Motor Schema Theory After 27 Years: Reflections and Implications for a New Theory

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The schema theory for discrete motor skill learning (Schmidt, 1975), originally published in 1975, has generated considerable interest and received strong challenges over its lifetime. In this paper, I focus on the findings generated since 1975 that bear on the theory and highlight those that produce difficulties for it and will be motivators for differing theoretical viewpoints in the future. At the same time, I examine other lines of evidence that seem to bolster the original lines of thinking. Finally, I provide some suggestions for a much needed new generation of motor learning theory, pointing out particular features from the schema theory that could be included and suggesting gaps and omissions that will need additional data and theorizing in future attempts.

Key words: dynamical systems, generalized motor programs, practice organization

Motor schema theory, originally presented in 1974 at a national meeting of the North American Society for Psychology of Sport and Physical Activity (NASPSPA) and published formally the following year (Schmidt, 1975), has experienced an interesting “ride” over the past 27 years. I am pleased to say that it has generated great interest—far more than I had ever imagined it would. It seemed to appeal to scientists in traditional motor learning at whom it was aimed, but it also resonated with people interested in children’s motor learning and motor development, workers in speech, physical, and occupational therapy, and various groups concerned with applying these ideas to various real world tasks and activities. At the same time, properly so, it has received considerable scrutiny and criticism. Some of this has come from the natural processes associated with doing empirical tests of the theory’s predictions. But more has probably been received from various competing points of view, such as the so-called dynamical systems perspective, that rejected the cognitively based assumptions of the schema theory.

The present paper, based on a presentation at the 2001 annual meeting of NASPSPA in St. Louis (see also Newell, 2003; Sherwood & Lee, 2003), attempts to take a long view of the theory’s history over the past 27 years. Surprisingly, many of the features that form the core of the theory have strong empirical and logical support and are even more appealing today than they were originally. At the same time, it has become clear, to me at least, that some of the ideas are incomplete and others have been contradicted by the empirical evidence or are just simply wrong. Still other aspects can be thought of as omissions that need to be added in later attempts.

As with other fields, we are driven by the need for a good, up-to-date theory that accounts for what we know and provides an understanding of the phenomena. And, paraphrasing Kerlinger (1973), there is nothing as practical as a good theory; having a good theoretical basis allows the application of numerous predictions to practical situations that were never considered in the original theory. All this suggests that someone needs to provide a new theory of motor learning—perhaps based on some of the ideas in schema theory, perhaps not—

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that can serve as the next “statement” of what happens when people learn skills.

Distinguishing Features of Schema Theory

Several features of the schema theory make it unique. These are discussed below.

Generalized Motor Programs

Certainly the most important idea in schema theory, and what caused it to deviate from Adams’ (1971) theory from which schema theory takes much of its heritage, is the idea of the motor program. To me, it has always (at least since my days with Franklin Henry) been obvious that quick actions somehow have to be organized in advance and represented in memory. Sensory information, or response-produced feedback, is too slow to account for control in these quick actions. A major drawback, in my view, of Adams’ theory was that it could not account for events that occur in rapid tasks (for a fuller discussion, see Schmidt & Lee, 1999).

It should also be mentioned that the schema theory was intended to be an account of discrete actions. Hence, continuous actions, such as steering a car or juggling, which are both of longer duration (allowing time for response-produced feedback to have a role) and more based on the performer’s interactions with the environment, were outside the area for schema theory. There was always (at least since James, 1890) the suggestion, however, that long-duration actions might be based on interplay between open-loop subactions and feedback-based corrections—a view taken later on by Meyer, Smith, and Wright (1982; Meyer, Smith, Kornblum, Abrams, & Wright, 1990). Interestingly, tasks such as juggling (Beek & Lewbel, 1995) seem appropriate for analysis in terms of the dynamical systems perspective, as I will discuss.

The theory added the unique feature that these programs are generalized—or are organized so that a given memorial structure or program could be executed in countless ways to allow for the fantastic number of variations we can do for the “same” action. I chose the mathematically simplest way of thinking about this, arguing that the program could be scaled linearly in time (by scaling a rate parameter) and in amplitude (by scaling a force parameter). Relative timing and relative force (i.e., the specification of the amount(s) of force over time) were thought to be rigidly structured “in” the program.

In this way, for schema theory, the problem facing the performer was to learn two things from practice. First, the performer needed to acquire the generalized motor program (GMP) that defined the “form” of the action. Second, the performer needed to learn the schemata that allowed the action to be scaled to the environment. However, as I discuss later, the theory was mute on how programs were developed.

Two Compartments of Memory

Another idea critical to learning theory in the early 1970s was the notion that memory be thought of in two states. This made good sense to Adams (1971), and I borrowed this idea as well. A recall memory in learning was responsible for producing a response, and it seemed natural to think of programs as the motor skills equivalent of recall memory in verbal skills. A recognition memory responsible for recognizing the correctness of a response translated naturally into the functioning of feedback and sensory experience in motor skills. The important extension of this idea to movement skills was that programs and recall memory controlled fast actions too quick to use feedback, whereas some combination of recall and recognition memory controlled slow actions that could use feedback for on-going control. Structuring the theory in this way allowed one to produce a fast action from recall and then evaluate it afterward from recognition, which spawned a number of studies on learning so-called error detection mechanisms in motor skills (e.g., Schmidt & White, 1972).

The Schema Concept

The schema concept was an old idea borrowed from Head (1926) and Bartlett (1932) and made more popular by Posner and Keele (1968, 1970) and Evans and associates (Edmonds, Evans, & Mueller, 1966). For the motor skills variant of this idea, the schema was a rule, developed by practice and experience across a lifetime, which described a relationship between the outcomes achieved on past attempts at running a program and the parameters chosen on those attempts. A recall schema was the relationship between parameters for the program on each trial and the outcome achieved by the program (e.g., distance thrown). A recognition schema was the relationship between the past sensory consequences generated by running the program and the outcomes of the program. Motor learning was conceptualized as the development of these schemata with practice and experience. The failure to make a distinction between a schema and a generalized motor program has been a common error among scientists writing about schema theory.

Storage and Novelty Problems

The combination of the idea of schemata with the ideas of generalized motor programs provided one way
of solving two rather persistent problems facing theorists in motor behavior. One was the so-called storage problem. The original idea of motor programs assumed that one needed a separate program for every action that we could do and implied massive storage capabilities for the central nervous system. The novelty problem was the notion that, unless we had inherited a given program for some action, there was no way to produce an action we had never produced previously and no way to produce novelty, which is so obviously a part of our capabilities.

Evaluation of the Constructs for Future Theorizing

In this section, I review some of the research that has evaluated several of the constructs mentioned in the previous section. A major goal will be to determine which of these ideas deserve retention in future versions of theories on motor learning and which should be modified or discarded.

Motor Programs

Without a doubt, the “hottest” issue surrounding motor behavior generally and schema theory in particular in the past 25 years was the idea of motor programs. Part of the issue was the emergence of the so-called dynamical systems perspective, which generated strong interest in the 1980s and throughout the last of the 20th century. A major assumption of this view was that order and regularity were not preprogrammed and stored but emerged from the dynamical interactions among many degrees of freedom in the motor system. Relative timing and relative force were measurable regularities of skilled actions, but it was too “easy” to account for their behavior by placing them in a cognitive structure called a motor program. For a time, and probably even continuing today, scientists in these areas have developed “camps” whose members (myself included) refused to be swayed by the arguments of the “enemy.”

These disputes remind me that science is, among other things, a social process complete with competitiveness, a focus on a few data points or minor differences that bolster one’s pet viewpoints, and vanity among its participants. I have seen a great deal of contention based on these issues over the years and even some blatant discrimination in areas relating to grant support and manuscript evaluation. This was one of the major reasons that I stopped working actively in the area. This formation of camps was, in many ways, unfortunate, but I suppose that is the way theoretical developments occur in most fields, with the camps arguing until one eventually falls or stops resisting. Newell (2003) contends that, by today, the two camps have “agreed to disagree,” which is probably not far off the mark.

Even so, today the evidence for motor programming is even stronger, and I would not shrink for a moment to include it in the next generation of theory in this area. As luck would have it, what I consider to be one of the most important papers in motor behavior, and the strongest support yet for motor programs, was published a few years after the schema theory—that being the article by Wadman, Denier van der Gon, Geuze, & Mol (1979). Their study was simple. They had participants make rapid elbow extension movements to targets and recorded electromyogram (EMG) from the agonist (extensor) and antagonist (flexor) muscles. The integrated records for these actions are shown as the darker traces in Figure 1. The pattern starts with a strong agonist burst to accelerate the limb toward the target, and then after about 80 ms this is terminated and replaced by an antagonist burst that presumably decelerates the limb. Finally, after another 50–60 ms, the agonist burst is terminated and replaced by a second agonist burst that probably stabilizes the action at the target (for a fuller discussion, see Schmidt & Lee, 1999, in press, Chapter 6).

For me, the most interesting question is: what determines or defines this pattern? More particularly, what terminates the agonist burst and activates the antagonist

![Figure 1. Agonist (triceps) and antagonist (biceps) EMG activity in a rapid elbow-extension task; the patterning remains largely unchanged even though the limb is mechanically prevented from moving (blocked; from Wadman, Denier van der Gon, Geuze, & Mol, 1979).](image)
burst after about 80 ms? People in a cognitive camp would argue that the motor program was responsible, with these things being a part of the “script” that runs off when the program is activated. The “other camp,” however, would argue that the dynamics of the limb’s movement and the sensory effects generated by the contracting muscles are received centrally by the spinal cord and initiate the next contractions (Feldman, 1986; Kelso, 1995); these patterns aren’t programmed at all but rather emerge from the dynamics.

The most interesting part of the results from Wadman et al. (1979) discriminates strongly between these two views. In addition to the procedures just mentioned, unpredictably to the participant, the experimenters would mechanically block the limb from moving from the starting location. From the dynamical perspective, this would radically alter the dynamics of the moving limb and, hence, would be expected to alter the EMG patterning. In fact, if the system depended on the joint reaching some angular position before the muscles were switched from agonist to antagonist action, then the muscles would never be switched if the movement never left the starting position. What happened is shown in the lighter traces in Figure 1. We can see that the offset time of the agonist burst and the onset time of the antagonist were not affected at all by the interruption. These bursts appear in time and amplitude just as if the limb were not blocked. This clearly contradicts a dynamical viewpoint in which these patterns emerged from the limb dynamics. And, these results clearly support a preprogrammed view in which the script is stored in advance and run off open-loop, without being affected by the momentary changes in the limb’s dynamics. Of course, later on (after 120 ms or so) when sensory information begins to have time to be used, we see changes in the blocked versus normal traces in Figure 1. I have taken these effects to mean that the motor program’s output is played out to the muscles and that the EMG record is the “signature” of this activity. I don’t see how these facts can be ignored in future theorizing about rapid actions, and future theories will have to account for them.

This general distinction—whether the timing involved in producing an action (as in Figure 1) is centrally represented or the product of dynamics—has been one of the truly long-standing issues during my tenure in this field. It remains still (see Newell, 2003). I agree that order and regularity do not necessarily imply a central timer. But my questions to people in the dynamical systems camp (both in personal conversations and in public presentations) referring to my interpretations of Wadman et al. (1979) has never yielded a satisfactory answer about where this patterning arises or why it is not disrupted when the limb is blocked. To say that it “emerged from the brain dynamics,” as one researcher claimed in response, is just nonsense to me and seems to be just another vague attempt to prevent any “retreat” toward admitting that something was stored in advance as program theory would have it. And, how can a general theory of motor control be developed without having some way to account for phenomena in fast movements? If I am wrong, and the dynamical perspective does have a way to account for this, I would certainly love to hear it, but at this writing I view this evidence as devastating to their view.

Generalized Motor Programs

This feature, which was an important part of the schema theory but also was of interest in movement control, has received considerable scrutiny over the years. Several of the issues are discussed below.

Separation of Programs and Parameters. A basic question was whether the proposed separation between factors that were a part of the fundamental pattern (the generalized motor program; GMP) and those that scaled it (parameters) had any psychological reality. A number of lines of evidence support this separation. Some are related to motor performance issues, such as the finding that the time to change a parameter for a given program was far less than the time required to change a program (e.g., Quinn & Sherwood, 1983). Others, though, are based on findings related to learning and are more relevant here, as discussed next.

Based on analyses designed by Wulf, Schmidt, and Deubel (1993), it is possible to decompose the errors in various movement patterning tasks into measures related to (a) errors in the scaling of the fundamental pattern (the GMP) in space and time versus (b) errors in the fundamental pattern itself. More interestingly, in a series of experiments on motor learning, we showed that factors affecting learning one kind of measure (e.g., of the GMP) do not affect learning scaling (parameterization), or they affect these measures in opposite ways. For example, decreasing the relative frequency of feedback presentations enhances learning the GMP, but degrades parameter learning (Wulf, Lee, & Schmidt, 1994; Wulf & Schmidt, 1989; Wulf et al., 1993). Randomized (as opposed to blocked) practice enhances the learning of the GMP but degrades the learning of parameterization (Wulf & Lee, 1993), but also see Sherwood and Lee (2003).

Relative Timing Seems Invariant. Given the idea that some kind of generalizable program is reasonable, a great deal of argument has occurred over what the invariant features and the parameters might be. The initial hunch, based on the evidence at that time (e.g., Armstrong, 1970), was that relative time (the temporal structure, or pattern) was invariant and that some “rate parameter” could scale this structure proportionately in
time. This expectation still seems to be reasonable. Many different studies have examined this prediction and, while there are examples where this prediction of exact invariance has not been found, most of the work supports an approximate temporal invariance (e.g., Heuer, Schmidt, & Ghosdarian, 1995; but see Gentner, 1987, and Schmidt & Lee, 1999, for a discussion). Heuer (1991) argued that the tendency toward temporal invariance is compelling enough for us to ignore temporarily the minor deviations from invariance that have been reported (e.g., Burgess-Limerick, Abernethy, & Neal, 1992).

Recently, this evaluation of relative timing invariance has been extended to bimanual actions, bringing it into the realm of the new and interesting area of coordination. Heuer et al. (1995) showed a very strong temporal invariance when participants were asked to produce simultaneous but different rapid actions with the two arms; they demonstrated that the temporal structure of each limb, as well as the temporal structure across limbs, was scaled linearly in time—that is, invariant relative timing was present for the whole bimanual task. These and similar findings extend the notion of relative timing invariance considerably and bring it in as a way to understand some of the phenomena that have, up to now, been considered as unique to coordination and the dynamical systems approach.

Even so, it should be clear by now that there are many kinds of actions for which this temporal invariance does not hold. First, this initial attempt at theorizing assumed a mathematically simple linear scaling, and it is certainly possible that temporal scaling might be more complex than this; a different type of scaling might reveal invariance. Also, many of these are tasks for which the performer cannot plan the action in advance because of the need to adjust for environmental disturbances and response-produced variability. Goal-directed reaching is one example for which the initial parts of the action appear to be scaled proportionately in time. But when the end of the action (including a grasp) is included, the whole reach-plus-grasp action does not show this temporal invariance. It is as if this latter phase occurred as a separate and subsequent action (Marteniuk, Leavitt, MacKenzie, & Athenes, 1990) to the reaching phase. Another example is juggling (Beek & Lewbel, 1995). Clearly, there are limits to the claim that discrete actions are run off with GMPs, and a newer theory must recognize this concern.

Relative Force. A more serious problem was the idea that relative force was also invariant in the GMP. The original idea was that two parameters were needed, one for scaling the rate (above), and a second for scaling the forces. In this way, the pattern could be scaled in both time and size. In many cases (e.g., Wulf et al., 1993) in which the participant moved a mechanically supported lever, this seemed to be quite reasonable.

But the more serious problem is that, with a GMP structured this way, it cannot account for actions involving gravity. One example is walking normally versus walking with a load, such as a backpack. Here, the extensor muscles and those involved in the stance phase must operate differently as a function of the load; however, the flexor muscles and those involved with the swing phase can operate essentially independently of the load. Simply scaling all the muscles proportionately will not accomplish this. Another example is producing an action in different planes (e.g., horizontally versus vertically); the effect of gravity in the flexors versus extensors will be different in horizontal versus vertical movements (e.g., Schmidt & McGown, 1980).

Almost certainly, this claim of relative force invariance is incorrect. Several possibilities come to mind as ways to improve this theorizing without abandoning the concept (that movements are somehow generalized) altogether. One is to argue that relative timing is invariant, but that relative force is not. In this view, the relative force would be a kind of parameter that scales muscles differently as they are affected by gravity or other factors. The problem with such proposals is that more and more free variables are created, which results in more that has to be specified before the action can be initiated. I am not sure how to solve this problem, but it certainly deserves attention as one of the important directions in this theorizing.

Variations on the GMP View

If one starts theorizing, as I would, with the idea that at least our quickest actions are pre-programmed, a number of ideas presented recently suggest some variations on this theme as well as some additional theoretical problems to address.

Motor Equivalence. We have long understood that people can achieve the same goals by using a variety of kinematic patterns that do not seem to be represented by a simple scaling of a given fundamental pattern. This phenomenon has been labeled “motor equivalence” (e.g., Abbs & Cole, 1987; Bertram, 2002). The problem is that if (a) the action is preprogrammed as I have argued, and (b) the action can vary in its fundamental pattern, how does the pattern on any given attempt become actualized? One view is the “new” action is planned that way in advance. Perhaps so, but this requires an explanation of how the programs for these different actions are generated and takes us back to the older issues associated with the storage and novelty problems. Another view is that the actions cited in support of motor equivalence are somewhat longer in duration than our quickest actions, allowing feedback-based processes to modify the movement while it is unfolding. I am not sure of the solution, but motor equivalence is a serious
omission in the schema theory that should be addressed in future attempts.

Hierarchical Control. Recent evidence suggests different control structures and processes for various limbs participating in a coordinated sequence. Dounskaia and colleagues (e.g., Dounskaia, Ketchem, & Stelmach, 2002; Dounskaia, Swinnen, Walter, Spaepen, & Verschuereen, 1998) found in drawing movements that wrist control was to be based on different processes than elbow control. In this view, these joints also serve different functions—the elbow moving the wrist as a whole and the wrist making fine adjustments to compensate for variations in the elbow action as well as to produce the final outcome of drawing. Such a view, of course, is not consistent with a notion that a single GMP governs the whole action. Perhaps the answer is that these actions are of relatively long duration, however, allowing the possibility that online feedback control is mainly directed at the wrist and not the elbow. In any case, these findings appear to raise additional questions for a view of GMPs.

Errors and Error Detection

An interesting aspect of the schema theory was the treatment of errors. One idea I particularly like is the notion that every action we produce (provided it receives feedback) generates schema learning, even if it is incorrect (i.e., scaled inappropriately for the environment). Incorrect actions generate the same relationship between parameters used and movement outcomes that the so-called correct ones do, and they increase variability in experiences that leads to increased movement generalization to new environmental situations.

Another aspect dealt with the detection of errors. Schema learning involved learning a recognition schema that was an "evaluator" of response-produced feedback and allowed improved capability to detect one's own errors with increased practice (e.g., Schmidt & White, 1972). But a curious prediction was that this error detection capability was only possible in rapid actions in which the GMP produced the action and the recognition schema evaluated it. However, in slow actions (e.g., linear positioning), the movement is actually produced by moving to that position recognized as correct (via feedback), so the performer can have no further error-detection capabilities after the action in these tasks. These predictions were borne out well by Nicholson and Schmidt (1991), who showed that in rapid timing tasks the performer had strong correlations between actual and estimated errors, whereas in slow timing tasks they did not.

Space limitations prevent more on this issue of error detection. But I think it is one of the badly neglected topics in motor learning. After all, if one can develop a capability to detect one's errors, then the learner can practice without a teacher or coach. Such findings have strong practical as well as theoretical implications.

How Do GMPs Originate?

A large problem with the schema theory is that it focused almost completely on how the performer learned to scale the GMP, and it never addressed the question of how GMPs were acquired or how they were modified. It simply assumed that the performer had a program for, say, throwing, and that motor learning was the process by which the person learned to scale it.

GMP Versus Parameter Learning. Over the years since 1975, a number of investigators have begun to ask about the development of GMPs. Wulf and Lee (1993), for example, showed that a powerful variable to determine learning of the GMP was randomized (as opposed to blocked) practice. Wulf et al. (1993) showed that reduced relative frequency of feedback facilitated GMP learning. All of this gives some initial insight into how these structures must be learned.

The idea that random practice and reduced feedback are involved in GMP learning encourages the idea that the learning is accomplished by a kind of reconstruction process. Recall that one of the dominant theories for understanding random practice effects was that random practice prevents producing the same response across trials (as opposed to blocked practice), forcing the learner to reconstruct the action again on the next attempt. This reconstruction process was thought to be important in developing the memory for the action. Combining these ideas, it is tempting to suggest that GMPs are learned by a kind of successive modification resulting from efforts at reconstructing them in practice. How this works, of course, is far from clear, but such questions are absolutely fundamental to understanding motor learning.

As an aside, I never have understood the criticism on the part of those in the dynamical perspective about learning being the creation of "more and better programs" (e.g., Newell, 2003), which, in terms of schema theory, it most certainly is. How would we want to consider learning a repertoire of separate gymnastics skills (such as I spent a considerable portion of my youth doing), or German vocabulary (another large portion of my earlier years)? What's wrong with the "more-and-better programs" metaphor, anyway?

In any case, it does seem likely that learning GMPs and parameters occur differently. First, the variables (a) random versus blocked practice and (b) relative feedback frequency act in opposite ways on the GMP versus parameter learning, with random practice and low relative frequency degrading parameter learning. This certainly does not support the idea that parameter learning is based on requirements for reconstruction, for ex-
ample. Rather, it seems more reasonable that parameter learning is based on schema modifications, as the original schema theory claimed.

When we first learned that reduced feedback frequency enhanced learning (Winston & Schmidt, 1990), I immediately foresaw the final demise of the original schema theory, because this seemed to contradict directly one of the predictions. The schema view predicted that withholding knowledge of results (KR) on a given trial produces no schema updating, because both (a) the movement outcome from KR and (b) the parameter used on that trial must be paired to make a kind of data point for upgrading of the schema, and the KR part was missing. Thus, finding that withholding KR on a portion of the practice trials enhanced learning appeared devastating for the schema theory.

But when Wulf et al. (1993) developed a method to separate the errors in GMP from errors in parameterization, we subsequently learned that the reduced KR frequency was acting to degrade only the parameterization part of the task. That is, reducing feedback frequency degraded the schema just as the original theory had expected (also Wulf & Schmidt, 1996). Apparently, when reduced frequency enhanced learning the overall task (in Winston & Schmidt, 1990), the benefits to the GMP learning must have overshadowed the decrements due to parameterization learning, although we did not know this at the time. This area is ripe for more work.

Random Practice Versus Variable Practice. An interesting hypothesis is that randomized practice only works when conducted with several different GMPs (see Magill & Hall, 1990). For random practice to be beneficial it seems to make little difference how different the tasks were so long as they were, in fact, based on different GMPs (e.g., throwing, kicking, and tapping). Variable practice, on the other hand, is conducted among task variants with a given GMP but with different parameters assigned over trials (e.g., throwing different distances). In terms of the theory, it was tempting to say that random versus blocked practice operated only on the GMP learning and that variable practice operated only on the parameter learning (or schema) part of the process. While there is considerable empirical support for this hypothesis, more work is needed to focus on some apparent contradictions to this generalization.

Overall, evidence favors the idea that the GMP is learned by different processes than parameterization. The idea of parameterization—the keynote process in the schema theory—seems defensible as a reasonable ingredient in a new theory. What is needed at this point is theorizing about how the GMPs are learned that could be added to the parameterization learning view to form a whole theory of learning. There continue to be interesting extensions for the notion of variable practice into a variety of related fields, which makes this area of research even more important (see, e.g., Roller, Cohen, Kimball, & Bloomberg, 2001).

Specificity of Learning. The schema theory assumed that when we learn a task, we really learn a collection of tasks that vary along the dimensions of the parameters involved. Then, producing this action involves selecting the program and the necessary parameters. An interesting question was whether or not anything specific was learned about the particular actions actually practiced, or whether they just contributed to the general class that was learned.

Young and Schmidt (1999) examined this question using skilled collegiate basketball players, who were asked to produce set-shots at distances of 9, 11, 13, 15, 17, 19, and 21 feet (2.7, 3.4, 4.0, 4.6, 5.2, 5.8, and 6.4 m, respectively) from the basket, positioned along the center line of the court. In this set, 15 feet (4.6 m) was the regulation distance for the free-throw shot in basketball (which is assumedly highly practiced), and the others were simply shorter and longer shots that may or may not be actually experienced in practice or playing the game. Is there something specific learned about the 15-foot (4.6-m) shot that distinguishes it from the others?

We plotted the regression line of proportion of correct shots against shot distance, using the three longer and three shorter distances as data; we then asked whether the proportion correct for the 15-foot (4.6-m) distance lay on this regression line. The correct proportion for the 15-foot (4.6-m) distance was significantly larger than the value predicted from the other six distances, suggesting that something unique to the 15-foot (4.6-m) distance was learned during its practice and the 15-foot (4.6-m) skill was not simply a member of a larger skills class ranging from 9 through 21 feet (2.7 through 6.4 m). The capability for performing this class of skills seems to be a combination of (a) GMP-based learning of a class of distances and (b) some extra proficiency achieved via practice at the specific 15-foot (4.6-m) target. This learning specificity is another factor that needs to be considered in the next generation theory for motor learning (see also Schmidt & Lee, in press).

Learning Curves and Time Scales. Newell (2003) talks about time scales and learning and argues that the work on exponential “learning curves” has been largely ignored. He is incorrect. One of the most important papers in my education was by Bahrick, Fitts, and Briggs (1957), entitled “Learning curves: Facts or artifacts.” This paper made it clear to me that the shapes of so-called learning curves (exponential or otherwise) were not reliable predictors of learning, were subject to temporary performance variables (e.g., fatigue), and that some other method was needed to assess learning. Our field seems to have chosen the transfer-test method that does not have these artifacts and seems to be well accepted today. It is not so much the ignoring of exponential
curves—but the rejection of them—that has been the source of lack of attention (see Schmidt & Bjork, 1992, for a further discussion). Any new theory built on these kinds of analyses will have a tough time overcoming many decades-old objections.

Practical Application. Finally, I think it is important for a new theory of motor learning to have at least some focus on practical application. This has been important for me for a long time (see Schmidt, 1977), but it became especially clear when I was writing my undergraduate textbook (Schmidt, 1991) in the late 1980s. I strove to create a chapter (Chapter 11) that identified as many of the solid empirical findings as possible and then structured implications for teaching motor skills. When I started thinking about it and writing down examples, I was amazed by the number of statements about how to teach that are possible from the empirical data surrounding schema theory and from the theory itself. All these applications are “true” to the data and defensible empirically. This was taken further with Craig Wisberg’s efforts in the next edition (Schmidt & Wisberg, 2000).

At the time I was doing this, I tried to think of what kinds of analogous statements could be made from a dynamical systems perspective. What practice variables are relevant? What does information feedback do? What is the role of errors and error detection? There are many others. All this made me realize that, while a dynamical systems perspective might have a great deal to say about the control of long-duration skills, where sensory information is integrated into actions (e.g., juggling) and program theory is on its weakest ground, it does not have much to say about learning at least that I can detect. The recent work of Zanolie and Kelso (1992, 1994, 1997), which suggested the acquisition of stable coordination states with practice and experience, might be one counterexample.

However, Newell (2003) and others before have argued that cognitive theorists have chosen tasks and independent and dependent variables that most closely align themselves with cognitive viewpoints and that these are not the only variables of interest. I agree, but let’s turn the table. What learning variables do people in the dynamical systems viewpoint want to discuss that can serve as the starting point for a new theory of learning? I simply don’t see any other than perhaps practice itself. When we planned this symposium one of my goals, at least, was to try to stimulate people from the dynamical systems view to give their views about how to structure a new learning theory, encourage different independent variables that were aligned to dynamical views, and create predictions for them that could be tested. There is not much of this in Newell’s (2003) response, despite his statement that the dynamical systems view “… clearly provides a theory of motor learning that is as or more encompassing than the original tenets of schema theory” (Newell, 2003). There is, however, plenty of the same old rhetoric about the problems with symbolic-cognitive theories and the promising contributions the dynamics perspective has for learning. I find this generally to be far overblown, and close analysis of what this view actually has to say about learning would reveal a wide void. I am doubtful that a viable theory of motor learning will “emerge,” if I may, from the dynamical systems viewpoint any time soon. If I’m wrong, I am willing to be shown. But I haven’t seen it yet.

Call for a New Theory

I argue here that, although the 1975 schema theory may have provided some interesting insights into several aspects of skill learning, it is deficient in a number of ways. Given the importance of theories in general for (a) organizing the many and diverse research findings, (b) directing empirical research, and (c) providing a basis for practical application, it is time the motor learning field developed a new theory for motor learning. If I were to do this, it would probably include many of the features of schema theory that have weathered the past 27 years. Also I would exclude or at least change many of the other features to include new data and thinking.

Others, such as those adhering to a dynamical systems viewpoint (e.g., Newell, 2003), probably see it differently and would structure a theory along different lines altogether. But the goal should be the same, regardless of how it is done. A major goal now, as it was in 1975, would be to develop a theoretical structure that accounts for as many of the reliable empirical findings pertaining to motor learning as possible and is contradicted by none of them.

References


**Author’s Notes**

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