorndike (1927) used motor learning for one of his important experiments on the Law of Effect. The issue was whether practice repetition alone could produce learning or whether knowledge of results after the response was required, as the Law of Effect implies. The part of the experiment with knowledge of results had the blindfolded subject draw 3-, 4-, 5-, or 6-inch lines. The experimenter said "right" if the movement was within a tolerance band around the correct length, or "wrong" otherwise. The percentage right rose from 13% in a pretest without knowledge of results to 54.5% in the final session after 4,200 lines had been drawn with knowledge of results. With practice repetitions alone, the percentage right remained unchanged over the drawing of 5,400 lines. Trowbridge and Cason (1932) supported Thorndike's work and showed that quantitative error produced faster learning than qualitative error like "right" or "wrong." It is from conceptual and experimental roots like these that modern analysts continue to emphasize the role of knowledge of results for motor learning (Adams, 1971, 1978; Newell, 1976a; Salmoni, Schmidt, & Walter, 1984; Schmidt, 1982b).

Distribution of Practice

Since Ebbinghaus (1885/1964), investigators have found that distributed practice produces better performance than massed practice for a variety of tasks (Hunter, 1929; J. A. McGeoch, 1942). One interpretation for verbal material is that the wider spacing of trials produces better performance because it increases the opportunity for covert rehearsal, but for motor behavior the interpretation has been in terms of effort, work, reactive inhibition, or fatigue. The distribution variable could also be a work effect for verbal responses, but work is such an obvious influence on motor responses that a work interpretation for lightweight verbal responses has been less convincing.

The distribution variable clearly affects the performance level of skills, as studies of fatigue have always shown, but this review aims at learning. Does distribution influence the fundamental nature of the skill being learned, or does it affect only the performance that is being observed, operating through a transitory agent like fatigue? Tolman (1932, p. 364) was the first to make the learning–performance distinction, based on data like Blodgett's (1929). Blodgett let rats wander around a complex maze without reinforcement and their performance showed no apparent learning. When given reinforcement, the rats quickly moved to the level of a control condition that had reinforcement throughout. They had learned something during the unrewarded trials that their performance did not reveal at the time. Perhaps massed practice works in the same way. Does massing depress only performance, not learning? A study by Lorge (1930) gave the first indication that this was so. Using mirror tracing of a star pattern as the task for his human subjects, Lorge found better performance under distributed practice than massed practice, but a group that was shifted to massed practice partway through training readily shifted to the level of the massed condition. The distribution variable was affecting the momentary level of performance. Doré and Hilgard (1938), using the Rotary Pursuit Test, had findings that could be interpreted in the same way. Gentry (1940), who also found this kind of trend with a task in which prose had to be translated into a code, and with the mirror reading of prose, was the first to see that Tolman's learning–performance distinction applied to the distribution variable. He concluded (1940, pp. 43–46) that distribution affected momentary performance, not the basic underlying skill that was being learned. These findings were given a theoretical rationale and were more fully exploited in the Middle Period.

Transfer of Training

Transfer of training, in which the learning of a response in one situation influences the response in another, is sometimes used as a way of making inferences about basic behavioral mechanisms, as when training on one stimulus value and transfer to another is a means of determining a generalization gradient; in such an experiment there is no interest in transfer for its own sake. Interest in transfer of training per se, however, often has an applied motive. Educational programs assume that knowledge acquired in them will transfer to situations outside the classroom, and the learning of skills on a training device are expected to transfer to an industrial or military job, but whatever the interest, it all began with Thorndike who, with Woodworth, formulated the identical elements theory of transfer. They were reacting to the doctrine of formal discipline, a version of faculty psychology that assumed that the mind was divided into faculties like reasoning, attention, and memory. Exercising a faculty would increase its strength, and its power would spread to all fields that required the faculty. The development of mathematical reasoning would increase one's capabilities in reasoning about religion, for example. Thorndike and Woodworth (1901a, 1901b, 1901c) laid the doctrine of formal discipline to rest with data and theory. To illustrate one of their experimental situations, they had subjects estimate the areas of different-sized shapes, and found little evidence of transfer of estimation from one task to another. The mind, they concluded (1901a, p. 248) is "a machine for making particular reactions to particular situations." In words that endured they said that the "spread of practice occurs only where identical elements are concerned in the influencing and influenced function" (p. 249). Transfer of training is not based on general faculties but is particular and limited, determined by identity of stimuli and/or responses made to them in the two situations.

As an early, non-Watsonian behaviorist, Thorndike expressed ideas and data in stimulus–response, not cognitive, terms. Cognitive findings were ignored or given a labored stimulus–response analysis. Orata (1928) criticized the identical elements theory of transfer for not being able to explain the cognitive findings of Judd (1908). Judd had two groups of primary school children throw darts at an underwater target. One group had the principle of refraction explained to them, the other did not. The groups performed equally well at the start when the target was submerged 12 in., but when the target depth was changed to 4 in. the group that knew the principle of refraction performed the best. There was no transfer from 12 to 4 in. without the principle of refraction. Judd presented a summary account, with no quantitative data, but the trend of his findings was replicated (Hendrickson & Schroeder, 1941) although the differences were small.

Overall, the transfer literature of the period was a mix of tasks and findings (Hunter, 1929; G. O. McGeoch, 1931; J. A. McGeoch, 1942, chap. 10; Whipple, 1928; Woodworth, 1938, chap. 8), making it difficult to see generalizations. The theory of identical elements emerged as the best principle of the period, and reviewers either believed that the theory accounted for much of transfer data (Hunter, 1929, p. 614; Whipple, 1928, p. 200), or were sympathetic to it (J. A. McGeoch, 1942, pp. 435–437). The most bewildering body of literature of the period is the field of part–whole transfer, which asks whether the most efficient way to learn a task is to practice repetitions of the whole task or to practice subtasks. Sometimes whole-task practice was found to be the best, sometimes part-task practice, with no clear principles emerging (G. O. McGeoch, 1931; Naylor, 1961).

Retention

Although the distinction between short- and long-term motor retention processes was not made until the 1960s, in 1909 Hollingworth came to but did not pass the threshold of the distinction.

Short-term retention. The linear positioning task, in which a subject moves a slide along a track and then attempts to recall its position after a retention interval, was a popular research tool in the 1960s. One way to view the linear positioning task is as a version of Thorndike's line-drawing task, but it was also used in its later mechanized form by Hollingworth (1909). Moreover, Hollingworth used the device to study the short-term retention of movements over seconds at a time when, in Ebbinghaus's tradition, the interest was in long-term retention over hours, days, and weeks. His retention functions were highly variable, with little compelling evidence for the rapid short-term losses that captured the interest of later investigators (e.g., Adams & Dijkstra, 1966). Probably the reason for uncertain short-term losses was that each retention interval had several lengths of movement associated with it, and a number of repetitions were given at each length. Later investigators (Adams & Dijkstra, 1966) found that practice repetitions slow short-term forgetting, and so Hollingworth unwittingly had used a procedure that minimized short-term forgetting. If Hollingworth had clearly established rapid short-term forgetting, and had set it off from long-term forgetting, the field of motor memory might have gotten off to an earlier and different start. Instead, motor memory continued to emphasize long-term motor retention until the 1960s.

Long-term retention. Some teachers assure students that motor skills are scarcely forgotten at all, and this is partly true. The foundation of the belief was laid in the early part of this century when a number of investigators found high long-term retention of motor skills. Typewriting was considered a revealing skill for the understanding of motor behavior, and its retention was studied in parallel with its acquisition. Bean (1912), Book (1925), Hill, Rejall, and Thorndike (1913), Swift and Schuyler (1907), and Towne (1922) found high retention of typing skills, sometimes over intervals of months. Swift (1905, 1910) found high retention of juggling two balls with one hand after intervals as long as 6 years. Tsai (1924) found high retention of stylus maze performance over 9 weeks. The contrast was with the retention of verbal responses that from Ebbinghaus (1885/1964) on, had been shown to have extensive forgetting. Thorndike (1913, p. 325) said that motor skills could be overlearned relative to verbal responses; the comparisons did not have equal levels of learning. Thorndike, however, acknowledged (p. 327) that motor responses may be intrinsically more resistant to forgetting than verbal ones.

J. A. McGeoch and Melton (1929) made the major effort of the period to compare retention of verbal and motor responses with degree of learning equated. The tasks were a stylus maze and the serial learning of nonsense syllables, each with three levels of difficulty. The criterion of learning was one errorless trial, with a retention interval of 1 week. Results were inconclusive. There was a tendency for the retention of mazes to be higher for some of the difficulty levels and some of the measures, but there were enough exceptions to cause McGeoch and Melton to back away from a strong conclusion.

Inconclusiveness of findings is probably the least of the difficulties with McGeoch and Melton's study. The assumption that the stylus maze is very motor and the verbal learning task very verbal, and thus that they are appropriate tasks for comparing the retention of verbal and motor responses, is partly wrong. Everyone would grant that the verbal learning task is verbal, but the stylus maze is less motor than it appears. Moves through a maze can be covertly encoded as "left, left, right, left," and so on, or in some higher order way. Responding to research on the verbalness of the stylus maze (Husband, 1931; Waters & Poole, 1933), Leavitt and Schlosberg (1944) used the Rotary Pursuit Test, which they believed to be near the motor end of the verbal–motor continuum, and reasonably so. Once again the comparison was with the serial learning of nonsense syllables. Their procedures failed to remove them from trouble. Because they were unfamiliar with intertrial intervals that constitute distributed practice on the Rotary Pursuit Test, their motor data had increasing gains (reminiscence) over the retention intervals, whereas the verbal data had increasing losses. No reasonable comparisons could be made. To escape this problem, Van Dusen and Schlosberg (1948) changed to a procedural task of throwing switches. This was a full-circle return to McGeoch and Melton's problems because procedural tasks like these turned out to have a large verbal component (Neumann & Ammons, 1957). The topic has been intractable ever since.

Individual Differences

The studies on individual differences and learning covered in this section did not always use motor tasks in the narrow sense of the term. The quasi-motor studies are included nevertheless because they are necessary background for the motor studies that came later.

Thorndike's legacy. Concerns with individual differences and learning date from the beginning of this century. Tolman (1938) was not thinking of individual differences and learning when he said that learning was a matter of agreeing or disagreeing with Thorndike, but it was true of individual differences and learning nevertheless. One of Thorndike's early studies (Thorndike, 1908), whose conclusions were repeated in a well-read textbook (Thorndike, 1914), provided the basis of research reaction for the next 30 years. The issue was phrased this way: We all know that group means improve with practice, but how do individuals of different initial performance levels profit from training? Do individuals of high initial performance on a task have superior learning ability and so profit at the same rate during practice and remain superior to individuals of low initial performance at the end of training? Individual differences should then remain the same or increase with training. Conversely, is initial superiority in a task the result of opportunities for extraexperimental practice of related materials that transfer positively to the task? Individuals of low initial performance have not had the benefits of training on related materials, and when given the opportunity to practice the task will overcome the deficit and approach the level of individuals of high initial performance. Individual differences should then decrease with practice. The vocational and social implications of the topic were not lost on investigators. Does training on the job and in school make people more alike or more different? Does a performance measure at the start of a job or school predict later performance? Using a mental multiplication task, Thorndike (1908) found that individual differences increased with training. Thorndike (1908, pp. 383–384; 1914, p. 305) gave his findings a nature–nurture turn. Those who were initially superior were naturally gifted and used their endowments to increase the advantage with practice.

The research on individual differences and learning that followed Thorndike used mostly mental tasks, although there were a few motor tasks. In a pivotal article, Kincaid (1925) reanalyzed 24 studies on individual differences and learning, including Thorndike's 1908 study. She found that 11 investigators concluded that individual difference increased with practice, 10 found decreases, and 3 had indifferent outcomes. This heterogeneity of findings was the foundation of her article. Some of the experimenters analyzed their data by correlation, some by variance, some by ratio of worst performers to best performers, and some by inspection. She decided that no decision could be made about individual differences and learning unless common measures were used, and so she reanalyzed the data of the 24 studies with the same seven methods of analysis. The essentials of her findings follow.

1. Scores were reliable. Scores on adjacent trials, both early and late in practice were highly correlated. The determination of reliability was necessary because other correlations are a function of it.

2. Correlations between initial and final performance levels were positive. Subjects tended to maintain their relative standing with practice.

3. Levels of initial performance and gain (the difference between final and initial performance) were negatively correlated; the subjects with higher initial performance learned at a slower rate. Subjects with high initial levels presumably start at a higher point on their learning curves, where acquisition rates are less. The subjects with lower initial levels learned at a faster rate but typically never reached the terminal levels of the subjects with the high initial levels (the positive correlation between initial and final performance level).

4. Correlation says nothing about the absolute differences between the scores of each set. The variance could increase or decrease from initial to final performance with the correlation coefficient remaining the same. Neither is variance informative by itself. Variance might increase from 5 to 50 from initial to final, but means might go from 10 to 1,000. Relative to means, the variance has actually decreased. Kincaid used the coefficient of variation (the standard deviation divided by the mean) to take this into account. Two-thirds of the studies had a smaller coefficient of variation by the end of practice.

Reed's monograph (Reed, 1931) was a scholarly critique of Kincaid (1925). He did not agree with the rationale for all of her measures, so he settled on three measures that he felt were defensible and used them to reanalyze the data of 58–70 experiments (including some that were analyzed by Kincaid), depending on the availability of data for the measure. The three measures were: (a) the ratio of the three highest to the three lowest performers at the beginning and end of practice, (b) the coefficient of variation at the beginning and end of practice, and (c) the correlation between initial performance and relative gain. Reed was obliged to agree with Kincaid: Individual differences converge with practice. Using Thorndike's reasoning, nature does not always triumph over nurture.

Factor analysis and learning. The orderly pattern of intertrial correlations of scores on a learning task has come to be called the *superdiagonal matrix,* which is defined as one in which (a) all correlations are positive, (b) adjacent trials have comparatively high correlations, and (c) correlations decrease with the remoteness of trials. The superdiagonal matrix was of interest because the orderliness of correlations suggested an orderliness of individual differences in learning and possibly a basis for an explanation of them. It appears that the superdiagonal matrix was discovered by Perl (1933, 1934). Her 1933 article on individual differences and practice was in the tradition of Thorndike, Kincaid, and Reed, but she observed that correlations of learning scores decreased with remoteness of trials. She knew of the early writings of L. L. Thurstone on factor analysis, and she factor analyzed the intertrial correlation matrices for each of the four learning tasks used in her 1933 article (Perl, 1934). Two similar factors emerged for each task, and she found it interesting that factor analysis applied to learning data gave an orderly structure like the factor analysis of a test battery. She used all of the data in a learning task to reveal the course of individual differences with learning, not just initial and final scores, and gain, as others had done.

Woodrow exploited Perl's initiative. Factor analysis emerged as a prominent analytic method in the 1930s (Thurstone, 1935, 1938), and Woodrow used it to make the most notable contributions on individual differences and learning in the Early Period (Woodrow, 1938a, 1938b, 1938c, 1939a, 1939b, 1939c, 1940). Most subsequent research done on this topic was anticipated by him.

Woodrow (1939a) believed that changes in factorial pattern could be an informative way to explain individual differences with practice and the superdiagonal matrix, but he found Perl's analysis self-evident in its outcome because she had factor analyzed only learning data. He said that in addition to learning data, an independent reference test battery was needed that defined the abilities whose changes over learning could be charted. In one of his major representative studies, Woodrow (1938a) gave 39 days of practice on each of seven learning tasks that showed unmistakable learning (Woodrow, 1938b) and that were sufficient for his purposes. Among the learning tasks were horizontal adding, which required the addition of six numbers arranged in a horizontal line, and an anagrams test. The reference battery was 12 measures from 9 printed tests administered before or after the learning tasks, which were intelligence tests, speed tests, numerical tests, and verbal tests. Woodrow included only an initial score, a final score, and a gain score from each learning task rather than a number of scores covering the full range of trials as Perl (1934) had done (the use of these three scores was in the tradition of Thorndike, Kincaid, and Reed, and also attests to Woodrow's interest in gain as a measure of the ability to learn, which is discussed later in this section). Thus the 3 measures from each of the seven learning tasks and the 12 measures from the reference battery gave 33 measures for the intercorrelations and the factor analysis. Nine factors were extracted that did not lend themselves to a clear interpretation, but nevertheless Woodrow saw unequivocal trends in his findings. A main trend was that factor loadings changed with practice—some increasing, some decreasing. Woodrow's interpretation was that the pattern of abilities required for goodness of performance changed with practice, with performance after practice requiring more of one ability or less of another than it did at the start.

Woodrow used these data and others to challenge the commonly held view that intelligence could be identified with the ability to learn. This is an old view. In 1881 the French Ministry of Education inaugurated popular education and was confronted with the problem that some children did not profit from instruction. Henri Binet's intelligence test was the answer, and it was interpreted as a measure of the ability to learn (Scarr-Salapatek, 1977). The General Classification Test, which is a kind of intelligence test, was used as a selection test in World War I by the U. S. military, who were assured that the test measured both general intelligence *and* the trainee's ability to learn (Woodrow, 1946). That IQ measures the ability to learn has popular appeal. Bright people learn faster, it seems. The implication of this thinking was that there was a general learning ability across tasks and that an intelligence test measured it. Both Binet's intelligence test and the military's General Classification Test had some success in predicting a measure of performance somewhere along the learning curve for academic or military skills, but that is where laymen and analysts went wrong. How far an individual has come along the learning curve could just as well be determined by transfer from past experience as by a function of the rate of learning; that an individual is performing well does not necessarily mean that he or she has learned faster. Woodrow's answers were to (a) use gain scores, or the difference between initial and final performance levels, as a measure of rate of learning (Woodrow, 1938a); and (b) fit functions to individual learning curves and use the value of the function's rate parameter as a rate measure (Woodrow, 1940). These measures correlated negligibly with intelligence test scores. Moreover, in his factor analyses, Woodrow found no factor common to gain scores. Woodrow (1946) concluded against an ability to learn, as did Simrall (1947), who also used gain scores. A qualification for this conclusion is that gain scores can be unreliable because their reliability is a joint function of the reliabilities of the two scores from which they are derived, and so they can suffer from the unreliabilities of each component score (Tilton, 1949). The low correlation between tests and gain scores could be attributable to this unreliability. The curve-fitting method is free of this criticism because it is not a difference score, and the main exception to Woodrow and Simrall's conclusion came in a study by Stake (1958), which used it. The correlations of an intelligence test predicting the learning rates of 12 learning tasks, both verbal and nonverbal, were positive but small, and they only weakly support the assertion that intelligence tests measure an ability to learn. Allison (1960) used the curve-fitting procedure also. There was no general tendency for three intelligence tests to predict the rates of learning 13 verbal and motor tasks.

Finally, with respect to intelligence tests, Woodrow (1938c) found that their correlation with performance on a learning task decreased with practice—a finding that loomed larger in the 1950s and 1960s, when interest was awakened in predicting performance at advanced stages of motor training.

Theories of abilities and learning. Woodrow turned these various findings into a kind of theory of learning, although he did not call it that. Woodrow (1938a, 1939a, 1939b) wrote that the change in factor pattern with practice is a change in the manner of performing the task. Taking "ability" literally as the capability of doing something, and equating it with "factor" as Woodrow did, implies that a subject would restructure his or her emphases on the various abilities called for by the task to improve, or learn. This reorganization of abilities has been called "work methods" (R. H. Seashore, 1930, 1939, 1940). Much of learning could be of this sort, but Woodrow alternatively hypothesized that abilities, being response capabilities, could be strengthened with practice. This idea did not fare well in Woodrow's (1939c) experimental test of it.

One seldom finds experiments to better explain why abilities change with practice, but Woodrow (1939c) is an exception. If some of the learning in a task is change in the strength of the constituent abilities required for its performance, and if the task and a test have substantial loadings on the same factor, then practice on one should transfer to the other if factors equate to abilities and if abilities are not at their asymptotes and can be strengthened with practice. Training for an experimental group was on verbal analogies and anagrams tasks, with eight reference tests administered before and after the learning tasks. A control group had only the two administrations of the reference tests. The learning tasks and the reference tests were all heavily loaded on a verbal factor. The training produced no increment in scores of the reference tests for the experimental group relative to the control group. Verbal behavior particular to a learning task might have been incremented but not a general verbal ability that would transfer to the reference tests. Woodrow (1946, p. 157) concluded that practice gains are task specific and are determined by the work methods the subject uses to achieve gains; learning is a matter of restructuring abilities in the repertoire, not strengthening them.

Middle Period

The struggle for direction in the Early Period was won by the time of the Middle Period because investigators saw clear roads ahead. Some of their directions were defined by empirical research that had uncovered important variables for study. Other directions grew out of theory, not theory sired by motor data, but theory from other domains. Hull's theory of behavior, which I discuss shortly, is the strong example. Theory that sprang from motor behavior itself did not appear until the Present Period.

Knowledge of Results

A tribute to Thorndike's ideas is that there was as much interest in them after his death as during his lifetime. The 1950s and 1960s saw a lively engagement with variables and ideas that were rooted in his conception of instrumental learning. Research on knowledge of results and motor learning was mostly nontheoretical empirical work. Theory about knowledge of results and motor learning became more prominent in the Present Period, and was related to some of the work done on theory and verbal learning (i.e., the informational view of learning, covered later in this section). To see motor learning in relation to the total effort, it is useful to review the theoretical ideas that were attracting investigators in this Middle Period.

When knowledge of results is presented after a response, performance improves. The two main ways to view the improvement are associative and motivational. Associative issues loomed important because of psychology's continuing curiosity about the fundamental nature of the learning process. Interest in the motivational view of knowledge of results came later, driven primarily by industrial and military hopes for improving personnel performance by setting goals. The associative interpretation has knowledge of results performing a directive, or guidance, function so that the subject learns what to do. The motivational interpretation has knowledge of results performing an energizing function such that the subject strives to make more of the responses that are already in his or her repertoire.

There were two positions on the associative process, one habit-based or behavioristic, and the other informational or cognitive. Given the origins of the Law of Effect in his thinking, Thorndike was committed to the habit interpretation. Thorndike (1898/1970) began his career with an attack on a cognitive view (it was not called that) of animal learning, which relied on analogues of conscious human thought—reasoning, awareness, planning, and decision making—and he was not about to recant. Thorndike said that reinforcement, or knowledge of results, automatically stamped-in the connection between the situation and the response without the intervention of conscious processes (Thorndike, 1927, 1933). The informational point of view relies on more mental machinery. A subject receives knowledge of results for a response on a trial, and remembers the situation, the response that was made, and the knowledge of results that was given. On the next trial the subject recalls the situation and the response that was last made, recalls the knowledge of results and plans a response that will eliminate the error embodied in it, and, finally, responds. Succeeding in these processes yields the improvement called learning. Research on the informational view was done almost entirely on verbal behavior and need not concern us here except to say that by the 1960s, the informational view was prevailing (for reviews, see Estes, 1969, 1971). Whether human learning required conscious awareness was a separate line of research, and it was conducted on verbal behavior also. Articles on the topic peaked in the 1960s and then declined in controversy and inconclusiveness (for reviews, see Kanfer, 1968; Krasner, 1967; Spielberger, 1965; Spielberger & DeNike, 1966).

The motivational view of knowledge of results has attracted less research (e.g., Annett, 1969, chap. 5; Locke, 1968; Locke & Bryan, 1966; Locke, Cartledge, & Koeppel, 1968) than have associative issues, perhaps because motivational paradigms translate into associative ones. Some of the motivational research used motor tasks. There are three ways that knowledge of results has been administered in experiments on it: to present the subject (a) a score that is his or her achievement on a trial; (b) a standard that is his or her goal, as well as the score he or she made on a trial; and (c) his or her error, the difference between the goal and the score. The first way does not increase performance (I. D. Brown, 1966; Chapanis, 1964), which is to be expected because the score is in relation to nothing and the subject has no cause to change his or her behavior. The second way, which is a motivational manipulation because the subject presumably strives for the goal, reduces to the third because the subject can take the difference between the goal and the score and have error just as if error was presented to him or her directly. With knowledge of error the subject has information that can be used to try to correct the error on the next trial, improve, and learn. Adams (1978, pp. 234–236) reviewed research on the motivational view of knowledge of results and indicates how it can reduce to an associative view, which is in agreement with Annett (1969, pp. 119–121).

Investigators of motor learning were convinced, as was Thorndike, that giving knowledge of results at the end of a movement sequence was the way that skills were learned. Practice repetitions, which laymen believe is the route to proficiency, are usually accompanied by knowledge of results, and so it is really knowledge of results that determines learning. Bartlett (1948b, p. 86) said it best: "The common belief that 'practice makes perfect' is not true. It is practice *the results of which are known* [original emphasis] that makes perfect." Although there were faint tremors (Lumsdaine, 1961) hinting that observational learning might be a profitable research route, there was the overriding belief that the main road was knowledge of results. Time has brought knowledge of results into a balanced perspective, but it cannot be said that the emphasis on knowledge of results was misplaced, because it is undeniably a strong variable for motor learning.

A number of reviews include the empirical work on knowledge of results during this period (Adams, 1971; Annett, 1969; E. A. Bilodeau & I. M. Bilodeau, 1961; I. M. Bilodeau, 1966; Holding, 1965, chap. 2; Newell, 1976a; Salmoni et al., 1984; Schmidt, 1982b), and it is unnecessary to travel the territory again. In the interest of historical trends, and to show why events take the turn they do, it is more useful to indicate the expectations for the research that was done, its outcomes, and the puzzlement resulting from the discrepancy between expectation and outcome. This puzzlement led to an eventual change in conceptualization.

There was a sentiment in experimental psychology that knowledge of results should have the same effect on human learning as positive reinforcement has on animal learning. The expectation had two sources—one was Thorndike. Thorndike's Law of Effect had its origins in animal learning and its realization in human learning, and he believed it was a general law that covered both. The other was behaviorism, which was riding high in the Middle Period (although stresses began to develop in the late 1960s). Behaviorists in general, like Thorndike in particular, believed in general laws that applied equally to animals and humans. Hull, who had the prominent theory of the period, moved freely between the data of animal and human subjects. There is no doubt that Hull saw his theory (Hull, 1943) as applying across the spectrum of organisms. An articulation of this belief of behaviorism did not come until the 1970s, when Seligman (1970) called it the "assumption of equivalence of associability" (p. 407), which holds that general laws of learning can be discovered that will work with any stimulus, response, or organism. It was sobering, therefore, for investigators of knowledge of results to sometimes find a lack of congruence between the findings of animal learning and their findings on human motor learning.

There was no dissonance between animal and human learning for acquisition when knowledge of results was administered on all of the trials. Performance steadily improved (e.g., E. A. Bilodeau, I. M. Bilodeau, & Schumsky, 1959; Trowbridge & Cason, 1932) just as it did with 100% reinforcement in animal learning. The issue was not so clear with knowledge of results on only some of the trials, however. E. A. Bilodeau and I. M. Bilodeau (1958a) administered knowledge of results on 25, 33, or 100% of the trials in the learning of a lever positioning task (a fixed-ratio schedule) and found no differences in acquisition. Such is not the case in instrumental learning with partial reinforcement in animals. Boren (1961) found that the rate of bar pressing in rats under various fixed-ratio schedules increased as the proportion of reinforcements decreased.

Withdrawal of knowledge of results is a manipulation that corresponds to extinction in animals. The expectation for it was that motor performance would decline, which it did (E. A. Bilodeau et al., 1959). One might have hoped for studies on knowledge of results derived from partial reinforcement effects in animals, in which resistance to extinction is a function of the schedule of reinforcement in acquisition, but this research was never done.

Delay of knowledge of results, corresponding with delay of reinforcement in animals, is the variable with the most discordant findings of all. It is axiomatic in animal learning that even a slight delay in reinforcement degrades the rate of learning (e.g., Grice, 1948), and so the recommendation for animal training is to use immediate reinforcement. Skinner (1953), who freely generalizes from animal research to humans, has written on the delay of knowledge of results and the learning of motor skills (p. 96) that "the reinforcement which develops skills must be immediate." The discordance arises because delay in the delivery of knowledge of results makes no difference in the acquisition of human motor skills. Lorge and Thorndike (1935) were the first with this finding for motor behavior, and others have replicated and extended it (E. A. Bilodeau & I. M. Bilodeau, 1958b; E. A. Bilodeau & Ryan, 1960; Boulter, 1964; Dyal, 1966; Saltzman, Kanfer, & Greenspoon, 1955). Other research (Boulter, 1964) attempted to determine the mechanism used by humans to bridge the delay. Still other research treated the delay interval as part of a larger problem that included the interresponse (intertrial) interval and the post-knowledge of results interval as well. Part of the research was methodological, trying to unscramble the confounding inherent in three intervals, such that manipulating one necessarily varies another (Denny, Allard, Hall, & Rokeach, 1960). Another part focused on the post-knowledge of results interval, although not always with motor behavior (Bourne, 1966; Bourne & Bunderson, 1963; Bourne, Guy, Dodd, & Justesen, 1965; Croll, 1970; Weinberg, Guy, & Tupper, 1964). Research on the post-knowledge of results interval was a prelude to the informational view of motor learning, which has as an issue what the subject does with knowledge of results in the time alloted to him or her, and how his or her processing of it influences the next response.

Distribution of Practice

The publication of Hull's (1943) classic theoretical work *Principles of Behavior* brought the topic of distribution of practice alive. Hull aimed for a general theory of behavior that covered the instrumental and classical conditioning of animal and human behavior, as the unqualified title of his book suggests. The theory stimulated thousands of experiments, with distribution of practice studies among them. His theory appeared to give unity, explaining what had been found before and what should be found. That Hull's theory eventually failed (for a discussion of problems with it, see Adams, 1963; Koch, 1954) should not cause us to forget how energetic a scientific force it was.

An outline of parts of Hull's theory is necessary to understand what was implied for the distribution variable. Basically, predictions came from his postulate of reactive inhibition. Whenever an organism makes a response, there is an increment of reactive inhibition that works in opposition to the reaction potentiality for the ongoing response and lowers performance. Reactive inhibition is a positive function of the amount of work involved in making the response and of the number of response evocations, and it spontaneously dissipates as a function of time between responses. Moreover, reactive inhibition is an aversive drive.

The second inhibition postulate was conditioned inhibition. Hull was a drive reduction theorist who required a decrease in motivation for an increment of habit. With reactive inhibition an aversive drive state, its dissipation between responses resulted in decreased drive and an increment of habit strength for the ongoing response of the moment, which is a resting response. The resting response conflicts with the response the organism is learning, and so it is inhibitory and joins with reactive inhibition in reducing the reactivity of the response being learned. When reactive inhibition and conditioned inhibition combine to lower the performance of the response being learned, it is the combination of a performance variable that is unstable and temporary (reactive inhibition), and of a learning variable that is permanent (conditioned inhibition) because habit was permanent in Hull's theory (forgetting notwithstanding).

Hull's interest in these postulates was mostly to explain extinction, and he borrowed their essentials from Miller and Dollard (1941, p. 43) and Mowrer and Jones (1943). With them he accounted for such extinction phenomena as the decline in responsiveness with the withdrawal of reinforcement (buildup of reactive inhibition), spontaneous recovery (the dissipation of reactive inhibition), and the failure of spontaneous recovery to be 100% (conditioned inhibition). What about work and distribution of practice in human motor learning? Like any good theory, it brings seemingly disparate empirical findings under a single conceptual umbrella; extinction may have been the motivator for Hull but his theory went beyond. Many motor studies were stimulated by Hull's theory. Here are representative studies and their Hullian foundation: The longer the intertrial interval the better the performance, because reactive inhibition has longer to dissipate between trials (verified by Adams, 1954). The greater the work involved in the response, the poorer the performance, because of the greater buildup of reactive inhibition (verified by E. A. Bilodeau, 1952a, 1952b; I. M. Bilodeau & E. A. Bilodeau, 1954). The longer the rest period after a session of massed practice, the greater the gain, or reminiscence, because of the dissipation of reactive inhibition (verified by Ammons, 1947b; Irion, 1949a; Kimble & Horenstein, 1948). Whenever there is substantial growth of reactive inhibition with massed practice, and it dissipates in intertrial intervals or the longer rest periods between practice sessions, conditioned inhibition should develop, and so massed practice should be irreversibly poorer than distributed practice. In the 1930s and 1940s studies showed that motor performance was controlled by the present conditions of distribution and was not impaired by previous massed practice (Cook & Hilgard, 1949; Doré & Hilgard, 1938; Gentry, 1940; Lorge, 1930). These studies were evidence against conditioned inhibition. Reflecting from these earlier studies and Hull's theory, Reynolds and Adams (1953) and Adams and Reynolds (1954) aimed deliberately at conditioned inhibition with a transfer design. They found that subjects who had massed practice on the Rotary Pursuit Test and then were switched to distributed practice performed as well on distributed practice as subjects who had distributed practice throughout. Stelmach (1969a) used the same kind of transfer design with a Bachman ladder (repeatedly attempting to climb a free-standing ladder) and a stabilometer (balancing on a pivoted board), with the same results. Whitley (1970) used a foot-tracking task of the same design and had the same outcome. The conclusions were that (a) conditioned inhibition as a theoretical concept is wrong, and (b) massed practice influences how well you perform, not how well you learn. The textbook generalization that learning with distributed practice is better than with massed practice is wrong.

Transfer of Training

Retroactive interference. Motor research on retroactive interference (Smode, Beam, & Dunlap, 1959) has been governed mostly by practical circumstances, although it entered the field of short-term motor retention, as is discussed later. Transfer occurs when the performance of one activity influences that of another. Tasks change in the practical world. Pilots go from one aircraft to another, in which control-display relations may be different. Is performance on a second aircraft adversely affected (negative transfer) by performance on a first? Then, if the pilot returns to the first aircraft after flying the second, will his or her performance be degraded (retroactive interference)? Of course, the hope is for positive transfer such that performance on one task benefits the other.

Research on retroactive interference for motor behavior had a spotty start before World War II (Buxton, 1940; Buxton & Grant, 1939; Buxton & Henry, 1939; Siipola, 1940, 1941; Siipola & Israel, 1933), and it was not until after the war that unmistakable interference was reported (Lewis, Shephard, & Adams, 1949). Lewis and his associates followed with the two major studies in the literature on retroactive interference for motor responses (Lewis, McAllister, & Adams, 1951; McAllister & Lewis, 1951) using the Complex Coordination Test (Melton, 1947). A pattern of lights on a display defined the positions of a control stick for the hand and a rudder control for the feet, and when the correct positions were attained a new pattern appeared that required new positions for the controls. The score on a trial was the number of patterns receiving a correct response. The interfering task was the same except that the controls had to be moved in the opposite direction for successful alignment. Their studies manipulated the amount of original learning on the first task and the amount of learning on the second, interfering task that was interpolated between learning and recall of the first task. There are methodological problems associated with measurement in these experiments (Schmidt, 1971), but the conclusion is that interference increases with the amount of original learning and the amount of interpolated learning. It might seem that the greater the original learning the greater the resistance should be to the interfering action of the interpolated task, but this is not so. As a motor response becomes increasingly refined, it appears to acquire a sensitivity to interference. Using the Star Discrimeter, in which the subject had to learn to position a stick in slots that were specified by colors on a display, Lewis and Miles (1956) varied the amount of interpolated learning on the interfering task, with the amount of original learning on the first task held constant. The interfering task was a reassignment of all color-slot relations. The results were the same as with the Complex Coordination Test: The greater the amount of interpolated learning, the greater the interference with recall of the first task.

Transfer and verbal mediation. There was little theory about the interference process itself and why one class of motor responses impairs another, but there was theory for positive transfer. Motor learning and transfer became the focal point of theory and research on verbal mediation in movement regulation. Alternative headings were "stimulus predifferentiation," if the stance was perception, and "verbal pretraining," if it was learning. (For reviews of theory and research see Arnoult, 1957; Cantor, 1965; Ellis, 1973; Gibson, 1969, pp. 62–73; Spiker, 1963).

The beginning of theory was an article by Gibson (1940), which emphasized perception even though the context was learning and conditioning principles. The question she addressed was how discrimination among verbal stimuli in learning is established, and her answer was through learning. When verbal responses are learned to verbal stimuli there is broad generalization among the stimuli at first, but as learning progresses the steepness of stimulus generalization gradients decrease and the stimuli are more discriminable. Though responses are learned to the stimuli, the benefits were considered to be perceptual because it was thought easier to differentiate the stimuli a second time around when they are paired with a new set of responses (a theory of transfer). Gibson (1942) found evidence for the theory within a context of verbal learning, but perception and motor learning came to be the main test grounds.

Perception psychologists saw the theory as a hypothesis about the classic question of how stimuli are differentiated with experience. How do we acquire the distinctive features of stimuli so we may distinguish among them? Gibson's theory held that one avenue is to learn responses to them, and it generated a number of experiments in which verbal labels were associated with visual patterns and the discriminability of the patterns was evaluated in recognition tests (for a review of this side of the research, see Ellis, 1973). Positive results were often obtained in these experiments, but Gibson (1969, p. 73) nevertheless came to disavow her response-oriented theory in favor of one in which stimulus appreciation comes from experience with the stimulus itself, without help from mediating responses.

Gibson's response-based theory of perceptual learning had its counterpart on the motor learning side in which verbal mediators were seen as implicit agents contributing to movement regulation (Goss, 1955). Gibson's theory was not independent of the behavioristic thinking of the period. Through learning, overt behavior can become attached to the response-produced stimuli of mediating responses, and so be governed by it. The best-known example of this kind of thinking is conditioned fear, which is a mediating response that comes to control reactions like avoidance behavior (Mowrer, 1947). The research on motor learning used these theoretical elements and the same empirical procedures as the perception psychologists in their experiments on pattern discrimination. In discrete visual-motor tasks, verbal labels were first associated with the stimuli of the task. The question was would this verbal pretraining transfer to the motor responses in the visual-motor version of the task that followed? The answer was usually yes. An experiment by McAllister (1953) that supported mediation theory is representative of the experiments that were done. Her task was the Star Discrimeter. Paired-associate pretraining was first given to color stimuli and verbal responses. Different groups learned verbal responses with varying degrees of relevance for designating the motor responses. The verbal learning transferred positively to the motor learning, with transfer related to verbal-motor relevance.

Verbal pretraining in tests of mediation theory is interesting for two reasons. First, mediation theory is a transfer theory, and transfer theories are scarce; transfer of training is a highly empirical domain. Second, it was behaviorism's way of handling the verbal regulation of behavior. Mediation theory passed away in the late 1960s and early 1970s as cognitive psychology supplanted behaviorism, but that does not mean that a better theory for the verbal regulation of movement has emerged (Zivin, 1979).

Part–whole transfer of training. This topic has always had an applied theme, and the theme became stronger in the 1950s and 1960s. The issue is the efficiency of learning a relatively difficult body of material, and one might suppose that interest in it arose because education, industry, and the military routinely teach difficult tasks to their trainees. Are there ways in which a complex task can be broken down into parts that can be learned more efficiently than the whole task itself? We do more part training than we realize, because some learning assignments are so complex that there is no alternative but to break them down. The mathematics that an engineer must know is not repeated as a whole but is taught as a progression of parts that begins with arithmetic.

The necessity for the part training of mathematics is self-evident, and the whole-task training of it absurd, but there are tasks in which part-task training is neither necessary nor self-evident and in which both part- and whole-task training are options. Learning to fly an airplane and execute missions is an example. A commercial airline pilot must fly from one city to another, and a military pilot must bomb enemy targets or destroy enemy aircraft in air-to-air combat. A training method used, both then and now, is to practice on flight simulators, which strive for whole-task mission simulation, and also to practice on the whole-task aircraft itself. There are, however, potentially valuable possibilities for part-task training, not as in the laboratory, where the whole task is entirely fragmented and the parts trained, but in the sense of singling out some of the parts for special training. Why would there be interest in the part training of some flying skills when obviously pilots can learn by the whole-task method? There are four reasons, some based on cost and some on skill (Adams, 1960, 1961b).

1. Flight simulators, whose use is whole-task training of flying skills, are complex and expensive, often costing millions of dollars. There is always the hope that less expensive ways of training can be found, and comparatively simple part-task training devices are considered to be a useful approach.
2. Parts of the flying task are critical to mission success and yet receive little practice, either because they are of relatively short duration and fail to accumulate enough practice time or because the pilot is busy sharing other activities and cannot attend to them.
3. Much of flight training involves experienced pilots who must learn to fly new aircraft or new missions. They have many highly learned flying skills, and only new elements must be taught. Part-task training devices could be built for training only the new response requirements; complex whole-task flight simulators thus might not be needed.
4. Manned space flight is the ultimate in flying. Some missions have lasted weeks, and someday they may last years. Forgetting of essential skills will occur. Can relatively simple part-task training devices be onboard to retrain forgotten responses? Of course, skills for conventional flight are also forgotten when they are unused.

Adams and Hufford's (1962) Experiment I is an example of the kind of research that is done on these practical aviation matters and the degree of success that can be achieved with part training. A pilot must know both how to control his or her aircraft and his or her "procedures," as they are called in aviation. There are normal procedures, like starting an engine, and emergency procedures, as in handling an engine fire. A pilot must know many procedural sequences. Some of these procedures can be taught in the air, but others, like certain emergency procedures, for safety reasons cannot. Should they be taught in a multimillion dollar whole-task flight simulator or could a simple device be devised solely for their training? Controls and instruments would be largely inoperative with this device, functioning only as far as the procedures require. Are there performance penalties when the procedures, learned in the simple device, must be performed in the whole task and time-shared with other flying activities? A flight simulator was used in Adams and Hufford's study to answer these questions. An experimental group learned the flight control for a complex maneuver separate from procedures that had to be performed concurrently. A control group had whole-task practice of the maneuver throughout. When the experimental group was shifted to the whole task after its part training, and compared with the control group, they showed positive transfer but did not perform as well as the control group. The new requirement for time-sharing the procedures with flight control produced a decrement in performance of the procedures, but it was transitory, lasting only one trial. Thereafter, the experimental group performed as well as the control group. Adams and Hufford concluded that much can be learned with part training, but that when the part skills must be time-shared with other activities some integrative whole-task practice is required.

What about part-task training for the reinstatement of forgotten responses? Cockpit procedures are readily forgotten, as discrete responses tend to be. Adams and Hufford's (1962) Experiment II used part-task procedures training for the relearning of procedures that showed almost complete forgetting over a 10-month layoff. A great deal of relearning was accomplished, but it was not complete. As with the acquisition of procedures, some additional whole-task practice was required. In related experiments, Naylor and his associates (D. R. Brown, Briggs, & Naylor, 1963; Naylor & Briggs, 1963; Naylor, Briggs, Brown, & Reed, 1963) used both part- and whole-task training for the reinstatement of forgotten responses, both discrete and continuous. In general they found positive transfer from part training, but whole-task training in relearning was the most effective. This summary of Naylor's work might be applied to the field as a whole. Positive transfer is easily found for part-task training, and sometimes part-task training is as good as whole-task training (Briggs & Naylor, 1962), but usually whole-task training is better (Adams & Hufford, 1962; Briggs & Brogden, 1954; Briggs, Naylor, & Fuchs, 1962; Briggs & Waters, 1958; McGuigan & MacCaslin, 1955). The hope that a regimen of part-task training could be better than whole-task training was not realized, then or now. This does not mean that part training cannot be cost effective, however. Part training may be less effective than whole-task training, but a combination of part- and whole-task training may be more cost effective than whole-task training alone.

Adaptive training. Other transfer of training topics have connections to the past, but adaptive training is an idea of the 1960s. Adaptive training requires computer technology for its realization, which is one of the reasons that it appeared in the 1960s. On the surface, adaptive training does not seem to have much to do with the transfer of training and yet transfer is at its heart.

The attractiveness of adaptive training is that it is individualized instruction, as computerized programmed instruction can be, in which the difficulty level of the task is continually adjusted to the performance level of the subject. Kelley (1969) listed three elements of adaptive training: (a) the continual measurement of performance, (b) an adjustable feature of the task that is changed in level of difficulty and called the adaptive variable, and (c) adaptive logic that automatically changes the adaptive variable, making the task easier or harder, as a function of performance measurement. In the beginning of training on a complex task, when the subject is having hardly any success at all, the adaptive variable is automatically regressed to an easy configuration, the subject succeeds, and presumably learns. As learning occurs, the task is automatically made more difficult. For example, consider a tracking task with a relatively high-frequency sine wave as the forcing function. A naive subject would find this task virtually impossible in the beginning. With frequency of the forcing function as the adaptive variable, the system would shift the input to a low-frequency wave, which the subject could follow much of the time. The frequency would then be increased until eventually the subject could be successful with the high-frequency input. Transfer of training enters because it is assumed that the adapting sequence will lead to more rapid learning of the criterion task or higher eventual attainment than practice on the criterion task alone; the adapting sequence will transfer more to the criterion task than an equivalent amount of practice on the criterion task itself.

The enthusiasm for this new idea in the 1960s and early 1970s led to the evaluation of various display and control variables as adaptive variables, primarily with tracking tasks, and the outcome of the effort was essentially negative (for reviews, see Lintern & Gopher, 1978; Lintern & Roscoe, 1980). Why did this intuitively attractive idea fail? There are three reasons, in addition to the usual rash of methodological and design problems that any area has.

1. Adaptive training is an uncritical translation of the methods of individualized computer-assisted programmed instruction to skill learning. Assume, for example, that a trainee is learning algebra with programmed instruction. Problems are regularly missed because he or she cannot multiply fractions. The system, detecting this shortcoming, can branch the student to exercises in fractions until a performance criterion is met, at which time the student is returned to algebra problems. Believing that these operations should work for skills learning is thinking by analogy, which is hazardous in science.

2. The independent variable of adaptive training is difficulty, but difficulty is a dependent variable, not an independent variable. Task difficulty is known from performance measures, and any psychological variable may contribute to it. To have difficulty as the primary manipulation of adaptive training is tantamount to saying that any psychological variable may influence transfer of training.

3. The adaptive variables studied were obviously performance variables, but it was not always clear that they were learning, and potentially, transfer variables (Mané, 1985). The old learning–performance distinction in psychology still has its explanatory uses.

Retention

Warm-up decrement. Athletes and their coaches seem convinced that there is a decrement after rest that is not forgetting and that quickly dissipates with practice of the criterion skill or with nonspecific "warming-up" exercises. Relative to control trials, the initial postrest trials will show a decrement and a rather steep reacquisition function before the gradual course of performance is resumed. This is the "warm-up decrement."

Observations about the warm-up decrement have appeared in the psychological literature since the nineteenth century but they were only passing observations, and it was not until this Middle Period that warm-up decrement was an idea that sustained research. Mosso (1906), for example, observed warm-up decrement in his studies of performance under conditions of protracted work, but the topic did not have a conceptual start until Thorndike (1914). Thorndike agreed with others on the definition of warm-up, but he went on to say (pp. 67–68) that warm-up activity that dissipates the decrement is the "fore-exercise of *other functions*, in order to get materials and motives with which and by which the given function is to work, than to an intrinsic alteration of it." In other words, the loss is in the behavioral support structure for the skill, and warm-up exercises reinstate what was lost. The structure, called the *set* for the goal response, might be orientation of the visual receptors, postures appropriate for the goal response, or muscular tensions. Adams (1961c) called warm-up decrement the second facet of forgetting because it is something other than loss of the goal response, which is what we ordinarily mean by forgetting.

The best evidence for loss of set as an explanation of warm-up decrement came from verbal learning research by Irion (1948, 1949b), and Irion and Wham (1951). Color naming, just before recall of paired adjectives after a retention interval, eliminated much of the warm-up decrement. Because it is unlikely that color naming would transfer associatively to paired adjectives, the reduction in decrement was presumably due to the reinstatement of set. Motor behavior entered the warm-up scene at this time because research on Hull's work-inhibition postulates often used the Rotary Pursuit Test, and the performance curves obtained with it showed a prominent warm-up segment in post-rest performance (e.g., Adams, 1952; Ammons, 1947a, 1947b; Reynolds & Adams, 1954). Efforts to locate a neutral task that would influence the motor warm-up decrement in a manner analogous to color naming failed (Ammons, 1951); there was no support for the set hypothesis for motor behavior.

Other theoretical attempts to explain warm-up decrement for motor behavior relied on manipulations of Hull's work-inhibition postulates (Eysenck, 1956), but they also failed (Adams, 1963). The set hypothesis for motor behavior has a lingering attraction, nevertheless. Nacson and Schmidt (1971) have a revised version of it and offer evidence in its behalf. Warm-up decrement is a phenomenon and an idea that will not go away, but there is currently little research interest in it.

Short-term retention. J. Brown (1958) and Peterson and Peterson (1959) demonstrated the forgetting of verbal items in seconds, and provided an empirical justification for a short-term memory system with properties different from a long-term memory system (e.g., Atkinson & Shiffrin, 1968). In earlier years memory was a unitary concept, populated by habits, and so a distinction between short- and long-term memory was stimulating for experimental psychologists. Most research done on the dual conception of memory used verbal materials; memory for movements was a shadowy land, outside of the new ideas and theory. Perhaps one reason was that motor retention had never generated much research interest, so there was not enough data to show how it would fit new theoretical ideas. Perhaps another reason was that the research on motor retention that had been done was on long-term retention in which the amount of forgetting was usually small. Forgetting is the stuff of memory theory, and it is hard to find a place for a response class that shows little of it.

Change came when Adams and Dijkstra (1966) reported a short-term motor retention experiment with procedures that were a motor analogue of the short-term verbal retention procedures used by Peterson and Peterson. In the verbal procedure a subject makes a designated response and then tries to recall it a few seconds later. Adams and Dijkstra had their subjects make a linear movement to a stop and then try to recall it after the retention interval with the stop removed. Contrary to the long-standing belief that motor responses were resistant to forgetting processes, the accuracy of motor reproduction decreased in seconds, just as with verbal reproduction. Posner and Konick (1966) confirmed the effect. In addition, Adams and Dijkstra used the number of practice repetitions, or moves to the stop before the retention test, as a variable. Motor forgetting was a decreasing function of the amount of practice, a finding as old as Ebbinghaus.

Two lines of inquiry could be opened at this point. One concerns the dual conception of memory, asking whether the distinction between short- and long-term memory had empirical believability for motor behavior. This line of thinking came to nothing because the criteria that were being used to distinguish short-term verbal memory from long-term verbal memory (Adams, 1967, chap. 3; Stelmach, 1974, pp. 1–5) had no counterpart for motor behavior. The other line of inquiry was forgetting theory—the process by which the retention loss occurred. Trace decay and interference were the prevailing theories of forgetting. Trace decay theory, which says that the passage of time causes forgetting, had credibility for Adams and Dijkstra's (1966) findings because a subject passively waited during the retention interval and yet forgetting occurred; no source of retroactive interference between learning and recall of the movement was evident. Proactive interference, in which interfering movements were made before the learning and recall of a criterion movement, was another possibility. Analysis showed that prior trials, in which other movements were learned and recalled and which could be potentially interfering, had no influence, and this was true in Posner and Konick's (1966) study as well. Without evidence for proactive and retroactive interference, trace decay theory gained in stature. The elevation of trace decay theory for movement did not fit an elegant conception of memory in which one theory of forgetting would serve. The interference theory of forgetting was favored for verbal memory in the mid-1960s (Adams, 1967), and it was expected to explain all. When it did not, a search began for sources of motor interference that would affect short-term motor forgetting as a means of refuting trace decay.

The research done on retroactive and proactive interference was a mix of operations and findings. Studies of retroactive interference were conducted in which interpolated activities in the retention interval were sometimes able to influence retention (Boswell & Bilodeau, 1964; Kantowitz, 1972; Patrick, 1971; Posner, 1967; Stelmach, 1970) and sometimes not (Stelmach & Barber, 1970). Studies of proactive interference had their failures too (Montague & Hillix, 1968; Schmidt & Ascoli, 1970), but positive evidence came forward when the studies were turned toward attention demand (Ascoli & Schmidt, 1969;

Stelmach, 1969b, 1969c; Williams, 1971). Attention demand is an information-processing hypothesis contending that short-term memory has limited capacity. Information in short-term memory is additive, and is lost and unavailable for responding if the sum of new information and old information already in the store exceeds capacity. According to the hypothesis, the reason Adams and Dijkstra (1966) did not find proactive interference effects from prior trials was that the subject was required to recall only the response of the current trial and could ignore the movements of all previous trials. If the subject had been required to maintain the responses of previous trials as well as that of the current trial in memory, then the responses could conceivably influence one another, possibly exceed the capacity of short-term memory, and produce decrement. The expectations from this reasoning were essentially confirmed by the studies, the most impressive of which was on directed forgetting by Burwitz (1974). Research on directed forgetting found that a subject could intentionally will the influence of material in memory away and eliminate much of its effect on to-be-recalled material (Bjork, 1972; Epstein, 1972). Burwitz reasoned that if the attention demand hypothesis is true, then instructions that directed the subject to forget about prior movements in memory should reduce proactive interference. His results were positive. Proactive interference is not a function of prior responses per se but of cognitive operations that involve them.

What can be concluded from these studies of interference about trace decay theory and interference theory as explanations for short-term motor forgetting? Not much. Something further was learned about the conditions of interference for motor responses, but it is doubtful that help was received in choosing between competing theories. In a discussion of the logic of interference theory, Adams (1967, p. 120) said that decrements produced with interference paradigms may not have the same causes as the decrements that are forgetting, and we should be wary of equating the two. Scientific proof of common causes follows more complex routing. Why we forget is in limbo for motor responses, as it is for all response classes.

Long-term retention. Very little conceptual advance was made for the long-term retention of motor skills during the Middle Period (Adams, 1967, chap. 8; Naylor & Briggs, 1961; Stelmach, 1974). Consistent with experiments on long-term motor retention that had been done in the previous 50 years or so, very high retention was found for performance in a three-dimensional tracking task (Fleishman & Parker, 1962), the Bachman Ladder (Meyers, 1967), and a flight simulator (Mengelkoch, Adams, & Gainer, 1971). Retention intervals ranged up to 2 years. About the only useful distinction that emerged during this period was that performance in a discrete, procedural task, like throwing switches in a sequence, is forgotten more readily than performance in a continuous task like tracking (Adams & Hufford, 1962; Ammons et al., 1958; Mengelkoch et al., 1971; Neumann & Ammons, 1957). Forgetting of procedural responses can be complete in about a year, although there are savings because relearning is rapid. Adams (1967, chap. 8; 1969) hypothesized that verbal components can be inherent in procedural tasks, making performance more vulnerable to forgetting processes. There is evidence for these verbal factors, but not necessarily for their role in forgetting. Neumann and Ammons (1957) found that 44% of their subjects used verbal cues in designating the location of switches.

Individual Differences

A surge of interest in individual differences and motor behavior during World War II was motivated by the need to select trainees for military aircrews; the postwar interest in individual differences and learning came as a spin-off partly of prewar research and partly of wartime research. Some background on wartime activity is useful as a matrix for postwar research. Between the great wars pilot selection was done by flight surgeons who interviewed the candidates and assigned "Adaptibility Ratings for Military Aeronautics." Bombadiers and navigators were selected by default from having washed out of pilot training. There was an expansion of aircrew training in 1941, on the eve of the United States' entry into the war, so the Medical Division of the Army Air Forces recommended the formation of a psychological agency with aircrew selection among its responsibilities. Testing, in World War I and since, had gained in scientific strength and stature. Between the wars the School of Aviation Medicine in the United States, and corresponding organizations in Great Britain and Canada, conducted research on printed and apparatus tests for aircrew selection, and a scientific literature on these tests developed. The new psychological agency, the Aviation Psychology Program with J. C. Flanagan as its director, launched a major effort in test development and use. The program was not limited to tests but diversified to other areas such as training, human factors and equipment design, and performance measurement. Tests, however, were the major effort because they were useful and cost-effective. For an overview of the roles and contributions of psychologists in World War II, see Bray (1948) and Flanagan (1948).

The use of motor tests to contribute to prediction of a criterion like success in flying school received little support from the research and ideas of university psychologists who had been studying the topic before the war. A common finding was that the intercorrelations of motor tests were low (Garfiel, 1923; Muscio, 1922; Perrin, 1921; R. H. Seashore, 1930), which led to the view that motor skills were highly specific (R. H. Seashore, 1930; S. Seashore, 1931). Even seemingly similar measures like reaction time and speed of movement had low correlations. Motor tests, therefore, would be unlikely to be valid because their specificities would deny shared variance between the tests and criteria that required motor performance. A study by S. Seashore (1931) is an example. The six tests of the Stanford Motor Skills Unit (R. H. Seashore, 1928) were used to predict success in operating winding machines in a knitting mill. S. Seashore had little luck, which he took as evidence of the specificity of motor skills and the unlikelihood that motor tests would predict industrial skills. R. H. Seashore (1940; R. H. Seashore, Buxton, & McCollom, 1940) factor-analyzed 21 motor tests in an effort to find clusters of tests that would be evidence for a small set of underlying abilities and against specificity, but did not consider his efforts successful. He isolated seven factors but said "our factors include such narrow groupings of performances as to make application to the prediction of complex practical skills a hazardous undertaking" (R. H. Seashore et al., 1940, p. 258).

Motor tests for aircrew selection were more successful than the pessimistic prewar research by university psychologists might have suggested. Working on their own, seemingly independent of psychological research in the civilian sector, military scientists in the 1930s were generating data on tests that had validity for pilot selection (for a developmental account of the Complex Coordination Test, the most successful test of the battery, see Melton, 1947, pp. 81–85). Early in 1942, a decision was made to include motor tests in the aircrew test battery along with printed tests, and so the tests had to be chosen quickly, based on sparse prewar validation data. Five tests were chosen and chosen well, because subsequent evidence showed them to be valid. A great deal of research was done during the war on promising motor tests, and occasionally some were added or deleted, but basically the first five were the core of the battery throughout the war. The motor tests contributed to validity beyond printed tests, although printed tests were given the greatest weight (Melton, 1947, p. 6).

Fleishman's research. Psychological research in the military diminished when World War II ended, but after a brief interim, increased again. In 1948 the U. S. Air Force inaugurated the Human Resources Research Center (later the Air Force Personnel and Training Research Center) under the direction of A. W. Melton. The Aviation Psychology Program of World War II had operational involvements in the routine testing of personnel for aircrew selection as well as research, but the Human Resources Research Center had only a research mission. Of the several laboratories, two conducted research on individual differences: The Personnel Research Laboratory, under the direction of L. G. Humphreys, was dedicated to the development of printed tests for selection; and the Perceptual and Motor Skills Research Laboratory, under the direction of R. M. Gagne, had, as one of its major projects, the development of motor selection tests. Much of this research picked up where the Aviation Psychology Program during the war left off, but an exception was research on individual differences and motor learning. The most visible investigator was E. A. Fleishman, a member of the Perceptual and Motor Skills Research Laboratory. Some of Fleishman's work was in the spirit of the Aviation Psychology Program of World War II. He conducted research on new motor tests that might improve personnel selection in the Air Force (Fleishman, 1953, 1956; Fleishman & Ellison, 1962; Fleishman & Hempel, 1956), but this research had a shortened half-life, partly for scientific and partly for administrative reasons. The scientific reasons were that it was difficult to find tests that would improve validities for Air Force criteria beyond that already contributed by motor tests. Motor tests in the battery were factorially complex and tests that would measure new valid variance were not easy to devise. The administrative reason was that the operational justification for motor tests disappeared because the Air Force stopped using them in 1956. The major source of recruits for pilot training became the Reserve Office Training Corps units in universities, and it was easy to administer printed tests at universities, but not motor tests. For whatever the contribution of Fleishman's research on motor tests for personnel selection, his most programmatic efforts were on individual differences and motor learning. Research on individual differences and learning is not as remote from personnel selection as it might seem because industrial and military personnel enter training programs before they graduate to a job, and a measure of proficiency in training or on the job becomes the criterion against which tests are validated. How well tests predict success at various stages of training and ultimately on the job is valuable information for sophisticated personnel selection. The common failure to chart the validity of tests throughout this progressive training process is a good reason for studying a laboratory analogue of it.

Overviews of Fleishman's research program on individual differences and learning are available (Fleishman, 1966, 1967, 1972a, 1972b), but it pays to examine representative studies in detail for the essentials of his approach. Fleishman and Hempel (1954) reported an experiment that typifies the approach. Extensive training was given on the Complex Coordination Test, which was a factorially complex motor task and served as the criterion. Printed tests and motor tests were administered as a reference battery. Measures from different levels of learning on the criterion task and the scores from the reference battery were all intercorrelated and the matrix factor analyzed, as Woodrow (1938a) had done. The results of the factor analysis were presented as the percentage of variance represented by each factor as a function of stage of practice on the criterion task. The number of factors with appreciable loadings decreased from early to late in learning, and their nature also changed. The factors primarily involved early were Psychomotor Coordination, Spatial Relations, Visualization, Mechanical Experience, and Perceptual Speed. Late in training there was a shift to a factor specific to the criterion task and to two predominantly motor factors, Psychomotor Coordination and Rate of Movement. The shift with training is from perceptual and cognitive to motor abilities.

The generalizability of Fleishman and Hempel's (1954) findings, which are the most widely cited, fit their next experiment (Fleishman & Hempel, 1955) reasonably well. The paradigm was the same as the 1954 study but a different criterion task was used. The criterion task on which training was given was the Discrimination Reaction Time Test, a four-choice reaction time task. Once again the reference battery had both printed and motor tests. In contrast to the 1954 study, the number of factors with appreciable loadings increased with practice on the criterion task. Two cognitive and perceptual factors, Spatial Relations and Verbal, primarily accounted for variance early in training. By the end of training two motor factors, Reaction Time and Rate of Movement (the factor specific to the criterion task), and to some extent Spatial Relations (which had both printed and motor test determinants) accounted for the variance. The decrease of cognitive and perceptual factors with training, and the increase of motor factors and the specific factor, are consistent with the findings of the 1954 study.

A study that used the Rotary Pursuit Test as the criterion task (Fleishman, 1960) ran into trouble with respect to generalizations from the 1954 study. Factor-analyzing scores from the Rotary Pursuit Test (the criterion task) and the reference battery en masse, as before, Fleishman extracted 11 factors. Only three had appreciable loadings on the criterion task. One was Control Precision, which accounted for about 25% of the variance. The other two with noticeable loadings were criterion-task specific. One specific factor dominated early in practice and the other late. It is difficult to reconcile an early and a late specificity factor with previous research that found one specific factor that increased with practice. In addition, when two of the three factors are task specific it is not possible to say much about the abilities required for the criterion task and how they change with practice.

The appeal of Fleishman's research is easy to understand. The interest in individual differences and the learning of skills goes back to the beginning of this century. Fleishman's finding that cognitive factors can dominate motor performance early in learning and give way to motor factors late in learning has always had appeal for psychologists (for the origins of the idea, see Bain, 1868, pp. 330–332; Spencer, 1881, pp. 450–452; James, 1890, chap. 4; for a summary of the idea, see Adams, 1981, pp. 262–264). In addition, the practical implications for selecting personnel at various stages of their training was not lost on Fleishman's readers. Notwithstanding, Fleishman has had his critics and the criticisms go deep. Bechtoldt (1962) was the most trenchant critic.

Bechtoldt contended that Fleishman's use of factor analysis, in which scores of the criterion learning task and the reference battery are intercorrelated in an omnibus matrix and factor-analyzed, is fundamentally wrong and cannot produce meaningful results. His two main points follow.

1. Factor analysis is not applicable to a dependency analysis, so common in science, when a dependent variable is predicted from one or more independent variables. Changes in the dependent variable are explained by changes in independent variables, and so constitute lawfulness; it is one of the meanings that scientists have for "explanation." Factor analysis, by contrast, is a technique of interdependency analysis in which all variables are coordinate and treated alike (Bechtoldt, 1962, pp. 323–324). Bechtoldt was drawing on the roots of factor analysis because Thurstone (1947) said the same thing:

> In factor analysis one does not select some one variable which is to be predicted or determined by the other variables. All the variables in factor analysis are treated alike in this sense. Whenever the investigator pivots his attention on one of the given variables which is central in importance and which is to be predicted by a set of independent variables, he is not talking about a factor problem. He is then talking about a customary statistical problem, involving a regression equation and multiple correlation. (pp. 59–60)

Fleishman used factors to explain individual differences in the criterion learning task, with factors as independent variables and scores from the criterion learning task as the dependent variable, but he used a technique that does not honor the distinction.

2. There is no independent definition of factors. Each factor is dependent on both the criterion learning task and the reference battery. The factors, which are to explain variation in the criterion learning task, are partly derived from it. Here again there is a failure to maintain separation of independent and dependent variables.

Bechtoldt is not alone in his criticisms of Fleishman. Humphreys (1960), arguing from the premises and logic of factor analysis, said that Fleishman's approach leads to wrong psychological conclusions. Corballis (1965) agreed. Alvares and Hulin (1972) examined all of the criticisms and endorsed them. Fleishman's method was Woodrow's method (Woodrow, 1938a), and so if Fleishman was wrong, then so was Woodrow.

Multiple-regression analysis is a methodologically legitimate way to go, so do results obtained with it differ from those obtained by Fleishman? Yes. Adams (1953) gave extensive practice on the Complex Coordination Test, which was the criterion task. The reference battery had three kinds of tests. One was a printed battery of 32 tests. The second was a simple motor battery of 13 fine manipulative tests like putting nuts on bolts, or sticking pins in holes. The third was a complex motor battery of six tests like the Rotary Pursuit Test and the Discrimination Reaction Time Test. Rather extensive practice was given on each of these complex tests so that scores early and late in training on them could be compared for their predictive power. The printed tests, the simple motor tests, and scores from early and late in training on the complex motor tests were each used as independent variables to predict initial performance on the criterion task as the dependent variable. Then the same analyses were performed with final performance on the criterion task as the dependent variable. The multiple *R*s for the printed tests, the simple motor tests, and the scores from early in learning on the complex motor tests decreased from initial to final performance on the criterion task. The zero-order correlations for these reference test scores tended to decrease with practice on the criterion task, which was an established finding (Reynolds, 1952; Woodrow, 1938c), but less interest was shown in them than in the β weights, which are contributions of tests with the effects of other tests partialed out. One of Fleishman's generalizations was that the factors shifted from verbal-cognitive to motor with practice on the criterion task. The β weights for the tests of the printed battery did not all sharply decrease from initial to final performance on the criterion task. In fact, for verbal and motor predictors alike, some of the β weights decreased, some remained the same, and some increased. It would appear that the prediction of individual differences in motor performance from early to late in training is not an orderly matter of transition from verbal-cognitive to motor. Another of Fleishman's generalizations was that performance on the criterion task becomes increasingly task specific as training progresses. If this generalization is correct, prediction of initial performance on the criterion task would always be better than prediction of final performance. Some multiple *R*s did indeed decrease from initial to final performance, but when final performance levels on tests of the complex motor test battery were used as independent variables, the multiple *R* from initial to final performance on the criterion task actually increased. Adams hypothesized a time-sharing ability, such that subjects learn to organize the component responses into the pattern required for proficiency; complex tasks have it in common at advanced stages of training. Furthermore, it was found that a multiple *R* that drew on the best predictors of each test battery was higher than the zero-order correlation between initial and final performance on the criterion task. If specificity was increasing with training on the criterion task, an intratask measure taken from anywhere in training on the criterion task would have more of the specificity entity than external tests and would best predict final performance on the criterion task. Yet, this was not so. The specificity hypothesis is one of despair because it implies that with training, performance on the criterion task will increasingly predict only itself, and that external tests will increasingly fail as predictors. Adams's findings are optimistic, saying that it is a matter of identifying abilities at the various stages of training and devising tests to measure them. In a parallel study, Adams (1957) replicated the essentials of his 1953 study with the Discrimination Reaction Time Test as the criterion task.

Theories of abilities and learning. Factor analysis is less a theory than a method, but it is a companion of theory when factors are equated to abilities, and even more so when abilities and changes in them are used to explain the patterning of individual differences in learning. Fleishman (e.g., 1966, 1967) distinguished between skill and ability. A *skill* is a level of proficiency in a particular task, as one might speak of skill in riding a bicycle or flying an airplane. Unlike a skill, which is a bedrock empirical notion and directly observable, an ability is an abstraction. An *ability* is an enduring and general trait or response capability, and there are a limited number of them. The ability of manual dexterity can be used to pick up a pin or screw a nut on a bolt. A spatial visualization ability can be used to read a map, land an airplane, or interpret a painting. Analysts like Fleishman see factor analysis as the statistical machinery by which the limited number of abilities in the human repertoire will be known; factors and abilities are equated.

Investigators of individual differences and learning have always been more interested in individual differences than in learning, but they are learning theorists more than they have known. The two theoretical approaches that have been taken are found in Woodrow (e.g., 1939a), and are reviewed by Alvares and Hulin (1972). The first approach hypothesizes that an individual has a limited set of very well-learned response capabilities, called abilities, that he or she brings to the task and uses in learning. Presumably they are response classes at the asymptote of their learning. What we observe as a learning gain is increasing efficiency in the use of the abilities. In efforts to attain the goal of the learning task, the subject brings different combinations or levels of abilities into play. Information about the adequacy of his or her responding (e.g., reinforcement, knowledge of results) will help him or her work out the optimum combination of abilities, and once this is done, the task is highly learned. The influence on Woodrow for this idea was R. H. Seashore (1930, 1939, 1940), who wrote of "work methods" in learning skills, or of arriving at the correct combination of response patterns to succeed in the task. The larger influence on the idea is a viewpoint in psychology that goes back to the nineteenth century and says that an organism has a repertoire of movement segments that can be assembled and reassembled to resolve the behavioral assignment confronting it (Adams, 1984).

The second approach does not necessarily assume that all abilities are very stable and at the asymptote of their learning curves. Being mostly learned, some of the abilities might fall short of their asymptote and be amenable to additional learning. Woodrow (1939a) said that if this is so, then practice on a learning task that requires an ability should transfer to tests that measure it. His one attempt at evaluating this good idea failed (Woodrow, 1939c), and so he opted for the other approach—learning is a matter of restructuring abilities, not strengthening them. Alvares and Hulin (1973) had some success with a transfer of training experiment like Woodrow's. Using flight training as their learning situation, they sought transfer to a test battery that measured some of the abilities required in flying, and positive transfer was obtained. Positive results notwithstanding, this important line of thinking is young and will profit from more empirical data. Greater scientific advance can come from determining the fundamental nature of abilities than from assuming that abilities are existential entities that await discovery with a diligent application of factor analysis.

Present Period

Research on skills was lively in the 1940s and 1950s, but began to decline by the 1960s. Why the decline?

1. Federal support decreased. It was commonly heard in military research circles that automatic control would soon replace manual responding, and so research on skills would not pay off. Therefore, research on skills fell from favor.
2. Ideas that motivated research on skilled performance in the 1940s and 1950s were not energizing investigators. Hull's theory had died and was no longer a research catalyst. Research on individual differences and learning was using the same ideas as before. Transfer of training research lacked a clear direction.
3. The rise of cognitive psychology in the 1960s was probably a factor in the decline. Only recently has cognitive psychology shown an interest in learning, which was always a motivator for the study of skilled performance.

Item 3 deserves more attention. Why was cognitive psychology, in its first 15 years or so, indifferent to learning when change with experience is one of the pervasive characteristics of behavior? There are three possible reasons. First, cognitive psychology was a reaction to behaviorism. Behaviorism was deficient in its handling of attention, memory, and higher mental processes, and when cognitive psychology arose to correct these shortcomings it rejected behaviorism, and learning along with it. The associative process was dear to behaviorism. Learning was central to the thinking of Watson, the founder of behaviorism. The great behavioristic theorists were learning theorists: Hull, Tolman, and Guthrie. Skinner, a nontheoretical behaviorist, has been consumed with the learning process.

The second reason, and a corollary of the first, is that cognitive psychology emphasized the active mind of a subject, seeking to extract information from the environment. Behaviorism had the unspoken assumption of a passive subject, and its view of learning exemplified this. In classical conditioning it did not matter whether the organism was consciously aware of the conditioned stimulus, the unconditioned stimulus, and the relation between them. If these stimuli occurred in proper sequential and temporal relation, learning occurred. This was also true for instrumental learning. If the reinforcement appropriately followed the response then learning occurred, whether or not the subject thought about the response-reinforcement relation or even knew about it.

The third reason is that cognitive psychology has been driven by the information-processing approach, which is a digital computer analogy (Bower, 1975; Bower & Hilgard, 1981; Estes, 1975, 1976), and has not had much to say about learning. The processes of a digital computer are most evident in storage, searching, and retrieval, and if artificial intelligence and its use of computers are invoked then pattern recognition fits also. The closest the information-processing approach ever came to an interest in learning during this time was the use of rehearsal to give items increasing stability with respect to time (e.g., Atkinson & Shiffrin, 1968). A systematic concern with learning was not part of the approach, presumably because computers do not learn. For criticisms of this neglect of learning, see Kolers and Roediger (1984) and Kolers and Smythe (1984).

Interest in motor learning would have gone even further downhill if it had not been for an unexpected ally in the discipline of physical education, which had its own reasons for studying skilled performance. Those involved in physical education did not seem to care whether emerging cognitive psychology was interested in learning or not. Physical education specialists had been training skills since the games of ancient Greece and they have both a great deal of interest in it and practical wisdom on how it is done. A major movement in physical education arose in the late 1960s and early 1970s under the influence of F. M. Henry, A. W. Hubbard, and A. T. Slater-Hammel, who were laboratory scientists within the field, and important influences on the graduate students they trained. The movement's philosophy was that experimental science is a source of new insights into the training of skills. The research that came from this movement was rationalized by the needs of physical education, but often it fit the mold of experimental psychology.

Coming at the historical hour that it did, research on skills in physical education was not untouched by the information-processing approach and its indifference to learning. The perspective took the form of an interest in motor control, as distinct from motor learning. As far as learning was concerned, a closed-loop theory of learning (Adams, 1971) appeared at the beginning of the Present Period and became a stimulus for revival. Schmidt's (1975) schema theory continued the momentum.

Theories of Motor Learning

Closed-loop theory. A closed-loop system is distinguished from an open-loop system. A closed-loop system has feedback from the response, error detection, and error correction, and an open-loop system does not. In a closed-loop system the response is fed back to a reference of correctness. The difference between the response and the reference is error and the system automatically corrects it. The automatic home furnace works this way. The thermostat setting is the reference and the heat output of the furnace is compared with it. If there is a discrepancy, the furnace will turn on or off until there is no discrepancy. An open-loop system is like a wood-burning stove. The heat output depends on the amount of wood piled on the fire. There is no sensing of too much or too little heat and a compensatory adjustment of the fire.

Instrumental learning, which is the way motor responses are learned with knowledge of results, is an open-loop system in the traditions of Thorndike and behaviorism. If energizing agents like motivation and the habit strength developed by reinforcement (or knowledge of results) are sufficient, the response occurs, otherwise it does not; there is no appraisal of the response for correctness or an adjustment if it is wrong. Open-loop conceptions of learning have made use of feedback stimuli generated by the response, but the use was in the behavioristic tradition of associating stimuli with responses. The response-produced stimuli of one response segment becomes connected to the next response segment as a way of explaining the learning of movement sequences and serial action; it was not the comparison of feedback stimuli with a standard of correctness for the detection and correction of error (Adams, 1968, 1984).

The closed-loop theory of motor learning was published by Adams in 1971, and dimensions of it are discussed in various articles (Adams, 1967, chap. 10; 1968, 1976, 1978, 1984; Adams & Bray, 1970). The theory was partly a reaction to perceived shortcomings of the open-loop tradition of motor learning, and partly to the attractiveness of the cognitive, informational view of knowledge of results. Central to closed-loop theory was the assertion that motor learning was not the strengthening of habit with knowledge of results but acquiring the capability to detect and correct errors, and the growth in this capability is central to the learning process. In addition, closed-loop theory rejected the Thorndikian idea that knowledge of results operates automatically on an unaware subject and contributes an increment to habit.

The closed-loop model was not discovered by the psychology of learning. The origins were servotheory in engineering, which engineering psychologists in the 1950s found useful for describing operators' tracking behavior as controllers of a system like an automobile or an airplane (Adams, 1961a). Other influences came from experimental phonetics (Fairbanks, 1954) and medicine (Chase, 1965a, 1965b). Some of the influences came from the Soviet Union. Anokhin (1961, 1969) and Sokolov (1969), in pursuing research on the orienting reflex, developed a closed-loop model of classical conditioning. The Soviet physiologist Bernstein was concerned with motor behavior and a closed-loop description of it, although learning was not a part of his thinking (Bernstein, 1967).

The closed-loop theory of learning was perceptual and cognitive, as well as closed-loop. Perception was in the reference mechanism for error assessment. The stimulus feedback accompaniments of movement, such as visual and proprioceptive stimuli, imprint a representation of themselves on each trial, just as experience with a visual scene would lay down a representation of itself in memory for later recognition of the scene. As the subject uses knowledge of results on each trial to improve performance, the response-produced feedback on each trial compounds to lay down an increasing representation of the feedback that the correct response should have. This representation is called the *perceptual trace.* When a subject at a relatively advanced stage of learning makes a correct response, knowledge of results shows no error and the matching of response-produced feedback with the perceptual trace tells the subject that the correct response was made. An effort is made to repeat the response on the next trial. If the response is substantially wrong, the knowledge of results will report it and the mismatch of feedback and the perceptual trace will produce subjective feelings of error also. The subject will see the necessity for changing the response on the next trial. Knowledge of results and the feedback–perceptual trace comparison are both sources of error information and complement each other on acquisition trials in which knowledge of results is provided, but in the absence of knowledge of results the subject still has information available, providing the perceptual trace is developed and response-produced feedback is sufficient. Thus, knowledge of results and the correspondence between feedback and the perceptual trace, as sources of error information, combine to produce the trial-by-trial changes that constitute learning. The axiom underlying this account of motor learning and performance is that motor learning is at heart a perceptual process. The subject relies on the perceptual trace whether he or she is asked to generate a response in recall or to recognize a response that has been made before. A second axiom qualifies the first by saying that another construct, called the *memory trace*, which is nonperceptual, is required to start the movement. There is no feedback before the response begins, so the perceptual trace cannot be used. The memory trace is a brief motor program that selects and initiates the response, preceding feedback and the use of the perceptual trace.

Knowledge of results was considered error information that was used in relation to the perceptual trace for making the next response different from the last one by having less error. In the interval between the response and knowledge of results the subject need only remember the response, but in the post-knowledge of results interval the subject uses the error information to form a hypothesis about the response for the next trial that will correct the error. The use of error information from knowledge of results and the feedback–perceptual trace comparison is a verbal and cognitive activity—hardly a Thorndikian stamping-in of habit.

Motor learning, in theory, does not always require knowledge of results. Of course knowledge of results is required early in learning because the subject has no idea of what the correct response is to be and so it must be shaped by an external source. As learning advances, and the perceptual trace becomes well developed, the subject has a basis of internal error sensing and can rely less on knowledge of results. Theoretically, the knowledge of results, as an external source of error information, can then be withdrawn and the subject can proceed with learning using the internal source of error alone. Learning without knowledge of results is called *subjective reinforcement*.

How much empirical support does the theory have? Effective theory, to avoid fantasy, must be empirically seeded at birth, consequently many empirical findings were compatible with the theory at the start (Adams, 1971). Since then, additional support has come from a number of studies. A crucial assumption of the theory is a strong role for feedback. Performance in acquisition trials with knowledge of results and on trials without knowledge of results should be positively related to the amount of feedback. Furthermore, the greater the experience with feedback over trials the more stable performance should be in trials with knowledge of results withdrawn. If the perceptual trace is strong enough, learning without knowledge of results should occur as the subjective reinforcement effect. In addition, what is most important, feedback should be related to error detection and correction, which is a requirement of a closed-loop system. Finally, as a basic assumption, the memory trace that starts the movement is independent of feedback and the perceptual trace that regulates the movement after it has started.

When the amount of proprioceptive feedback is varied by spring tension on a control (Williams, 1974), or when proprioceptive and visual feedback are varied (Adams, Goetz, & Marshall, 1972; Adams, Gopher, & Lintern, 1977), the greater the feedback the better the performance on trials both with and without knowledge of results. The greater the number of acquisition trials the greater the performance stability when knowledge of results is withdrawn (Adams et al., 1977; Newell, 1974). Moreover, the greater the number of acquisition trials the greater the subject's capability for estimating the accuracy of response (Adams et al., 1972; Adams et al., 1977; Newell & Chew, 1974; Schmidt & White, 1972), showing that an internal reference for judgment of error develops. Whether improvement in performance can occur without knowledge of results is unclear. The most common finding is that performance holds steady after enough trials with knowledge of results (Adams et al., 1972; Adams et al., 1977; Newell, 1974; Schmidt & White, 1972; Williams, 1974), but gains that can be called learning are usually not found. An exception with an interesting experimental design was a study by Newell (1976b). Newell reasoned that if observational learning techniques were used to give perceptual experience with feedback stimuli, the subject should develop a strong perceptual trace even though no movement had been made. When given practice on the motor task without knowledge of results the subject should learn because the perceptual trace is the basis for error perception, corresponding to knowledge of results from an external source. The task was a ballistic move of 10.16 cm in 100 ms. A subject first listened to auditory cues for the movement when an expert performed it. The subject then transferred to the task without knowledge of results. Not only was there positive transfer from the auditory pretraining, but improvement occurred, as theorized. In addition, feedback and amount of practice were related to error detection and correction (Adams & Goetz, 1973), which is essential for a closed-loop account. Lastly, is feedback, and the perceptual trace that it creates, independent of the memory trace that initiates the response? Following Schmidt and White (1972), Newell and Chew (1974) used a fast ballistic movement and argued that after practice the elimination of auditory and visual feedback should affect error estimation based on the perceptual trace but not response initiation based on the memory trace. Their results were positive. This remains the only deliberate test of the two-trace notion, perhaps because the theory's emphasis is perceptual and on one of the traces. The theoretical assertion that error detection and correction, and movement regulation, are fundamentally perceptual processes has been directly confronted and has received some empirical support (Kantowitz, 1974; Marshall, 1972), but not all analysts are convinced (Laabs & Simmons, 1981).

The handling of knowledge of results in the theory was informational and cognitive. The findings for knowledge of results tilt toward the theory, although they are somewhat mixed (Newell, 1976a). The theory says that the interval between the response and knowledge of results is relatively innocuous, with only memory for the motor response required, and Shea and Upton (1976) have shown that to be likely because interpolation of memory for other motor responses in the interval is interfering. The interval between the knowledge of results and the next response, however, is mentally active as the subject processes the error and plans the next response to eliminate it. In the review of the Middle Period a number of experiments were cited with no effect of delay on knowledge of results, and that generalization continued to be supported (Schmidt & Shea, 1976). The studies on the post-knowledge of results interval usually have taken the form of liberalizing or constraining opportunities for processing error in the interval. The two procedures that have been used were to shorten or lengthen the interval, or to fill the interval with an activity that would either compete with the processing or not. Weinberg et al. (1964) used a line-drawing task with an interval of 1, 5, or 20 s. Shortening the interval to 1 s impaired performance, suggesting there was insufficient time to process the knowledge of results. Other studies, using positioning tasks, failed to find an effect of interval duration (Boucher, 1974; Magill, 1973, 1977). Experiments that interpolated either verbal or motor activities in the interval in an effort to interfere with processing, which have been criticized (Schendel & Newell, 1976), have sometimes had positive results for theory (Boucher, 1974) and sometimes negative results (Magill, 1973, 1977). Rogers (1974) varied the length of the interval as well as the precision of knowledge of results, ranging from qualitative knowledge of results with reports like "too far" to quantitative knowledge of results with reports that varied in precision, like 3, 3.2, or 3.214. The hypothesis was that information processing should become more difficult with shorter intervals and more complex knowledge of results. His findings were positive. Performance was degraded as the complexity of knowledge of results increased, but not when the interval was long enough for the subject to process the information. The length of the interval and the complexity of the knowledge of results joined to determine performance. Barclay and Newell (1980) took a different approach to the post-knowledge of results interval. They allowed the interval to be self-paced, and found that performance in a ballistic movement task was related to duration of the interval as theory says, although the relation was not entirely anticipated. Both error in movement time and duration of the interval decreased as a function of trials. Presumably the subjects became increasingly adroit at processing the error information in knowledge of results as trials progressed. Barclay and Newell also found that only about 1 s was required to process knowledge of results in their simple task. The mixed results of the other studies of processing in the interval could be a failure to appreciate the speed of error processing in simple tasks.

Motor programming and schema theory. The closed-loop theory of learning has had two lines of criticism (Schmidt, 1975): (a) that the theory relies too much on response-produced feedback and consequently fails to consider that movement sequences can be run off centrally without the aid of feedback; and (b) that it fails to consider response variability, not of the random error kind but of the productive kind in which responding is flexibly adapted to a changing situation. The first criticism is of the theoretical structure and the second says that additional theoretical conceptions are required to account for empirical data.

Theory by Schmidt (1975, 1976a, 1976b, 1980, 1982a, 1982b, 1982c) was offered to accommodate these criticisms. The theory is called *schema theory,* although it relies heavily on the concept of motor programming as well as schema. The concept of the motor program said that movements are structured and driven centrally, and that substantial portions of a movement can be run off without the regulatory assistance of response-produced feedback. The use of feedback is not denied, however. There is much evidence supporting feedback as a variable for motor behavior, and it would be unwise for a theory to ignore it (Mulder & Hulstyn, 1984), nevertheless the concept implies that movements of some unspecified duration can occur without feedback. The origin of the motor program lies almost entirely in research on deafferentation of lower animals, in which an attempt is made to surgically deny afferent input from a movement by sectioning the dorsal roots of the spinal nerves. Deafferented animals typically show some competence, and it is argued that the agents of movement do not always need peripheral information and so must be central. These data and arguments are generalized to human motor behavior, usually learned behavior. (For reviews of the literature, with arguments for and against the motor program, see Adams, 1968, 1971, 1976; Fentress, 1976, 1977; Keele, 1968; Keele & Summers, 1976; Schmidt, 1975, 1976a, 1976b, 1980, 1982a, 1982b, 1982c; Taub 1976.)

The schema concept originated in neurophysiology (Adams, 1976), but soon found its way into psychology, and it is primarily used to account for versatility in behavior. Accounting for behavioral versatility is nothing new in psychology, such as when stimulus generalization is used to explain the same response to changed stimuli (e.g., Hedges, Dickinson, & Modigliani, 1983), but advocates of schema would regard stimulus generalization as a pale explanation for the kind of behavioral versatility that impresses them. On the perceptual side, schema is seen as the power of categorical recognition, in which all members of a class can be identified, like any triangle or any vehicle. On the recall side, schema is seen as a categorical concept that yields the ability to hit a tennis ball many different ways to the same place or, more simply, to take several behavioral routes in touching a desk corner. In addition, schema is used categorically in response recognition, in which the response-produced feedback of the various responses initiated by the recall schema are recognized as either correct or not. Pew (1974) implied schema as an abstraction that governed movement, and for subsequent and more explicit discussions of schema with respect to movement see Adams (1976, 1984), Schmidt (1975, 1976b, 1982a, 1982c), and Van Rossum (1980).

Schmidt has taken these concepts of motor program and schema as anchors and defined them in a theory that uses response-produced feedback less than Adams's theory and whose behavioral centerpiece is response versatility. The motor program is an abstract structure in memory that is prepared in advance of the movement, and it contains the patterns of muscle contractions and relaxations that define movement. The motor program, by definition, does not need response-produced feedback to run off the movement, but if something should perturb the course of the movement, then feedback transmits the information centrally and an error correction is initiated. Adams (1971) uses the concept of memory trace as a motor program of a few milliseconds duration to get the movement started, after which feedback and the perceptual trace take over, but Schmidt has the motor program operating for 1 s or more before, presumably, another is selected (Schmidt, 1982a, p. 207). Not every movement segment is assumed to have its own motor program, because it is believed that memory could not store them all, so Schmidt conceived of a generalized motor program. Throwing a ball is not a composite of many small programs but is one program, and the contention is that the comparatively small number of these omnibus programs easily fits within the brain's storage areas.

We can throw a ball in an infinite number of ways, so how is a particular way of throwing it specified for the generalized program? By the selection of values for movement parameters, like force, duration, and movement amplitude. Recall schema and response recognition schema enter at this point. Recall schema selects the values of the movement parameters that specify the particular movement to be made from among those in the movement category, and the response recognition schema evaluates the correctness of the movement that is made. The schemata get their capabilities by abstracting information from the parameters, knowledge of results, feedback consequences, and initial conditions over the many times that the movements of a response category have been made. In a particular situation the recall schema reacts to the initial conditions that call for the movement and specifies the parameters, and the response recognition schema is the reference against which feedback from the movement is evaluated for error. Knowledge of results is used along with subjectively perceived error to redefine the parameters if the movement is to be made again.

Schmidt's schema theory has a number of learning issues associated with it, like how the generalized motor program is acquired and how the schemata acquire the abstract rules that govern their operations. These learning issues have been diminished by the overriding theoretical purpose of explaining movement versatility. What kind of research on versatility has the theory generated and what has been its outcome?

The typical experiment on recall schema has an experimental group that is trained on varied instances of a motor response, and a control group that is trained on the same motor response. Both groups are then transferred to new variations of the motor response that neither have made before. Schmidt's theory predicts that varied training would produce the best performance in transfer, and in a review of these experiments Shapiro and Schmidt (1982) noted that the findings are mixed, but tend to be positive. The positive effect is stronger with children, perhaps because adults have had so much varied training in their extraexperimental lives that the varied training they receive in the laboratory adds little to the capabilities they already have. The positive findings for children notwithstanding, recall schema for motor behavior will probably not go far as a concept until there is a way of defining the response class to which a recall schema applies. In perceptual studies of categorical learning, the categories, such as triangles or birds, are given a priori to the experimenter; this does not happen in motor studies. Without a definition of response class, a failure of varied training to produce the advantage expected from schema theory may mean only that the varied training has unwittingly spanned two or more response classes, not that schema theory is wrong (Zelaznik, 1977).

The response recognition schema is more difficult to study because the sensory consequences of response might seem to require a response to generate them and thus be confounded with recall schema. There is only one memorable study on response recognition schema (Zelaznik, Shapiro, & Newell, 1978), and it gets around this potential problem by having the subjects only listen to a recording of the sensory consequences of responding for a ballistic movement of 10 cm in 130 ms. Varied training was listening to the auditory feedback of movements of different durations around 130 ms. The constant training was always listening to a movement of 130 ms duration. The subjects then transferred to practice on the task itself, without knowledge of results, and those with varied training performed the best, as the theory predicts.

Knowledge of Results

Most of the modern research on knowledge of results has either enlarged on empirical findings of the Middle Period or has tested hypotheses derived from contemporary theories of motor learning, and some of it was discussed in the section on *Theories of Motor Learning.* The excellent reviews (Newell, 1976a; Salmoni et al., 1984) available of knowledge of results and motor learning should be consulted for complete coverage of the topic. Two developments of the Present Period, however, one empirical and one methodological, deserve discussion.

Kinematic knowledge of results. Adams (1978, 1984) has reviewed the historical reasons why knowledge of results is almost always a score related to a movement outcome in relation to an environmental goal, and why there has been little research on the refinement of a movement sequence required in getting to the goal. Yet, much of motor learning in the practical world entails the shaping of long movement sequences. The diving coach tries to cultivate an ideal pattern of movements throughout a dive, and the instructor of classical ballet succeeds only when traditional movements have been refined. That a research interest in the shaping of movement sequences is now taking place does not mean that past research on outcome knowledge of results is destined for oblivion. New scientific knowledge builds on old, and our principles of outcome knowledge of results should continue to be useful. One reason for survival is that outcome knowledge of results, as the informing event at the end of the sequence, can exert an influence on the movement sequence that precedes it (Adams, 1984, 1985).

The shaping of movement sequences has been called *kinematic knowledge of results* (kinematics is the branch of mechanics that deals with pure motion without reference to the masses or forces involved in it; (Newell & Walter, 1981). So far, the research literature indicates three ways to present kinematic knowledge of results.

1. Show a subject the pattern of his or her response sequence only. Error in the pattern must be inferred.
2. Show a subject the pattern of his or her response sequence along with the ideal pattern that is to be achieved. The difference between the two is error and is seen directly.
3. Give a subject error information for some or all of the segments that make up the movement sequence. An instructor might point out several mistakes that the subject made as he or she advanced to the goal.

There are no studies of the comparative merits of these three ways of administering kinematic knowledge of results but there are data that address each and sometimes compare them to outcome knowledge of results. Newell, Quinn, Sparrow, and Walter (1983, Experiment 2) illustrated the merits of the first way by showing that a velocity–time display of an arm movement worked better for minimizing movement time, which was the task goal, than a report of movement time itself, which is outcome knowledge of results.

Following Howell's (1956) lead, Newell, Sparrow, and Quinn (1985, Experiment 2) demonstrated that there are advantages in using the second way. The task was to reproduce a particular force–time function with an isometric control. The subjects of the key experimental group had kinematic knowledge of results in which the actual force–time function was superimposed over the ideal function. The comparison was with other groups that had outcome knowledge of results in terms of integrated absolute error or peak force. The subjects who received kinematic knowledge of results performed the best.

The third way is common in athletic training and the performing arts. Videotaping of a performance is used by instructors to show trainees their movement patterns. Rothstein and Arnold (1976) have reviewed the literature on videotaping as a way of training athletic skills. Watching a videotape without critical comments may sometimes have its benefits, but it is better to draw the trainee's attention to specific aspects of his or her movement sequence. Wallace and Hagler (1979) also demonstrated the importance of directing the trainee's attention to segments of the movement sequence. The task was shooting a basketball through the hoop from a fixed position. All subjects saw the results of each shot as outcome knowledge of results but, in addition, one group was given kinematic knowledge of results in the form of information about stance and motion. A second group received only general encouragement. Both groups learned and there was no difference between them in the acquisition phase in which treatments were administered. When treatments were withdrawn in a test phase, however, the group that had received kinematic knowledge of results was superior. An aspiring basketball player can learn alone, with only outcome knowledge of results, but a coach who adds information about the course of the movement sequence also makes a contribution.

Adams (1985) compared kinematic and outcome knowledge of results in the learning of a movement sequence. A movement sequence was seen as composed of segments, each of which contributes to outcome, that differ in weight and do not contribute equally to outcome. The position of the golf club at the start of a swing may not contribute as much to the distance the golf ball travels as do the arc and speed of the swing. When only outcome knowledge of results is available (a common case, as when the golfer sees only where the golf ball goes), the trainee must use the time between trials to draw inferences about pertinent segments, segment weights, and segment errors in relation to goal error. That most performers attain only a modest level of performance for popular skills over a lifetime of practice is testimony to the difficulty of this inference process. The complexities of problem solving can be greatly reduced if the performer receives segment knowledge of results along with outcome knowledge of results because he or she will then have little left to figure out alone. The task is even easier if the trainee has an instructor who also tells him or her what to do about segment errors. Adams pursued this reasoning by using a three-segment acceleration task and a mathematical model of a movement sequence in relation to it. Experimental variables were segment weights computed with the model, outcome knowledge of results, and segment knowledge of results with outcome knowledge of results added. Learning occurred for the entire movement sequence when only outcome knowledge of results was given, with better performance on the most heavily weighted segment. The segment that was most heavily weighted is the big contributor to outcome knowledge of results, and a subject could easily infer that concentration on the segment would be the easiest way to reduce goal error. The addition of segment knowledge of results to outcome knowledge of results produced the best performance of all. Learning the movement sequence is comparatively easy when the subject is unburdened of the inference process.

Methodological considerations. Salmoni et al. (1984) turned their review of knowledge of results into a methodological reexamination of the topic, and they urge that the topic be restructured and cast anew. They looked at knowledge of results from the standpoint of the learning and performance distinction. Salmoni et al. said that knowledge of results, when it is being administered in acquisition trials, has guidance and motivational functions. In guidance the subject relies on the informational properties of knowledge of results that guide him or her in responding on the next trial. Knowledge of results is presumed to be motivational also because subjects are said to be indifferent without it and to work harder with it. When knowledge of results is performing these functions, it is hard to see what it contributes to learning, just as it is hard to see what is being learned when performance is depressed by massed practice. Test trials without knowledge of results are required to assess learning, it is argued. Salmoni et al. examined the literature for examples of the transfer paradigm with respect to variables for knowledge of results. They found that for some variables performance is affected one way when knowledge of results is present in acquisition and another way when it is withdrawn. For example, Boulter (1964) attempted to understand processes in delay of knowledge of results by interpolating verbal and motor activities in the delay interval during acquisition. Performance was unaffected by the interpolated activities in acquisition but they had pronounced effects in test trials with knowledge of results withdrawn.

Observational Learning

Kinematic knowledge of results is one way of learning a movement sequence, and observational learning is another, although they may not be equally efficient. Both of these areas are underdeveloped, and for different reasons. As mentioned previously, kinematic knowledge of results is underdeveloped because of the emphasis that had been placed on outcome knowledge of results, in the tradition of instrumental learning. Similarly, observational learning is underdeveloped because adherents of the psychology of learning have dedicated their energies to instrumental learning and classical conditioning. If everyday experience bears out that observational learning can sometimes work, and research bears out everyday experience (e.g., Rosenthal & Zimmerman, 1978), then why is observational learning an underdeveloped area? One historical reason is that dominant fathers of the psychology of learning in the United States, which has always been a center of learning research in the world, never had much luck with observational learning, and so learning operations that worked moved center stage. Thorndike was a strong advocate of instrumental learning because it worked well with animals and observational learning did not (Thorndike, 1898/1970, 1901/1970). Watson, the founder of behaviorism, had no luck with observational learning in animals either (Watson, 1908), and so he cast his lot with classical conditioning. That observational learning never had much credibility in mainstream experimental psychology did not prevent its growth in developmental psychology (e.g., Piaget, 1962; Rosenthal & Zimmerman, 1978). Despite the ebb and flow of themes and theories, developmental psychologists have always accepted observational learning because of their belief that children learn much of their language and social skills in that way. Observational learning has also been accepted at the practical level in physical education, such as when a skilled instructor demonstrates required movements to a novice (Gould & Roberts, 1982), but research on it in physical education has been far less extensive than in developmental psychology. Not until the 1970s, under the spur of cognitive psychology, did observational learning acquire a larger following. Bandura (1971, 1977), with his social learning theory, was its most influential proponent.

Watching someone else perform is the common approach to observational learning, although other modalities than vision have been used (e.g., Newell, 1976b). According to Bandura, a model's performance imparts a cognitive representation (not a template) to the observer. The cognitive representation could be verbal, as with a verbal description of the model's behavior, or nonverbal, like an image. Bandura hypothesizes two uses of the cognitive representation. One is to guide the observer's behavior when he or she is called on to reproduce what was observed, and the other is a standard of correctness for the detection of error between the response and the representation. The observer must be motivated to learn and must pay attention to the model.

Bandura's theorizing has influenced research on the observational learning of motor behavior (Landers & Landers, 1973; Landers, 1975), and an experiment by Martens, Burwitz, and Zuckerman (1976) with children is an example of the kind of research on motor behavior that it has stimulated. Films were used for observational training in the task of rolling a ball up an inclined plane to a target. The training was only slightly successful. Success in observational training was achieved in a subsequent experiment in which the ball task was changed to benefit from a play strategy. Some of the subjects saw a film that demonstrated a successful strategy, and others saw a film with an unsuccessful strategy. The observational training did not affect the subjects' motor behavior very much, perhaps because not enough training was given, but the films were clearly successful in teaching strategy.

One of the reasons that observational learning is incomplete is that there are important dimensions of the movement that are unavailable to the subject's view. Examples of unavailable dimensions are pressures, muscular tensions, and external features of the movement that cannot be seen (Adams, 1984). The cognitive representation is incomplete without these dimensions, and so the reproduced movement will be imperfect. Observational learning may not be destined to be incomplete forever though, because ingenious ways might be devised to make the unobservable observable. Carroll and Bandura (1982) demonstrated the importance of making observable those parts of the model's movements that are normally out of view. A complex arm movement was modeled, and a television system was used to make the unobservable sides of the movement observable. Benefits to the observational learning were obtained.

Carroll and Bandura required their observers to learn the spatial requirements of the task, not the timing of the spatial elements, but Adams (1986) showed that the timing of the segments of a movement sequence can be learned by observation also. Observational learning was combined with the informational view of knowledge of results. The procedure was to have the observer privy to the model's knowledge of results or not as the model learned. An observer who watches a model learn, with no other information available, will passively see the model's behavior change over trials and something undoubtedly will be learned. When, however, the observer sees both the learning model's response and his or her knowledge of results, he or she not only forms a cognitive representation but can join the model in other cognitive activities as well. The observer should be able to use his or her developing cognitive representation to perceive error in the response being observed, and can test his or her perception of error against the model's knowledge of results. Furthermore, the observer should be able to use this error information to project the correction required on the next trial and then, on the next trial, see whether the model did it. An observer who receives the model's knowledge of results should show the benefits of these mental activities when required to perform the motor task because he or she has an enriched cognitive representation. A group that had this observational learning–knowledge of results procedure was compared with a group that only watched the model learn and was not given the model's knowledge of results. This latter group showed some benefits of observation when tested in the motor task, but the group that received the model's knowledge of results as well had benefits that were more stable and enduring. Bandura is on the right track in saying that the cognitive representation is more than a template.

How much can be learned by observation, even when remote features of the movement are made accessible as Carroll and Bandura (1982) did, is not known. Newell, Morris, and Scully (1985) hypothesized that the observational learning of movement is limited. Observation may teach about movement segments, their order, and the bounds of their operation, but thereafter knowledge of results must take over and refine the sequence in all of its dimensions. More research is needed, however, before this hypothesis can be accepted or rejected. We do not know salient cues in the perception of movement and how to bring them to the attention of the observer (Scully & Newell, in press). We need to refine our methods of independently measuring the cognitive representation and relating it to the motor behavior based on it, as Carroll and Bandura (1985) did. In their study subjects were shown, in scrambled order, photographs of the components of the motor pattern that had been observed, and were required to place them in an order that corresponded to the pattern. Scores on this test correlated with the motor behavior that had been learned by observation. Tests of this kind are a route to evaluating Newell et al.'s (1985) hypothesis that a cognitive representation acquired by observation is inherently impoverished and incapable of wholly governing a refined, expert skill.

Distribution of Practice

Isolated experiments on fatiguelike processes have not joined to define memorable scientific activity. Driven by Hull's theory, distribution of practice was a heavyweight in the Middle Period and is remembered. When Hull's theory fell, the interest in distribution of practice fell with it and has not risen again.

Transfer of Training

Experiments on proactive and retroactive interference for motor behavior that were visible in the Middle Period dropped from sight in the Present Period, perhaps because corresponding research on verbal behavior also dropped from sight. Verbal interference was the most active of the two research areas but its associative, behavioristic slant was enough to cause its decline when cognitive psychology arose. Research on verbal mediation and transfer had the same fate for the same reason. Verbal mediators were seen as being linked in a chain with the motor responses they instigated, but cognitive psychology would not conceptualize verbal behavior in such a simplistic way. Moreover, the response-chaining hypothesis is dead (Adams, 1984).

Part–whole transfer of training has dropped from interest also, but for different reasons. Part–whole training research has always had a constituency with an applied orientation. The issues have gravitated around devices that train skills, usually flying skills, with part-task training devices among them. Part-task training devices have the difficult problem that no one has devised a design methodology for them. The design concept of a whole-task training device is comparatively easy because the device is usually made as much like the parent system as engineering technology and funds allow. The specification of a part-task training device, however, is intuitive. An analyst uses knowledge of the parent system and its operation to specify a part that should be represented as a training device. The assumption is that the part is important and training with a device that represents it will produce positive transfer. The assumption can be correct, but what is needed is a method whereby one or more part tasks most closely associated with success of the parent system can be identified as a guide to the development of part-task training devices (Mané, 1985). Useful hints about directions to follow might be found in a review of the part–whole literature for tracking by Wightman and Lintern (1985). They distinguished between studies that have used segmentation, in which the parts are sequentially arranged along temporal or spatial dimensions; fractionation, in which two or more parts are performed simultaneously; and simplification, in which a dimension of the whole task is made easier, as in adaptive training. The most encouraging results for transfer of training were found when the whole task allowed segmentation of the parts.

A promising new idea for transfer of training is called *contextual interference* (see Shea & Zimny, 1983, for a review). Shea and Morgan (1979) did a prototypical study of it. The basic task was knocking down six barriers in a prescribed order by hand as quickly as possible. High contextual interference was random presentation over trials of three variations of the task, in which the order of the barriers differed for each variation. Low contextual interference was blocked presentation, in which all trials for a variation were completed before going on to the next one. Training with high contextual interference produced the highest retention and transfer to new variations of the task. These advantages of varied training invite a schema explanation, but Shea and his associates prefer the explanation that high contextual interference encourages multiple processing strategies in acquisition that benefit learning and retention.

Retention

The conceptual distinction between short- and long-term memory systems (as distinct from the empirical distinction between short- and long-term retention) had viability for verbal retention, and in the 1960s it appeared to have value for motor retention also. It was hoped that verbal retention and motor retention could be lawfully and theoretically described in the same way (Adams, 1983). By the 1970s there was an unspoken understanding that the conceptual distinction between short- and long-term memory had little to offer an understanding of motor behavior. Research on motor retention became more of an empirical undertaking, with efforts aimed mostly at short-term motor retention.

Short-term retention. The assumption had always been that the representation of a response was stored in memory as a unitary entity, and it was made for both motor and verbal responses, but by the 1970s some memory investigators were brushed by perception (e.g., Gibson, 1969) and saw the stored response representation as an aggregate of features. Features are different dimensions of a response and, presumably, can be a function of different variables and be differentially retained. Research on the separate storage of distance (extent) and location (end point) of a movement directed analysts away from the traditional view that movement is stored as an entity. An experiment that separates distance and location will have the same extent of movement to a different location each time when the retention of distance is being studied, and the same end point but different extents of movement when the retention of location is being studied.

Using the short-term motor retention paradigm, Posner (1967) suggested that visual and kinesthetic cues were coded separately in memory, and Laabs (1973) concluded that kinesthetic location information and kinesthetic distance information were separately stored. Verbal factors appear to be implicated. Ho and Shea (1978) found that the recall of position benefited from verbal labels. Laabs (1973) and Diewert and Roy (1978) found that subjects used counting to remember distance. Nonverbal encoding of motor features has been encouraged by modern thinking on imagery but has gone virtually nowhere (Housner & Hoffman, 1980, 1981; Walsh, Russell, & Imanaka, 1980). Studies with blind subjects, arguing from imagery premises that the blind cannot visualize location and distance as well as sighted subjects, have also been inconclusive (Dodds & Carter, 1983; Hermelin & O'Connor, 1975).

Long-term retention. Two reviews of the retention of motor skills underscore the impression that long-term motor retention is a domain empty of productive ideas and in which only a little research is being done (Prophet, 1976; Schendel, Shields, & Katz, 1978). The distillation of the research literature by Schendel et al. came up with established principles, some of them known since the time of Ebbinghaus (1885/1964)—such as forgetting increases as a positive function of the retention interval, overlearning is beneficial for retention, relearning after a retention interval is more rapid than the original learning, discrete procedural responses are forgotten more readily than continuous motor skills, and the like.

Individual Differences

Time-sharing ability. There is a modern interest in time-sharing ability, coming from contemporary cognitive psychology more than from mainstream research on individual differences and learning. Not all of the research on time-sharing ability, old or new, is conceptually convincing or methodologically secure. Adams (1953) found that predictor measures from late in learning on learning tasks did a better job of predicting final performance on a motor criterion task than measures from early in learning. Adams postulated a time-sharing ability that was common to tasks at advanced stages of training. After component responses are acquired earlier in learning, the subject concentrates on integrating the parts into a patterned whole, which is time-sharing. Subjects differ in this time-sharing capability, and so it is an individual differences variable. Adams's hypothesis is reasonable, but a hypothesis nevertheless. In more recent times cognitive psychology has come to time-sharing by way of dichotic listening, a paradigm used for the investigation of attentional processes. The requirement is to listen to two channels at once and respond to only one of them. The adroitness with which subjects do this is a measure of their time-sharing ability. Gopher (1982) relied on this reasoning for the development of a dichotic listening test for the test battery the Israeli Air Force uses for pilot selection. The test successfully accounts for new, valid variance in the criterion. Gopher reasoned that flying an airplane requires the time-shared processing of multiple task demands, and the new test measures a trainee's capacity for doing it. This project of Gopher's applied research is an example of recent work that has been sound and successful. There have been other recent studies, but Ackerman, Schneider, and Wickens (1984) criticized them severely and their contributions are questionable.

Resource pools. The concept of time-sharing seems more secure than some of the other ideas that have been borrowed from cognitive psychology and used to explain individual differences in complex tasks. The notion of "resources" is one of them, and it has its advocates for explaining aspects of behavior in dual tasks (for a review, see Lane, 1982). Resources is a branch of the field of attention because a resource is a capability pool that can be allocated to the subtasks as whole-task demands dictate or as the subject wishes. When the resource is conceptualized as one pool, the subject may choose to direct his or her attention to a central subtask of importance and, if this subtask is not overly demanding, there will be plenty of resource remaining for response to a second subtask with high efficiency. If, however, the central subtask absorbs most of the pool, then there is little left over for the second subtask and performance on it is degraded. A modified point of view is that several resource pools can be allocated to subtasks. There might be a verbal pool distributed among the verbal parts of the task, an auditory pool for the auditory parts, and so on (it is not clear how many pools there are, their relative sizes, or whether they operate serially or in parallel). Heuer (1984) has taken the idea of multiple resources and turned it toward individual differences and motor learning. He assumes that early in training in a complex task a set of resource pools is called on, and with training the emphasis among these resource pools may change, some of the pools may drop out, or new ones may be activated. Navon (1984) has recently lectured the investigators of resources on requirements of scientific concepts and theory, and his lessons apply to Heurer's hypotheses. Of what value is the concept of resources for the explanation of individual differences with learning if it is just abilities renamed? What Heuer said about resource pools is very reminiscent of what Woodrow and Fleishman said about abilities.

Projections for the Future

What inferences can we draw from past trends and the present research climate to project directions for the future? This article began with research on the characteristics of learning curves as a function of prolonged practice in complex tasks (Book, 1925; Bryan & Harter, 1897, 1899). Today we again see interest in behavior under conditions of prolonged practice. This modern research has centered in perceptual learning, but it is an important direction for research on motor learning and it seems likely to go there. This topic in perception is called *controlled versus automatic processing,* and its protagonists have been Schneider and Shiffrin (Schneider & Shiffrin, 1977; Shiffrin & Schneider, 1977).

Schneider and Shiffrin contend that the perceptual learning of search for discrete items on a visual display passes from the controlled processing of information to automatic processing. Controlled processing is slow and deliberate, requires attention that the subject must allocate, is capacity limited, and allows the performance of only one operation at a time; processing is serial. Controlled processing dominates early in learning but the processing becomes automatic after very extensive training. Automatic processing is effortless and fast, does not require attention, and is not capacity limited and so is tolerant of concurrent response requirements; processing is parallel. A shift from conscious and deliberate allocation of attention to automatic processing with extensive practice is an old idea (Bain, 1868, pp. 330–332; Spencer, 1881, pp. 450–452) that is usually ascribed to James (1890), who explained it with the response-chaining hypothesis (Adams, 1984). Since James, psychologists, without much evidence, have believed in the shift from controlled to automatic processing for motor behavior (Adams, 1981, pp. 262–264). The topic over the decades was informally structured and defined by anecdotes, like consciously thinking about one thing and unconsciously doing another, or doing two things at once without interference after extensive practice. Doing two things at once without interference appeared to be empirically testable and a way of getting at automaticity. There have been many recent research studies on attention allocation in dual-task situations (for a review, see Lane, 1982), some of them involving motor behavior, but the amounts of practice have been small. Only a few dual-task experiments have administered extensive training, and usually the behavior has been nonmotor (Hirst, Spelke, Reaves, Caharack, & Neisser, 1980; Spelke, Hirst, & Neisser, 1976). An exception is a study by Bahrick and Shelly (1958) that used a dual task involving motor behavior. Practice on two concurrent discrete tasks, one visual-motor and the other audio-motor, was administered over 25 daily sessions. There was some reduction in interference with practice, but interference remained at the end of training. Adams and Creamer (1962) used an audiovisual, dual-motor task, and did not administer as much training as Bahrick and Shelly, but they nevertheless brought the learning curves to asymptote and beyond. Interference was found at the end of training. One way of interpreting interference in these two studies is that cognitive, nonautomatic factors were operating at the end of training. Adams (1981) concluded the same thing with an approach that used interference with the verbal and imagery mediators that could govern behavior in a discrete motor task. There is, then, no evidence of a shift from controlled to automatic processing for motor behavior—a point of view that Annett (1985), in his review of the topic, agrees with.

There have been few studies on controlled and automatic processing in motor behavior so far, but interest in the topic, which is presently settled in the field of visual perception, is likely to spread to motor behavior. Studies of extensive training on dual motor tasks will (a) extend the findings of dual-task studies of allocation of attention that have used small amounts of practice, (b) test and extend the generalizations about controlled and automatic processing that have come from the field of perceptual learning, and (c) cultivate understanding of the conditions of interference, or the lack of it, when two responses are performed together. Beyond these gains, it would seem that a concern with behavioral mechanisms and how they change with practice could accelerate interest in the mechanisms and theory of action, another direction in which research is likely to go.

Experimental psychology has often been preoccupied with disembodied perceptions and higher processes, and indifferently concerned with translating perceptions and higher processes into "action," or goal-oriented motor behavior. Today, in cognitive psychology, there is sensitivity to this long-standing deficit (e.g., Harvey & Greer, 1980). It should be kept in mind that experimental psychologists have been interested in mechanisms and theories of action for 100 years, as this article attests, but nevertheless there is a void in the failure to relate higher processes to action. A major omission of contemporary theories of motor behavior is that they give scant treatment of the environmental stimulus array, its perceptual processing, and how the percept comes to govern motor behavior. Theory about current perception of the world as a determinant of motor behavior may be easier than theory about inner cognitive representations, however, which contemporary theories also ignore. If the position is held that an image is a faint, probably imperfect, representation of the real world (Finke, 1979, 1980, 1985) and, further, that the image can be mixed with language descriptions of the world (Bandura, 1971, 1977) to make a compound representation, then this remote structure is complex and its relation with movement regulation correspondingly complex (Adams, 1984; Annett, 1985). There could be several ways of getting at the cognitive representation via our present-day arsenal of ideas and methods.

1. Observational learning of movement, which is cognitive learning and which I have already reviewed, can be a valuable testing ground for investigating the cognitive representation and how it regulates movement.

2. Mental imagery has had explanatory successes for the learning and retention of verbal and pictorial materials, but infrequent successes in motor learning (Gopher, 1984). Imagery might have played a low-key role in studies of mental practice, however, and was not always identified as such. Mental practice has been a research interest of physical education, and its governing hypothesis is that imagining the effective execution of a skill will improve it. Some of the research findings have been positive (Johnson, 1982), but not all (for reviews, see Corbin, 1972; Richardson, 1967a, 1967b).

3. In covering the Middle Period, I reviewed the verbal mediation hypothesis, now defunct, which was an attempt to get at language and its control of movement. Investigators of the verbal mediation hypothesis usually restricted their attention to discrete verbal responses, but language is capable of more complex organization for the control of movement. There is little systematic knowledge of the relation between language and movement (Annett, 1983, 1985). A limit to which language can regulate skilled performance must exist because language is crude relative to the subtleties of skills, but there is no doubt that language is an influential agent for the control of movement.

The temptation may arise, if cognitive factors turn out to be important determinants of motor behavior, to view movement as a slave to cognition and to decide that cognitive learning is about all there is to motor learning. That viewpoint would be a simplification, complex though it may be. A limitation of a cognitive view of motor learning is that a subject cannot do everything that he or she knows (Adams, 1984, pp. 20–23).

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Psychological Documents to Resume Operation

On June 16, 1986, the on-demand publication system *Psychological Documents,* published by the American Psychological Association from 1971 through 1985, was sold to Select Press. Select Press will begin publishing new volumes this year and, as of June 16, 1986, began fulfilling orders for documents accepted into the system while it was published by APA.

Peer-reviewed documents were published by APA under the experimental system (formerly Journal Supplement Abstract Service) for 15 years. A catalog containing synopses of each document accepted into the system was published on a subscription basis. Those wishing to have a copy of the full-text of a document could order a copy in either microfiche or paper.

During periodic evaluation of the service, however, APA found that as a result of low volume, the difficulties of providing service within existing systems, the expenses related to fulfilling orders, and the cost of maintaining an editorial office, it was extremely difficult for APA to maintain service that was both timely and economical. After an extensive review of the history of the system and intensive evaluation of the expenses related to it, the APA Council of Representatives voted in 1985 to discontinue publication of *Psychological Documents* with publication of the December 1985 catalog. APA was to continue to fulfill orders for individual copies of documents until December 1986, assuming that no alternative publisher could be found. Possible alternative publishers included APA divisions, individuals, and commercial publishers. In mid-1985, Select Press approached APA and negotiations were begun.

Select Press will continue to operate the system as a peer-reviewed "journal" or document service. It will continue to feature specialized documents suitable for individual circulation such as technical reports, annotated and technical bibliographies, original data sets, test instruments, test manuals, and papers that would ordinarily be too long to be considered for regular journals. Select Press expects to expand the system to cover a broader range of documents including interdisciplinary content and possibly brief, early announcements of new findings. Select Press also publishes the interdisciplinary *Journal of Social Behavior and Personality.* Further information about *Psychological Documents* may be obtained from Select Press at P.O. Box 9838, San Rafael, CA 94912.