

# MOTOR CONTROL

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## Issues and Trends

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## Issues for a Closed-Loop Theory of Motor Learning

*Jack A. Adams*

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### I. Introduction

In 1971 I published a paper on a closed-loop theory of human motor learning (Adams, 1971). The ideas of the paper were motivated by shortcomings in open-loop conceptions of motor learning that had dominated the field since its beginning, and by the weaknesses in extant closed-loop descriptions of motor behavior that were little more than interesting analogies drawn from servo theory in engineering. These engineering models were uninteresting to learning psychologists because they had strict assumptions which behavior could not abide, they did not contain learning variables, and they were not rooted in empirical findings for motor behavior (Adams, 1961). I sought to avoid the

pitfalls of earlier closed-loop theorists by operationally defining the constructs of my theory so that it could be empirically tested, by including variables which are known to influence motor learning, and by securing the theory in empirical data. It is my conviction that the formulation of good theory is grounded in empirical data. Theory not fixed in data can be a fanciful dream that will have little chance of saying anything about the real world, and it can have an apparent scientific pertinence that is attractive to the unwary.

Today there are ideas abroad in the realm of motor behavior which suggest that it is time to change and enlarge motor theory. This trend is a natural progression in science and is to be encouraged, but some of the recommendations for change are ideas which are thin for want of empirical support and which are poor candidates for theory, at least by the canons of theorizing which guide me. The recommendations most frequently heard are to incorporate the concepts of motor program and schema into theory. After summarizing my closed-loop theory of motor learning as a background for the discussion, I will take a critical look at these scientific concepts and document their ineligibility for formal theory status at this time.

## II. Review of Adams' Closed-Loop Theory of Motor Learning

The starting points for my closed-loop theory of motor learning were doubts that I had raised about open-loop conceptions of behavior (Adams, 1967, 1968), and some closed-loop theorizing which I had attempted for paired-associate learning (Adams and Bray, 1970). A stimulus for my theorizing was a discontent with the S-R reinforcement view of learning.

The S-R reinforcement position of learning is one of empirical reinforcement. Any event that follows a response, is correlated with its occurrence, and produces an increment in the probability of occurrence for the response class, is considered a reinforcer. The food pellet which the rat receives when he presses the bar in a Skinner box is a reinforcer because it increases the chances that he will press it again; and the experimenter saying "Right" when the subject draws a 3-in. line is a reinforcer because it increases the probability that he will draw it again. There is no theory of reinforcement that explains the action of these diverse reinforcer events with a common principle. This lack of conceptual elegance does not destroy the scientific usefulness of empirical reinforcement because a great deal of prediction and control of behavior can be exerted with the various reinforcing events that are known to affect behavior.

In motor learning, and often in verbal learning, we call an empirical reinforcing event "knowledge of results," and there is reason to believe that the empirical reinforcement position is wanting and that we can begin to go beyond surface

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correlations and specify underlying processes. The literature suggests three of these processes:

1. An empirical reinforcement interpretation of knowledge of results says that information about the correctness of the response will lead to an increase in occurrence of the response. Elwell and Grindley (1938) observed that a subject in a motor learning experiment does not repeat a response like a pigeon repeating a key-pecking response for grain reward. Rather, the subject attempts to vary responses and correct error, not repeat responses.

2. Motor behavior is guided by covert verbal behavior in the early stages of learning. Subjects accompany the ~~learning process~~ with hidden verbal activity, where they form hypotheses and plans about the next movement on the basis of the knowledge of results which they have just received. Perhaps from the days when motor behavior was studied by physiologists as a "spinal" activity, there have been those who have identified motor behavior with the lower senses and remote from the upper reaches of the mind. Actually, motor behavior is draped with more cognitive activity than most are willing to admit. The acknowledgment of a role for verbal factors in motor learning does not mean that motor behavior is always under verbal control, however. As William James (1890a,b) said a long time ago, motor sequences are under verbal control at the outset of learning but eventually become "automatic," or nonverbal, as the learning becomes advanced.

3. The error in a motor movement is known by the subject. We have self-knowledge of the adequacy of our movements, and we regulate them on the basis of it.

I believe that we can be carried beyond an empirical reinforcement view of motor learning by incorporating these three processes in a closed-loop theory of motor learning. In my 1971 paper, I distinguished between open-loop and closed-loop systems in the following way:

An *open-loop* system has no feedback or mechanisms for error regulation. The input events for a system exert their influence, the system effects its transformation on the input, and the system has an output. A poorly operating open-loop system (error) is because of characteristics of the input and/or transformations imposed by the system. A traffic light with fixed timing snarls traffic when the load is heavy and impedes the flow when traffic is light. The system has no compensatory capability.

A *closed-loop* system has feedback, error detection, and error correction as key elements. There is a reference that specifies the desired value for the system, and the output of the system is fed back and compared to the reference for error detection and, if necessary, corrected. The automatic home furnace is a common example. The thermostat setting is the desired value, and the heat output of the furnace is fed back and compared against this reference. If there is a discrepancy the furnace cuts in or out until the error is zero. A closed-loop system is self-regulating by compensating for deviations from the reference [p. 116].

The empirical reinforcement conception of motor learning is open-loop. The response outcome for the system is primarily determined by system changes which reinforcement has made, which has often been conceptualized as habit. By contrast, a closed-loop system has a reference mechanism that specifies the correct response required of the system, feedback which informs of the response which the system has made, a comparison of feedback with the reference mechanism for a determination of error, and a correction of error. An open-loop system treats errors incidentally as evidence of system incompetence because the focus is on occurrences of the correct response, but errors and their processing lie at the center of a closed-loop system.

The trick for devising a closed-loop theory of motor learning is to specify how the mechanisms of a closed-loop system link to learning variables. How is the reference mechanism learned so the subject comes to know when he is performing correctly? What are the sources of feedback and how do they work? What role does knowledge of results play?

#### A. Knowledge of Results

An interpretation of knowledge of results can be in direct empirical terms, where it is an event whose occurrence affects response probability, but the empirical reinforcement view is insensitive to the fertile workings of the human mind and the operations that it performs on knowledge of results in behalf of the next try at the motor response. One does not have to introspect very hard to know that motor learning can be a problem-solving situation, where the verbal human takes the knowledge of results that he receives and uses his language capabilities to form strategies and hypotheses about how to solve the motor problem that is confronting him. Knowledge of results is information, and how the subject will transform and use it will depend on the type and accuracy of knowledge of results and on the kind of motor task.

#### B. The Reference Mechanism

A movement must be started, and it must have direction and extent. The starting of the movement is a separate problem, and it will be dealt with in the next section. This section deals with the continuous regulation of the movement once it has started.

The learning of a movement in closed-loop theory requires the acquisition of a reference mechanism which is the basis of the subject knowing the correctness of a response insofar as he has learned it. In addition, the subject needs knowledge of results to inform him about the correctness of the last movement, and response-produced feedback stimuli to inform about the progress of the current movement. The reference mechanism is called the perceptual trace in my theory. At the start of a trial the perceptual trace is aroused in anticipation of feedback

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from the forthcoming movement, and as the movement proceeds the feedback is compared with it and the appropriateness of the movement is assessed. If the perceptual trace and the feedback match the error signal is zero, and the subject proceeds confidently with the movement. But if the match is absent and there is error, the subject has reduced confidence in the correctness of the movement and moves to eliminate the error. The process is one of continuous error nulling throughout the course of the movement.

The strength of the perceptual trace is a positive function of experience with the various sources of feedback in the situation. Motor movements will have proprioceptive feedback associated with them, and often there is visual feedback as well as auditory and tactual feedback. The perceptual trace is a motor image (not necessarily a conscious one), and the comparison of feedback stimuli with it is an act of recognition, just as the image of a picture from visual experience is the basis for recognizing the picture when it is presented again. The perceptual trace is conveniently referred to as a single state, but actually it is considered to be a distribution of traces resulting from the responses of all of the learning trials. The movement on each trial lays down a trace.

One might stop in this point in the theorizing because the essentials of a closed-loop system have been achieved, but the theory would be pale stuff for learning because it would not have tied the important learning variable of knowledge of results to the perceptual trace and feedback stimuli. A simple error-nulling system will not do because in the beginning the perceptual trace is ill-defined and if the subject adjusted his behavior with respect to it he would be repeating his own errors, which is failure to learn. Instead of repeating his past responses, he must vary his behavior and make his next response different from the last one. The perceptual trace is stored information about past movements that have been made, and knowledge of results is information about the adequacy of the last movement that was made, and the subject uses the two in relation to one another to make the next move better than the last one. The result over trials is the gradual improvement which we call learning. Because knowledge of results and the verbal behavior based on it has a strong role at this stage, it is called the Verbal-Motor Stage. As learning progresses, and the subject has been making little or no error for some time, the perceptual trace becomes a solid reference for the correct response and the subject can now respond with respect to the perceptual trace alone by comparing feedback with it and nulling the perceived error. Knowledge of results is no longer needed. This is called the Motor Stage.

### C. The Starting of the Movement

Elsewhere (Adams, 1971, pp. 125-126) I have discussed the logical and empirical reasons for a separate theoretical state, distinct from the perceptual trace, whose function is the initiation and selection of movement. I call this

agent the memory trace. Once a movement has started the perceptual trace and feedback regulate it, but it takes the memory trace to start it in the first place.

### III. The Motor Program

My closed-loop theory relies fundamentally on response-produced, peripheral feedback stimuli and the perceptual trace, which is the reference of correctness, for the regulation of movement. This structure gives the theory its closed-loop quality, and it has historical continuity with closed-loop theory from engineering which uses peripheral feedback as a source of information about the system's response.

The challenge to a closed-loop view of motor behavior is an open-loop version that is based on the motor program. The motor program has a centrally stored plan for the movement sequence, and the plan controls the movement during its course. The implication is that feedback is unnecessary for the regulation of movement, although it has been suggested that feedback is used intermittently to report on the movement's progress and to adjust the ongoing motor program (Keele, 1973, Chapter 6; MacNeilage, 1970). Being central, the motor program is a challenge to revise my closed-loop theory of motor learning because my theory turns strongly on peripheral feedback for movement regulation. It pays, therefore, to examine the empirical foundations of the motor program concept.

At the outset I must point out that a very limited idea of a motor program is necessary for any theory of movement because a movement must be started and feedback does not occur until a fraction of a second later. My concept of memory trace performs the movement initiation function, so in this restricted sense I am an advocate of the motor program. The standard view of the motor program, which comes down to us in the history of motor learning, is not concerned with movement initiation but with extended movement sequences like moving the arm, swinging a bat and hitting a ball, or running down a maze. That there is a central agent which runs off long motor sequences without feedback is the issue.

#### A. Deafferentation

##### 1. K.S. Lashley, The Founder of the Motor Program Hypothesis

The strongest platform for the motor program is deafferentation, where the nerve fibers from the muscles and joints to the motor centers of the brain are cut and the behavioral competence of the organism is observed. That some competence is found in the absence of proprioceptive feedback is taken as support for the central control of movement and a downgrading of feedback.

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Lashley (1917) invented the motor program hypothesis during a clinical study of a patient who had gunshot injury to the spinal cord, which is accidental deafferentation. Lashley found that the blindfolded patient never made a mistake in the direction of a voluntary movement, and accuracy in the extent of movement compared favorably with a control subject. From that time on Lashley was a centralist who denied the role of feedback in movement regulation, and he led a crusade against feedback with experiment and argument for the rest of his life. The campaign began with this statement in the 1917 paper:

We may conclude that the anesthetic subject's control of his movements is not significantly less accurate than that of the normal individual, and it is not clear that for the simple movement studied the afferent impulses from the moving limb contributed anything to the accuracy of movement in the normal subject. The chief mechanism for the control of movement is located in some other body segment than that of the moving organ [p. 185].

The clinical work was only a clue, but Lashley saw it and moved it into the laboratory where he subjected his feedback ideas to experimental attack. Lashley and McCarthy (1926) used rats and insulted the cerebellum with lesions and found that the earlier learning of a complex maze was retained rather well. Lashley and Ball (1929) denied proprioceptive feedback by cutting the spinal afferent paths and they also found good retention of maze performance. Here again Lashley spoke for a central organization of motor activity.

Lashley was not without his critics. W.S. Hunter was a prominent experimental psychologist on the American scene for a long time and his analysis of Lashley's position cut it with surgical neatness. Hunter (1930) said that maze running is behaviorally complex, involving virtually all of the senses, and to deny the animals only one sense ally should not impair performance very much. Learning can make some of the other senses informative too, and they remain influential when proprioceptive feedback is denied. Feedback was still guiding behavior and so the concept of the motor program was unjustified, Hunter contended. Pavlov (1932) also sliced at Lashley's position. He made arguments similar to Hunter's, and at one point (Pavlov, 1932, p. 114) dismissed Lashley's programming notion as a "bodiless reaction." In turn, Lashley (1931) defended himself, but he had to admit that the motor program was a concept unfulfilled:

The notion of central processes controlling sequences of behavior was little more than a denial of the adequacy of the motor-chain theory [feedback from one movement segment determines the next, J.A.A.] as an explanation of maze running or of thinking and a suggestion that we can profitably study the central-nervous coordinating process. As an explanatory process it is empty, but it may perhaps be filled by continued research [p. 18].

Hunter, Pavlov, and other critics of Lashley (e.g., Honzik, 1936) were correct in their analyses, of course, but this did not deter Lashley. In his best-known paper (Lashley, 1951), Lashley expressed the program idea again, and the



empirical foundations were no stronger than they were twenty years before. The motor program might have been the right idea for the wrong reasons, because later investigators tried different research approaches to it and some claimed support for it. As we shall see in sections that follow, the later studies are not without their difficulties either.

## 2. Recent Deafferentation Studies

Taub and his associates have been the most persistent pursuers of deafferentation in recent times (Knapp *et al.*, 1963; Taub *et al.*, 1965; Taub and Berman, 1963, 1968; Cohn *et al.*, 1972). Theirs is an attempt to correct some of the difficulties with earlier work, and they interpret their data in favor of the motor program. Advocates of the motor program frequently cite the Taub findings as positive evidence, but there is reason for the enthusiasm to be restrained.

Lashley had the problem that nonproprioceptive sources of feedback could come to guide the response through learning, and Taub and his colleagues have the same problem. Consider the experiment by Knapp *et al.* (1963). They used monkeys and sought to eliminate visual feedback by strapping an animal in a chair with an opaque collar during avoidance conditioning trials. The conditioned response was forelimb flexion, and the opaque collar obscured vision of it, but not vision of the surrounding room. Correlated movement of the head or eyes could have given visual feedback that would come to be informative about movement progress over learning trials; there is no reason to believe that only visual feedback of the responding limb is informative about the course of movement. Or consider the conditioned stimulus as a source of auditory feedback. The conditioned stimulus was a buzzer which came on for 7 sec and the unconditioned stimulus, an electric shock, came on for the last 3.5 sec. If the animal made the conditioned response the buzzer terminated, which was informative feedback about the movement. One of their findings was that deafferented animals were unable to use the deafferented limbs in a free situation but readily used them to avoid shock in the conditioning situation. The most likely reason seems to be that the conditioning trials created reliable sources of feedback information about movement that were lacking in the free situation.

In subsequent experiments (Taub and Berman, 1963; Taub *et al.*, 1965), feedback from the conditioned stimulus was eliminated by using trace conditioning where the conditioned stimulus is off by the time the unconditioned stimulus and the response occurs, but the same lack of visual control persisted. That visual feedback can provide guidance for deafferented limbs has been found in several studies. Ataxia is greatly accentuated in blindfolded deafferented animals and can be absent without it (Twitchell, 1954; Bossom and Ammaya, 1968; Knapp *et al.*, 1963). Pavlov (1932, p. 188) observed that ataxic subjects with *tabes dorsalis* can stand on one leg when the eyes are open but fall when the eyes are shut. Leont'ev and Zaporozhets (1960, Chapter 1) studied war

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veterans who had restricted movements because of battle injuries. Subjects with restricted movement of the shoulder joint were instructed to close their eyes and raise the damaged arm as high as possible. Then they were asked to repeat the act with eyes open. The second movement was 7° higher than the first.

If these difficulties are not enough, Bossom and Ammayya (1968) criticize the surgical techniques which Taub and his colleagues use, and suggest the possibility that some dorsal root fibers might have remained intact. If so, this is a serious criticism because the existence of only a few fibers is sufficient for coordinated behavior after recovery from surgery (Bossom and Ammayya, 1968; Mott and Sherrington, 1895; Twitchell, 1954). Recently a paper from Taub's laboratory (Cohn *et al.*, 1972) discussed the difficulties with the surgical procedures in deafferentation. As an added check on the completeness of the deafferentation, they tested to see if motor movement could produce evoked potentials at the cortex. None were found, and they asserted confidence in their current surgical methods.

So, like Lashley, all that Taub and his colleagues have shown is that skilled responding is possible without proprioceptive feedback, not that the motor program is a valid concept. There can be no test of the motor program hypothesis in the absence of controls on the other sources of feedback that can come to be endowed with informative properties for movement through learning. Even the autonomic nervous system can provide cues for movement, and learning psychologists have used this possibility in their theorizing. For example, Mowrer (1947) had fear stimuli as the cues for motor avoidance responses in his two-factor theory of emotional learning. In the literature domain that is particularly pertinent to our inquiry here, Mott and Sherrington (1895), Twitchell (1954), and Bossom and Ammayya (1968) found that a deafferented animal can have his coordination restored when he is emotionally aroused, suggesting a role for emotional stimuli in the maintenance of manipulative and ambulatory movements. Perhaps the fundamental observation is not that we can move without proprioception but that learning can make any source of feedback informative for movement.

#### B. The Motor Program as a Determiner of Certain Insect Behaviors

It is common for advocates of the motor program to cite research on insect behavior as a strong line of positive evidence for the program idea, and it is justified. Experimental biologists have amassed evidence for feedback-free behavior (Hinde, 1969a,b; Evarts *et al.*, 1971, Chapter 2), and this is the kind of behavior that supports the program concept. The best-known work is by Wilson (1964, 1965) on the wingbeat of the locust. The sexual behavior of the mantis and the cockroach is held to be programmed (Roeder *et al.*, 1960). Insect walking behavior is under a degree of programmed control (Pearson, 1972;

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Pearson and Iles, 1973; Wilson, 1966). That the concept of the motor program is applicable to the insect domain is important to know because it means that the idea has some validity, but to leap from genetically determined behavior in insects to learned behavior in animals, including man, is a jump that is too large for many scientific temperaments. That biological evolution can genetically determine behavioral sequences is grounded empirically and believed by us all, but the issue is learning, the ultimate in adaptability, not restricted programmed sequences which genetics has endowed.

### C. Proprioception and the Control of Fast Movement

Always, defenders of the motor program move Lashley's (1951) example of the pianist to center stage. The anecdote has it that a pianist's fingers can move at the rate of 16/sec, and this is too fast for closed-loop proprioceptive feedback control of movement. Lashley's anecdote continues to be a compelling example for some, despite modern physiological research on proprioception and the fact that no one has ever bothered to measure the speed of a pianist's fingers. The serial ordering of speech is an instructive example on the control of fast movements. Is speech a series of open-loop commands, or is tactual and proprioceptive feedback control sufficiently swift to do the regulatory job?

MacNeilage (1970) admits to a possible rôle of the closed-loop control of speech, but mainly he defends open-loop programmed control where the target end points are specified and the muscles can take various routes to them. Implicit throughout his paper is the sentiment that proprioceptive feedback is not fast enough to control the speech apparatus. Most psychologists, in conducting this argument, cite Chernikoff and Taylor's (1952) finding that kinesthetic reaction time is 119 msec. It is true that if the motor stimulation-brain-motor response loop ordinarily took this long to operate-it would be difficult to argue for closed-loop control. However, as we shall see in this section, modern work on the physiology of proprioception provides lines of evidence which show that proprioceptive feedback can be much faster than the work of Chernikoff and Taylor implies, and so a closed-loop interpretation is becoming more plausible than ever before. Sussman (1972), in a reply to MacNeilage (1970), has speech as his battlefield and marshals his physiological evidence and his closed-loop forces very effectively. His arguments are secured in the physiological research of Bowman (1968) and Bowman and Combs (1968, 1969a,b) on muscle spindles in the monkey's tongue. The tongue is a remarkably skilled organ when we stop to think about it. Its intricate positioning and timing in the oral cavity is a prime factor in that complex learned activity which we call intelligible speech, and the tongue's skills may be a good focus for some of these issues that concern us.

Muscle spindles are absent in the tongue of lower animals like the cat, but not so for primates. The monkey and the human tongue have their intrinsic and

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extrinsic muscles richly laced with spindles (Bowman, 1968), and they are arranged in transverse, vertical, and longitudinal dimensions to communicate the position of the tongue in three-dimensional space (Bowman and Combs, 1968). Ten different response patterns of the stretch sensitive units of the hypoglossal nerve were identified. Different amounts of stretch gave different amounts of impulse frequencies, and many of the stretch sensitive units gave velocity responses during the dynamic phase of the stretch. Moreover, the lingual nerve responded to stretch in any direction and with an acceleration in their discharge frequency. The adequate stimulus for the lingual nerve was distortion of the tongue's surface. Bowman and Combs (1968) see the sensors of the tongue as the structural substrate for a "highly discriminative feedback system [p. 117]." Sussmann (1972), in his comment on these findings, said:

The finding by Bowman and Combs substantiates the view that there is a vast potential existing within the oral cavity for a detailed, one-to-one mapping of the oral area onto corresponding cortical areas. Hence, the two neural systems (hypoglossal and lingual) can provide an extensive repertoire of information to the higher control centers to bring about closed-loop control of target attainment during the articulatory gestures of speech [p. 269].

A subsequent study by Bowman and Combs (1969a) is a reply to the argument that there is insufficient time for the brain to be informed about movement. Stimulation of the monkey's hypoglossal nerve produced cortical potentials with a latency of 4–5 msec. Other studies have shown an equally fast response. Bowman and Combs (1969b) stimulated the deep radial nerve of the monkey's elbow and obtained a response in the cerebellum in 4 msec.

Fuchs and Kornhuber (1969) found that stretch of the extraocular muscle of the cat produced a response in the cerebellum in 4 msec and a response in the brain stem in 3–4 msec. Citing evidence by Cohen *et al.* (1965) that the time from the cerebellum back to the eye muscle takes 5.5 msec, Fuchs and Kornhuber estimate the time for the complete trip from muscle receptor to brain back to eye muscles to be 10 msec. Evarts (1973) studied the motor input of a learned hand movement, where a monkey was taught to move a handle back to a correct position after it had been mechanically displaced. Cortical and EMG responses were recorded. The time from the motor stimulus to a cortical response was 10 msec. EMG response to the motor stimulus (cortically mediated) was only 30–40 msec. In a similar fashion, Sears and Davis (1968) used a human subject and made EMG recordings in the respiratory muscles to a change in pressure load on the lungs. They obtained a latency of the EMG response in the 50–60 msec range. Proponents of the motor program have always assumed that the motor system does not have the neural speed for the closed-loop regulation of movement, but these recent physiological research findings suggest otherwise. Lashley's pianist may be a closed-loop system after all.

#### D. The Motor Program, Feedback, and Learning

Motor program advocates are concerned with memory but they are not much interested in the learning operations that store the movement sequence in memory in the first place, so they fail to specify how the program is acquired. Is feedback irrelevant throughout the learning of a program? Or does feedback only play a role early in learning, with the program being a developing independence of feedback as learning progresses? An experiment by Adams *et al.* (1972) was designed to test these possibilities. Linear positioning was the task that they used, and it was learned under either augmented feedback or minimal feedback. With augmented feedback there was full vision, the subject could hear the slide which he manipulated move along its track, and he had spring tension on the slide to give heightened proprioceptive feedback. Minimal feedback was none of these. After low or high amount of practice with knowledge of results, the knowledge of results was withdrawn and the feedback either remained the same or was switched, depending upon the experimental condition. If the motor program is independent of feedback throughout its learning then feedback change should make no difference whether the amount of learning was low or high. But if the learning of a program is gradual liberation from feedback, then feedback change should produce a performance difference at the low level of learning but not the high level. Neither of these things happened. Feedback change made a big difference for both levels of learning. Moreover, the difference was the greatest for the high level of learning, which is contrary to the possibility that a motor program develops with practice. The authors interpret their data in support of my closed-loop theory of motor learning which has a role for feedback throughout all stages of learning.

#### E. Error Detection and Correction

My closed-loop theory of motor learning uses a comparison of the perceptual trace and peripheral feedback for the determination of error. We know that feedback is fundamental for motor sequences (e.g., Adams, 1968) but an issue for closed-loop theory is more specific than that: Does feedback play a role in error detection and correction? An experiment by Adams and Goetz (1973) took deliberate aim at this question. They found that the accuracy of error detection and correction was positively related to amount of feedback and amount of practice, as my theory contends.

When one's theoretical stance is the motor program, where feedback is played down, there is the problem of accounting for error detection. How is it done? MacNeilage (1970) and Keele (1973, Chapter 6) use the motor program to generate the movement but with response-produced feedback stimuli entering intermittently, reporting on movement error and modifying the program if

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necessary. This is a compromise hypothesis which seems very difficult to test. It is insufficient to show that either the motor program or feedback are governing agents for movement (the issue which is in the center of our scientific eye at the moment). Rather, it must be shown that both the motor program and feedback are operating and, also, the circumstances of their interaction must be demonstrated.

#### Corollary Discharge

Defenders of the motor program are looking with increasing fondness at corollary discharge to assume a burden in error determination. Corollary discharge is central feedforward stimuli, in contrast to feedback stimuli. Supposedly, the motor program discharges stimuli about the movement-to-be to sensory centers of the brain where there is a comparison with an image representation of the desired movement and error is determined. An idea like this seems required for a motor program approach because if feedback stimuli are unimportant then error assessment must come from other sources, presumably central ones.

The term "corollary discharge" was invented by Sperry (1950) in his research on the visual behavior of fish. He contended that the advent of an eye movement was accompanied by "an anticipatory adjustment in the visual centers specific for each movement with regard to its direction and speed [p. 488]." This idea has been used to explain the old puzzle in visual perception about how we distinguish movement in the world from voluntary movement of the head and eyes. Since both result in movement of visual stimuli on the retina, how do we know one from the other? The answer that is commonly given is that corollary discharge informs the visual centers of the brain that the movement about to be perceived is self-generated, and so the world seems stable in the face of a changing visual scene. The world moves when changing visual stimuli are unaccompanied by corollary discharge. Corollary discharge has been discussed and used by various writers (e.g., Evarts *et al.*, 1971; Konorski, 1962; Taub and Berman, 1968; Teuber, 1964). The empirical validity of a central, anticipatory sensory discharge that is unrelated to external stimuli is established for lower animals (e.g., Johnstone and Mark, 1969, 1971).

The issue for corollary discharge is not whether it exists or not but how much information it carries. For a motor program notion to work the situation must arouse an image representation (or what I call the perceptual trace) in anticipation of the response-to-be, and the motor program must then transmit the corollary discharge which contains all of the information about the forthcoming movement for a comparison with the image for a determination of error. This means that all of the characteristics of the movement must be coded in the corollary discharge, but there is no evidence that this is so. The empirical data show that there is a signal to a sensory center of the brain but no one knows

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how much information it carries. Perhaps it is only a simple signal of the movement, with no information about movement features whatsoever. Certainly the argument that is made about the explanatory value of corollary discharge for eye movements and visual perception can be sustained with a simple corollary discharge signal. For a stable visual world, the brain need only know that the eye is about to move, not the speed, direction, and terminal position of the move. In my theory, corollary discharge could be the anticipatory signal that galvanizes the perceptual trace to action and readies it for the feedback that will arrive when the movement is begun.

The use of corollary discharge as a sister concept of the motor program is in the weak tradition of physiological explanations of behavior because there has been no bridging of the interface between physiological and behavioral data. The research on deafferentation is on sounder ground because physiological indices of feedback, or its absence, are directly related to behavior in the same experiments. If experiments on deafferentation and motor behavior were done in the same way as the experiments on corollary discharge and motor behavior, there would be studies that would cut afferent fibers and show their failure to transmit proprioceptive stimuli, and there would be other studies that would show motor behavior related to proprioceptive stimuli, but there would be no experiments that related deafferentation and motor behavior in the same experiment. We have yet to show a direct relationship between corollary discharge and motor competence.

#### IV. Schema

My closed-loop theory of motor learning has each movement separately generated and its stimulus consequences separately stored. The advocates of schema for motor behavior contend, as a counterproposal, that what really happens is that a general plan for a class of movements develops which the performer flexibly uses to respond to the momentary demands of the situation. There are many response routes to the same goal, and a schema allow any one of them to be chosen. This generalized plan for a class of movements will be called the recall schema, and it is closely analogous to concept behavior. To know a concept is to be able to recognize an instance of a stimulus class even though it has never been experienced before. To have the concept "three" is to know three objects even though a particular aggregation of three objects has never been encountered before (e.g., three Martian maidens). The recall schema, it is contended, will allow you to make a movement of a class even though you have never made that particular movement before.

Schema has been used so often to explain findings in visual recognition that

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one might think that it was devised for that purpose, but not so. The schema was first used by the British neurosurgeon Henry Head to explain aspects of motor behavior. Head's interest in motor behavior came out of his neurological studies of the injured of World War I (Head, 1920a,b), but his research on aphasia is also classic (Head, 1926a,b), and Frederic C. Bartlett collaborated with him on it. Bartlett, being a psychologist, brought the schema into psychology to explain verbal recall primarily, and it is Bartlett's version (1932) which most psychologists know. The schema also is used to explain form recognition (e.g., Attneave, 1957) and motor recall (Bartlett, 1932; Bernstein, 1967; Bruner, 1970; MacNeillage, 1970; Pew, 1970, 1974; Schmidt, 1975). In order to understand schema, and to assess its credibility for modern theory of motor behavior, it is instructive to trace the convolutions of the schema concept from its beginning with Head. The different meanings of schema have not always been appreciated.

### A. Three Conceptions of Schema

#### 1. Head's Conception of the Schema

That motor images are influential in the evaluation of movement was an idea prevalent in the early part of this century, and Head rejected it. Allow a patient with sensory damage to his arm to see its position, and then have him close his eyes. Change the position of the arm with the eyes remaining closed and ask the patient about the location of his arm. He will say that his arm is in its original position, not the changed position, and Head said that this is because he has a visual image of the original position of the arm but no motor image to inform him of the move and the second position. A conscious motor image, then, cannot be the basis of movement recognition, said Head. Theorizing anew, Head (1920b, pp. 604-608) defined a spatiotemporal model of movement which is derived from experience and which is the internal standard for the recognition of movement. Head (1920b) wrote:

For this combined standard, against which all subsequent changes of posture are measured before they enter consciousness, we propose the word "schema." By means of perpetual alterations in position we are always building up a postural model of ourselves which constantly changes. Every new posture of movement is recorded on this plastic schema, and the activity of the cortex brings every fresh group of sensations evoked by altered posture into relation with it. Immediate postural recognition follows as soon as the relation is complete [p. 605].

Schema for Head is the internal reference for response recognition. It resembles my concept of perceptual trace (or vice versa).

#### 2. Bartlett's Conception of the Schema

Bartlett's use of the schema (1932, Chapter 10) has two thrusts. One was accounting for his data on the recall of prose, and the other was accounting for



the motor behavior in popular British sports. Bartlett related these disparate topics with the schema.

Bartlett's work on verbal recall had his subjects learn a brief story and then recall it repeatedly after various retention intervals. Bartlett found that the core of the story's idea usually would be retained and, according to Bartlett, would be used to reconstruct the details of the story, some of them correct and some of them not. Bartlett's use of the schema as the central idea that is stored, and from which the protocol at recall is constructed, not only accounts for Bartlett's empirical findings but was, at the same time, a rejection of an account of memory where each response element at recall is based on a separate memory trace. The stored agent is a generalized schema, according to Bartlett, not a set of separate memory traces, and the schema gives a measure of creativity in responding because responses can occur which have never been made before.

Bartlett reasoned the same about motor movement. The execution of movement is not the activation of a passive set of memory traces. Rather, the movement which occurs is never exactly old or completely new but is "manufactured" out of the postural and situational schemata that prevail. Skill in a complex game like tennis does not come from practice and storage of the large number of moves that comprise proficiency, but rather is the development of a much smaller number of schemata that allow the required movements to be created to meet the demands of the situation.

For our purposes here, the important thing to notice about Bartlett's position is that he gave Head's concept of the schema a new function. Head used schema as the internal model for response recognition, but Bartlett used it for the generation of responses—the schema became the basis of recall.

### 3. Attneave's Conception of the Schema

Attneave (1957) is responsible for bringing the concept of schema to the realm of stimulus (form, pattern) recognition. A classical view of pattern recognition is template matching where experience with the stimulus stores a representation of the stimulus pattern in memory, which has often been called an image. At the recognition test the stimulus is presented again, and if the image is aroused the subject will say that he recognizes it. A variation of template matching is feature matching, where only key features of the stimulus are stored. Another possibility is that the proper kind of experience with members of a defined class of stimuli can result in the storage of a more general representation of the pattern, or an abstraction of the pattern. The abstraction subsumes particular stimulus instances, in the same fashion that a concept subsumes particular instances, and it gives the power to recognize all of them. Given the abstract pattern, a subject can recognize instances of a pattern that have never been experienced before. Attneave gave empirical credibility to this idea, and he called the stored abstraction of a class of forms a schema.

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Head used schema as an explanatory mechanism for response recognition, and Bartlett used it for recall, but Attneave had schema as the mechanism for stimulus recognition.

## B. The Empirical Validity of Schema

Three uses of schema have been identified, and users of schema have not always been meticulous in specifying their meaning of schema and distinguishing it from other meanings. Notwithstanding, the human has undeniable powers of abstraction, and schema seems to have been the label that various analysts have used for conceptlike behavior when they thought they had identified it. To what extent have these analysts produced acceptable empirical evidence for the three kinds of schema that have stepped forward in the past fifty years or so? If theory must have guidance from substantive empirical data to embrace a concept, to what extent are these three kinds of schema ready for their theoretical debut?

### 1. Stimulus Recognition Schema

This is the schema that has the strongest support of all. Research on this version of the schema has accelerated in recent years and a satisfying amount of empirical support has developed for it (e.g., Attneave, 1957; Charness and Bregmen, 1973; Peterson *et al.*, 1973; Posner *et al.*, 1967; Posner and Keele, 1968; Posner, 1969). The experimental paradigm for developing a schema is experience with the prototype of the stimulus class or with varied instances of the stimulus class. It is not contended that the schema explains all recognition, because it is unlikely that a schema will develop after only one brief exposure to a stimulus, and yet we have power to recognize it. Nevertheless, we have good capability for recognizing all instances of a stimulus class (all triangles, all humans) even though we have directly experienced only a subset of them.

### 2. Recall Schema

Schema means versatility, and the recall schema is response versatility where the subject has the option of various response routes to the goal. Bartlett's skilled tennis player has, say, the goal of placing the ball in the far left corner, and he can elect various behavioral routes to fulfill this plan. The performer is not stimulus-response bound, where he must have practiced all of the possible moves that a proficient game requires. Once the schema has been acquired he can make responses that he has never made before.

The discomfiting problem with recall schema for skills is that there is no reliable evidence for it. From Bartlett's intuition about skills and his background with Head came the hypothesis of recall schema, but his evidence in behalf of skills was anecdotal accounts of feats on the courts and playing fields. Perceptive

analyses of everyday behavior can be the beginning of a worthy scientific hypothesis, but only a beginning. First analyses of this sort hardly qualify as empirical data that can support a theoretical concept. Bartlett's versatile tennis player could have had thousands of hours of practice, as is common among skilled athletes, and instead of recall schema it is just as reasonable to believe that practice has been given to the many moves that make up his impressive game. Couple this practice with some verbal control of motor behavior (Adams, 1971), and we have an alternative account of versatile skills.

A good starting point for research on motor recall schema would be something like the paradigm that is used to test stimulus recognition schema. The experimental design should be a transfer of training design, and as a minimum it should contain the conditions shown in the following tabulation:

Group	Training	Transfer
Experimental	Training on varied instances of a class of motor responses	All groups transfer to one or more new instances of the motor response class
Control I	Training on a single instance of the class of motor responses	
Control II		

If a recall schema has developed the Experimental Group will perform better than the control groups on the transfer test. Maybe Control Group I will perform better than Control Group II on the transfer test. Adams (1954) and Morrisett and Hovland (1959) found positive transfer under such circumstances in a problem-solving task.

This would seem to be an easy experiment to run, and yet it will not be easy because so many motor responses have accompanying verbal behavior; the recall schema may be no more than a consistent verbal mediator that is guiding the motor response. Suppose the motor response class chosen for study is the drawing of circles. The Experimental Group draws many different kinds of circles in training and all subjects covertly label their motor responses with the same verbal description each time. Assume that the repeated drawing of a single circle by Control Group I leads to less frequent labeling. In the transfer test the subjects of the Experimental Group could say to themselves "He wants me to draw a circle again," and they would proceed to do so. Control Group I subjects label less frequently and perform less well on the test and so the conclusion would be in behalf of a motor recall schema. The conclusion would be inappropriate because it is an experiment on verbal mediation as a determinant of motor behavior, not recall schema.

If a motor recall schema means no more than verbal mediation then we know more about it than we think we do. I am quite sure, however, that motor recall schema is intended to mean a wholly motor agent which might sometimes have

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verbal accompaniment but which is quite capable of producing motor versatility alone. To get at the fundamental motor core of motor recall schema will be difficult because of the ubiquitousness of verbal behavior. Possible research possibilities are to use motor sequences that defy verbal description, or to use preverbal children or animals. Another difficulty will be the definition of a motor response class. It is one thing to define a class of patterns for stimulus recognition experiments (all triangles, all dwellings), and quite another to define a class of movements whose definition sets it off from other classes of movement.

### 3. Response Recognition Schema

The recall schema has anecdotal evidence in shaky support of it but the response recognition schema lacks even that. The gist of the response recognition schema is that the subject can develop a capability to recognize and decide about the appropriateness of a class of motor responses, even though some of them have never occurred before. Response recognition is the function which I give to the perceptual trace in my closed-loop theory of motor learning, but I have the sensory consequences of each motor movement stored and entering a distribution of traces, which becomes a primary governing agent for the behavior. My theory would say that a movement outside the distribution of responses that has not been practiced would be poorly recognized as a member of the class, but the concept of response recognition schema would predict otherwise (providing the new response can be defended as a member of the motor response class).

## V. General Conclusions

A theory to be scientifically fruitful must receive good guidance from empirical data when it is conceived, as I said in the beginning of this chapter, and I believe that the concepts of motor program and schema are insufficiently supported at this time to be given meaningful definitions in motor theory; they should be held in the wings and regarded as pretheoretical until data justify moving them on to the stage that is theory. So I am temporarily satisfied with the omission of motor program and schema from my closed-loop theory of motor learning, although I am fascinated with the research challenge that they pose.

## References

- Adams, J.A. (1954). *J. Exp. Psychol.* 48, 15-18.  
Adams, J.A. (1961). *Psychol. Bull.* 58, 55-79.

- Adams, J.A. (1967). "Human Memory." McGraw-Hill, New York.
- Adams, J.A. (1968). *Psychol. Bull.* 70, 486-504.
- Adams, J.A. (1971). *J. Mot. Behav.* 3, 111-149.
- Adams, J.A., and Bray, N.W. (1970). *Psychol. Rev.* 77, 385-405.
- Adams, J.A., and Goetz, E.T. (1973). *J. Mot. Behav.* 5, 217-224.
- Adams, J.A., Goetz, E.T., and Marshall, P.H. (1972). *J. Exp. Psychol.* 92, 391-397.
- Attneave, F. (1957). *J. Exp. Psychol.* 54, 81-88.
- Bartlett, F.C. (1932). "Remembering: A Study in Experimental and Social Psychology." Univ. of Cambridge Press, London and New York.
- Bernstein, N. (1967). "The Co-ordination and Regulation of Movements." Pergamon, Oxford.
- Bossom, J., and Ommaya, A.K. (1968). *Brain* 91, 161-172.
- Bowman, J.P. (1968). *Anat. Rec.* 161, 483-488.
- Bowman, J.P., and Combs, C.M. (1968). *Exp. Neurol.* 21, 105-119.
- Bowman, J.P., and Combs, C.M. (1969a). *Exp. Neurol.* 23, 291-301.
- Bowman, J.P., and Combs, C.M. (1969b). *Exp. Neurol.* 23, 537-543.
- Bruner, J.S. (1970). In "Mechanisms of Motor Skill Development" (K. Connolly, ed.), pp. 63-92. Academic Press, New York.
- Charness, N., and Bregman, A.S. (1973). *Can. J. Psychol.* 27, 367-380.
- Chernikoff, R., and Taylor, F.V. (1952). *J. Exp. Psychol.* 43, 1-8.
- Cohen, B., Goto, K., Shanzler, S., and Weiss, A.H. (1965). *Exp. Neurol.* 13, 145-162.
- Cohn, R., Jakniunas, A., and Taub, E. (1972). *Science* 178, 1113-1115.
- Elwell, J.L., and Grindley, G.C. (1938). *Brit. J. Psychol.* 29, 39-54.
- Evarts, E.V. (1973). *Science* 179, 501-503.
- Evarts, E.V., Bizzi, E., Burke, R.E., DeLong, M., and Thach, W.T., Jr. (1971). *Neurosci. Res. Program, Bull.* 9, Whole No. 1.
- Fuchs, A.F., and Kornhuber, H.H. (1969). *J. Physiol. (London)* 200, 713-722.
- Head, H. (1920a). "Studies in Neurology," Vol. I. Oxford Univ. Press, London and New York.
- Head, H. (1920b). "Studies in Neurology," Vol. II. Oxford Univ. Press, London and New York.
- Head, H. (1926a). "Aphasia and Kindred Disorders of Speech," Vol. I. Macmillan, New York.
- Head, H. (1926b). "Aphasia and Kindred Disorders of Speech," Vol. II. Macmillan, New York.
- Hinde, R.A. (1969a). "Animal Behaviour." McGraw Hill, New York.
- Hinde, R.A. (1969b). *Quart. J. Exp. Psychol.* 21, 105-126.
- Honzik, C.H. (1936). *Comp. Psychol. Monogr.* 13, Whole No. 64.
- Hunter, W.S. (1930). *J. Gen. Psychol.* 3, 455-468.
- James, W. (1890a). "Principles of Psychology," Vol. I. Holt, New York.
- James, W. (1890b). "Principles of Psychology," Vol. II. Holt, New York.
- Johnstone, J.R., and Mark, R.F. (1969). *Comp. Biochem. Physiol.* 30, 931-939.
- Johnstone, J.R., and Mark, R.F. (1971). *J. Exp. Biol.* 54, 403-414.
- Keele, S.W. (1973). "Attention and Human Performance." Goodyear, Pacific Palisades, California.
- Knapp, H.D., Taub, E., and Berman, A.J. (1963). *Exp. Neurol.* 7, 305-315.
- Konorski, J. (1962). *Brain* 85, 277-294.
- Lashley, K.S. (1917). *Amer. J. Physiol.* 43, 169-194.
- Lashley, K.S. (1931). *J. Gen. Psychol.* 5, 3-19.
- Lashley, K.S. (1951). In "Cerebral Mechanisms in Behavior" (L.A. Jeffress, ed.), pp. 112-136. Wiley, New York.

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- Lashley, K.S., and Ball, J. (1929). *J. Comp. Psychol.* 9, 71-106.
- Lashley, K.S., and McCarthy, D.A. (1926). *J. Comp. Psychol.* 6, 423-434.
- Leont'ev, A.N., and Zaporozhets, A.V. (1960). "Rehabilitation of Hand Function" (B. Haigh, transl.). Pergamon, Oxford.
- MacNeilage, P.F. (1970). *Psychol. Rev.* 77, 182-196.
- Morrisett, L., Jr., and Hovland, C.I. (1959). *J. Exp. Psychol.* 58, 52-55.
- Mott, F.W., and Sherrington, C.S. (1895). *Proc. Roy. Soc.* 57, 481-488.
- Mowrer, O.H. (1947). *Harvard Educ. Rev.* 17, 102-148.
- Pavlov, I.P. (1932). *Psychol. Rev.* 39, 91-127.
- Pearson, K.G. (1972). *J. Exp. Biol.* 56, 173-192.
- Pearson, K.G., and Iles, J.F. (1973). *J. Exp. Biol.* 58, 725-744.
- Peterson, M.J., Meagher, R.B., Jr., Chait, H., and Gillie, S. (1973). *Cog. Psychol.* 4, 378-398.
- Pew, R.W. (1970). *J. Mot. Behav.* 2, 8-24.
- Pew, R.W. (1974). *Hum. Perf. Ctr. Tech. Rep. No. 48*. Univ. of Michigan, Ann Arbor.
- Posner, M.I. (1969). In "The Psychology of Learning and Motivation" (G.H. Bower and J.T. Spence, eds.), Vol. 3, pp. 43-100. Academic Press, New York.
- Posner, M.I., and Keele, S.W. (1968). *J. Exp. Psychol.* 77, 353-363.
- Posner, M.I., Goldsmith, R., and Welton, K.E., Jr. (1967). *J. Exp. Psychol.* 73, 28-38.
- Roeder, K.D., Tozian, L., and Weiant, E.A. (1960). *J. Insect Physiol.* 4, 45-62.
- Schmidt, R.A. (1975). *Psychol. Rev.* 82, 225-260.
- Sears, T.A., and Davis, J.N. (1968). *Ann. N.Y. Acad. Sci.* 155, 183-190.
- Sperry, R.W. (1950). *J. Comp. Physiol. Psychol.* 43, 482-489.
- Sussmann, H.M. (1972). *Psychol. Bull.* 77, 262-272.
- Taub, E., and Berman, A.J. (1963). *J. Comp. Physiol. Psychol.* 56, 1012-1016.
- Taub, E., and Berman, A.J. (1968). In "The Neuropsychology of Spatially Oriented Behavior" (S.J. Freedman, ed.), pp. 173-192. Dorsey, Homewood, Illinois.
- Taub, E., Bacon, R.C., and Berman, A.J. (1965). *J. Comp. Physiol. Psychol.* 59, 275-279.
- Teuber, H.-L. (1964). *Acquis. Lang. Mongr. Soc. Res. Child Develop.* 29, 131-138.
- Twitchell, T.E. (1954). *J. Neurophysiol.* 17, 239-252.
- Wilson, D.M. (1964). In "Neural Theory and Modeling" (R.F. Reiss, ed.), pp. 331-345. Stanford Univ. Press, Stanford, California.
- Wilson, D.M. (1965). In "Physiology of the Insect Central Nervous System" (J.E. Treherne and J.W.L. Beament, eds.), pp. 125-140. Academic Press, New York.
- Wilson, D.M. (1966). *Annu. Rev. Entomol.* 11, 103-122.

, 391-397.

Social Psychology."

ments." Pergamon,

Connolly, ed.), pp.

3, 145-162.

971). *Neurosci. Res.*

1-722.

s, London and New

s, London and New

I. Macmillan, New

II. Macmillan, New

1-939.

r, Pacific Palisades,

15.

Jeffress, ed.), pp.