THE VARIABILITY OF PRACTICE HYPOTHESIS: MANIPULATION OF TWO TASK PARAMETERS

by

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ABSTRACT

While numerous studies exist in the literature which have examined the variability of practice hypothesis by comparing how different practice schedules affect transfer to a novel variant of the practice task, there exists a paucity of such studies which have sought to provide variability via the manipulation of more than one task parameter. The experiment reported here attempted to address this issue. One hundred and ninety-two university students served as subjects in a novel handle-pulling task. Handle displacement caused a simulated ball on a video monitor to execute a parabolic trajectory towards a specified target. The target location could be altered, and the distance the ball moved was directly dependent upon the extent of handle displacement. Subjects had one second after initiating a pull in which to complete their movement. The resistance against which subjects pulled could also be altered. Such manipulations did not directly affect the flight path of the ball, but did change the muscular activity required to effect a given handle displacement. There were six possible practice resistances, and ten possible practice targets. Practice groups received twenty practice trials before transferring to two novel variants, one within, and one external to, the practice boundaries of both task parameters. The control group received no pre-test practice. Five groups received practice which involved manipulations of target distance, five received manipulations of handle resistance, and five received manipulations involving both parameters. Within each of these five groups, two had high levels of practice variability, two had low levels of variability, and the fifth was a constant practice group. One of the high variability and one of the low variability groups experienced a random presentation order during practice, while the other two received a sequential presentation order. Analyses involving constant error and variable error of the last four of five test trials, as well as mean first test trial performance, were conducted on both transfer tasks. Results indicated that the parameter manipulated during practice was a significant determinant of first trial performance on both tasks, as well as of variable error score on the interpolated transfer task. A significant effect of test task presentation order was also noted for the extrapolated transfer task, as well as interaction effects involving parameter and practice variability level, and parameter and presentation order.

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CHAPTER 1

INTRODUCTION

Many investigators in the field of motor learning and behaviour tend to view the latter part of the nineteenth century as the beginning of scientific study in the area (e.g. Adams, 1987; and Irion, 1966). The seminal studies of Bryan and Harter (1897) on the acquisition of telegraphic skill are often considered to have developed awareness in many psychologists of the fruitful use of simple skills in the elucidation and explanation of fundamental learning processes. Early workers examined, among other issues, such questions as how the distribution of practice affects skill acquisition (e.g. Digman, 1956; and 1959), whether it is more beneficial to practise a given skill in parts or as a whole (e.g. Barton, 1921; and Knapp & Dixon, 1952; or see Wightman & Lintern, 1985, for a more thorough exploration of this topic), the effects of warm-up (e.g. Adams, 1952; and 1961), what is the most effective schedule on which to provide learners with knowledge of results (KR) (e.g. Bilodeau & Bilodeau, 1958), how proactive and retroactive interference work (e.g. Stelmach, 1969; and Williams, Beaver, Spence & Rundell, 1969), and various questions regarding transfer of training effects (e.g. Lordahl & Archer, 1958). While many of the dimensions of learning and the parameters studied during the early investigatory phase of motor learning research may now be of historical interest only, transfer of training has been a recurrent issue in the field of study (Adams, 1987). Indeed, many of the issues considered, both historically and presently, while not always transfer of training per se, bear relationships of varying intimacy with this phenomenon. For example, retention and forgetting, proactive and retroactive interference, whole versus part practice, bilateral transfer, practice schedules, temporal arrangement of skills based on relative difficulty level, warm-up, and even to some extent changes due to the effects of maturation, may all be considered to involve, at one level or another, the question of transfer of training. The ubiquitous, albeit frequently underlying, nature of transfer provides an obvious incentive for researchers to improve their understanding of the mechanisms involved in this process.

Aside from the fact that transfer of training is a pertinent topic in the examination of such a variety of issues within the motor learning domain, there are other reasons for its continued position as an important theme in the field of study. Firstly, an understanding of transfer of training is important for an understanding of the general acquisition process. For the vast majority of skills it is impossible to regard any adult as a novice. There are always inherent in the learner previous experiences which may be relevant to the new skill to be acquired. In the oft-quoted words of Bartlett (1932) regarding tennis stroke production:

> ...When I make the stroke I do not, as a matter of fact, produce something absolutely new, and I never merely repeat something old. (p.202).

Such reconciliation of past experiences with present demands may be said to occur in almost all human performance.

Secondly, it is the case that much of the consistent attention of researchers to the topic of transfer of training may be attributed to the practical value of understanding the processes involved. Much of the training strategy from instruction in education, to sports, to industrial and military skills, relies on the appropriate use of organized stages of skill progressions, or transfer of training designs. With such widespread potential applicability, it is not surprising that quite a number of researchers have examined transfer of training issues. A brief account of the general history of transfer of training, focussing on issues most relevant to the area of motor learning, is included in Appendix I.

A specific question regarding practice schedules and transfer, which has received considerable attention in the last several years, is how variability of practice affects transfer of training within a movement class. This current interest in variability of practice has been largely compelled by the work of Schmidt (1975; 1976; 1980; 1982a; 1982b; and 1982c), and his development of Schema Theory.

1.1 Schema Theory

The trend which began with Thorndike in 1907, to consider motor learning from a behaviourist perspective, or S-R orientation, remained central to motor learning until the 1970's. Even Holding's (1976) transfer paper, which was published after significant criticism of an S-R approach to motor learning had resulted in relatively few studies with such an orientation, nevertheless summarized transfer from an S-R perspective. More recently attempts have been made to renew the examination of transfer from a cognitive orientation. (Judd, 1908, is perhaps one of the earliest researchers to consider the cognitive aspects of transfer of a motor task). The first steps in this direction were taken by Schmidt (1975) in his development of Schema Theory.

> ...(I)t is obvious that the schema concept belongs squarely within the cognitive theoretical framework in psychology. Schmidt (1975) used this concept, while retaining much of the valuable contribution of Closed Loop theory, and hence may be thought of as finally declaring, in theoretical terms, the divorce of motor learning theory from the S-R behaviourist tradition and a move to a more cognitive orientation. (Dickinson & Goodman, 1986, p.33).

Schema Theory was originally presented as an improvement over an earlier motor learning theory, Adams' (1971) Closed Loop Theory. While Schema Theory was primarily a theory about motor learning in general, it also lent itself particularly well to the specific issue of transfer of training. Before addressing the implications of Schema Theory for transfer of training phenomena, however, the theory will be discussed in more general terms.

As just mentioned, Schmidt (1975) addressed numerous specific short-comings of Adams' Closed Loop Theory. First, Closed Loop Theory is pertinent only as an explanation for the learning of slow, positioning tasks. Clearly, not all human motor behaviour falls into this category. Furthermore, Closed Loop Theory is not always an accurate predictor of the performance of even such a limited range of movement types. Rapid movements are not handled at all by Adams' theory.

A second problem with Closed Loop Theory was that it cannot explain accuracy in the absence of practice on the specific task itself, since Adams' (1971) has specified that the memory trace required to generate an accurate response can only be developed via such task-specific practice. An example of empirical evidence which illustrates the inadequacy of such a theoretical premise is provided by Williams and Rodney (1978), who demonstrated that subjects who practised a series of arm positioning movements surrounding a criterion distance could produce that criterion movement as well as subjects who only practised the criterion. Thus, response variability may be productive, and is not always disruptive as Closed Loop Theory would predict.

A third difficulty with Closed Loop Theory is that it fails to consider movements which do not rely on feedback for successful completion. Numerous studies involving animals (e.g. Taub & Berman, 1968) and humans (e.g. Lashley, 1917) have demonstrated that peripheral feedback is unnecessary for accurate movement, thus implying the capacity for central control of movement.

Perhaps the two most recounted weaknesses of Closed Loop Theory involve what have come to be known as the storage problem and the novelty problem. Adams has suggested that there is a memory trace which initiates every movement made. Thus, a trace for each movement must be stored in memory. While it is true that the capacity of the central nervous system is immense, it seems unlikely that it could accommodate the incredible volume described by a lifetime of movement. At best, the notion is less than parsimonious. Furthermore, Adams has postulated that every movement an individual makes is then stored in memory in the form of a perceptual trace. While it is true that Adams has also claimed that these perceptual traces decay over time, it is not unreasonable, based on our knowledge of human movement memory capabilities, to believe that many of these perceptual traces must exist in memory at any given moment. This brings the total memory space requirements for movement to astronomical proportions.

A difficulty related to this storage problem, although of separate concern, is how new movements are generated. This novelty problem provides a pitfall for any theory which purports to address human motor learning and does not successfully handle the problem, since obviously humans quite readily make novel movements, and often do so rather well. In the case of Closed Loop Theory, it is difficult to explain how a memory trace which is a product of task experience can exist a priori to initiate a novel movement. Since, as Bartlett (1932) pointed out, repetitions of the same movement are rarely, if ever, executed in precisely the same way, due to changing initial conditions and/or task demands, it becomes problematic for a single memory trace to initiate "novel" variants of the same movement. Adams developed his theory to address acquisition of slow positioning movements only, and it is obvious, based on the problems identified, that Closed Loop Theory does little to address the issue of transfer of training phenomena.

Schmidt (1975) solved these problems by postulating that individual movements are not stored in memory, and that a specific trace is not required for each movement to be initiated. Rather, he suggested that generalized rules, or schemata, are formulated over time for each type of movement made, and that individual movements affect a rule rather than being stored individually. In his view, positive transfer to novel variants of a learned class of movements emanates from these developed schemata or generalized motor programs. The conceptualization of transfer as a function of associations and response learning is, therefore, replaced in this view by the abstraction and storage of movement-outcome relationships. These relationships may be used successfully to produce novel responses by interpolating from those relationships previously experienced to new response specifications or parameters.

The notion of the schema is not a new one. In fact, Schmidt (1975) has acknowledged that his theory is a synthesis of the work of many people:

> ...(T)he lineage of the major ideas can be traced to Bartlett (1932) in terms of the notion of the schema, to Adams (1971) for his application of closed-loop theory to learning of motor skills, to Pew (1974) for the suggestions about the application of the schema to motor skills, and to Lashley (1917) for his lead in characterizing man as controlling his movements centrally with "motor programs". (p.231).

The schema concept is obviously crucial to Schmidt's theory. Although Bartlett (1932) was not the first to discuss schemata, he modified previous conceptualizations and presented a formal, clear definition of the construct. The following quote, although lengthy, demonstrates the power of Bartlett's formulations. His definition of schema is naturally similar to Schmidt's (1975) current interpretation of the concept, and is perhaps preferrable to another contemporary notion of schemata as "rules" for prototype production (see, e.g., Evans, 1967).

> 'Schema' refers to an active organization of past reactions, or of past experiences, which must always be supposed to be operating in any well-adapted organic response. That is, whenever there is any order or regularity of behaviour, a particular response is possible only because it is related to other similar responses which have been serially organised, yet which operate, not simply as individual members coming one after another, but as a unitary mass. Determination by schemata is

> the most fundamental of all the ways in which we can be influenced by reactions and experiences which occurred some time in the past. All incoming impulses of a certain kind, or mode, go together to build up an active, organized setting: visual, auditory, various types of cutaneous impulses and the like, at a relatively low level; all the experiences connected by a common interest: in sport, in literature, history, art, science, philosophy and so on, on a higher level. There is not the slightest reason, however, to suppose that each set of incoming impulses, each new group of experiences persists as an isolated member of some passive patchwork. They have to be regarded as constituents of living, momentary settings belonging to the organism, or to whatever parts of the organism are concerned in making a response of a given kind, and not as a number of individual events somehow strung together and stored within the organism. (p.201).

Thus, in Bartlett's view, a schema is not so much the prototypic representative of a class, as Evans (1967) would suggest, but a general, responsive rule formed from the synthesis of all examples experienced by an individual. While the distinction is a subtle one, the Bartlett definition implies a greater flexibility for change based on experience. Such adaptability would seem beneficial in the motor domain, where generally the question is not "Does that item constitute an example of this class?", but "How do I execute that example of this movement class?" Identification is not problematic; performance is. Thus, a prototype in memory might prove less useful than an integrative rule governing input-output relationships. Schmidt (1975) has recognized the necessity for something more than a prototype:

> ...The schema notion requires some extension from the original pattern-perception idea...in that in the motor case it is the relationship among the arrays of information that is abstracted rather than the commonalities among the elements of a single array. (p.235).

Specifically, Schmidt has hypothesized that four discrete pieces of information are necessary for schema formation. These are: the initial conditions (i.e. preresponse status of the musculature and environmental conditions); the response specifications (i.e. specific details regarding speed, force, etc. which must be added in some way to the general motor program); the sensory consequences (i.e. all afferent information, both proprioceptive and exteroceptive, resulting from the response); and the movement outcome (i.e. knowledge about the results of the response acquired via KR, if present, and via sources intrinsic to the response itself). Schmidt's diagrammatic representation of the relationships between these four variables is included in Figure 1.1. As can be seen, there are actually two schemata in this model. One, the recall schema, is formed from movement information regarding initial conditions, response specifications and movement outcomes, and is responsible for generating appropriate response specifications. The other, the recognition schema, is formed from movement information including initial conditions, sensory consequences and movement outcomes, and serves to generate expected sensory consequences.

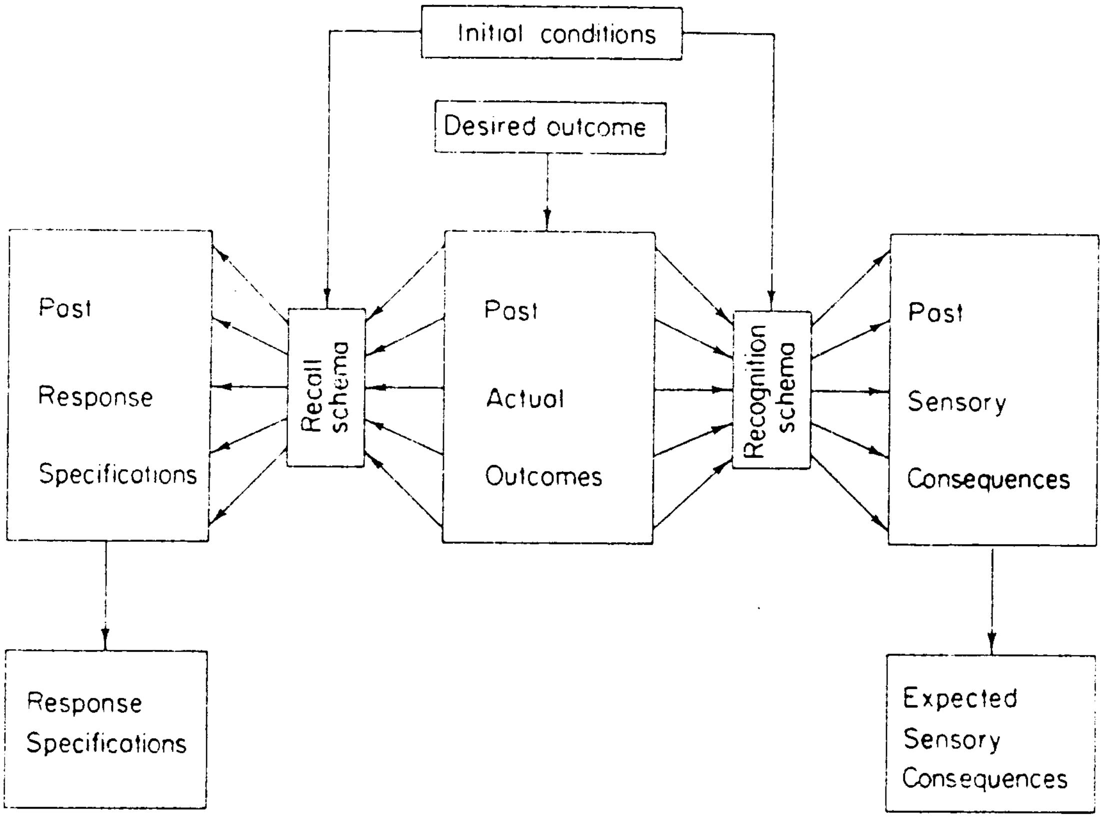


Figure 1.1. Schmidt's Recall and Recognition Schemata.
The schemata are presented in relation to various sources of movement information. (From Schmidt, 1976).

Schmidt has also outlined how the schema operates in producing a motor response. His diagram is reproduced here in Figure 1.2. After the initial conditions and desired outcome are determined, new specifications are identified for the motor program based on previous response specification-outcome relationships. These are used in the executed motor program, and the resultant response leads to several sources of feedback. The feedback in turn is compared to pre-specified sensory consequences, and any errors may be used in a closed loop fashion to correct subsequent and/or ongoing (slow) responses. The relevant information can then also be used to update the recall and recognition schemata.

Schmidt's (1975) theoretical formulations provided the impetus for a great deal of empirical examination, just as Adams' (1971) paper had done earlier. A number of researchers tried to establish evidence for the existence of the two separate memory states (recognition and recall) which Schmidt had suggested were necessary for movement. Williams (1978), for example, compared three variable practice groups on recognition ability for distances which either had, or had not, been experienced during practice on a linear slide apparatus. One of these groups made passive movements during practice, one made constrained movements, and the third made active movements. Thus, all three groups were provided with the elements necessary for recognition schema formation. No differences existed between them on the recognition tasks employed, and Williams concluded

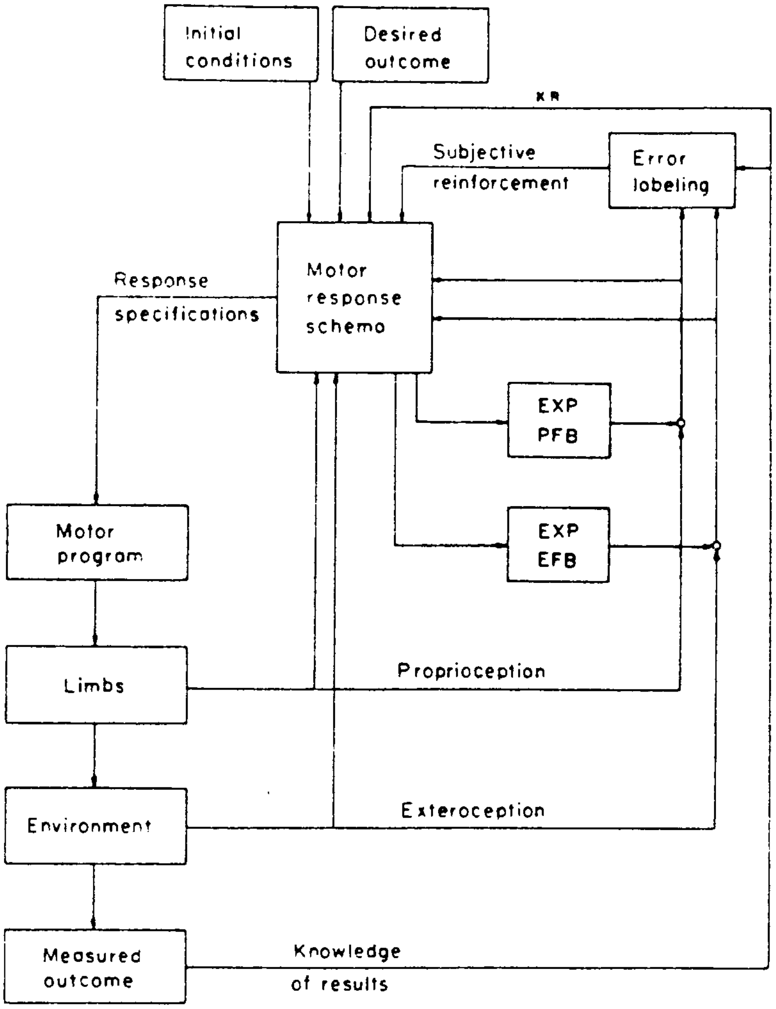


Figure 1.2. Operation of Schmidt's Motor Response Schema.
Recall and recognition schemata are combined for clarity, and are depicted in relation to the events occurring within a trial. (From Schmidt, 1976).

that support for the formation of recognition schemata had been found. Williams went on, in a second experiment, to provide evidence for the formation of recall schemata. Employing the same task, he compared passive and active groups on their ability to reproduce specific practice movements, as well as generate novel ones. The superiority of the active group confirmed Schema Theory predictions regarding recall schemata.

Evidence for recall and recognition schemata was also provided by Wallace and McGhee (1979). They compared active groups with those who only received endpoint experience on a linear positioning task, and found superior performance on a criterion distance by the former group. These results provided evidence for the development of recall schemata by the active group but not by the non-active group, which is compatible with Schema Theory predictions. A passive and an active group were compared in a second experiment. Both demonstrated evidence for recognition schema development, again supporting Schema Theory predictions.

Zelaznik and his colleagues (Newell, 1976; Zelaznik, Shapiro & Newell, 1978; and Zelaznik & Spring, 1976) have produced results which are difficult to reconcile with the idea that recall and recognition are independent memory states. They found that allowing subjects to hear others making rapid linear positioning movements proved beneficial for these subjects' subsequent performance. Furthermore, variable listening was even more effective than constant listening. Such auditory input should have strengthened the recognition schema, but left the recall schema unaffected, since subjects were not actually producing responses. McGhee (1981; cited in Shapiro & Schmidt, 1982) has attempted to explain these contrary results by suggesting that such auditory experience was serving as an information source to subjects, rather than functionally linking the two memory states.

1.2 Focus of the Present Research

While some researchers pursued empirical evidence for the existence of schemata, others turned to an examination of the implications of Schema Theory. In particular, Schema Theory led to the testing of what quite quickly came to be known as the variability of practice hypothesis (Moxley, 1979). In brief, this hypothesis states that the greater the variety of experience with a movement class, the stronger the schema development will be. It is hypothesized that this greater schema strength based on knowledge concerning various manifestations of the input-output relationship is accompanied by a greater ability on the part of the subject to acquire new examples of the movement class. In other words, greater variability of practice leads to greater ability on novel variants of the same movement class.

Numerous researchers have conducted experiments designed to test this hypothesis. A discussion of this literature is included in Chapter Two of this volume.

Research concerned with the variability of practice hypothesis has failed to provide unanimous support for the superiority of variable practice in producing positive transfer to novel examples of a given movement class. Of those research projects involving adult subjects, only five of the eighteen reviewed demonstrated clear support for the variability of practice hypothesis, although by far the majority of the remainder had results in the predicted direction. The research involving children was somewhat more positive for Schema Theory, with fourteen out of twenty studies yielding results which were clearly favourable to the variability of practice hypothesis.

One issue which appears to have received scant attention in the literature is the quantitative analysis of variable practice. Most researchers have been interested in a qualitative analysis. In other words, the question most frequently asked has been "Is the presence of variable practice beneficial to transfer performance?", rather than "Is increasingly variable practice of increasing benefit to performers who are presented with a novel variant of the movement class?" Magill and Reeve (1978), in one of the few studies to address this question, found their low variable practice group to be superior on transfer in a linear positioning task to both a high variable and a constant practice group. Magill and Reeve defined quantity of variability in this study by the range of practice movements made, rather than by the number of different distances moved. The actual number of different variants experienced by the low and high variable groups was equal. Their results may have been affected by the procedural difficulty that subjects practiced under constrained conditions, and then transferred to a free-movement protocol during testing.

Turnbull and Dickinson (1986) eliminated this methodological problem in their study by having subjects practice unconstrained movements, as well as testing them during transfer under these conditions. In addition, they operationally defined quantity of variability based on the number of variants, rather than on the range of practice movements, experienced. Subjects experiencing maximal variability in this study (fifteen different variants) tended to outperform low variable groups (three different variants), as well as constant and control groups. This finding lends some support to the notion that the relative degree of variability may be an important consideration, along with simply examining its presence or absence, when testing variability of practice hypothesis predictions.

The degree of variation provided during practice may be controlled, not only by manipulating task requirements in a single dimension, but by introducing variation in more than one parameter. In all of the research on the variability of practice hypothesis, experimenters have maintained direct control over only one task parameter. (A task parameter is defined here as any externally manipulable characteristic or dimension which the task possesses, and which influences the motor behaviour of the performer. In essence, task parameters may be viewed as the environmental demand characteristics which a task possesses. This is contrasted with the response reaction of the performer confronted with these demand characteristics, who controls movement parameters. At their most elemental level, task parameters may be viewed as stimuli for the performer's movement parameter responses). Thus, numerous studies have used paradigms in which subjects were required to perform movements which varied only in terms of distance, or speed, or force requirements. As Kerr (1982) has observed, however, the fact that only one task parameter is being actively manipulated does not mean that variability inherent in the human motor system is excluded.

> "(T)he natural inconsistency of the human motor system is seen even in repetitions of well practiced movements; the pattern of movement tends to vary from trial to trial." (Kerr, 1982, p.220).

Furthermore, it is generally the case that more than one movement parameter must be actively adjusted to accommodate a single task parameter shift. An example of this may be seen when subjects are asked to throw projectiles at a target from different spatial locations (e.g. Moxley, 1977; and Kerr & Booth, 1977). An alteration in target distance (a task parameter) requires concomitant adjustments in muscular force output, angle of release, etc. (movement parameters). Thus, it is obvious that simple practice variability, as it has been achieved in experimental manipulations, has often involved a somewhat more complex manipulation of movement parameters.

A potentially interesting experimental situation would be one in which the experimenter maintains control over more than one task parameter. Subjects presented with simultaneous, controlled variation of two or more task parameters would be facing a somewhat different learning environment than would individuals in a more traditional variable practice paradigm. It is true, as discussed above, that subjects in traditional experiments often must adjust more than one of their movement parameters to perform variable practice. However, it is rare that they have to do so in response to task demands which are varying on more than one level. Of interest is whether the superiority of variable practice holds even when such mixed variation is implemented. The answer to such a question would of course be valuable, since frequently in ecological settings performers are faced with precisely this type of task variability. One obvious example is Bartlett's tennis player who, as well as being faced with an ever-changing set of initial conditions (e.g. different ball speeds, angles of return, body postion, etc.) also wishes to execute strokes which direct the ball to various locations at various speeds. Clearly, more than one task parameter is changing as a tennis rally progresses, and it seems highly likely that more than one of these task parameters is serving as a functional stimulus for the tennis player. Thus, there is obvious ecological curiousity involved in the desire to examine manipulation of more than one task parameter.

The present experiment attempted to address the issue of how maipulations which influence practice variability affect transfer to a novel variant of a movement class. A novel handle-pulling apparatus was developed which permitted experimental manipulation of handle resistance (and thus force output requirements for a given movement extent) and movement extent requirements. The most popular experimental tasks employed in research on the variability of practice hypothesis appear to have been those requiring positioning movements of the upper limb (e.g. Gerson & Thomas, 1977; Magill & Reeve, 1978; Newell & Shapiro, 1976; and Zelaznik, 1977). While occassionally movement velocity has been treated as the independent variable in these studies (e.g. Newell & Shapiro, 1976), generally movement extent has fulfilled this role (e.g. Gerson & Thomas, 1977; and Magill & Reeve, 1978). In addition to allowing limited comparative analysis between experiments sharing this variable, utilization of movement extent is a likely candidate for research because it is relatively easy to manipulate, allows for ease of measurement, and provides no great difficulties for administration. Because force output is a critical variable for task outcome (Schmidt, 1982a), it also seems a likely candidate for experimental attention. One would be hard-pressed to imagine successful motor performance of any sort which did not involve control of muscular force output.

The quantitative analysis of practice variability was approached in the present experiment in two ways. First, variability level was manipulated in either one or both of the task dimensions (i.e. movement extent or handle resistance) under consideration. Manipulation of two task parameters was operationally defined as being of higher variability than manipulation of only one task parameter.

Second, within each of the three parameter manipulation conditions (i.e. either movement extent, handle resistance or both), variability was manipulated by altering the number of task variants provided to subjects during practice. The provision of only three variants was operationally defined as low variability, while the provision of six or more variants (the actual number possible was dependent upon parameter condition) was operationally defined as high variability.

A question of subsidiary interest was that of the influence of practice structure on transfer performance. Shea and Morgan (1979) have demonstrated that random variable practice leads to better transfer performance than blocked variable practice, and Lee and Magill (1983) have extended this finding by demonstrating that serial variable practice is similar to random variable practice in eliciting this effect. (See Appendix I for a brief discussion of Contextual Interference Theory in the context of transfer of training). These researchers have provided a clear warning that practice schedules must be carefully controlled when testing variability of practice hypothesis predictions. Variability level in the Lee and Magill study was much lower than the high variability conditions in the present experiment. Because of the potential complexity involved in highly variable practice here (namely, variability introduced in two dimensions), the question arose as to whether or not random and serial practice schedules would always be equally successful in eliciting positive transfer. Specifically, it was of interest to discover whether or not subjects experiencing highly variable practice would benefit from externally provided organization during practice (i.e. serial practice).

In summary, then, the present study attempted to quantitatively assess the effectiveness of variable practice for eliciting future positive transfer, both through manipulating the number of task parameters varied, and through altering the number of variants provided during practice. In addition, the effect of practice structure on transfer performance was examined. Predictions were as follows:

Prediction 1:

Provision of pre-transfer practice, regardless of type, will yield improved performance on both an interpolated and an extrapolated transfer task, and thus the control group will be the least adept on transfer of all the experimental groups. This prediction is based on the obvious, but generally tacit, expectation that experience with a movement class facilitates performance on novel variants of that class, and leads to some higher degree of performance than could be expected without such prior experience.

Prediction 2:

Manipulation of two task parameters will lead to better performance (on both an interpolated and an extrapolated transfer task) than will manipulation of only one, since performers will be able to acquire knowledge about response specifications and task outcomes based on both independently varying task parameters, and thus schema formation should be more complete.

Prediction 3:

Groups experiencing practice variability will outperform constant practice groups on both an interpolated and an extrapolated transfer task, as predicted by the variability of practice hypothesis.

Prediction 4:

Groups experiencing high levels of variability during practice will outperform those experiencing low levels of variability on both an interpolated and an extrapolated transfer task. This prediction is again in line with previous research by Turnbull and Dickinson (1986).

Prediction 5:

Random presentation schedules are equally effective in eliciting positive transfer as are serial presentation schedules when practice conditions are not tremendously complex. However, at very high levels of practice variability, a presentation schedule incorporating externally imposed structure (i.e. serial practice) is better. This prediction is based on intuitive appeal, rather than on empirical data, since practice schedules do not appear to have been considered in quite this way before. Specifically, it is predicted that:

(i) Within the groups receiving distance or resistance manipulations (i.e. manipulation of only one task parameter), the random groups and their non-random counterparts at both high and low variability levels will perform equally on transfer. This pattern will be evident for both an interpolated and an extrapolated transfer task. This prediction is based on previous work by Lee and Magill (1983) reviewed in Appendix I.

(ii) Within the groups receiving mixed manipulations (i.e. manipulation of two task parameters), the non-random groups will outperform their random counterparts at both the high and the low variability levels. This pattern will be evident for both an interpolated and an extrapolated transfer task.

CHAPTER 2

LITERATURE REVIEW

The Variability of Practice Hypothesis

As discussed in Chapter One, numerous researchers have been concerned with the theoretical implications of Schema Theory, and in particular the variability of practice hypothesis has drawn a great deal of empirical attention. A review of the literature which has emanated from that experimental attention is included here.

Researchers have employed a variety of motor tasks in tests to determine whether variable or constant practice is superior for eliciting positive transfer to novel variants of the task under consideration. Magill and Reeve (1978), for example, compared three groups of subjects who received much, little or no variability during practice, on their ability to estimate a novel distance on a linear slide apparatus. The group which experienced little practice variability tended to outperform the other two groups on the transfer task, even after knowledge of results was withdrawn. These results provided, at best, limited support for the variability of practice hypothesis, and thus for Schema Theory. One point which should be made, however, is that subjects in the group receiving little variability actually practised the same <u>number</u> of different distances during practice as the high variability group, and it was merely the <u>range</u> of those movements which was altered. Thus, the manner in which variability was defined in this study may have been responsible for the equivocal support found for Schema Theory.

Although designed to test Closed Loop Theory predictions, an experiment by Williams and Rodney (1978) served equally well as a test of Schema Theory predictions. They also employed a linear slide apparatus to compare practice groups on a transfer task. One group received constant practice of the criterion distance; another moved to progressively closer locations surrounding the criterion, ultimately receiving the criterion for the last two trials; the third group practised a random sequence of targets around the criterion; and finally, one group experienced an organized sequence around the criterion. Little difference was found between these groups, or between similar groups who moved in a sagittal rather than a frontal plane. These results were contrary to both Closed Loop and Schema Theory predictions. A major difficulty with the experiments by Williams and Rodney (1978), as well as that by Magill and Reeve (1978), is that subjects practised under constrained conditions, and then transferred to a situation in which they determined their own endpoint. Thus, the transfer task in each case may have been significantly different from the type of task for which schemata were developed during practice. (See Turnbull & Dickinson, 1986, for a more detailed discussion of this issue).

Zelaznik (1977) had subjects perform a linear positioning task under various practice conditions. A low trials constant group, a high trials constant group, a high trials variable group, and a no-practice control group all transferred to the no KR criterion distance for eighteen trials, and then performed eighteen more trials with KR so learning could be observed. While differences were not significant, groups were ordered contrary to Schema Theory predictions. The constant practice groups performed better than the variable group on both immediate transfer (both constant groups), and on subsequent learning trials (high trials constant group only). Zelaznik suggested that this failure to support Schema Theory predictions may have been indicative of the fact that the variable practice was spanning more than one response class. An alternative explanation for these results is that the constant groups, who practised movements more similar to the criterion than the variable group, were evolving generalization gradients which incorporated the criterion, and thus were able to perform in a superior fashion upon transfer (see Dickinson & Hedges, 1986, and Hedges, Dickinson & Modigliani, 1983, for evidence of generalization gradients produced by movement stimuli).

Turnbull and Dickinson (1986) used a linear positioning task to examine the hypothesis that increasing levels of practice variability have increasingly beneficial effects on transfer performance. In other words, they hypothesized that if some practice variability is good, then more will be better. They found tentative support for this idea. While differences between specific groups were not significant, a maximally variable group (i.e. one which was allowed no repetitions) tended to perform better than low variable and constant practice groups. This difference was apparent on an immediate transfer test, and was even more accentuated after a one week retention interval.

Johnson and McCabe (1982) employed a ball bushing task to compare various constant groups with a variable group and a no-practice control group. Although their data tended to be in the direction prescribed by the variability of practice hypothesis, differences were not significant. Margolis and Christina (1981) did provide statistical support for Schema Theory predictions. They used a rapid aiming task to show that variable practice groups outperformed non-variable ones when transferred to a novel variant. Likewise, McCracken and Stelmach (1977) demonstrated superiority of a variable practice group over a constant and a control group on a barrier-knocking task.

Frohlich and Elliott (1984) developed a unique computer-controlled game which involved synchronous knob turning to cause a cursor on a video monitor to move down a trackway. They compared a control group, a variable practice group, a free practice group and a constant practice group on transfer performance of (1) the constant group's pathway, and (2) a novel variant. The variable and constant groups were approximately equivalent on the trackway which the constant group had experienced during practice, and the variable group was superior on the novel variant. These results were consistent with the variability of practice hypothesis.

Various sorts of timing tasks have also been employed to test the variability of practice hypothesis. Newell and Shapiro (1976) conducted an experiment on a rapid linear timing task. They had subjects practice movement times of either 70 msec, 130 msec, or both, and then transfer to either 100 or 180 msec. Results indicated that the variable group was superior, although only on the extrapolated task and only if practice order involved the 70 msec condition followed by the 130 msec condition. A second experiment reported in the same paper, which involved more extensive variability of practice, yielded results which were in the appropriate direction to support Schema Theory predictions, but were not significant.

Wrisberg and Ragsdale (1979) examined the variability of practice hypothesis using a coincident timing task. They had subjects either observe a series of runway lights, or respond to those lights with their non-preferred hand. Within each of these groups, runway speed was either variable or maintained at a constant velocity. Subjects then transferred to a novel interpolated speed condition in which they responded with their preferred hand. Wrisberg and Ragsdale found superiority of the variable group, but only if they had actually made responses during practice. Catalano and Kleiner (1984) extended the work of Wrisberg and Ragsdale by demonstrating that variable practice on a coincident timing task led to superior performance over constant practice on a novel variant, even when that novel variant lay outside, rather than within, practice boundaries.

Cummings and Caprarola (1986) attempted to identify a schema range for a rapid linear positioning task in which movement extent was maintained at a constant value and subjects were asked to produce this movement at different velocities. They measured subjects' rates of learning of these different movement velocities, and then identified the range of velocities with learning times which did not differ significantly from one another. This range of velocities was considered to represent the schema range. They then compared various constant and variable practice groups which practised movement velocities within this previously identified schema range. Cummings and Caprarola found no significant differences between groups. The use of rate of learning is obviously only an indirect method of inferring schema range. It is unclear why slower movement velocities, which necessarily involve longer movement times, should require the formation of a schema separate from that for the faster movement velocities. It seems more likely that the absolute error scores which Cummings and Caprarola used to assess rate of learning were, at least partially, a function of the overall movement time. If such was the case, then the schema range which these researchers identified may have been an artifact resulting from their method of determining that range. Regardless of the correctness of their identified schema range, Cummings and Caprarola were unable to provide support for the variability of practice hypothesis.

In their extensive survey in 1982 of the literature pertaining to Schema Theory, Shapiro and Schmidt included numerous theses and other unpublished documents. Of those which reported on tests of the variability of practice hypothesis using adult subjects, four indicated no significant differences between variable and constant practice groups, while two demonstrated partial support for Schema Theory predictions.

The evidence just cited has been less than conclusive in its support of the variability of practice hypothesis, and thus, of Schema Theory. However, all of these experiments employed adults as subjects. It has been suggested that it is easier to find evidence for schema development in children because they are less likely than adults to have a relevant schema already developed (Shapiro & Schmidt, 1982). A small number of trials performed by an adult in a laboratory setting is unlikely to produce much change in a schema which has evolved over a relatively longer lifetime of movement. Children, younger and less experienced, are perhaps more frequently operating with a less well-defined schema, if indeed they have a relevant formulation at all. Thus, a large number of researchers have chosen to use children as their subjects in tests of Schema Theory predictions.

Gerson and Thomas (1977) tested 5 to 6 year old female children on a curvilinear positioning task, and found that those who experienced three practice locations were better on a transfer task than were those who experienced only two locations. However, it should be noted that these results were potentially confounded by a practice effect. Kelso and Norman (1978) found evidence for the effectiveness of variability of practice for children using a ball bushing task. Male and female children ranging from 2 years 11 months to 4 years of age served as subjects in this experiment. Children receiving variable practice outperformed constant and control groups on both an interpolated and an extrapolated transfer task.

Robert Kerr and Bernard Booth have conducted a number of studies which demonstrated the superiority of variable over constant practice for children learning a simple throwing task (Kerr, 1977; Kerr & Booth, 1977; 1978). Seven year old male and female subjects who practised two throwing distances demonstrated superior ability on transfer over the constant group which actually practised the criterion distance in Kerr's (1977) study. Blindfolded subjects in this study threw bean bags at two and four foot targets (variable practice), or at a three foot target (constant practice). All subjects were then tested on their proficiency on the three foot target. Kerr and Booth obtained basically the same results in 1977 using 7 and 9 year old children, and again in 1978 using 8 and 12 year old subjects, on a similar throwing task. Kerr (1977) also tested the variability of practice hypothesis using a stylus-and-paper location task. He asked blindfolded seven year old children to strike specified locations on a gridded piece of paper using a dart in place of a pen. Differences between variable and constant groups on this task were as predicted by Schema Theory, but were non-significant.

Moore, Reeve and Pissanos (1981) also employed a throwing task to examine the effects of variable practice on children. They found no significant difference, during separate tests for accuracy and maximum distance, between kindergarten children who had freely experimented with five different projectiles, and those who had received direct throwing instruction on one projectile. The provision of instruction for the constant group may have served to outweigh the benefits of variability in this case.

Moxley (1979) had 6 to 8 year old boys and girls throw shuttlecocks at a target on the floor. Subjects practised from either one or four locations and then transferred to a novel position. The variable group proved superior on transfer, providing empirical support for the variability of practice hypothesis. Carson and Wiegand (1979) also employed a throwing task to compare a variable, a constant, and a criterion practice group with a control group. Three to 5 year old boys and girls served as subjects in this task where variability was introduced via altering the weights of the bean bags being used as projectiles. The variable and criterion practice groups were significantly better than the others on a variety of immediate posttests, including throws with the criterion group's bean bag weight, throws with a novel weight, and throws with a novel projectile at a wall target instead of the practised floor target. However, only the variable group maintained their performance level over a two week retention interval, which lends support to the variability of practice hypothesis.

In one of the few studies involving children which failed to support Schema Theory predictions, Wrisberg and Mead (1981) tested 6 to 7 year old male and female subjects on transfer performance using an anticipatory coincident timing task. A constant group which experienced one practice speed was significantly better than a no-practice control group, while a variable group which was provided with four practice speeds was not significantly different from either of the other two groups. These results held for both an interpolated and an extrapolated transfer task.

A second study using children as subjects which did not support Schema Theory predictions was conducted by Pease and Rupnow (1983). They used a linear slide apparatus with an adjustable brake shoe to provide 9 to 11 year old male and female subjects with a movement task requiring variable force production. Subjects were required to make a 15 inch movement in 500 msec ± 50 msec. The variable group practised two different forces, the constant group practised one or the other, and the control group had no pre-transfer practice. The constant group tended to perform the interpolated transfer task better than the variable group, although the difference was not significant.

Miller and Krantz (1981) tested males and females between the ages of 29 and 63 months on a battery of fine and gross motor tasks. Each fine motor task was paired with an analogous gross motor task involving the same cognitive demands but different muscular activity. For example, fine motor rapid pickup involved two-handed pickup of 30 marbles which were placed into a cup, while its gross motor equivalent required 30 tennis balls to be picked up and put into a barrel. Miller and Krantz found no significant correlation for performance on fine motor tasks or for performance on gross motor tasks. However, significant correlations were evident between members of six out of the ten task pairs, supporting the hypothesis that a common underlying schema was responsible for performance on each member of a pair.

Evidence for schema development in educable mentally retarded (EMR) boys has been provided by Poretta (1982). He matched 10 year old EMR boys with nonretarded boys on either mental age (MA) or chronological age (CA), and then provided them with either variable, constant or no practice. Subjects practised a ball kicking task over four different terrains (variable practice), over one terrain (constant practice), or were engaged in an unrelated throwing task (control group). CA matched normals outperformed both other groups on transfer to a novel terrain, while the EMR boys and their MA equivalents did not differ significantly from one another. These results were not unexpected, based on the relative MA's of these three groups. In addition, the variable practice group within each classification of children performed in a superior manner to the constant and the control groups, thus supporting the variability of practice hypothesis. Dummer (1978) failed to find evidence for schema formation in TMR (trainable mentally retarded) children on a linear ballistic (ball bushing) task. However, since she failed to include CA or MA matched children in her study, it is unknown whether the task was simply too difficult for children of the tested mental age to learn.

Kerr (1982) examined practice variability in a somewhat unconventional light. He made a distinction between variability intentionally introduced via manipulations by the experimenter, and variability inherent in the motor system which leads to differences between "repetitions". Kerr hypothesized that, while experimental variability may be beneficial to schema development and deployment, inherent variability could act as a source of interference during the establishment or application of a schema, particularly during early learning. He had 12 to 14 year old subjects make positioning movements which were structured so that inherent variability could be eliminated, limited or left untampered with, and compared a variable with a constant practice group. The variable group performed equally on transfer Task A with the constant group, which had practiced this criterion transfer task. This finding provided support for Schema Theory, since the variable group was demonstrating facility with a variant it had never before encountered. Groups were also equivalent on accuracy for transfer Task B (an extrapolated task), which was not predicted by the variability of practice hypothesis. However, subjects within each group who had been prevented from experiencing inherent practice variability demonstrated less variable error than did other subjects. Kerr interpreted this finding as support for his original hypothesis that inherent practice variability can act to interfere with schema development.

Of the unpublished materials reviewed by Shapiro and Schmidt (1982) which employed children as subjects, all five provided results in the pattern predicted by the variability of practice hypothesis, and four of these yielded significant differences between practice groups.

In brief, then, it may be concluded from this review that while frequently experimental results from tests of the variability of practice hypothesis have been in the predicted pattern, significant differences between variable and constant practice groups have been somewhat more rare. Results from studies employing children as subjects have more dramatically favoured the variability of practice hypothesis than have those involving adults, proabably due to the relative inexperience of child motor performers with respect to their adult counterparts.

CHAPTER 3

METHODS

3.1 Subjects

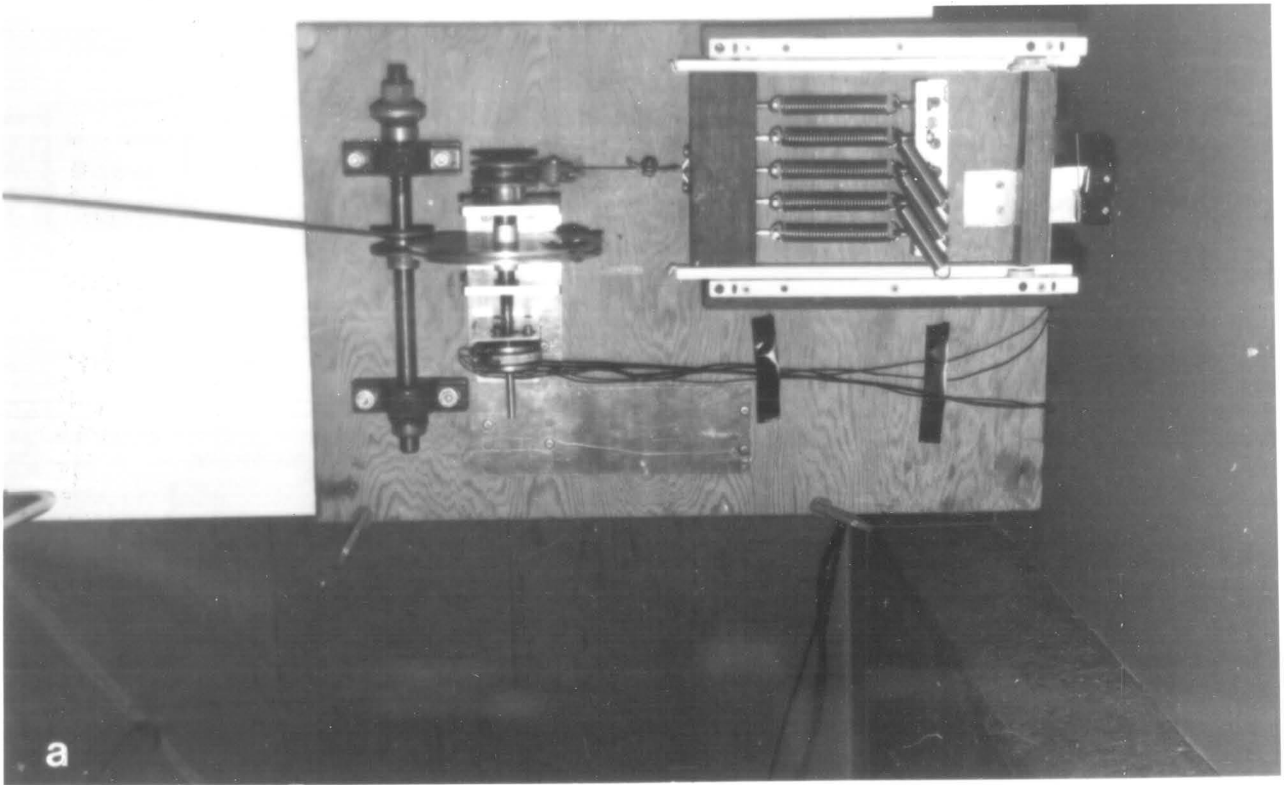
Subjects were 192 graduate and undergraduate volunteers from the student population at Simon Fraser University. In an attempt to ensure that all subjects felt motivated to perform well on the task, a ten dollar prize was offered to that subject in each of the 16 experimental groups who performed with the least amount of error on the transfer trials (as measured by their constant error on those trials).

3.2 Apparatus

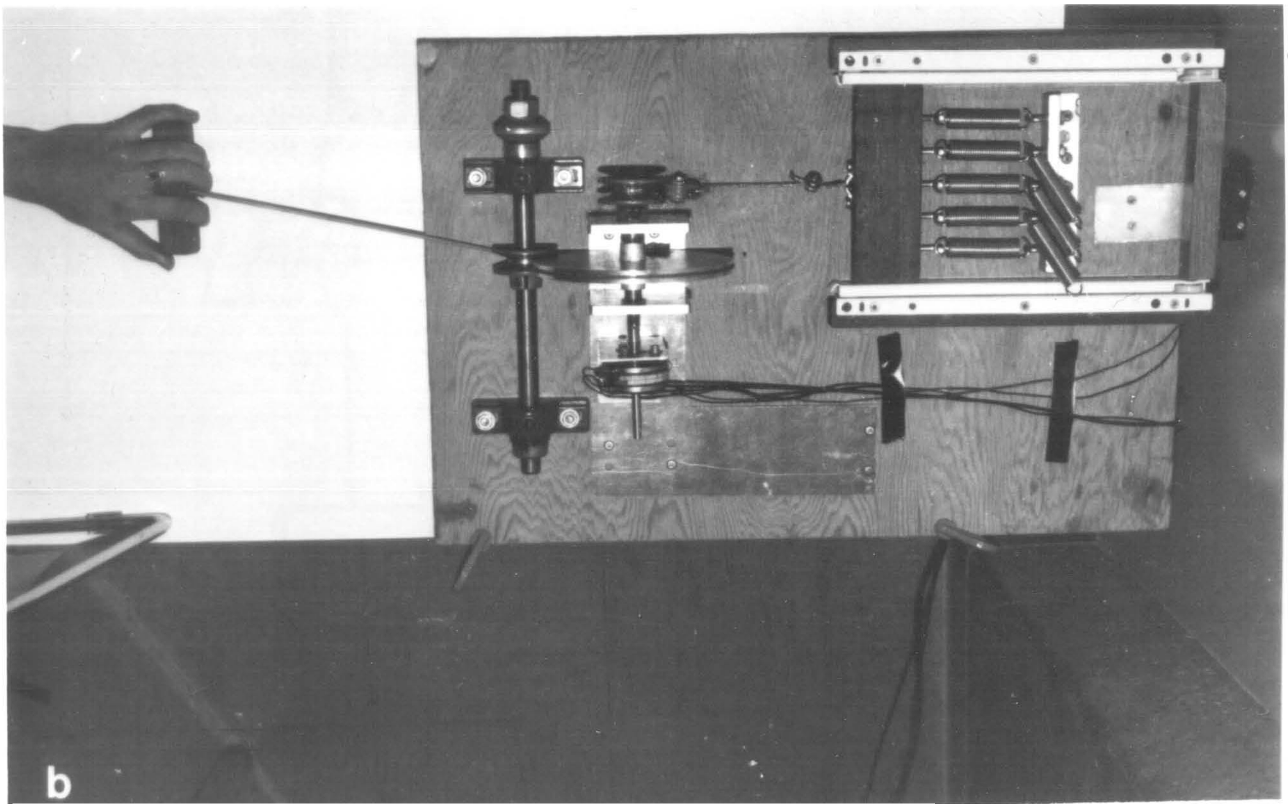
Subjects performed a handle-pulling task on a novel apparatus. The handle was attached to a length of 2000 pound-test chain saw cord which fed through a pulley and attached to a metal wheel. This wheel was soldered to a smaller, pulley-type wheel, which in turn was connected via metal cable to a wooden framework containing a system of springs. Any number of springs from one through eight could be engaged, in parallel, to alter the resistance against which subjects pulled. Initial displacement of the handle opened a microswitch located at the back of the wooden framework containing the springs. Opening of this microswitch triggered an Apple-type microcomputer to begin a one second sampling period. Displacement of the handle also caused simultaneous displacement (i.e. rotation) of the metal wheel to which the chain saw cord was attached. In addition to being soldered to the pulley wheel, the main wheel was linked to a five thousand ohm potentiometer (Precision Potentiometer; Helipot, Division of Beckman Istruments, Inc., Toronto, Ont.) located adjacent to the wheel frame. This arrangement of the apparatus permitted the transduction of length changes produced by subjects in the form of handle displacement, to electrical changes in the potentiometer in the form of voltage changes. A five volt power pack supplied the system, and both the microswitch and the potentiometer were linked to the microcomputer via a 12 bit analogue-to-digital (A-D) converter (AI13, Interactive Structures, Inc., Bala Cynwyd, PA). Software programming specified a sampling rate of 250 hertz. The entire system is displayed in Figures 3.1(a), 3.1(b) and 3.2.

When a subject pulled the handle, the main wheel to which it was directly attached was rotated. When this wheel was rotated, two events occurred. First, the spring framework was pulled forward a proportionally shorter distance than the actual handle pull, opening the microswitch and triggering the initiation of the one second sampling period. Second, an alteration (increase) in the voltage level of the potentiometer resulted. The voltage level increased as a direct function of the distance the wheel was rotated, or in other words as a direct function of the distance the handle was pulled. The starting position of the wheel coincided with a 0 volt potentiometer reading, while maximum displacement of the wheel (i.e. maximum handle

Figure 3.1. The Handle-Pulling Apparatus.
(a) The apparatus is displayed during a handle pull, with the microswitch in the open position.
(b) The apparatus is displayed with the microswitch in the closed position (i.e. before the subject has initiated a handle pull).



a



b

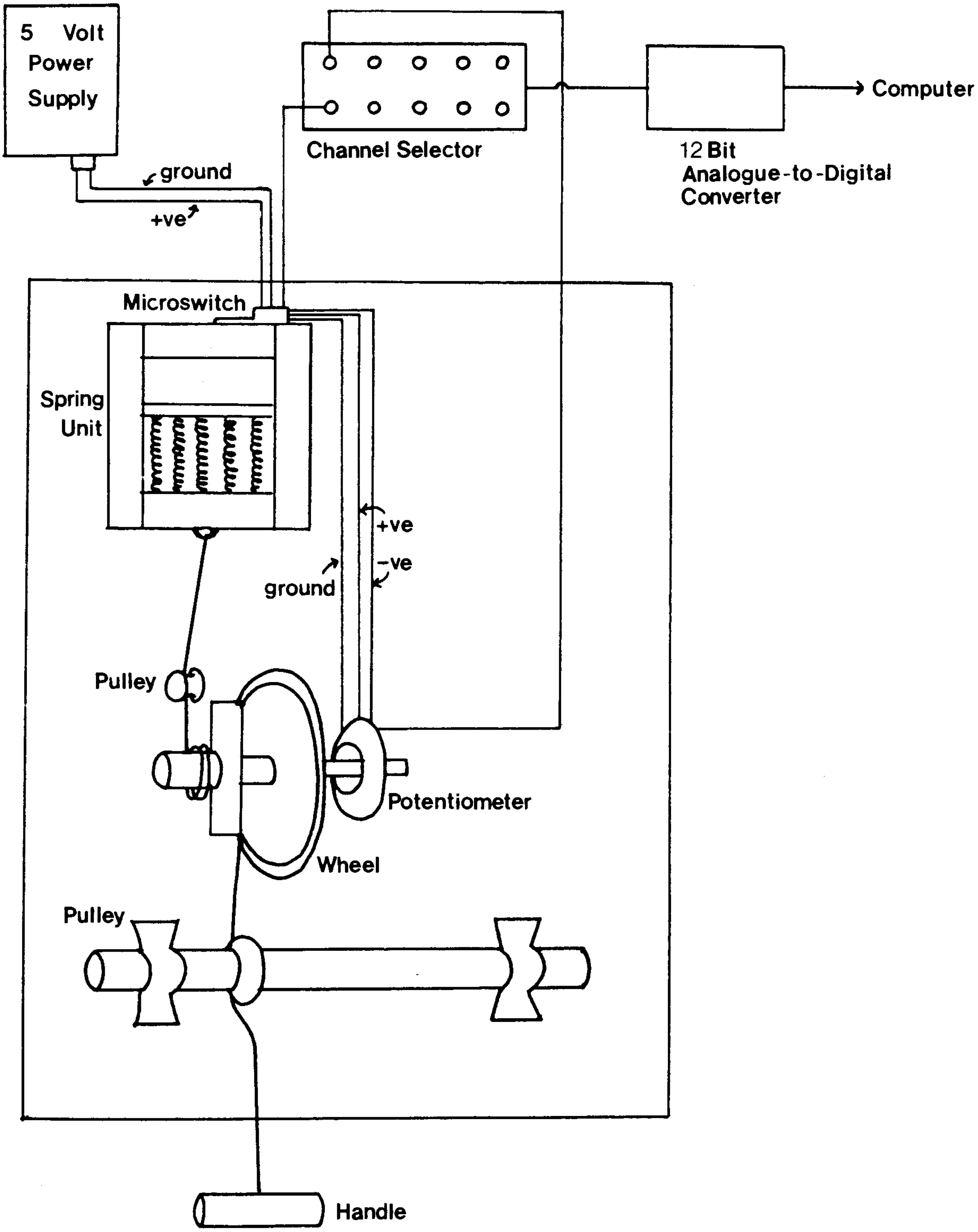


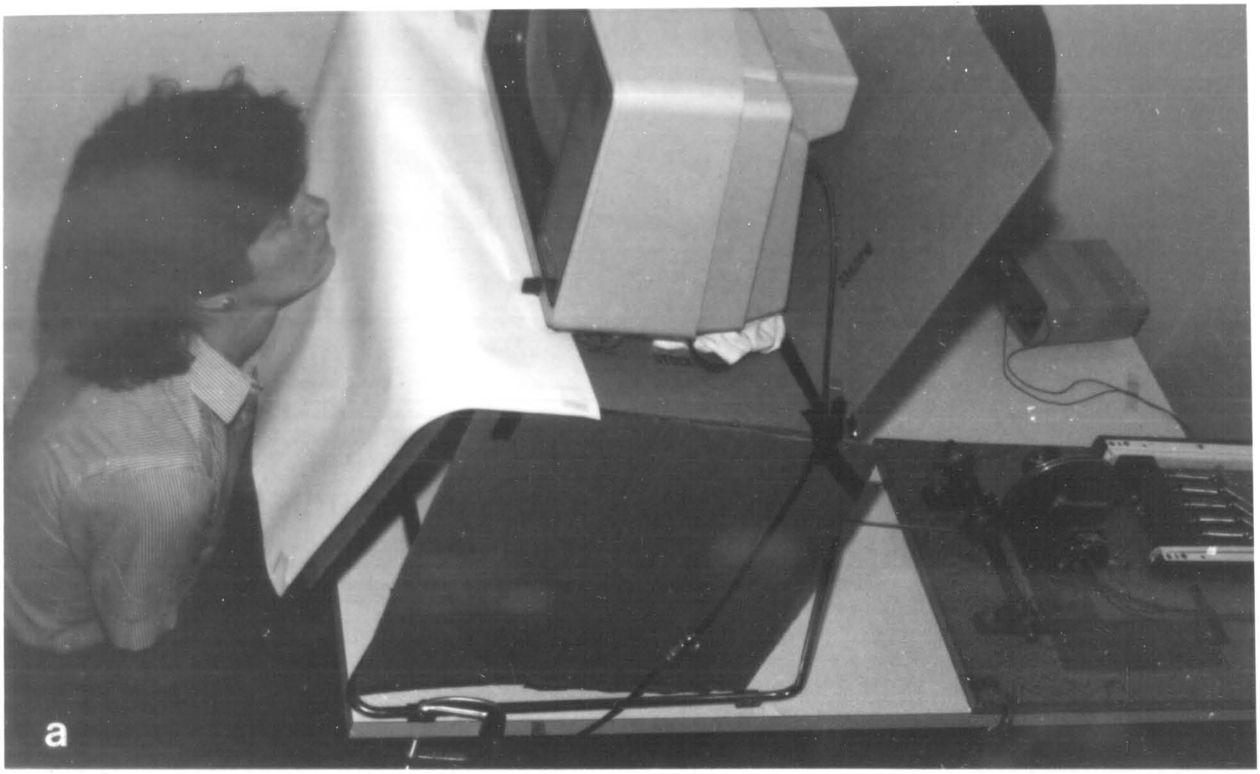
Figure 3.2. A Schematic of the Apparatus.

displacement by the subject) resulted in a 4.96 volt potentiometer signal. This 0 to 4.96 volt range was transduced by the A-D converter to a range of 0 to 4096 units. The A-D converter reading was then used by the microcomputer in calculating a parabolic trajectory for a simulated ball on a video monitor. (See Appendix II). Ball speed was constant across all trials. Maximum handle displacement by the subject resulted in maximum trajectory for this ball. Thus, the further the subject pulled the handle, the further the ball moved on the screen.

The video monitor was raised 46.5 centimeters above the surface of the table on which the apparatus was positioned. A skirting surrounded the base of the monitor, and the handle and apparatus were situated behind this skirting out of view of the subjects. Thus, subjects reached under the skirting to grasp the handle, losing view of their arm in the process. Subjects were further discouraged from watching their movements by the fact that they had to keep their head tilted up in order to keep the monitor in view. [See Figures 3.3(a) and 3.3(b)].

The specific target which was relevant for any given trial was visible on the screen before and during that trial. A target was simply defined by a horizontal line 0.5 cm in length. The starting position of the simulated ball (0.5 cm in diameter), which remained constant for all subjects during both practice and testing, was in the lower lefthand corner of the screen. Targets were located at various distances to the right of this

Figure 3.3. (a) and (b) Subject in Relation to the Apparatus.


a


b

ball in the same horizontal plane. The ten possible practice targets required pulls of 5.80 cm, 7.87 cm, 9.94 cm, 12.01 cm, 14.08 cm, 20.97 cm, 23.04 cm, 25.11 cm, 27.18 cm and 29.25 cm. The two test targets required pulls of 17.53 cm and 32.70 cm.

As mentioned previously, the number of springs engaged during any given trial could be altered. While manipulation of the springs in the system affected the force with which a subject was required to pull the handle in order to displace it to any given distance, changing the number of springs did not directly affect the simulated flight of the ball. Each additional spring required approximately 49 Newtons of additional force for the handle to be maximally extended.

3.3 Procedure

There were 16 experimental groups, including one no-practice control group. Of the 15 practice groups, five received manipulations of target distance, five received manipulations of the resistance against which subjects pulled, and five received manipulations of both target distance and handle resistance. A parallel set of five practice groups was repeated within each of the three parameter conditions. These five groups were: random, highly variable practice; non-random, highly variable practice; random, low variable practice; non-random, low variable practice; and constant practice.

Each of the subjects in the 15 practice groups received a total of 20 practice trials. The object of all trials, both practice and test, was to pull the handle in such a manner so as to cause the simulated ball to land on the centre of the presented target. A direct hit resulted in a score of zero; if the ball fell short of the target the score was negative, and if the subject overshot the target then the score was positive. The actual magnitude of the score was dependent upon the distance the ball landed away from the target, with larger negative or positive scores indicating a greater distance away from the target. Scoring was designed so that a range of 201 units (including zero) was available. The location of zero within this range (and thus the relative sizes of the positive and negative scoring ranges) was dependent upon which target was currently on display. After subjects had completed pulling the handle during practice trials (i.e. after the trial was over), they were shown the flight path of the ball, its landing point, and their numerical score for that trial.

There were ten possible practice targets and six different possible practice spring conditions, or resistances. The actual selection of these which a given subject encountered was dependent upon which group he or she was in. All targets were separated by at least one just-noticable-difference, or JND (determined during preliminary testing---see Appendix III). The spring conditions were also all identifiably different from one another. There were an additional two targets and two spring conditions which were considered test items. One test target was located in the centre of the range of practice targets, while the other was located beyond all of the practice targets. Both were separated by at least two JND's from all practice targets. The shorter, centrally located test target was always associated, during testing, with the test resistance condition which fell in the middle of the range of practice resistances. The second test target was always presented, during testing, with the test resistance which was lighter than any of the resistances used during practice.

While a test target was only presented with a test resistance during the actual test trials, the central, or interpolated, test target was experienced during practice by subjects in the Resistance groups, and the interpolated test resistance was experienced during practice by subjects in the Distance groups. (See the description of groups below). However, in each case the test parameter was paired with a practice example of the other parameter, and thus the overall interpolated test target was a novel transfer task. The test target and test resistance which lay beyond practice boundaries (i.e. the extrapolated conditions) were never encountered by any subjects during practice. Thus, there were two test tasks: Task 1 involved task demands within the boundaries of practice but never before precisely experienced by subjects (although, as mentioned, subjects in the Distance and Resistance conditions did experience one <u>or</u> the other of the test task parameters during practice), and Task 2 involved task demands which were entirely outside the range of conditions previously experienced during practice. In Schmidt's (1975) terms, Task 1 would require interpolation from an existing schema, while Task 2 would require extrapolation.

All subjects, regardless of group, completed five trials on Task 1 and five trials on Task 2 during testing. Half of the subjects within each group attempted Task 1 first (Order 1), while the other half were presented with Task 2 first (Order 2). KR was withdrawn during testing. Thus, while the initial visual information presented on the screen to subjects at the beginning of each trial paralleled conditions during practice (i.e. the ball was visible in the lower lefthand corner and the target was situated to the right of this ball in the same horizontal plane), the ball now disappeared from the screen after the handle was pulled and neither its trajectory nor its landing point were made available to the subject. Furthermore, the subject was no longer appraised of his or her numerical score during the test trials. This lack of KR was the only procedural change subjects encountered upon transferring from the practice to the test trials.

When a subject entered the laboratory he or she was seated in front of the apparatus in the position to be occupied during testing. The experimenter then read to the subject the information and instructions contained in Appendix IV. When the subject indicated an understanding of the instructions he or she was requested to sign an informed consent form. Then the subject grasped the handle and, when ready, initiated trial number one. The subject then proceded with the practice schedule appropriate for his or her group. Upon completion of the practice trials, the subject was informed that the test trials would commence, and told to begin when ready. After testing was completed the subject was shown all of his or her scores for both practice and test trials, and the session was terminated.

Because some software alterations were necessary for the three different types of parameter manipulations (i.e. Distance, Resistance and Mixed), all of the groups in the Distance conditions, plus the control group, were run first, followed by all of the groups in the Resistance conditions, and finally by all of the groups receiving mixed manipulations. Within any one of these three experimental subsections, subjects were sequentially assigned to one of the five appropriate groups (i.e. the first subject was put into group 1, the second into group 2, and so forth).

A description of the 16 experimental groups follows:

DISTANCE GROUPS: The resistance for all of the distance groups was permanently set at five springs during practice, which was equal to the test resistance for transfer Task 1. Thus, only the target distance was manipulated during practice.

(1) High variable, random (DHR) - Subjects were presented with all ten of the practice targets in a random order. Each target was presented twice, for a total of twenty trials.

(2) High variable, non-random (DHN) - Subjects in this group also received all ten of the practice targets two times each. However, half of the subjects were presented with the targets in

sequence from nearest to furthest (i.e. ascending order), while the other half experienced the sequence from furthest to nearest (i.e. descending order). (See Note 1). The sequence was repeated twice in either case, for a total of twenty practice trials per subject in this group.

(3) Low variable, random (DLR) - Five practice targets were short of the test target for Task 1, while five were beyond this test target. Subjects in this group experienced three randomly selected targets, with the provision that one target was from the short group of practice targets, one was from the long, and the third was randomly selected from either side. Since the Task 1 test target was located between the long and short groups of practice targets, it was definitely within the range of practice targets experienced by this group. The three practice targets were presented in a random order such that two occurred seven times each, and the third occurred six times.

(4) Low variable, non-random (DLN) - Targets were selected in the same fashion for this group as for the group described previously (DLR). However, targets were presented sequentially in an ascending order for half of the subjects in this group, and sequentially in a descending order for the other half of the group. This order was repeated until the subject had completed twenty practice trials.

(5) Constant (DC) - Each subject experienced any one of the possible practice targets a total of twenty times. The group was balanced such that at least one subject did each target, and of the two remaining subjects, one did a target from the first half of the practice range, while the other received a target selected from the second half of the range.

RESISTANCE GROUPS: All subjects in the resistance conditions were always presented with the target distance coinciding with that of transfer Task 1. Thus, only the resistance was manipulated during practice. Alterations in handle resistance were accomplished by changing the number of springs connected to the apparatus framework during any given trial.

(1) High variable, random (RHR) - Subjects received all six of the possible resistance conditions, four three times and two four times. Presentation order was randomized.

(2) High variable, non-random (RHN) - The same resistances were presented to this group as to the previous group (RHR). However, half of the group received an ascending sequential order, while the other half received a descending sequential order. This order was repeated until the subject had completed twenty practice trials.

(3) Low variable, random (RLR) - Three practice spring conditions provided lighter resistance than that in transfer

Task 1, while three practice spring conditions provided heavier resistance. Subjects in this group received three randomly chosen resistances, one from the lighter range and one from the heavier, plus a third resistance randomly chosen from either range. They were presented with these resistance conditions in a random order such that two occurred seven times each, and the third occurred six times.

(4) Low variable, non-random (RLN) - Resistances were selected in the same fashion for this group as for the group described previously (RLR). However, half of the subjects received a repetitive ascending sequential order, while the other half received a repetitive descending sequential order.

(5) Constant (RC) - Each subject experienced any one of the possible practice resistances a total of twenty times. The group was balanced such that two subjects practised each of the six resistances.

MIXED GROUPS: Subjects in these practice conditions were presented with resistances and target distances which differed from those found in either of the transfer tasks. Both resistance and distance were manipulated during practice.

(1) High variable, random (MHR) - All possible target distances and handle resistances were presented to subjects in this group. Targets and resistances were randomly paired and randomly ordered, with the proviso that all targets were used twice, and four of the resistances were used three times each, while the remaining two were used four times each.

(2) High variable, non-random (MHN) - All possible targets and resistances were presented to this group, as in the group above (MHR). However, half of the subjects in this group received systemmatic pairings of resistances and targets such that the largest resistance was paired with the shortest target and the smallest resistance was paired with the longest target, and there was a gradual transition of distance-resistance pairings within the two extremes. Four of the resistances were used twice (i.e. with two of the targets), while the remaining two were used only once (i.e. with one of the targets) during each of the two repetitions of the sequence of ten targets. The other half of the subjects received the reverse pairings (i.e. the largest resistance was associated with the longest target, the smallest resistance was presented with the shortest target, etc.). Half of the subjects in each of these divisions (i.e. a quarter of all subjects in the group) received a sequential ascending presentation order, while the other half received a sequential descending presentation order.

(3) Low variable, random (MLR) - Subjects in this group received three randomly chosen targets selected on the same basis as those for the DLR group, randomly paired with three

resistances chosen in an identical fashion to those selected for the RLR group. Two of the targets appeared seven times, while one was presented six times. Likewise, two of the resistances were used seven times, while the remaining one appeared six times.

(4) Low variable non-random (MLN) - Resistances and targets were selected as for the group MLR above. For this group, however, pairings were made on the same basis as for the group MHN. In other words, half of the subjects received pairings where high resistance accompanied the longest of the targets, and low resistance was paired with the shortest of the three targets being employed in any given case. For the other half of the subjects the reverse matching (i.e. high resistance with short target, etc.) was employed. Half of the subjects in each format received a sequential ascending presentation order, and the other half received a sequential descending presentation order (as in group MHN, above).

(5) Constant (MC) - Each subject received one of the practice targets with a randomly matched resistance for a total of twenty trials. At least one and not more than two subjects experienced each of the possible practice targets (as in group DC, above), and each of the six possible resistances was presented to two subjects.

CONTROL GROUP: Subjects in this group received no practice of any kind, but were immediately presented with the test trials.

CHAPTER 4

RESULTS

Of the five predictions to be tested in this experiment, three were appropriate for analysis by means of regression analysis. A number of authors (e.g. Cohen, 1968; and Pedhazur, 1982) have outlined the benefits of regression analysis over the more commonly used analysis of variance (ANOVA). A major benefit of regression is that this method permits a greater amount of experimental error to be accounted for (over, e.g. ANOVA or preplanned contrasts) by considering the over-riding structure of the practice variables imposed by the experimental design rather than treating each group as a totally individual entity. Groups in this experiment were distinguished from one another in a number of ways, but at the same time they were not totally unrelated to each other. As previously described, groups were organized into three main divisions based on the task parameter which was manipulated (i.e. Distance, Resistance or Mixed). Within each of these three conditions, groups were differentiated based on the variability level of the practice they received (i.e. high, low or constant). In addition, subjects in the high or low variability conditions were further separated into groups which experienced either random or non-random (sequential) presentation conditions, while subjects in the constant groups were intrinsically limited to a non-random (blocked) presentation schedule. Finally, half of the subjects in each group were presented with one of the two possible transfer task orders, while the other half received the second presentation order. To summarize, then, the experimental variables which were manipulated included parameter, variability, randomness and order. The three predictions tested by the use of regression analysis were Predictions 2, 4 and 5.

While regression analysis was the best method for eliminating superfluous variability in making pre-planned comparisons, only a subset of the sixteen experimental groups could be examined using the regression model chosen here. The control group obviously does not lend itself to the group structure delineated above, since the independent variables which distinguish the groups are almost all practice variables, and the control group did not receive any practice. Thus, the no-practice control subjects were excluded from the regression analyses. The constant practice groups were also problematic for this model. These groups represented the only variability level which was not further delineated based on randomness. It is logically impossible that a constant practice condition be anything other than non-random, blocked in design. The high and low variability conditions were structured such that each was sub-divided into both random and non-random, sequential conditions. Thus, to include the constant practice groups in the analysis would have altered the symmetry of the nested design and inappropriately influenced the randomness/non-randomness dichotomy. In addition, the possibility existed that the distribution of scores for a non-random division which included the constant groups would be skewed, and this would violate an assumption of regression. Therefore, the constant groups were also excluded from consideration in the regression analyses, and were dealt with separately. This meant that it was necessary to test Predictions 1 and 3 by using *t* ratios. Dunn's multiple comparison procedure, also known as Bonferroni *t*, was chosen in order to avoid exceeding the experiment-wise error rate (specified as alpha=.05 for all predictions).

Every subject had five attempts at each of the two transfer tasks, for a total of ten test trials per subject. Constant error, or CE, and variable error, or VE, (where $CE=\Sigma X/n$, $VE=[\Sigma(X-\bar{X})^2/n]^{-\quad 2}$, X=score on any given trial, and $\bar{X}$=mean score for subject over n trials) were calculated for both tasks using only the last four trials (i.e. with the first trial of each testing sequence excluded).

Because subjects were unaware of the resistance they would be facing until they actually initiated a handle pull, it was felt that the first trial of each transfer task sequence was probably serving an exploratory function, and that the final four trials of the sequence probably better reflected the subjects' abilities to execute the task at hand. However, since the first trial in a transfer task paradigm may be the best time to observe the effects of any previous training, the first trials of each task were also analyzed separately. The analyzed scores are designated in all subsequent figures, tables and discussion as follows: T1(CE1) is first trial performance on transfer Task 1 (the interpolated task); T1(CE4) is CE for the last four trials of Task 1; T1(VE4) is VE for the last four trials of Task 1; T2(CE1) is first trial performance on transfer Task 2 (the extrapolated task); T2(CE4) is CE for the last four trials of Task 2; and T2(VE4) is VE for the last four trials of Task 2. Each prediction was tested by analyzing all of these scores. The mean scores for Task 1 are included in Table 4.1, and those for Task 2 are displayed in Table 4.2.

4.1 Analysis of the Experimental Predictions

4.1.1 Analysis of Prediction 1

In accordance with Kirk (1968), Dunn's procedure was followed for testing this prediction, as well as Prediction 3, without first performing an overall test of significance for differences between groups. Dunn's procedure indicated that the control group was significantly different from all other groups on T1(CE4), T1(VE4) and T2(VE4). (See Table 4.3). Thus, it may be concluded that, as predicted, the control group performed more poorly on both transfer tasks than did the practice groups. Practice led to significantly more accurate performance on trials two through five on Task 1, and to significantly less variability on both Task 1 and Task 2 during the last four trials.

GROUP	CE1	CE4	VE4
RHR	-19.50	-6.40	8.03
RHN	3.75	-4.08	7.89
RLR	-6.50	-6.00	11.21
RLN	-16.08	-4.75	7.83
RC	-14.00	-7.48	8.51
DHR	-28.00	-7.44	11.99
DHN	-19.83	-5.06	12.59
DLR	-25.50	-9.77	8.74
DLN	-19.33	-5.72	9.99
DC	-2.83	-9.52	10.17
MHR	-25.67	-8.48	14.31
MHN	-27.50	-10.69	12.40
MLR	-18.25	-9.17	11.96
MLN	-17.75	-14.02	11.18
MC	-25.17	-5.10	11.89
Control	-22.83	-23.60	15.32

Table 4.1. Group CE1, CE4 and VE4 Scores for Task 1.
Means have been collapsed across order within each group.

GROUP	CE1	CE4	VE4
RHR	-35.42	-15.40	16.79
RHN	-19.42	-17.02	13.20
RLR	-49.83	-17.27	17.46
RNR	-25.75	-10.98	17.32
RC	-9.5	-12.95	21.94
DHR	-12.25	-2.46	15.86
DHN	-24.75	-13.15	17.62
DLR	-43.58	-13.56	19.32
DLN	-22.33	0.52	17.35
DC	-36.08	-11.63	13.10
MHR	-43.00	-4.10	14.34
MHN	-35.92	-12.17	17.64
MLR	-11.50	-16.52	20.36
MLN	5.75	-14.19	20.06
MC	-22.50	-7.02	14.13
Control	-33.67	-21.63	26.49

Table 4.2. Group CE1, CE4 and VE4 Scores for Task 2.
Means have been collapsed across order within each group.

SCORE	CONTROL GROUP MEAN	PRACTICE GROUPS MEAN	d_{crit}	d_{calc}
T1(CE1)	-22.83	-17.48	20.18	5.35
T1(CE4)	-23.60	-7.58	9.06	16.02*
T1(VE4)	15.32	10.58	3.65	4.74*
T2(CE1)	-33.67	-25.74	37.53	7.94
T2(CE4)	-21.63	-11.19	13.68	10.44
T2(VE4)	26.49	17.10	7.10	9.39*

*Significant at the .05 level.

Table 4.3. A Comparison of the Control Group Versus All Other Groups. Dunn's critical differences for alpha = .05 (d_{crit}) are displayed, along with the actual differences obtained (d_{calc}).

4.1.2 Analysis of Prediction 2

As discussed above, a series of regressions were performed on the various CE and VE scores to examine Predictions 2, 4 and 5. Each of these scores, for the six high variability groups and the six low variability groups, were regressed on parameter (P), variability level (V), randomness (R) and order (O). In addition, in order to account for the maximum amount of variation in the data which could be controlled, all possible interactions were included in these regression analyses. These interactions were: PxV, PxR, PxO, VxR, VxO, RxO, PxVxR, PxVxO, VxRxO and PxVxRxO.

None of the six full model regression analyses (i.e. those which included all main order and interaction effects) were significant at the .05 level. This may have been a result of the fact that a large number of dummy variables were required to specify all real variables and interactions, and thus information was diluted by the large number of degrees of freedom incurred in the attempt to estimate so many interaction effects. Therefore, the data was re-analyzed using a simpler model. In this second series of regression analyses, only main and first order interaction terms were included for consideration.

Only one of these simpler regression analyses led to a significant overal F-value. The regression on T2(CE1) was significant at the .05 level ($F_{14,143}$=1.84; $p<.05$). A number of effects were significant within this regression. These included main effects for parameter and order, and the interaction effects PxV and PxO. The F-values for all of these effects included in this regression may be found in Table 4.4. Mean T2(CE1) scores for the three parameter conditions are displayed in Figure 4.1. A post hoc Scheffe's analysis proved too rigorous to identify any one parameter condition as significantly better than any other. Figure 4.1 displays the mean absolute T2(CE1) scores for the three parameter conditions, while Figure 4.2 displays the PxV interaction. It would appear that the significant PxV interaction is due to the very low error scores produced by the low variable Distance groups relative to the high variable Distance groups. Within the Resistance and Mixed groups, this pattern was not evident, and the low variable groups did not outperform the high variable groups.

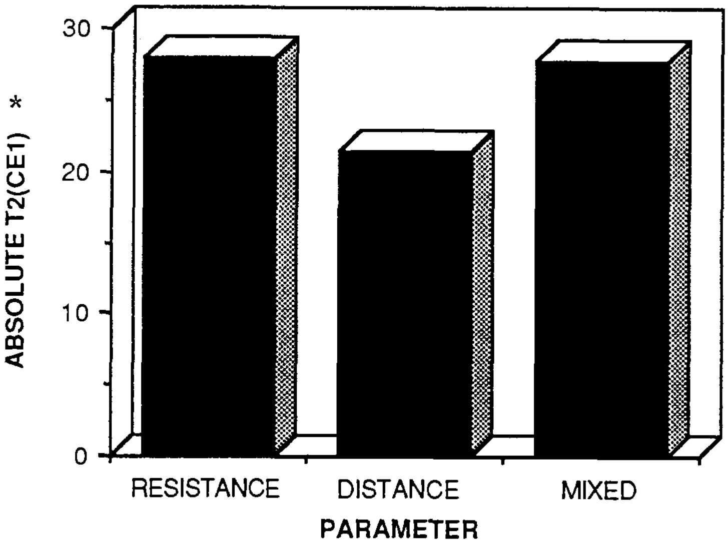
Figure 4.3 provides a visual display of the PxO interaction. The Mixed groups appear to have benefitted greatly from presentation Order 1, while for the Distance groups, Order 2 led to lower T2(CE1) scores. Order appears to have had little effect on the performance of the Resistance groups on this measure.

Although included only as a counterbalancing measure, and thus not a variable of primary interest, order also proved a significant main effect in this regression. Individuals experiencing Order 1 during testing had an average T2(CE1) score of -21.68, while those subjects in the Order 2 condition had a mean of -31.17.

EFFECT	T2(CE1)*	T1(CE1)**	T1(VE4)**
P	4.96***	5.30***	5.32***
V	.04	.33	1.26
R	.09	1.37	.61
O	4.50***	2.36	.01
PxV	4.21***		
PxR	.36		
PxO	4.00***		
VxR	1.10		
VxO	1.38		
RxO	.83		

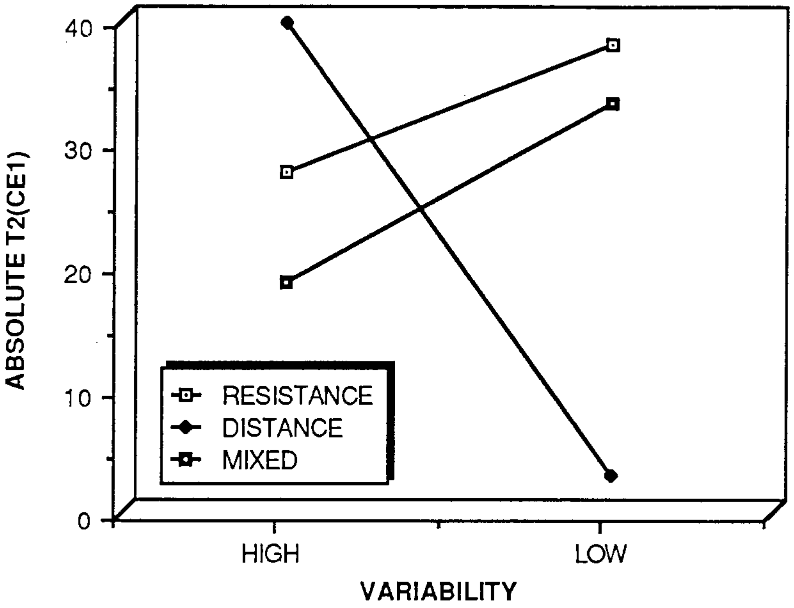
*Regression on main effects and first order interactions.
**Regression on main effects.
***Significant at the .01 level.

Table 4.4. F-Values for Significant Regressions.

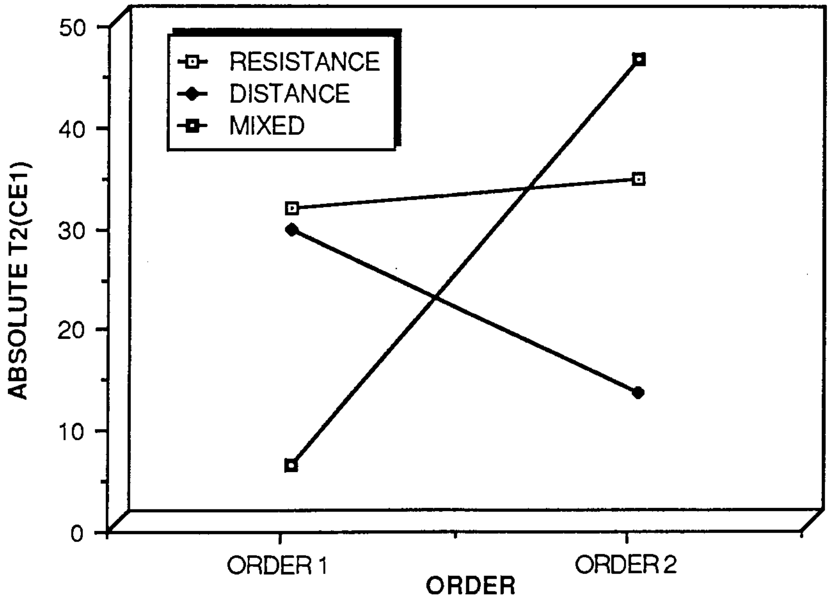


4.1. Mean T2(CE1) Scores for the Three Parameter Conditions.

*Units here and in all subsequent graphs in this section are arbitrary units based on the scoring system described in the text.



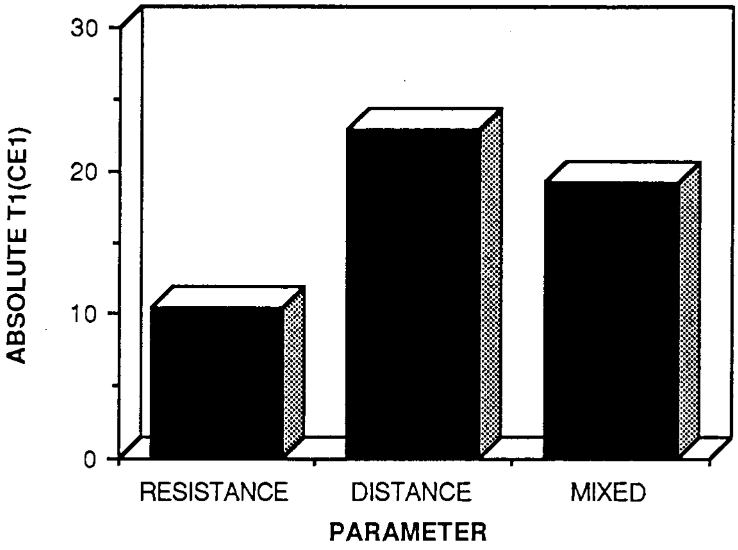
4.2. Interaction Between Parameter and Variability Level on T2(CE1).



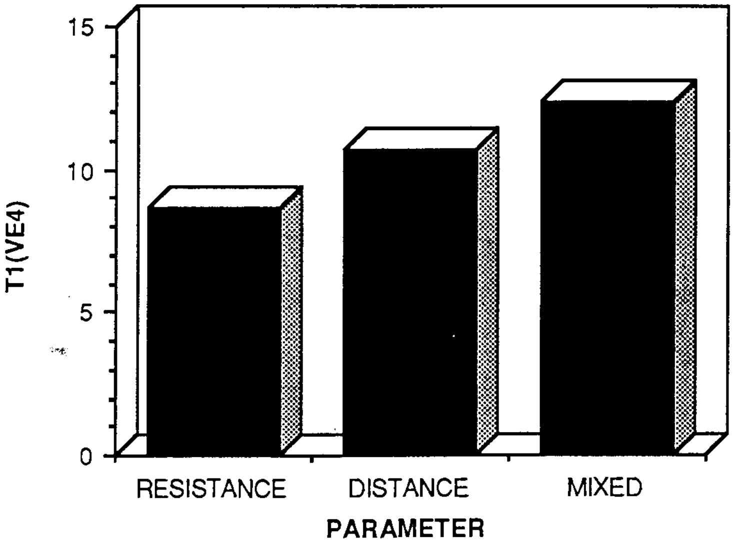
4.3. Interaction Between Parameter and Order on T2(CE1).

In order to examine the five remaining error scores, T1(CE1), T1(CE4), T1(VE4), T2(CE4) and T2(VE4), in regressions diluted by as few degrees of freedom as possible, analyses were conducted which included only the main order variables. Only the regressions on T1(CE1) and T1(VE4) yielded significant overall F-values ($F_{5,143}=3.05$, $p<.05$; and $F_{5,143}=2.51$, $p<.05$, respectively). Within each of these two regression, parameter proved a significant main effect. The complete sets of F-values resulting from these regressions are included in Table 4.4. Equations for the three significant regression analyses are presented in Appendix V. Mean T1(CE1) scores for the three parameter conditions are presented in Figure 4.4, while the means for T1(VE4) are displayed in Figure 4.5. A post hoc Scheffe's analysis indicated that the Resistance groups scored significantly lower on T1(CE1) than the Distance groups, while the Mixed groups were not significantly different from either of the other two conditions. For T1(VE4), Scheffe's analysis indicated that the Resistance groups were significantly better than the Mixed groups, while the Distance groups were not significantly different from either of the other two conditions.

In summary, it may be concluded that Prediction 2 was not supported by the data, and that variability provided by the manipulation of two task parameters did not lead to superior performance on the transfer tasks over variability provided in only one dimension.



4.4. Mean T1(CE1) Scores for the Three Parameter Conditions.



4.5. Mean T1(VE4) Scores for the Three Parameter Conditions.

4.1.3 Analysis of Prediction 3

Dunn's multiple comparison procedure was again employed to examine differences between the variable and constant groups. Table 4.5 contains the results of these comparisons. As can be seen, the variable practice groups were not significantly different from the constant practice groups, and thus Prediction 3 was not substantiated by the data.

4.1.4 Analysis of Prediction 4

The only significant finding in the regression analyses which involved variability level was the PxV effect identified for T2(CE1). (See Figure 4.2). The Resistance and Mixed high variable groups outperformed their low variable counterparts, which supports Prediction 4. However, this pattern was dramatically reversed in the Distance groups, where the low variable groups clearly displayed more accuracy on trial one than any other groups, and especially with respect to the high variable Distance groups, which were the worst of all on this measure. The overall means for the high and low variability levels were -28.46 and -24.54, respectively. Thus, the high and low variability conditions actually led to performances on this measure which were patterned opposite to that prescribed by Prediction 4, although the differences were slight.

SCORE	VARIABLE PRACTICE GROUPS MEAN	CONSTANT PRACTICE GROUPS MEAN	d_{crit}	d_{calc}
T1(CE1)	-18.35	-14.00	12.61	4.35
T1(CE4)	-7.63	-7.34	5.66	0.29
T1(VE4)	10.68	10.19	2.28	0.49
T2(CE1)	-26.50	-22.69	23.45	3.81
T2(CE4)	-11.37	-12.36	8.55	0.99
T2(VE4)	17.28	16.39	4.44	0.89

Table 4.5. A Comparison of the Variable and Constant Practice Groups.
Dunn's critical differences for alpha = .05 (d_{crit}) are displayed, along with the actual differences obtained (d_{calc}).

4.1.5 Analysis of Prediction 5

Randomness did not surface as a significant effect at all in the regression analyses, and thus Prediction 5 also proved incorrect. Practice schedule did not play a significant part in determining transfer performance on either Task 1 or Task 2.

CHAPTER 5

DISCUSSION

5.1 Prediction 1

The practice groups were not significantly better than the control group on the first trial of each transfer task. This is probably reflective of the fact that even subjects with previous experience within the movement class needed one trial to familiarize themselves with the particular handle resistance they were facing in order to effectively identify the required movement specifications. With this one "exploratory" trial, subjects who had previous practice experience were able to reduce their variability on both transfer tasks (as measured by VE), and also to improve their accuracy on the interpolated task, transfer Task 1. The control subjects, who had had no previous opportunity to acquire relevant experience, were unable to demonstrate these adjustments. Thus, it may be concluded that Prediction 1, that previous experience on this task would facilitate transfer performance to novel variants, was supported by the empirical results emanating from this study.

5.2 Prediction 2

Manipulation of two task parameters did not lead to significantly better transfer performance than manipulation of one task parameter, and thus Prediction 2 was not supported by the data. The Resistance groups proved significantly better than the Distance groups, but not the Mixed groups, on the first trial of Task 1. In addition, they demonstrated significantly less variable error on the last four trials of this task than the Mixed groups, but not the Distance groups. The superior performance of the Resistance groups may be explained by the fact that task outcome was dependent upon how far the handle was pulled, rather than the force required to generate a given movement extent. Since the Resistance groups were practicing the criterion target for Task 1 (but not the criterion resistance), they were relatively well-prepared to execute the required movement extent when faced with Task 1, in spite of the novel handle resistance. In addition, their previous experience allowed them to maintain greater consistency over subsequent trials, relative to the other two conditions.

For transfer Task 2, which lay beyond the practice boundaries of all groups, the Distance groups displayed the least error on initial transfer (i.e. the first trial). Again, since task outcome was dependent upon movement extent, it is to be expected that the practice condition which led to the greatest knowledge about the relationship between movement extent and task outcome would also lead to the best performance on this transfer task. No condition had previous experience with the criterion target for this task, and thus the advantage enjoyed by the Resistance groups for Task 1 was eliminated here. Subjects in the Resistance conditions were learning to generate a number of different forces to produce a specific movement length during practice, since they faced a constant target and changing handle resistance. Thus, they had little opportunity to experience the relationship between variable movement distance and variable "ball" trajectories (aside from that provided via variability inherent in the motor production system---i.e. errors), and virtually no opportunity to learn that increasing force output could increase movement distance in any reliable way, since a given force output did not produce the same movement outcome unless the handle resistance remained constant. Thus, subjects in the Resistance groups were limited in their ability to perform Task 2 by two factors: (1) Their experience with variable target distances was non-existant, since they practiced a constant target; and (2) They were prevented from experiencing a situation in which increasing force output led to reliably increased movement lengths.

The Mixed groups had previous experience with variable distances, but this experience was confounded by the presence of variable handle resistance as well. This two dimensional variability may have created a learning environment which was overly difficult for optimal development of a schema, or rule, for task execution, particularly since the number of practice trials was relatively low. In essence, while the Mixed groups were being provided with task demands (i.e. targets) which changed in such a way as to elicit variable movement production, just as were subjects in the Distance groups, they also were deprived of the opportunity to learn a clear relationship between force output and movement extent, just as the Resistance groups had been. Thus, subjects in the Distance groups probably had the best opportunity to learn the relationship of primary importance for task outcome here, which was the specific relationship which dictated how increasing movement extent increased ball trajectory. It should be noted that, as discussed in the Results, the superior performance of the Distance groups was entirely the product of very low error scores produced by the low variable Distance groups, and that the high variable Distance groups performed poorly relative to other groups. In addition, the superiority of the Distance groups over the other two parameter conditions was transient in nature, and disappeared after the first trial. Thus, parameter was not an exceptionally strong influence on performance of Task 2.

A PxO interaction was also observed for first trial performance on Task 2. It was expected that Order 1 would lead to superior performance on Task 2, while Order 2 would lead to superior performance on Task 1, since in these situations the task in question would be in the end position in the testing sequence. For example, subjects assigned Order 1 had the benefit of experience with Task 1 before being presented with Task 2. This has the obvious advantage of providing such subjects with some previous experience with the testing procedure (which involved withdrawal of KR), as well as augmenting the total quantity of experience with the movement class prior to attempting transfer Task 2. In other words, the first transfer task presented acted as additional pre-test practice for the second transfer task to be encountered. These benefits would also be expected to accrue for performance on Task 1 for those in Order 2. Indeed, these expected benefits were the rationale behind the original division of subjects into Order 1 and Order 2 within each group.

Results indicated, however, that order was not a particularly strong influence in this experiment. The PxO interaction effect for first trial performance of Task 2 was the only significant order effect identified. The Mixed groups yielded results for this measure which were very obviously in the expected direction, while the Distance groups ran contrary to expectations and the Resistance groups appeared to be largely uninfluenced by presentation order. It may be the case that the Mixed groups were particularly sensitive to the effects of preferential presentation order because their learning environment was so complex. Transfer Task 1 represented the first opportunity these Order 1 subjects had to experience consecutive trials which did not vary. This "constant practice" experience may have been of critical importance for allowing Mixed group subjects to consolidate the information acquired during their "true" variable practice trials.

It is less clear why such an advantage did not hold for the Distance and Resistance groups on this measure, or why in fact Task 1 appeared to proactively interfere with Task 2 for the Distance groups. At any rate, the influence of order disappeared after the first trial on Task 2, and did not appear at all on Task 1. It is probable that order, being a relatively weak effect, only became influential under the most difficult circumstances, where the room for improvement was the greatest. Thus, Task 1, being inherently easier than Task 2 (see Note 2), precluded an advantage of optimal presentation order being realized.

5.3 Prediction 3

The variable practice groups were not significantly better than the constant practice groups in this experiment, and this result is directly contradictory not only for Prediction 3, but also for the variability of practice hypothesis, on which it is based. This lack of positive influence resulting from variable practice may be an indication that schema formation did not occur in this study, rather than that the variability of practice hypothesis is incorrect. The large number of studies which have supported Schema Theory predictions (e.g. Catalano & Kleiner, 1984; Margolis & Christina, 1981; and Moxley, 1979) are certainly not overshadowed by the present experiment. An obvious limitation of the study reported here is the low number of practice trials provided to subjects. Rabbitt (Note 3) has reported observing improvements in reaction time tasks, even after as many as two thousand trials. He has stated that he believes quantity of practice is the single most important determinant of learning. Although pilot testing demonstrated large improvements in performance after only ten trials (see Appendix VI), twenty trials was patently a very short time in which to expect subjects to learn the novel handle-pulling task employed in this experiment. In addition, the fact that the apparatus employed here was novel, is in no way insurance that the task was a totally novel one for subjects. It is entirely possible that subjects already had a relevant schema, or hierarchy of schemata, developed for tasks such as the one employed here. If such is the case, then it is unlikely, as discussed in Chapter One, that twenty practice trials were sufficient to alter significantly a pre-existing schema.

5.4 Prediction 4

Given that there were no differences between the variable and constant practice groups, it is not surprising that there were no significant differences between the high and low variable practice groups. The only significant effect for variability level involved a PxV interaction for T2(CE1). Within the Resistance and Mixed conditions on this score, differences were in the predicted direction, with the high variable groups tending to outperform their low variable counterparts. However, the Distance groups displayed a strong reversal of this pattern. This certainly was not predicted, and is somewhat more difficult to explain. Although no clear evidence for Prediction 4 was provided by the present study, the issue of degree of variability is probably one which is worthy of further examination.

5.5 Prediction 5

It had been hypothesized that providing subjects with a random, non-sequential presentation order during practice would facilitate schema formation in a manner similar to non-random, sequential practice when practice was relatively simple, and that a non-random schedule would be better when variability was such that it substantially increased the compexity of practice. Such would be the case, for example, when two task parameters were being manipulated. However, no significant effects involving randomness were identified, and thus only the first part of this prediction found support in the empirical results. This finding must be interpretted as supporting the position of those who espouse Contextual Interference Theory (e.g. Del Rey, 1977; Lee & Magill, 1983; and Shea & Morgan, 1979), that as long as learners are forced by their practice schedules to continually reconstruct action plans during skill acquisition (i.e. serial repetitions are prevented), then future retention and transfer performance will be enhanced. However, it is probably premature to entirely abandon the investigation of the relationship between practice schedules and variability level.

5.6 Limitations of the Present Study

While there was a significant effect of practice in the present study, and thus pre-transfer experience with the task was of some benefit to performers, there were few dramatic differences between groups. The large number of experimental groups tested resulted in a rather large statistical burden in terms of degrees of freedom, and it is possible that some noteworthy differences between groups were masked by this factor. In addition, groups tended to be quite variable, and this also may have contributed to the pausity of significant effects. Limitations imposed by the low number of practice trials and the possibility that the task was not a totally novel one for all subjects have already been discussed.

A limitation which extended beyond statistical considerations was the fact that only one task parameter under manipulation was affecting task outcome. While this does not mean that only one task parameter was contributing to variability, it certainly means that they were contributing to variability in different ways. One manipulation (handle resistance) required subjects to do different things to achieve the same outcome, while the other (target location) required subjects to do different things to achieve different outcomes. In this latter situation the relationship controlling action and outcome was stable. However, when both manipulations were made concurrently, subjects were required to do different things to achieve different outcomes, and the action-outcome relationship was no longer rigidly defined. The required external action upon the environment (movement extent) was still predictable, but the internal processes (force outputs) required to achieve such an external action were not.

In order to effect an accurate limb placement, a performer must be able to identify and specify the force output requirements of a task. Subjects in the present experiment had one second after initiating a trial in which to evaluate the force requirements of the task at hand and, if necessary, modify initial response specifications to achieve the goal. The relative variability level of this evaluation and adjustment process experienced during practice, was dependent upon the type of experimental manipulation being made. The Distance groups received no variability of this type, since they experienced a constant resistance. However, both the Resistance and Mixed groups did receive such variability. The Resistance groups had a constant goal, which simplified their task relative to the Mixed groups, who were forced to make variable adjustments to achieve variable goals. The net result of these differences was that, while all conditions were provided with task parameter variability, and consequently practised under conditions of movement parameter variability, the Distance groups practised meeting variable goals by employing a constant execution strategy, the Resistance groups practised variable execution to achieve a constant goal, and the Mixed groups practised variable execution to achieve variable goals. Thus, if schemata were being developed by these groups, it is possible that they were qualitatively different, as opposed to differing merely in relative strength. The variability of practice hypothesis is concerned with schema strength, and does not address possible comparisons between schemata which have been developed for the same task, but which are structurally different. Thus, the present study may have limited applicability as a test of the variability of practice hypothesis.

NOTES

Note 1. Newell and Shapiro (1976) demonstrated that presentation order was a potential influence on transfer performance. Subjects in their study who performed a ballistic timing task showed better transfer to a slow task if they practised a rapid task prior to a slow one, than vice-versa. Since it was not known if an analogous effect would be found for length and/or force manipulations, all groups involving sequential presentation orders were counterbalanced to eliminate such potential group biasing effects. In other words, in all non-random groups, half of the subjects received an ascending sequential order, while the other half received a descending sequential order.

Note 2. Task 2 involved a longer movement length than did Task 1, and increasing error is associated with increasing movement length (Woodworth, 1899). In addition, increasing movement variability is associated with increasing movement length (Schmidt, 1982a).

Note 3. Patrick Rabbitt discussed the importance of number of practice trials for affecting improvements in performance during an invited speech at the 1986 annual conference of the Canadian Society for Psychomotor Learning and Sport Psychology held in Ottawa, Ontario.

APPENDICES

Appendix I

A Brief History of Transfer of Training

Transfer of training was examined early in the century by Thorndike (1914), who postulated that transfer occurred only as a function of identical components between tasks. That is, Thorndike believed that transfer occurred between tasks only to the extent that they contained identical elements. This position was in marked contrast to that of the faculty psychologists of the nineteenth century, who assumed that transfer could be attributed to a "trained mind". (See, e.g. James, 1890, and his tests of the Theory of Formal Discipline). Thorndike's Identical Elements Theory of transfer summarized research to that date and represented a first step towards moving the study of transfer in the direction of a behaviourist rather than an introspectionist orientation. This perspective on transfer remained dominant for the following seventy years.

While Thorndike's Identical Elements Theory was the foundation for a great deal of work in the area of transfer of training (e.g. Cheng, 1929; and Harden, 1929), it was inadequate to explain all of the research findings accumulating in the literature. Judd's (1908) classic study stands as one marked illustration that Identical Elements Theory is incomplete as an explanation of transfer of training phenomena. Judd demonstrated that school children instructed in the principle of refraction transferred to a novel underwater target depth in a dart throwing task better than did those who had not received prior instruction. His Generalized Principles Theory emphasized that basic principles, as well as specific skill components, were transferrable between tasks.

Thorndike's theory was also challenged on another front. While his view provided a means of explaining positive transfer, it could not account for negative transfer. Studies demonstrating negative transfer surfaced in the 1920's and have continued to appear in the literature since that time (e.g. Gibson, 1941; and Zelaznik, 1977). It became apparent from these investigations that simply analyzing the elements of a task into identical and non-identical components and then basing predictions of transfer on this dichotomy, was an over-simplification. Investigations demonstrated that along a continuum of similarity there existed distinctions in the magnitude and direction of transfer. (See Gagne, Baker & Foster, 1950, for a more complete discussion). These studies were summarized by Skaggs (1925) and Robinson (1927) and their views became known as the Skaggs-Robinson hypothesis of transfer. The relationship between similarity and transfer according to this hypothesis is illustrated in Figure I.1.

Over the following 20 years, tests of the Skaggs-Robinson hypothesis showed that, although an improvement over the Identical Elements Theory, it was still an over-simplified account of the process of transfer. The major flaw in the

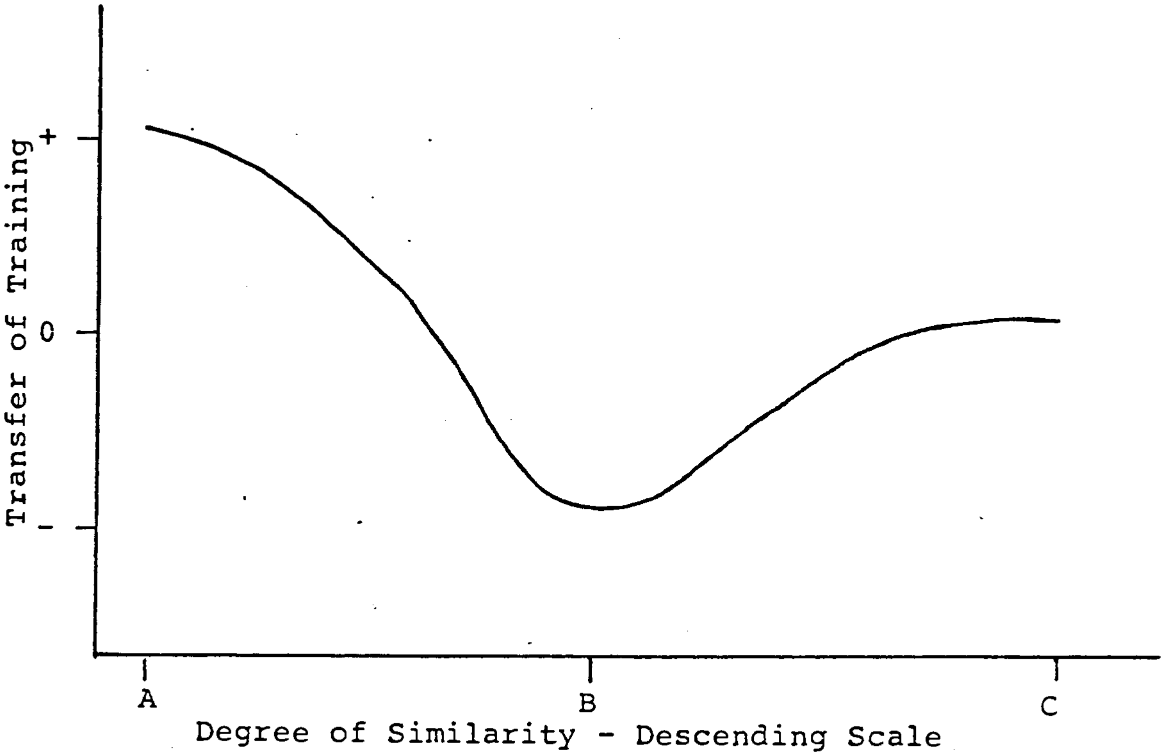


Figure I.1. The Skaggs-Robinson Hypothesis. Point A specifies maximum similarity (identity) and point C minimum similarity (neutrality) among the successive practiced materials; point B merely indicates the low point in the curve for efficiency of recall. (From Osgood, 1949).

conceptual relationship expressed in the Skaggs-Robinson hypothesis was the unidimensional view of similarity. As the S-R (stimulus-response) Associationist tradition in psychology gained in momentum during these decades under the guidance of Hull (1943), Guthrie (1935), Tolman (1932) and Skinner (1938), so it became apparent that similarity between tasks could be manipulated on either the stimulus or the response side of the S-R relationship. Studies in which these two elements were manipulated independently revealed a complex relationship which was finally summarized by Osgood in 1949. The transfer surface generated by Osgood is shown in Figure I.2.

The relationship between task similarity and both direction and magnitude of transfer may be identified using this surface for both stimulus and response components. Since the 1950's modifications to this surface have been recommended by a number of investigators, but it has remained a standard point of reference for researchers to the present time.

Two of the modifications will be discussed here. Martin (1965) was critical of Osgood's surface on the grounds that it dealt only with the associations formed between stimuli and responses in the two tasks. Martin suggested that in order to represent the transfer process in its entirety, two additional processes relevant to acquisition needed to be included. Firstly, response learning was ignored in the Osgood surface. That is, positive transfer resulting from the learned material itself rather than the associations between stimuli and

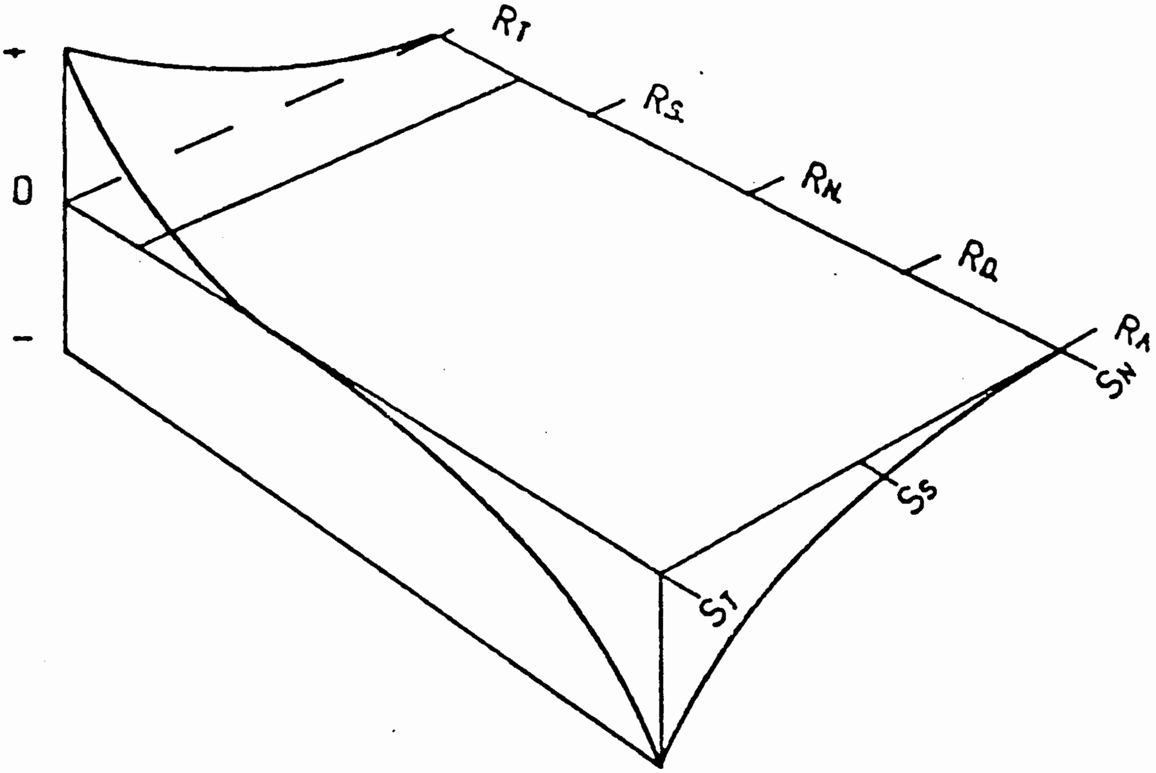


Figure I.2. Osgood's Transfer Surface. The medial plane represents effects of zero magnitude. Response relations are distributed along the length of the surface, and stimulus relations are distributed along its width. (From Osgood, 1949).

responses is not reflected in the Osgood surface. Secondly, evidence had accumulated that in forming associations between stimuli and responses, backward (R-S) associations were formed, as well as forward (S-R) associations (e.g. Deese & Hardman, 1954; and Porter & Duncan, 1953). It was Martin's contention that such associations could also have an influence on the transfer process. Accordingly, Martin developed three transfer surfaces designed to represent the impact of similarity between tasks upon transfer for these three components of learning. His transfer surfaces are illustrated in Figure I.3.

It should be noted that these surfaces were developed in the verbal learning context and predictions based upon them have not been tested in the motor domain. In addition, backward associations have not been demonstrated in a motor learning context. Nevertheless, the surfaces produced by Martin are important because they serve to solve a major problem with the Osgood surface. Conflicting results emerged repeatedly in the testing of the AB-CB transfer design. AB-CB transfer refers to those situations in which an individual learns to make some response "B" when confronted with some stimulus "A", and then transfers to a situation in which "B" must now be executed in response to a new stimulus, "C". At this corner of the surface Osgood predicts zero transfer, whereas both negative and positive, as well as zero, transfer have been reported (e.g. Porter & Duncan, 1953; and Yum, 1931). In fact, the preponderance of support in the motor learning domain is for

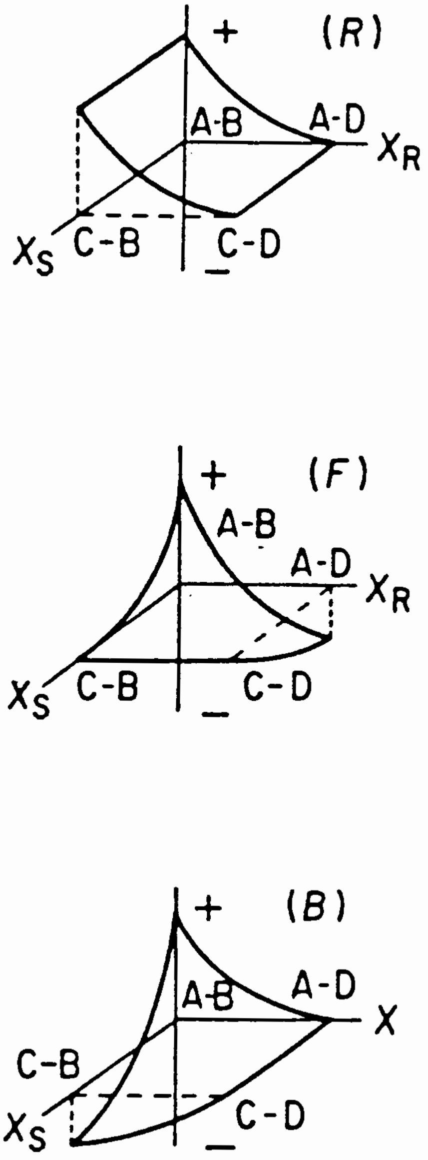


Figure I.3. Martin's Component Transfer Surfaces. The surfaces R, F and B represent the transfer of response availability, forward associations and backward associations, respectively, (From Jung, 1968).

positive transfer with this transfer paradigm. Zero transfer is to be anticipated if only forward associations are considered (since subjects have not experienced stimulus C before). However, backward associations are likely to produce negative transfer (since B has interfering associations), and response learning will produce positive transfer. Martin (1965) proposed therefore that net transfer may be either positive, negative or zero depending on the relative contributions of these associations.

A different form of modification to the surface was made by Holding (1976). In one respect this modification is more pertinent since it was explicitly designed to represent results from the motor learning domain. The Holding transfer surface is shown in Figure I.4.

Two differences between the Holding surface and that produced by Osgood are noteworthy. Firstly, Holding's surface incorporates evidence regarding response learning as well as forward associations. Thus, the AB-CB corner of the surface shows a low level of positive transfer. This is typical of the motor domain. In Martin's (1965) terms, this would indicate that the positive transfer from response learning more than compensates for any negative transfer generated by backward associations. The majority of the evidence with AB-CB designs in the motor context supports the view that there is positive transfer, but there is no evidence to suggest that the transfer is reduced by negative transfer from backward associations. (See

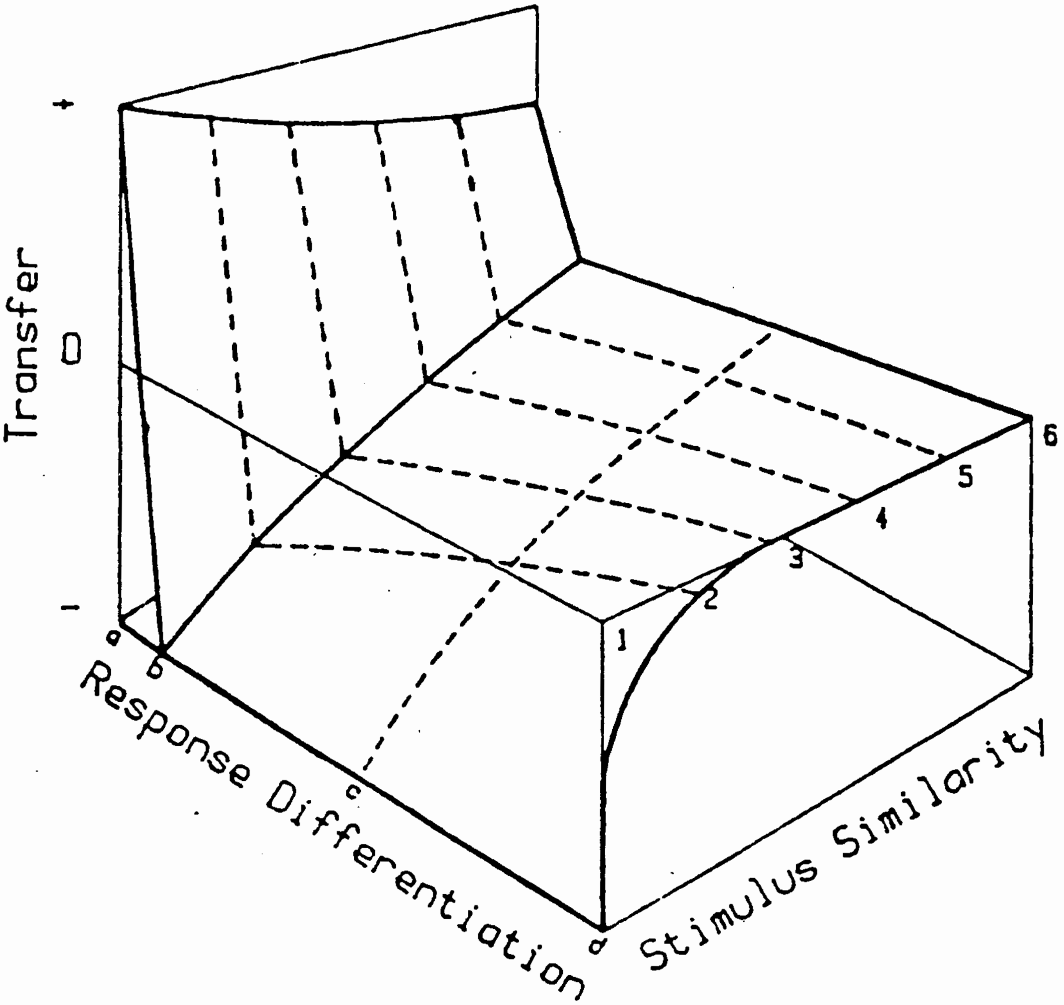


Figure I.4. Holding's Transfer Surface. Expected interference between two tasks is dependent upon their input and output characteristics. (From Holding, 1976).

Holding, 1976, for a review of the pertinent literature).

Secondly, in the transition from AB-AB to AB-AD, Holding suggests a step-function transition from positive transfer to maximum negative transfer as a function of decreasing similarity between responses. This transition occurs at a point where responses are no longer functionally identical. On the other hand, it will be noted from Figure I.2 that Osgood depicted this transition as a gradual increase in negative transfer. Recent evidence (Kleven, Herring & Dickinson, 1986) supports Holding's view.

While the transfer surfaces described above have been useful in providing some orientation for workers in the area, they were designed and have functioned primarily as descriptive tools rather than as theories about how humans learn motor skills. Indeed, early motor learning research was marked by a paucity of theoretical formulations. The few which existed were generally borrowed from other areas of psychology. It was not until the last third of the twentieth century that motor learning made a significant move to separate itself from mainstream psychology.

In 1971, Jack Adams introduced his Closed Loop Theory of motor learning. This theory was a landmark for the motor learning area because it was perhaps the first theory specifically tailored to address the empirical evidence which was accumulating in motor learning research. While not designed to address the issue of transfer of training, Adams' theory quickly provided partial impetus (Schmidt, 1975) for the creation of yet another motor learning theory, Schmidt's (1975) Schema Theory. This theory was broad enough to include transfer of training phenomena. A discussion of Schema Theory is included in Chapter One of this volume. In addition, the empirical research which Schema Theory generated regarding how variable practice influences transfer of training is included in Chapter Two. While more of this experimental research has proven favourable to Schema Theory than not, there were a substantial number of studies which failed to yield the predicted results.

The fact that support for Schema Theory has been mixed has left the field open for alternative theoretical formulations. Shea and Morgan, in 1979, borrowed from researchers in verbal and rule learning (e.g. Battig, 1972), and introduced the idea of contextual interference effects as an explanation for the equivocal findings in the motor learning literature. In Shea and Morgan's (1979) view, the success of variable practice for transfer of training to novel task variants was dependent upon the order of presentation of practice trials, rather than variability per se. If trials were blocked, then contextual interference was low and variable practice tended not to be any more successful than constant practice in eliciting positive transfer. Shea and Morgan provided empirical support for their position, finding that a random variable group performed better than a blocked variable group on retention and transfer tests of a barrier knocking task. (See also, Shea & Zimny, 1983, for a partial review of this issue).

Lee and Magill (1983) extended Shea and Morgan's (1979) work by demonstrating that serial variable practice was equally successful in eliciting retention and transfer effects as random variable practice. Since serial practice contains elements of both random and blocked practice, some serious speculation on the mechanism underlying the effectiveness of contextual interference was possible. Initial uncertainty existed as to whether contextual interference effects were attributable to the cognitive processing requirements of non-blocked practice, or to the event uncertainty inherent in random practice schedules. Since serial practice has cognitive demands similar to those of random practice, but is as predictable as blocked practice, the culpable factor was identifiable. Lee and Magill (1983) concluded that the cognitive processing demands of non-blocked practice were responsible for the superiority of this form of practice over its blocked counterpart. They have continued to provide empirical support for their position (e.g. Lee, 1985; Lee & Magill, 1983; and Lee, Magill & Weeks, 1985). Wrisberg and Mead (1983), and particularly Patricia Del Rey and her co-workers, have also found a great deal of support for the effects of contextual interference (Del Rey, 1982; Del Rey, Whitehurst, & Wood, 1983; Del Rey, Whitehurst, Wughalter & Barnwell, 1983; Del Rey, Wughalter & Whitehurst, 1982; and Whitehurst & Del Rey, 1983), although they have also reported a failure to find superiority of a high contextual interference practice group in one study (Del Rey, Wughalter, DuBois & Carnes, 1982). Del Rey et al (1982) suggested, in this last paper, that their contrary finding was due to a need for cued recall. Nevertheless, the majority support for Contextual Interference Theory would imply that order effects of trial presentations during practice must be carefully controlled in any study of variability of practice and its implications for transfer.

While Contextual Interference Theory has been eliciting a great deal of empirical attention recently, Schmidt's (1975) ideas have not been abandoned. His cognitive approach to transfer of training has been extended in a provocative way by Newell and Barclay (1982). They suggest that, while it may be appropriate to consider schemata existing at a motor level, this may represent only one level in a hierarchy of schemata. Their view is that acquisition of a skill is a process of developing an organization of schemata varying in their degree of abstraction. At the most abstract or symbolic level, a schema may consist of knowledge about actions which the learner is not able to produce. The more detailed schemata (analogous to those proposed in Schmidt's Schema Theory) may consist of kinematic or kinetic features of specific movements. Transfer between tasks may occur at any level in this hierarchy.

Newell and Barclay's position may be seen to represent a synthesis of ideas which were originally formalized by Thorndike (1914), who addressed the transfer of stimulus-response components, and by Judd (1908), who addressed the transfer of more cognitive components. In other words, Newell and Barclay's conceptual framework represents a synthesis of the cognitive and motor aspects of transfer. It may be extended, without disruption to the concept of a hierarchy of schemata, to include both S-R associations and stimulus generalization. Dickinson and Hedges (1986) have pointed out that the lowest level in the transfer hierarchy may consist of stimulus and response generalization which may "automatically" provide response strength to new instances of the same class of movements. That is, schemata concerning links between stimuli and responses may be involved in transfer within specific skills at this level. Kleven et al (1986) have suggested that the most molecular level of transfer involves previously learned movements having an impact on subsequent acquisition via biasing of afferent and efferent physiological systems.

The extent to which the different levels of schemata will contribute in any specific transfer situation will vary with the complexity (both cognitive and motor) of the particular skill. Thus, at the simplest level of skill, existing knowledge about skills (i.e. symbolic schemata) may enable the skill to be performed perfectly without practice. Conversely, other skills may involve transfer of motor components or kinematic features.

Appendix II

Calculation of Ball Trajectories

The apparatus permitted a handle displacement which ranged from 0.1 cm (at which point the microswitch opened) to 38.5 cm. As discussed in Chapter Three, this handle displacement range was matched to the potentiometer's range of 0 to 5 volts (actually 4.96 volts, as limited by the power supply), which in turn was matched to the 12 bit A/D converter's range (0 to 4096 units), which finally translated to an error range of ±100 scoring units for a centrally positioned target. Through continuous sampling (at 250 Hz) the maximum handle displacement for a trial was determined. This value was then used as the input parameter (termed V_i) in determining the ball trajectory and final score, using standard equations for projectile motion. Specifically, the A/D converter supplied the computer with a value which was treated as the initial vertical velocity component, and the vertical ball displacement for the handle pull in question was calculable. Since the horizontal displacement of a trajectory is dependent upon its vertical displacement, the horizontal ball displacement was ultimately calculable. The derivation of these equations follows:

Given $V_{Vf}^2 = V_{Vi}^2 + 2ad_V$,
where: V_{Vf}^2 = final vertical velocity
= 0 m/s (ball at rest after landing),
V_{Vi}^2 = initial velocity
= value from A/D converter,
a = accelaeration = $g = -9.8\ m/s^2$, and

$$d_V = \text{vertical displacement},$$

$$\text{then } V_{Vi}^2 = -2(-9.8 \text{ m/s}^2) d_V$$

and, ignoring units, which are irrelevant for purposes of this simulation,

$$V_{Vi} = \left[(19.6) d_V\right]^{1/2}$$

The resultant initial velocity (V_{Ri}) for a projectile is dependent upon both an initial vertical velocity (V_{Vi}) component,, and an initial horizontal velocity (V_{Hi}) component. V_{Hi} and V_{Vi} are related to the angle of take-off as follows:

$$V_{Hi} = \frac{V_{Vi}}{\tan 0}$$

The simulated ball was assigned a constant take-off angle of sixty degrees. Thus, setting $0 = 60$, $\tan 60 = 1.732$ and

$$V_{Hi} = \frac{\left[(19.6) d_V\right]^{1/2}}{1.732}$$

The dimensions of the video monitor on which the trajectories were to be displayed were such that it was necessary to increase the initial horizontal velocity component slightly in order to employ the full width of the screen. The denominator for V_{Hi} was consequently multiplied by .831, yielding initial velocity components for the ball as follows:

$$VY = V_{Vi} = \left[(19.6) d_V\right]^{1/2}, \text{ and}$$

$$VX = V_{Hi} = \frac{\left[(19.6) d_V\right]^{1/2}}{1.44}$$

Appendix III

JND Testing

Six subjects were tested in a short study to determine the just noticeable difference (JND) for adjacent targets in this task. A cardboard frame around the screen eliminated visual cues regarding target positions from the edges of the video monitor. The criterion target (the target associated with Task 1 in the main study) was presented to each subject ten times in order to provide an opportunity for familiarization with this target. Following these active practice trials, the Method of Constant Stimuli (see Dickinson, 1974) was employed to determine JNDs. The Method of Constant Stimuli here involved presenting a series of targets either longer or shorter than the criterion and gradually approaching that criterion, and then switching to the opposite range of targets and again approaching the criterion. Before each comparison target the subject was presented once with the criterion in order to maintain a strong referent.

After executing a handle pull in an attempt to hit the target presented on a given comparison trial, subjects were asked to report whether the comparison target was shorter than, or longer than, the criterion target. Since they pulled the handle on each trial in an attempt to hit the target, the visual information available to subjects was augmented by any cues which may have become available through task-related movement. At the point at which subjects switched their report of the quality of the comparison (i.e. changed from a series of reports of "longer than's" to a "shorter than", or vice-versa), then the current sequence (either ascending or descending) was abandonned and a new sequence was begun in the opposite direction. This process was repeated until each subject had completed four sequences in each direction.

Handle resistance was maintained throughout the entire testing procedure at five springs (which was the criterion value for Task 1 in the main study). Targets were continuous such that, if all targets were presented on the screen at one time, they would form an unbroken line. This meant that the centres of each adjacent pair of targets were separated by five pixels, which was the target width employed.

Subjects demonstrated a relatively high sensitivity to adjacent targets. Ninety-two percent of the time subjects altered their response within one target of the criterion. The average interval within which both responses were given, across all trials for all subjects, was 5.8 pixels. Practice targets employed during the main study were 15 pixels apart from centre to centre. The centres of the test targets were separated by 25 pixels from adjacent targets. Thus, targets in the main study were more than adequately separated to ensure that they were identifiably different.

An initial attempt was made to determine JNDs for handle resistance. However, no subjects could be found who had any difficulty distinguishing between the resistances provided by any number of springs.Thus, it was concluded that the spring conditions were all identifiably different from one another, and JND testing for handle resistance was abandonned.

Appendix IV

Instructions to Subjects

The motor learning experiment in which you are about to participate involves a simple handle-pulling task. The pull which you exert on the handle is translated into a trajectory for the ball which you see on the screen above your head. This translation is based on a straight length relationship. (In other words, the further you pull the handle, the further the ball will move). The object of the task is to pull the handle in the appropriate manner to cause the ball to hit the target. As soon as you start to pull the handle a beep will sound, indicating that the computer has started to monitor your pull. One second later the computer ceases to monitor your activity. Thus, you must complete your pull within one second after you begin. In order to do this successfully you must pre-plan your pull, and then execute it quickly and smoothly. (A physical demonstration was made by the experimenter "in the air" at this point to indicate to the subject an appropriate movement velocity and ballpark range of motion). The ball itself will not actually begin to move until after this one second sampling period is over. At the end of each trial you will see the landing location of the ball as well as a numerical score. A positive score indicates that you have overshot the target, a negative score signifies that you were short of the target, and a score of zero means that you hit the direct centre of the target. You will receive twenty practice trials, followed by ten test trials. The object of every trial is to hit the target and score a zero. The target at which you are aiming and/or the resistance against which you are pulling may change. I will inform you when the test trials start. The individual in each experimental group with the best overall performance on the test trials will win ten dollars. I will contact the winners. Are there any questions?

Appendix V

Resultant equations for significant regressions:

Significant Regression on Main and First Order Interactions:

$T2(CE1) = -4565 + 6147\beta_0 + 2137\beta_1 - 343\beta_2 - 546\beta_3 + 3818\beta_4 - 5113\beta_5 - 330\beta_6 - 771\beta_7 - 1668\beta_8 - 5507\beta_9 - 3551\beta_{10} + 1686\beta_{11} + 1891\beta_{12} - 1470\beta_{13}$

r^2=16.7%

r^2=7.6%, adjusted for d.f.

Significant Regressions on Main Effects:

$T1(CE1) = -1705 + 99\beta_0 + 1334\beta_1 - 214\beta_2 - 436\beta_3 - 573\beta_4$

r^2=9.9%

r^2=6.7%, adjusted for d.f.

$T1(VE4) = 1153 - 163\beta_0 - 372\beta_1 + 105\beta_2 + 72.6\beta_3 + 8.6\beta_4$

r^2=8.3%

r^2=5.0%, adjusted for d.f.

where:

T1(CE1) denotes trial one score for Task 1,

T1(VE4) denotes variable error for trials two through five on Task 1, and

T2(CE1) denotes trial one score for Task 2;

and where:

β_0 and β_1 specify parameter,

β_2 specifies variability level,

β_3 specifies randomness,

β_4 specifies order,

β_5 and β_6 specify PxV,

β_7 and β_8 specify PxR,

β_9 and β_{10} specify PxO,

β_{11} specifies VxR,

β_{12} specifies VxO, and

β_{13} specifies RxO.

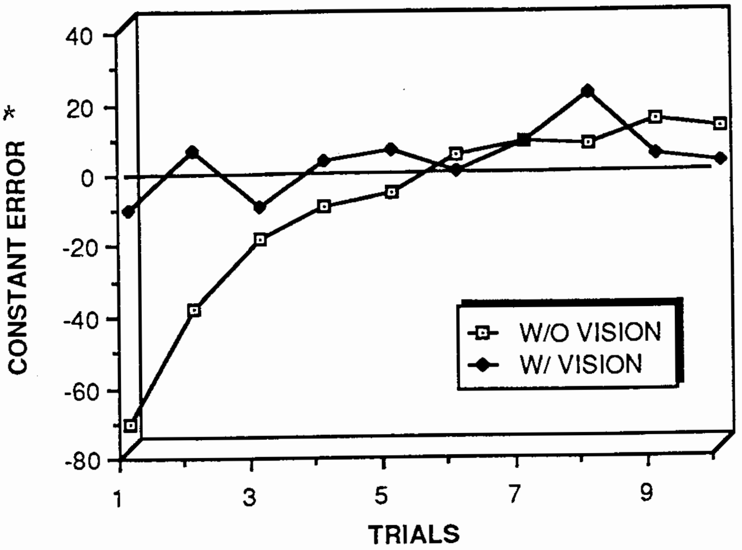
Appendix VI

Pilot Study

A preliminary study was conducted to examine whether varying either movement extent requirements or handle resistance was an effective means of providing practice variability. Four subjects experienced variable targets during forty practice trials (ten trials at each of four targets) before transferring to the criterion target, which they attempted ten times. Spring resistance was constant throughout the fifty trials. Eight subjects experienced variable handle resistance during their forty practice trials (again, ten trials at each of four resistances). Four of these subjects practised under visual conditions (i.e. subjects were freely able to view their arms during movement), while the remainder were blindfolded during movement only (i.e. these subjects were permitted to view the monitor after they had completed the handle pull for each trial). The target distance for these subjects remained constant throughout practice and testing. Three subjects practised the criterion target with the criterion resistance for the full fifty trials, two with vision and one without. The criterion task was identical for all 15 subjects.

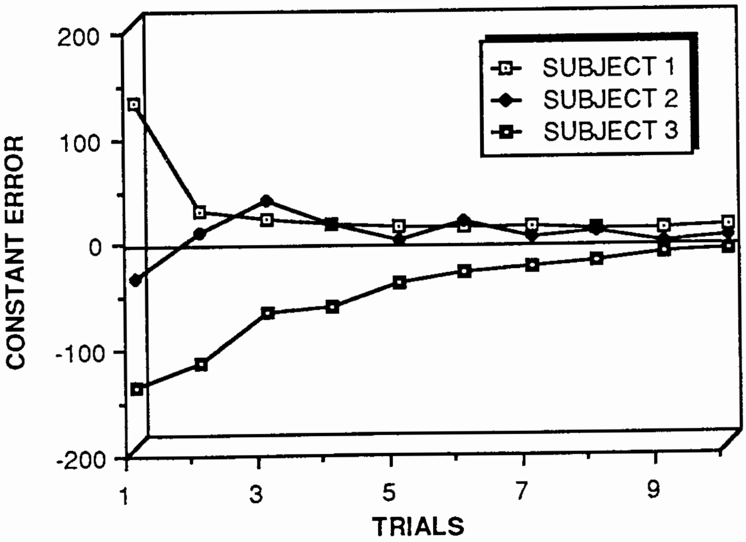
While the sample size for this initial investigation was very small (N=15), and thus no statistical analysis of group differences would have been meaningful, some observations were possible based on the data available. First, the task appeared to be of a sufficiently challenging nature that learning did occur over trials, without being so difficult that mastery was impossible. (See Figures V.1 and V.2). It was readily apparent that subjects were reducing their errors over trials. In fact, it appeared that subjects achieved an asymptotic performance level within approximately ten trials, and little improvement was observed after this time. (See Figure V.3). For this reason, it was decided that twenty trials would provide sufficient practice time for subjects in the main experiment. The wide variations in the graphed results is due to the extremely small sample size. Little averaging across subjects was possible, and most of the lines plotted represent data from only one subject.

A second observation drawn from these data was that type of parameter manipulation made appeared to influence performance on the criterion task. Specifically, variability of resistance appeared to be more beneficial for transfer performance than did variability of targets. (See Figure V.4). However, this superiority of handle resistance practice was only evident for those subjects who were permitted vision during their practice trials. Thus, the decision was made to prevent all subjects in the main experiment from visually monitoring their arms during trials.

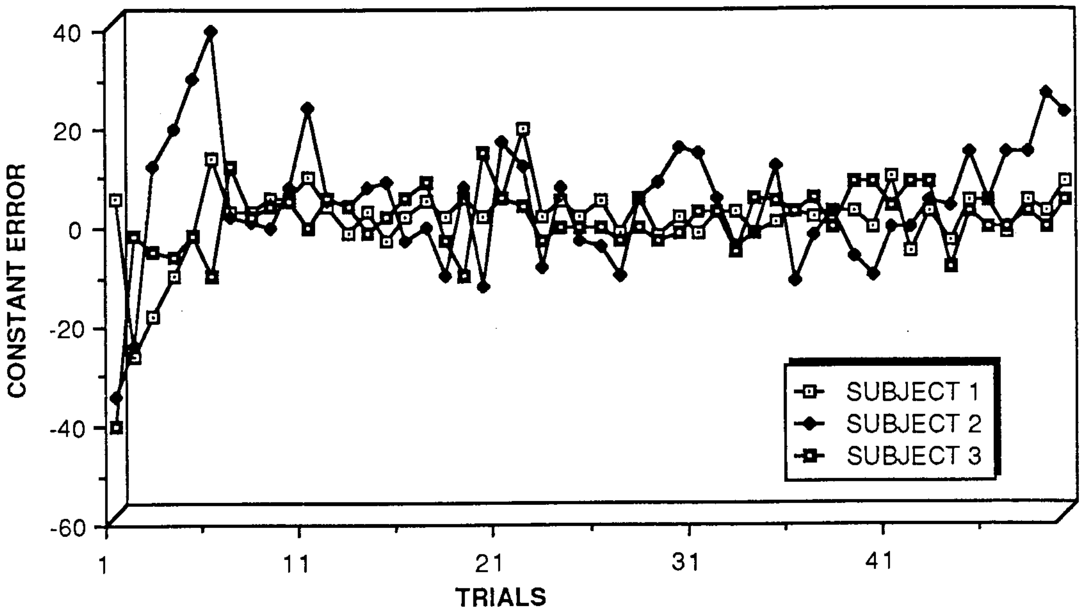


VI.1.Pilot Study: Variable Handle Resistance Practice.

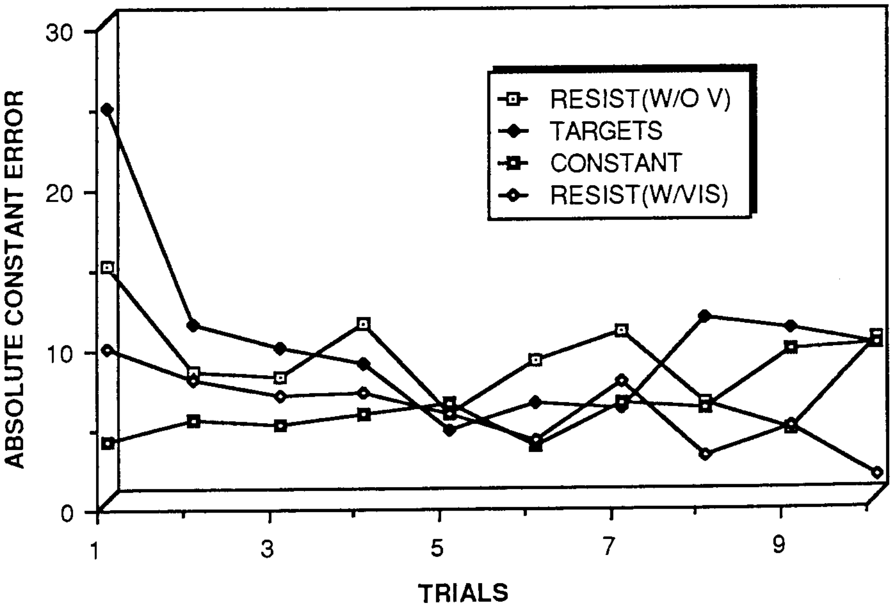
*Units here and in all subsequent graphs in this section are arbitrary units based on the scoring system described in the main body of the text.



VI.2.Pilot Study: Variable Target Practice.



VI.3.Pilot Study: Constant Practice of Criterion Task.



VI.4.Group Performances on the Transfer Task.

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