LEARNING RESEARCH AND SCHOOL SUBJECTS

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LEARNING RESEARCH AND SCHOOL SUBJECTS

EDITED BY

ROBERT M. GAGNÉ
University of California, Berkeley

AND

WILLIAM J. GEPHART
Phi Delta Kappa

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PREFACE

In 1959 Phi Delta Kappa inaugurated a series of research symposia with a discussion of the nature and status of educational research. Some of the most reputable persons in the field were brought together to present and discuss aspects of this topic.

To continue its service to the field, Phi Delta Kappa is pleased to present in this volume the eighth in this annual symposium series. Commendations are due for Robert M. Gagné for organizing a most stimulating series of presentations and discussions and to the University of California, Berkeley, Phi Delta Kappa Chapter for its efforts in making this symposium an enjoyable experience.

It is the purpose of this Annual Research Symposium to bring together the finest minds available on an educational topic of recognized importance, and, through such an assemblage, to push thinking on the topic as far as can presently be attained. Phi Delta Kappa is confident that the insights to be obtained from the material in this volume merit its placement among the publications of the other seven symposia, all of which as shown on the preceding page are available at Phi Delta Kappa Headquarters.

October 31, 1967

WILLIAM J. GEPHART
DIRECTOR OF RESEARCH SERVICES
PHI DELTA KAPPA

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INTRODUCTION

As research attains increased respectability and importance in the minds of educational practitioners, there is a correspondingly heightened need for establishing mutual understandings of learning as it is viewed in the psychologists' laboratories and in the schoolhouse. As one contribution to this rapprochement, five distinguished psychologists who have approached the study of learning from different points of view were asked to prepare statements which examined their areas of interest for empirically based knowledge of relevance to learning as found in the classroom. These people were brought together in a symposium held at Berkeley, California on October 28 and 29, and were augmented by twice their number of equally reputable investigators who were invited to analyze and criticize their reports.

The symposium was conducted in five sessions, each initiated by a formal presentation, followed by a period of discussion and questioning in which the general audience participated. Following this, the two designated Discussants commented on the presentation, followed in each case by further discussions by members of the symposium panel. Beginning with a first session on the learning and teaching of concepts, the symposium continued with the topics of perceptual learning, management of instruction, transfer of training, and the learning of values. The commentary of discussants originated a lively discussion in every instance.

Perhaps the most general impression to be obtained from this stimulating conference is to the effect that there is an exciting, complex, and rather long road to traverse in proceeding from the learning of the laboratory to the learning of the classroom. Another suggestion to be gleaned from these interchanges is the need for alterations in ways of approaching the problem of learning, not only in the laboratory, but also in the classroom, if progress toward the goal of educational improvement is to be made.

ROBERT M. GAGNÉ
WILLIAM J. GEPHART

CHAPTER ONE

CONCEPT LEARNING AND CONCEPT TEACHING

BY

ROBERT GLASER

LEARNING RESEARCH AND DEVELOPMENT CENTER
UNIVERSITY OF PITTSBURGH

Between basic elementary behavioral processes such as stimulus perception, discrimination, and associations, and more complex intricate behavioral processes such as problem solving, thinking, and creating, lies the domain of concept learning.[1] Concepts are built and molded out of simple behavioral acts and form the basic elements that are processed in higher-order behaviors. Because concepts occupy this unique position as the complex aspects of simple behavior and basic elements of more complex behavior, they have been of serious and sustained interest to psychologists and educators.

THE NATURE OF CONCEPTS

In a flat and uniform universe, concepts probably would not be very important. A uniform object or idea would have a unique label and definition which could be used for general discourse. However, in our rich, changing, multistimulational environment, concepts

[1] The preparation of this chapter was performed pursuant to a contract with the Office of Education, U.S. Department of Health, Education, and Welfare and to contract Nonr-624(18) with the Personnel and Training Branch of the Office of Naval Research. Grateful acknowledgement is made to Dr. Joan Sieber of Stanford University for her thoughtful criticism of the manuscript.

provide a kind of stability, and concept learning is a process of constant revision to maintain this stability. The concept we call "river" can be applied to a single object or instance, like the Monongahela River, but is not properly restricted to the one instance; it applies to a class of instances with properties and dimensions in common. The child who applies the term "river" to only a single object of his experience has an incomplete concept. It will take some time (learning time) for him to use the concept in a way that is socially standardized so that he can share the concept with his fellows and also can use it inventively and metaphorically to talk about a "river of fear" or a "river of generosity" (Brownell & Hendrickson, 1950). In learning to do this, he will make some errors and have some difficulties in applying the concept of river when contrasted to a stream or a creek.

Fundamental questions for psychologists are how the child learns and what he responds to, in order to make such distinctions and classifications. There are attributes or dimensions, as I shall call them here, of rivers and streams which permit their identification and classification; in the course of learning to respond to these dimensions, the child has intellectual mishaps and so adjusts his concepts accordingly. Dictionary definitions attempt formally to mark off the boundaries of a concept by indicating its *genus* (that is, what it has in common with other things and experiences) and its *differentia* (that is, the extent to which it differs from these things). When the dimensions of a definition are overlooked, misunderstood, or mistaught, then an error in the use of the concept occurs. For example, a tarn differs from a lake or a pool, but when one overlooks the dimensions of size and height, it is possible to call Lake Erie a tarn (Carroll, 1964).

Concepts are sharpened by the inclusion and exclusion of appropriate and inappropriate exemplars. Particular properties of these exemplars are abstracted and become the stimuli according to which an instance is classified as a member or not a member of a concept class. The individual then is able to respond to classes of experience in relatively stable ways, and is not faced with the chaos of treating each new experience with a new response. The "blooming, buzzing confusion" of stimuli in the environment is reduced to somewhat manageable proportions.

The formation of concepts and the process of abstraction are probably never complete. Perhaps as the child grows up, the concept of river and the concept of redness become reasonably stable, but in the course of living, a challenging instance usually waits around the bend. In school such concepts as subtraction, a grammatical class like prepositions, longitude in geography, and mass and weight in physics, constantly grow and change so that response of the individual is probably never quite restricted to a specific collection of properties, although for many "practical" purposes a particular set of properties will dictate the student's behavior.

The dimensions by which concepts are abstracted and classified may be obvious, such as particular colors and shapes; or more subtle, such as those which define Mozart's music and the paintings of the Dutch masters; or extremely subtle, such as the properties that define instances of justice and freedom. Concept dimensions can be direct perceptual experiences such as light and sound, or they can be indirect, that is, experienced through words and symbols. Concepts also have different structures, depending upon how their dimensions are combined to form the concept. They may be additive or conjunctive, such as orange and round, or they can be relational, like the concept of time.

One of the functions of education and of school learning is to transmit relatively definitive meanings of certain concepts, and at the same time transmit the ability to amend and revise concepts, as well as the ability to reorganize instances of experience into newly discovered or personally held concepts.

The Definition of a Concept

I shall derive a definition of concept learning from the nature of the tasks that psychologists have studied in their experiments. As a result, the nature of this definition may reflect special scientific interests such as investigation of the primacy of the physical dimensions which define a concept, the verbally mediated properties of a concept, the limiting characteristics of a mathematical model, or the information-processing aspects of concept attainment. Despite the limitations of such specialization, in recognition of the central role of concept learning in human behavior, the literature of psychological experimentation has a large and extensive backlog of experimental studies. From the experimental situations used by psychologists to study the variables that influence concept learning, a general definition emerges, although there are departures and special cases.

As a first approximtaion, it can be stated that concept learning involves learning to make a common response to a set of stimuli; that is to say, a group of stimuli is assigned to a single response category. In contrast, in paired-associate learning, a different response is learned for each stimulus. In concept learning, the individual learns to respond in one way to one set of stimuli and in another way to another set of stimuli. He learns to categorize instances, and in doing so, discriminates between instances and noninstances, generalizing his behavior so that a new instance with relevant properties can be included in the concept class.

The properties of the instances presented to the learner have a further defining requirement. They involve a number of possible dimensions according to which they can be categorized (Garner, 1962). Some of these dimensions are relevant to the concept being formed, and others are irrelevant. The concept-learning tasks defined by experimental psychologists require that the learner be confronted by more than one dimension. Let me give an instance of what is not defined as concept learning: Given a set of blocks differing in size, the task of the learner is to sort the large blocks into one category, the middle-size into another category, and the small blocks into another; here we have a situation in which common responses are made to sets of stimuli. However, what is not present in this task is the necessity for eliminating irrelevant dimensions and for determining which dimension or combination of dimensions defines the concept.[2]

The criteria, then, for identifying conceptual behavior are twofold: (a) multiple stimuli and a common response and (b) stimuli which are multivariate in nature so that the learner must determine which dimensions are relevant, which can be ignored, and how the appropriate values of the relevant dimensions are combined. The reinforcement contingencies of the situation establish those combinations of dimensions which the experimenter or the community consider relevant. When a child learns the concept of red, he learns to call all objects that emit certain physical properties of light "red." He learns this because he is reinforced for making the response "red" in the presence of this stimulus; at the same time, all other properties of the stimulus object are essentially randomized with respect to

[2] Of course, it might be said that the concept of size for a young child involves concept learning. It does, indeed, if he does not already have the concept of size, and this task requires him to ascertain the relevant and irrelevant properties of the stimulus. The entering behavior of the learner obviously contributes to defining the task.

that particular stimulus-response contingency. Object shape, size, distance from the viewer, position, etc., all are randomized in the child's experience with the exemplars of red so that no other physical property becomes associated with that response.

Learned or mediated dimensions also can be the basis upon which stimuli can be categorized. For example, the learned meaning of "preposition" can comprise a concept dimension so that the words *in, to, above, below, beside,* and *near* form the concept class of function words called prepositions. An important point to make is that if we consider concept formation as learning to categorize classes of stimuli so that stimulus equivalence is set up within members of the class and discriminations are learned between classes, then we accept this behavorial process and accept the reality of different kinds of stimulus dimensions. At the present state of knowledge of concept learning, it seems to me to represent something other than scientific parsimony to include or exclude concept-learning situations on the basis of different kinds of stimulus dimensions. There is a tendency to do this among some psychologists, who imply that direct, nonmediated stimulus situations are not instances of conceptualization. This is implied in Osgood's (1953) critique of Hull's early experiment with Chinese figures. It is also implied in Eleanor Gibson's note (1959) on the definition of generalization as a psychological concept, which suggests that primary generalization occurs in the absence of conceptualization and is essentially an inability to discriminate among a group of stimulus objects; secondary or mediated generalization, in contrast, is a result of classifying or categorizing on the part of the organism, in which similarities and differences are both learned. She suggests that secondary generalization obviously increases with learning, whereas primary generalization, being defined as the inability to discriminate, decreases with age and learning.

To my way of thinking, the limits and precision of discrimination and generalization in both cases are, to a large extent, learned phenomena, and these limits can be expanded and contracted as a function of learning and environmental events. Although mediated stimuli may require additional behavioral processes, it seems not unreasonable to assume, from examining the experimental literature, that the basic processes of conceptual learning involving discrimination and generalization are similar for both nonmediated and mediated dimensions. This is also the conclusion reached by Bourne (1966) in his recent monograph on human conceptual behavior:

> A single theme underlies most of the current research on verbal concepts. The primary aim has been to study much the same variables as have been shown to be important in non-verbal task settings. Indeed, there is little substantial difference either in purpose or results between programs of research on verbal and nonverbal concepts. The main distinction is simply a change in stimulus materials from objects or their pictorial representations to their conventional linguistic symbols. While it is interesting to know that such a change can be made without serious modifications in the principles and functional relationships that govern performance in problematic situations, much deeper questions and issues seem hardly to be touched by current experimentation [p. 119].

On the basis of my discussion so far, then, I will admit direct (non-mediated) and indirect (mediated), non-verbal and verbal dimensions as the stimuli in concept formation, holding open the possibility that as we obtain more knowledge we may find it convenient to make distinctions on the basis of the kind of stimulus and task variables involved. I will not hesitate to say that rats who jump toward a triangular form discriminated from other forms despite variations in size, shading, position, etc., are displaying a form of concept learning. Stimulus patterns such as words, while they do not necessarily contain direct stimulus features in common, do elicit common mediating or thematic responses which establish dimensions for concept learning. Any one of these situations may be no truer a form of concept learning than the other.

When concept learning is defined in this way, it encompasses a wide variety of stimulus situations—from the dimensions of color and form which define the concepts of redness and triangularity, to verbal, structural dimensions which define syntactic classes, and to semantic dimensions which contribute to word meanings (Rhine, 1958; Brown, 1958; Brown & Fraser, 1964; Carroll, 1964). I would also include attitudinal concepts which involve value words like those which define the concepts of Turk, Negro, and Jew. Incidentally, stereotypes can be thought of as established concepts which include false generalizations (Brown, 1958). I would possibly include affective concepts, where a generalized response of fear or pleasure is made to a particular class of environmental objects or events. Lest I be accused of being too all-embracing, I hasten to point out that while differential behavioral processes may exist for these different stimulus dimensions, common to these situations is the fact that similar responses are made to different stimuli; the behavior of the individual is brought under the control of a single property or a special combination of properties (called a rule or definition) while being freed, for that particular concept, from the control of other properties. The number of category responses is smaller than the number of stimulus patterns, and the specific stimuli presented in the course of concept learning may be or may never be exactly the same ones.

More Specific Definition (Stimulus Aspects)

The basic notion of conceptual behavior involving generalization (or stimulus equivalence) within classes and discrimination between classes has been schematized by Mechner (1965) and is shown in Figure 1.1. Each of the ovals surrounds a set of geometric figures

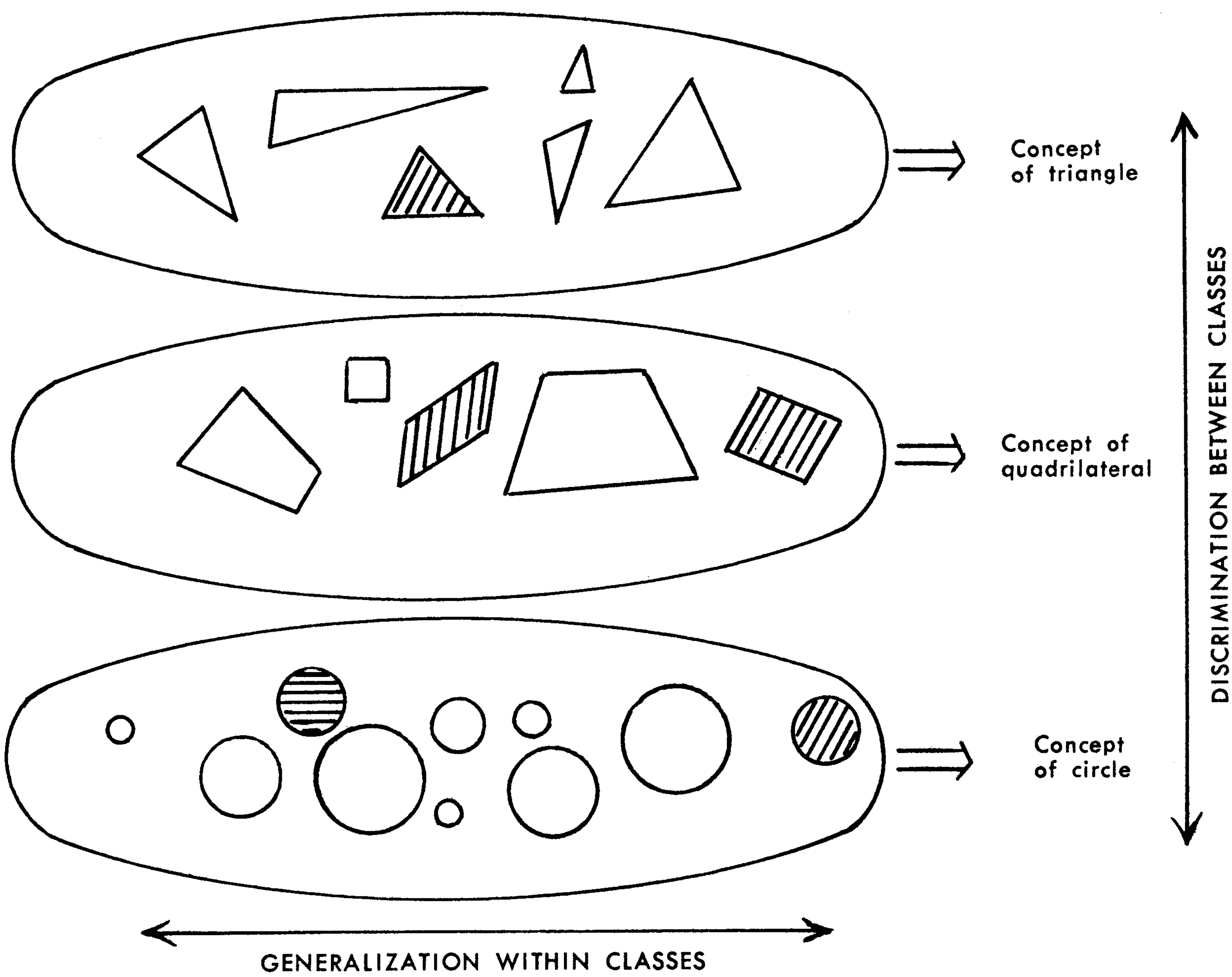

Fig. 1.1. A schema illustrating generalization and discrimination in learning the concepts "triangle," "quadrilateral," and "circle." (Mechner, 1965, p. 462.)

having common dimensions—triangles, quadrilaterals, and circles, respectively. The student learns the three concepts when he generalizes among the figures in each oval and categorizes them correctly, and also when he discriminates between the figures in the different ovals by placing them in different categories. An example of a more complicated subject matter, the interpretation of an electrocardiograph pattern, is illustrated in Figure 1.2 (Mechner, 1965). The concepts involved are the electrocardiograph patterns of three medical concepts. What is involved is not the meaning of the concept names themselves; it is assumed that these are already known, just as a subject in a typical concept formation experiment would know the meanings of shape, size, and color. The boxes in Figure 1.2 enclose electrocardiograph tracings that have common dimensions indicating patterns of ischemia, injury, and infarction, respectively. The student learns to generalize within a set of tracings and to discriminate among sets.

These two examples illustrate a further consideration in conceptual behavior which relates to the familiarity with, or previous degree of learning of, the dimensions which define a concept. In the first example, the stimuli of triangle, quadrilateral, and circle may already be learned patterns, at least for an adult, but in the second example it may be necessary to learn to discriminate the particular wave characteristics involved in the electrocardiograph tracing. Not only the stimulus discriminations (or the perceptual distinctions) involved may need to be learned; in addition, the rule by which the stimuli are combined to form the concept may be more or less familiar. Whether the dimensions and rules involved in a concept are new or established discriminations, or initially clear-cut or easily confusable, obviously influences the nature and rate of conceptual learning.

The two examples are similar in the respect that they both involve what I have been calling direct stimulus dimensions. In Figure 1.3 the sets of words in the boxes have something in common, and concept classes which are defined involve the meaning dimensions of *round,* *small,* and *white,* respectively. The individual learns to generalize within and discriminate among these word classes. An aspect of difficulty here, like the degree of familiarity in the previous examples, is the extent to which each word can be labeled or coded by a well-learned concept name which supplies an underlying dimension of meaning. The similarity and dissimilarity of the words in each class also influences difficulty. The number of word stimuli in a class also influences the complexity of the stimulus set, to the extent that task difficulty, in terms of an easily elicited common class meaning, is a function of the amount of information which the

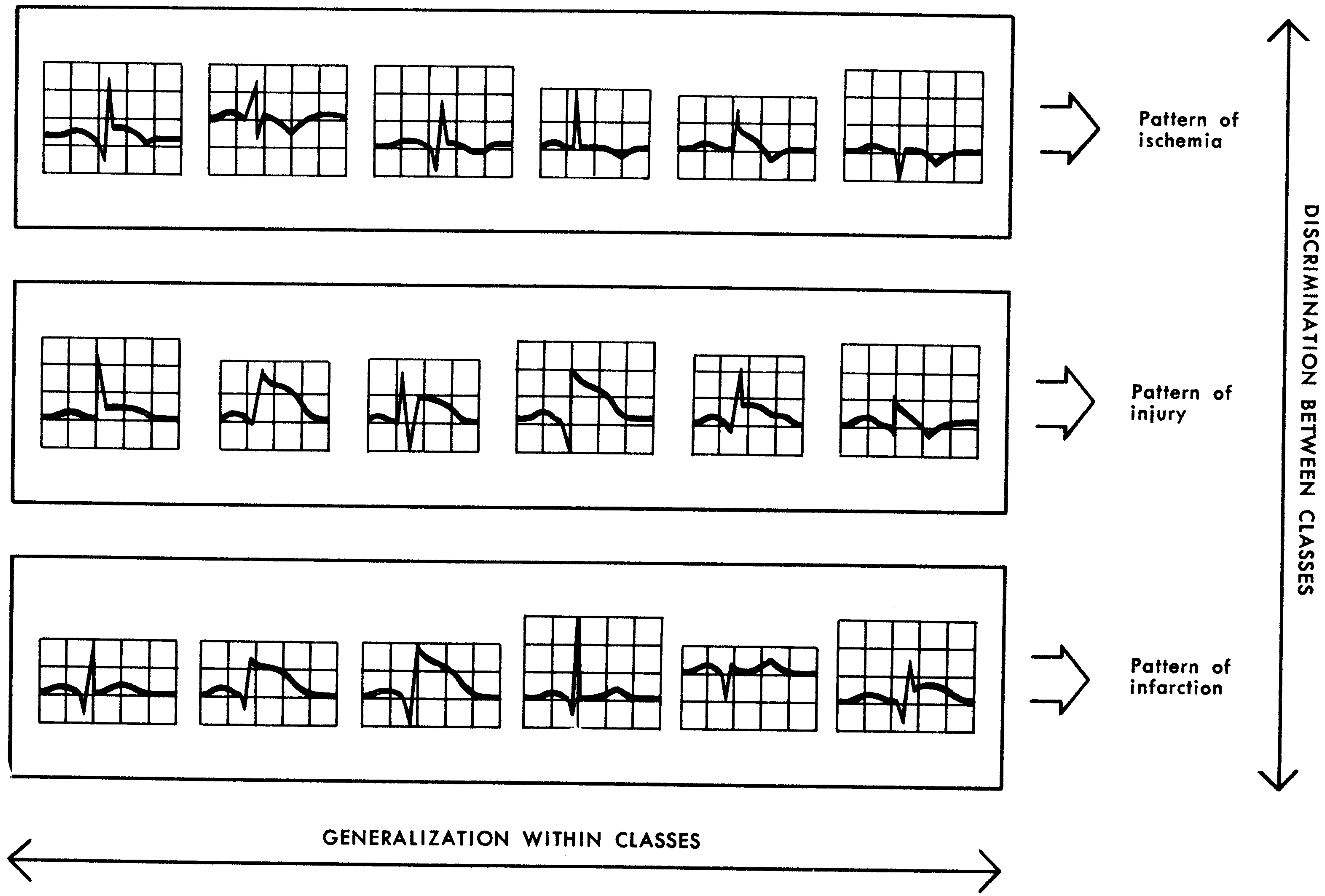

Fig. 1.2. The electrocardiograph tracings indicate three medical concepts, illustrating the three classes of stimulus patterns among which generalizations and discrimination occur in concept learning. (Figure reprinted from Mechner, 1965, p. 465.)

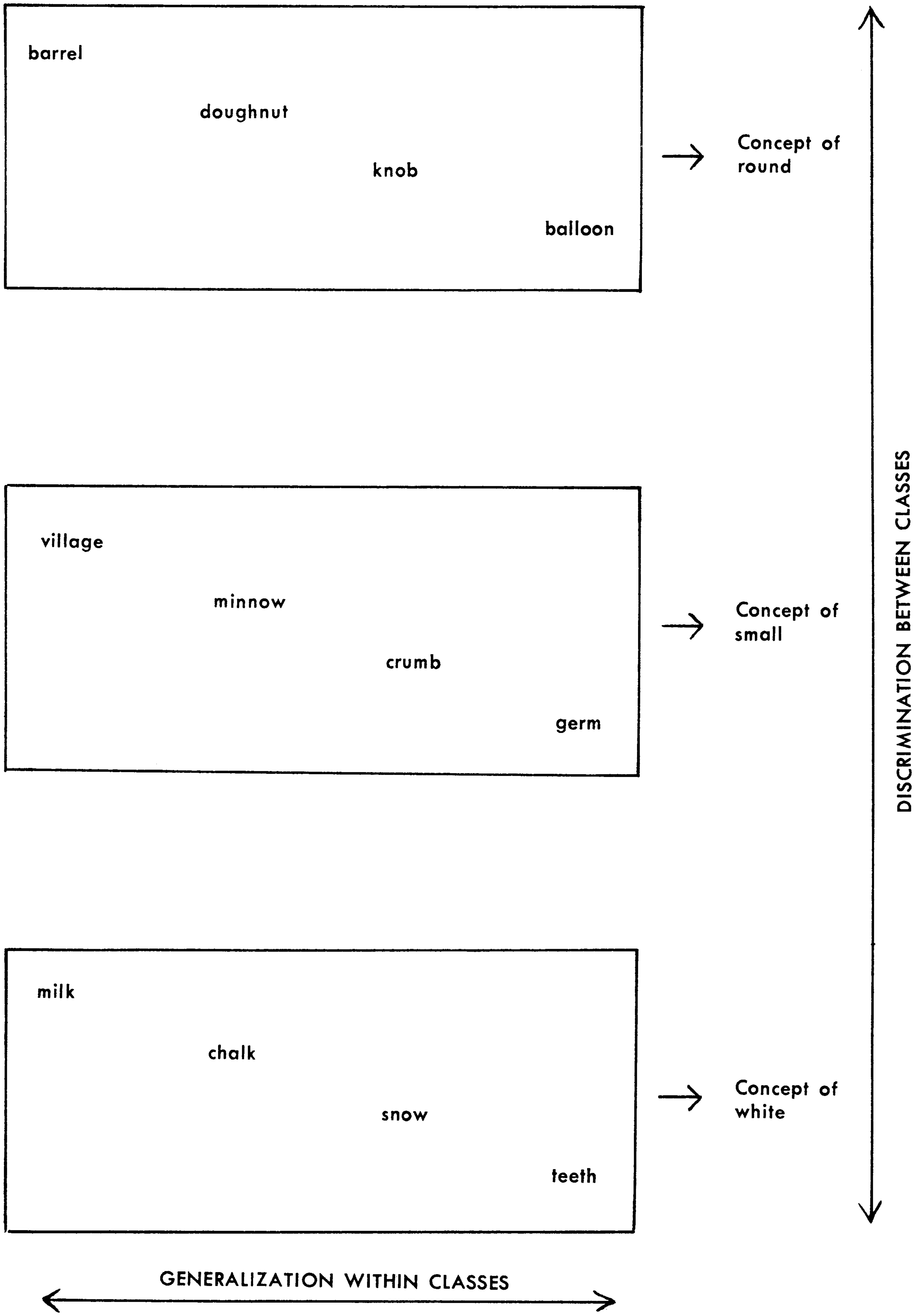

Fig. 1.3. Generalization and discrimination in mediated stimulus dimensions defining verbal concept classes. (Adapted from Underwood & Richardson, 1965b.)

individual has to reduce to come up with a common response (Posner, 1964).

More Specific Definition (Response Aspects)

So far, the nature and definition of conceptual behavior has been considered primarily with respect to the stimulus aspects to which the learner responds, although it is practically impossible to discuss stimuli without implying response characteristics. Now, however, I shall consider response properties in greater detail. Knowledge about whether concept learning has taken place is obtained when the learner responds by making appropriate category responses; he is able to apply the "classification rule" to a new set of instances involving the concept dimensions. It is possible also that classification responses can be made when the individual cannot verbalize the rule involved. This is apparent in young children who have learned to use the grammatical class of verbs correctly but may not be able to verbalize the definition and rule of verb usage. Lack of verbaliza-

TABLE 1.1
Response Systems Illustrating the
Nature of the Response in Concept Learning

Stimulus Dimension			Response System			
Size	Color	Shape	I	II	III	IV
1	1	1	A	A	A	A
1	1	0	B	A	A	A
1	0	1	C	A	B	$\bar{A}$
1	0	0	D	A	B	$\bar{A}$
0	1	1	E	B	C	$\bar{A}$
0	1	0	F	B	C	$\bar{A}$
0	0	1	G	B	D	$\bar{A}$
0	0	0	H	B	D	$\bar{A}$
1 = *large* 0 = *small*	1 = *red* 0 = *blue*	1 = *square* 0 = *circle*				

Note—Adapted from Garner, 1962, p. 312.

tion seems particularly obvious in attitudinal and affective concepts. Some psychologists require the learner to verbalize the categorizing rule involved. Hunt (1962), for example, restricts the definition of concept learning so that the learner, without using examples, must instruct a human to apply the categorizing rule. The bulk of the literature, however, allows that conceptual learning is not necessarily specified by verbalization of the dimensions and dimension combinations involved.

An analysis of the kinds of the combinatorial rules that have been used in laboratory studies of concept formation further illustrates the nature of response required. The kinds of classificatory schema and response systems vary considerably in the literature, and it is difficult to relate experiments to one another. The most useful general representation is the one presented by Garner (1962) and illustrated with some adaptation in Table 1.1. The table shows four different kinds of response systems applied to eight different stimuli generated from three dimensions, each dimension having two stimulus values: size (*large* and *small*); color (*red* and *blue*); and shape (*square* and *circle*). The first response system, I, has eight different instances assigned to a completely unique response: stimulus 1 1 1 (*large, red,* and *square*) is assigned response *A*; stimulus 1 1 0 (*large, red,* and *circle*) is assigned response *B*. Since every stimulus has a unique response, there is no stimulus equivalence or categorizing rule involved; hence, this is not defined as a concept-learning situation.

The second response system, II, has a categorizing schema combining dimensions so that response *A* is appropriate to four instances and response *B* is appropriate to the other four instances. In this case, the response is reinforced with respect to the dimensions of size only; color and shape are uncorrelated with the response. Thus the categorizing response comes under the control of the two values of the size dimension (the response *A* for *large* and response *B* for *small*) and the other dimensions are ignored. It is possible that with this response system, the learner can learn the responses to each of the eight instances by rote, as a paired-associate list. This, according to the properties of conceptual behavior so far described, would not be a case of concept formation, and it would be necessary to have the learner apply his learning to new instances or to verbalize the rule in order to detect this possibility. Perhaps it is just this kind of possibility from which emerges the concern in school learning about whether the student has "just memorized" or "really learned" a concept.

In the third response system, III, the dimensions are again combined according to a rule similar to the second system. It is now necessary for the subject to learn to categorize instances into four classes. In this categorizing scheme, the response is reinforced with respect to combinations of the dimensions of size and color, and is uncorrelated with shape. The four conjunctive classes are *large* and *red*, *large* and *blue*, *small* and *red*, and *small* and *blue:* two relevant dimensions and one irrelevant. The fourth response system, IV, is again similar to the second and third in that the concepts are defined in terms of two variables, one variable being irrelevant. The defining rule is the joint presence of two values of the two relevant dimensions. The learner classifies his response into A or not A ($\overline{A}$). The correct concept is *large* and *red*, and nothing else. This kind of analysis illustrates quite well the kind of relatively simple conjunctive combination of dimensions that generally make up the standard task in concept-formation studies.[3] The relevance of this type of conceptual task to school subject-matter concepts is a question I shall examine later on.

VARIABLES THAT INFLUENCE CONCEPT LEARNING

One indication of knowledge in a field of science is the number of reliable laws that have been established in the field and the relationships among them. These laws express functional relationships between variables; however, the sheer number of variables studied by experimenters may reflect a richness of knowledge or a casting around for information. The variables that have been studied experimentally in concept learning primarily reflect the latter state, but some consistencies may be apparent.[4] I shall attempt here to review what appears to me to be some of the more important knowledge about concept learning.

[3] Some more complex analyses of concept structures used in experimentation are given in Garner, 1962; Hovland, 1952; Shepard, Hovland & Jenkins, 1961; Haygood & Bourne, 1965; and information-processing descriptions in Hunt, 1962; Reitman, 1965; Simon & Kotovsky, 1963; and Gregg, 1967.

[4] A listing and classification of the variables studied in concept learning has been prepared by Klausmeier, Davis, Ramsay, Fredrick, & Davies (1965).

Positive and Negative Concept Instances, and Relevant and Irrelevant Dimensions

Experimental studies make concluding statements like the following: (a) Consistent evidence shows that subjects learn more efficiently from positive instances than from negative instances (Bourne, 1966). (b) Subjects show an inability or unwillingness to use information efficiently based on exemplars of what a concept is not (Bruner, Goodnow, & Austin, 1956). (c) Problems defined by positive instances are more easily solved than those defined by negative instances; mixed sequences are of intermediate difficulty (Hovland & Weiss, 1953). Even when the possible informational content of positive and negative exemplars is equated, there is still an advantage to the presentation of positive instances (Hovland & Weiss, 1953). On the other hand, there is some indication (Huttenlocher, 1962) that negative instances in a mixed series of positive and negative instances can result in efficient learning. In these latter two studies, problem complexity differed, negative instances being more useful in the study involving single-dimension concepts.

The Use of Negative Instances. Few experiments (one exception being Freibergs & Tulving, 1961) have been concerned with the effects of prior training in the use of negative instances in concept learning. This experiment indicates that while practice time was longer for learning to use negative instances than positive ones, subjects eventually used negative instances almost as well as positive. Apparently, students, in our society at least, are more accustomed to dealing with positive instances; negative instances appear to involve an indirect test of the hypothesis and usually provide unreliable or ambiguous information. Studies show that subjects persist in sorting out relevant from irrelevant instances until they are able to make direct tests with positive instances; information from negative instances does not seem to be trusted (Bruner *et al.*, 1956), even though one can logically determine what is the minimal number of positive and negative instances required to define the concept correctly (Hovland, 1952).

The simplest generalizations that can be made seem to be the following: (1) as the information required to define the concept is increasingly carried by negative rather than positive instances, concept learning becomes increasingly difficult; (2) concerning irrelevant dimensions, the difficulty of concept learning is related to the number of relevant instances that define the concept (Shepard *et* *al.*, 1961) and the addition of even a single irrelevant variable adds considerably to the difficulty of the task.

Redundant Information. When redundant information is presented in a concept-learning situation, redundancy of relevant information facilitates learning and redundancy of irrelevant information interferes (Bourne, 1966). Redundancy exists when dimensions overlap. For example, if all red patterns are large and all green patterns are small, the size of pattern is predictable from knowledge of its color, and the information contained in the two dimensions is redundant. However, while performance improves as the result of the increase of the number of redundant relevant dimensions, this improvement is related to the number of irrelevant dimensions in the situation; as the number of irrelevant dimensions increase, there is greater improvement due to additional redundancy of relevant dimensions (Bourne & Haygood, 1959, 1961). The implication is that when many irrelevant dimensions are presented, concept learning can be facilitated by increasing relevant redundancy.

Irrelevant Dimensions. The nature of the irrelevant dimensions that interfere with concept learning has not received the attention that has been devoted to the study of relevant dimensions and their combination into classification rules. Most frequently, the learner's task is to discover some kind of rule or principle of internal structure concerning the relevant dimensions that define the concept. This is made easier if the irrelevant dimensions do not also form an easily perceived structure among themselves (Garner, 1962). In a concept-learning situation, the subject is oriented to find structure among the stimuli presented to him; if the relevant variables are seen as structured, and the irrelevant variables as unstructured, then the concept structure will be easier to induce. Interference, however, will occur if structure is also perceived among the irrelevant attributes.

Order and Sequence

The order and sequence in which instances of a concept are presented to the learner would seem to have a significant influence on concept learning. However, sequence variables have not been studied very much. Hovland and Weiss (1953) studied sequence by presenting mixed orders of positive and negative instances, but reported no effects of special importance. A most interesting study of sequence (Detambel & Stolurow, 1956) concerned itself with change in the sequence of presentation of relevant and irrelevant dimensions from trial to trial. It was assumed that learning would be facilitated when the same relevant dimension was presented on adjacent trials if as many as possible of the irrelevant dimensions had different values on these trials, and, correspondingly, when two different relevant dimensions were presented on adjacent trials, if as many as possible of the irrelevant stimuli were kept constant. This "asynchrony" between relevant and irrelevant stimuli was hypothesized to maintain the discriminability of the relevant stimuli and to enhance concept learning. In this experiment the asynchronous presentation sequence showed a striking superiority in contributing to the efficiency of concept learning.[5]

Reception and Selection Paradigms. The two main experimental paradigms used in the study of concept learning call attention to the variable of sequence. The classical method, originally carried out by Hull (1920), has been most used and is called the "reception" paradigm. In this procedure the subject is told that his task is to categorize a group of stimulus instances. Each instance is presented to the subject one at a time, and he is required to respond by placing it in the appropriate category. After he is told whether he is correct or not, the stimulus is removed and a new one presented. The second paradigm is called the "selection" paradigm (developed largely in the work of Bruner *et al.*, 1956). In this procedure, the subject is presented with the entire population of stimulus instances at the outset. The experimenter designates one member of the population as a positive instance. The subject then is allowed to select an instance and ask whether it is positive or negative. This procedure allows the subject to set up his own sequence and to decide upon a strategy in solving the concept problem.

Induction and Deduction. The similarity rather than the difference between these two paradigms brings up an interesting point about sequencing: almost exclusively, all studies of concept formation have been studies of inductive behavior, that is, the instances are presented and the rule must be induced. In contrast, it appears that in school instruction a concept often is taught by stating the rule first and then providing the learner with instances that illustrate

[5] R. C. Anderson (unpublished paper) followed up this study using a longer series of transitions than adjacent trials and confirmed only part of the findings of the Detambel & Stolurow (1956) study. Anderson concluded that his study "extends the previous result by showing that the effective variable is not merely the number of relevant stimulus dimensions in the concept learning task, but the number of these that changes from trial to trial."

and refine it. The question to be asked is whether studies of concept formation tell us anything about modes of instruction which present rules as "instructions"[6] rather than inductions. There has been little work on how behavior is brought under control by "instructions," although this is an effective and frequently employed means of teaching.[7] I shall return to this point later in considering the teaching of concepts, particularly stressing the distinction between relatively clear-cut and more subtle concepts.

Salience, Dominance, and Perceptibility

The early experiments of Hull (1920) and Heidbreder (1946a, 1946b, with Bensley & Ivy, 1948, with Overstreet 1948) point out that dimensions of a concept may be more or less conspicuous or perceptible. Generalizations made about the influence of number and combination of relevant and irrelevant dimensions seem to require qualification with respect to the salience of the dimensions involved. Heidbreder's experiments report that concept attainment occurs with decreasing ease when the instances consist of concrete objects, form, and number, respectively. Follow-up experiments by Grant, Jones, and Tallantis (1949) and Grant and Curran (1952) tend to agree with this order of relative difficulty in a somewhat different task, and indicate that the salience seems inversely related to perseveration on an irrelevant dimension in later stages of learning. For example, a number sorting task in which the number dimension was of low salience was distinguished by marked perseveration; this is interpreted to mean that it is more complex for the learner to abstract. Taken as a group, these experiments by Grant *et al.* and Heidbreder and more recent ones by Wohlwill (1957) do not lead to any clear-cut interpretation of the influence of the salience of concept dimensions, although a possible generalization is that concrete objects are more readily conceptualized than abstract attributes. Salience seems compounded with response sets and individual differences that are brought to the situation. These response sets can be idiosyncratic, or they can be determined by societal norms which influence developmental and associative patterns. For example, there

[6] I shall use the word "instructions" in quotes to mean giving directions or describing a rule, as distinguished from the word *instruction* in the more general sense of an instructional system.

[7] Goldiamond (1966) makes the distinction between "instructional" and dimensional control in concept learning, and Bourne (1966) and Carroll (1964) emphasize the necessity for studying "rule learning."

is a consistent finding that young children prefer color and older children prefer form, and that such preferences influence the salience in concept formation (Brian & Goodenough, 1929; Lindberg, 1938; and Suchman & Trabasso, 1966). However there are no direct studies of children's perceptual preferences and their influence upon concept learning.

Prior Associations. In verbal learning there has been some investigation of the effects of prior associations to stimuli that are used in concept-learning tasks. Dissimilar verbal labels serve to facilitate discrimination between concept classes, and similar verbal labels facilitate within class generalization (Kendler, 1961). Concept learning with verbal dimensions occurs more effectively when the concept instances have a strong common descriptive response (a high dominance association) than when they have a weaker common association (Underwood & Richardson, 1956b). When the class-descriptive word is of low dominance, there are more intruding associations, which slow down concept learning. (See also Baum, 1954; Gelfand, 1958; and Freedman & Mednick, 1958.)

Stimulus Patterns. Studies of the way in which stimulus patterns are learned are of interest with respect to stimulus perceptibility in concept learning. For example, in a preliminary experiment Binder (1966) studied the discrimination of geometric stimuli on the basis of their patterns and of partially relevant subsets of these patterns. The subsets were found to be important during early trials; in later trials full patterns rapidly started dominating, and discrimination accuracy on the basis of common subsets decreased. This was interpreted to mean that except for the preliminary exploration of stimuli, components and intermediate patterns of a stimulus are not learned until the full patterns are adequately learned. It is, then, in later learning that a closer examination of stimulus characteristics is involved. This can imply that the learning of appropriate responses to components and intermediate patterns in a complex stimulus which defines a concept rule occurs by initially learning responses to the total pattern, and then learning to abstract details and responding accordingly. It is of interest to juxtapose with the study just described another study which involved training in the recognition of complex, visual stimuli (Marx, Murphy & Brownstein, 1961). In this latter study it is reported that the recognition of complex stimuli was more effective when the learner was trained on simplified abstracted versions of the stimulus pattern rather than on the final stimulus itself.

TABLE 1.2

TASK CONDITIONS AFFECTING CONCEPT LEARNING

I. The Definition of the Task
What does the person take as the objective of his behavior?
What does he think he is supposed to do?

II. The Nature of Instances Encountered
How many attributes does each exhibit?
How many attributes are defining and how many noisy?
Does he encounter instances at random, or in a systematic order?
Does he have any control over the order in which instances will be tested?
Do instances encountered contain sufficient information for learning the concept fully?

III. The Nature of Validation
Does the person learn each time an instance is encountered whether it is or is not an exemplar of the concept?
Is such validation only available after a series of encounters?
Can hypotheses be readily checked or not?

IV. The Consequence of Specific Categorizations
What is the price of categorizing a specific instance wrongly and the gain from a correct categorization?
What is the price attached to a wrong hypothesis?
Do the various contingencies—rightness and wrongness of a categorization of "X" and "not-X"—have a different price attached to them?

V. The Nature of Imposed Restrictions
Is it possible to keep a record of instances and contingencies?
Is there a price attached to the testing of instances as a means of finding out in which category they belong?
Is there pressure of time to contend with, a need for speedy decisions?

NOTE—Adapted from a discussion by Bruner, Goodnow, and Austin, 1956, p. 56.

On this topic of salience, dominance, and perceptibility the studies are preliminary and difficult to interpret with respect to their general relevance to concept teaching. On the other hand, such properties of the stimulus seem to influence the way in which conceptual stimulus dimensions are learned; as such they need to be emphasized as a relatively unexplored area which requires further study.

Feedback and Response Contingencies

There have not been many experiments which explore feedback contingencies in concept learning. This is surprising, since contingencies of reinforcement and information feedback are apparently powerful variables in their influence on behavior, and have been intensively studied in other areas of psychology. Along these lines, references can be made to the following studies: reinforcement schedules (Green, 1955); probability matching (Grier & Bornstein, 1966); the nature of information feedback (Buss, 1953; Buss & Buss, 1956; Suppes, 1965); probabilistic concepts (Azuma & Cronbach, 1966; Bruner *et al.*, 1956; Suppes, 1966); time factors in feedback relationships (Underwood, 1952; Green, 1955); and memory requirements (Bruner, *et al.*, 1956; Hunt, 1962).

Task Conditions and Types of Concepts

In the studies by Bruner, Goodnow and Austin (1956), a detailed discussion was given of task conditions that affect concept learning. These are listed in Table 1.2. I refer you to their discussion and make the comment that we have barely scratched the surface in analyzing such variables.

Concept learning experiments also have begun to investigate the learning of different types of concepts. For the most part, different concept types have been defined in terms of logical operators, primarily conjunction and disjunction (Bruner *et al.*, 1956; Hunt & Hovland, 1960; Neisser & Weene, 1962; Haygood & Bourne, 1965; and Bourne, 1966) and to a lesser extent, exclusion, negation, and certain conditional rules (Haygood & Bourne, 1965). In general, the empirical finding is that more complex concepts are more difficult to learn and that logical complexity is a factor in this difficulty. It also seems apparent that, because of prior experience in society, certain logical relationships like conjunction are more familiar than others; and this influences our experiments.

Examination of the experimental literature further makes clear that in the press for experimental purity and for the building up of a fund of knowledge around a consistent type of laboratory task, many types of concepts related to school learning have been neglected. This neglect relates to at least two characteristics. First, many of the concepts in school learning are relational rather than conjunctive or disjunctive (the latter two being the primary types of laboratory concept studied). To say this another way, many difficult school-learning concepts deal with relations among dimensions rather than their combined presence or absence. For example, concepts like "many," "few," and "average" require the learner to think in terms of the relationships between a base quantity and a reference quantity.

Secondly, new concepts learned in school often depend upon dimensions which themselves represent concepts, that is, they depend upon a network of prerequisite concepts. "One cannot very well learn the concept of derivative, in the calculus, until one has mastered a rather elaborate structure of prerequisite concepts (e.g., slope, change of slope, algebraic function, etc.)" (Carroll, 1964, p. 190). This notion of the hierarchical structure of concept learning has been emphasized by Suppes (1966) in his work on teaching mathematical concepts to children. He points to a structural ordering of concepts of identity and suggests how positive and negative transfer might be predicted as the learner proceeds along a hierarchical structure. Suppes points out that in the usual laboratory concept-formation study there is little possibility of defining any sort of substantial substructure of the kind existing in "real" subject-matter learning. Research on the hierarchical nature of school concepts will undoubtedly emphasize, more than ever, the importance of measuring transfer effects as a way of assessing the effectiveness of instruction.

Consideration of types of concepts brings up other matters. It has been pointed out (Bourne, 1966) that concept-learning problems in which the rule is neither familiar nor simple have not been studied very often. Typical concept-learning studies have emphasized the identification of relevant attributes, and once the relevant attributes have been identified, the rule involving them is trivial or previously indicated in some way to the learner. "Problems wherein the rule is an element to be learned or discovered have simply not been studied very often" (Bourne, 1966, p. 74). A further point regarding types of concepts is that little information is available on concept formation in different sensory modalities, for example, auditory concept formation, which seems relevant to the teaching of music and sensitivity to language tones.

The Influence of Language. As implied throughout this chapter, language concepts and the influence of language on concept learning are essential aspects of school learning. Words come to stand for concepts that may have been learned pre-verbally or learned with the assistance of verbal labels. Some psychologists have defined concepts as meaningful words which label classes of otherwise dissimilar stimuli (Archer, 1964). However, it is to be noted that most of the work called concept learning by psychologists has been limited to laboratory situations involving direct, non-verbal dimensions. Any empirical knowledge that has been obtained is primarily relevant to situations in which there is a one-to-one correlation between a categorizing response and the presence of a physical stimulus dimension or pattern. There have been some notable exceptions, however. (For example, Underwood, 1952; Underwood & Richardson, 1956b.) In these latter studies, common associations between words form relevant and irrelevant dimensions. For example, for two lists which represent two different associative categories, cross-category associates can be considered as irrelevant dimensions which interfere with learning.

An especially significant difference between studies of non-verbal and verbal concept formation is the necessity, in the verbal studies, of obtaining normative association data prior to carrying out an experiment. The dimensions along which discrimination and generalization take place exist as a result of prior verbal habits whose presence must be ascertained since they are not as readily observable as behavior brought to the experimental situation relating to direct, non-verbal dimensions. One cannot help but wonder whether the determination of normative data prior to the conduct of the traditional concept-learning studies (for example, with respect to salience), might not have resulted in more definitive findings. Suppes (1966) appears to be investigating the question of prior learning with respect to the behavior that children bring to the perception of geometric forms.

There is evidence concerning the influence of words in traditional concept-learning studies which indicates that the ability to use words is an important factor in the speed of concept acquisition. For example, children learn to categorize non-conventional forms faster when distinctive names are applied to them (Dietze, 1955).

Verbalization, when compared with non-verbalization, facilitates concept learning in four-year-olds, but makes little difference for seven-year-olds because they verbalize the solution anyway. However, instructing the seven-year-olds to verbalize the irrelevant dimension delays the learning of a reversal shift (Jensen, 1966). The correlation between verbalization of a rule and correct responding is unclear. For example, in a study with college students, verbalization covaried with correct responses, but was not necessary to making the correct response (Green, 1955). In studies with children, verbalizations are not always a guarantee that their choice or behavior will be appropriate (Kendler & Kendler, 1962).

The introduction of language into the study of concept learning brings up the question of the definition of the stimulus dimensions involved and emphasizes mediational processes such as have been studied by the Kendlers (1961, 1962). The fact that there is a difference between children and adults in performing solution shifts in concept problems recalls the previous point concerning the determination of prior verbal habits. The nature of the verbal repertoire that exists prior to concept learning will determine the way the concept is learned, and consequently, the way in which it can be taught. Even though we know little about postulated mediational processes, it would probably be profitable to study empirically the influence of the entering verbal habits of the learner in investigations relevant to instructional design.

Individual Differences

With respect to the interaction between individual differences and learning variables, the situation in concept learning is no different from the state of knowledge in other fields of learning. On the whole, the two disciplines of psychometrics and experimental psychology have developed in separate ways, and the amount of work on individual differences in learning is meager. While experimentalists interested in learning and concept formation have expressed concern with individual differences, this concern for the most part has never risen above the threshold of sustained action (Glaser, 1967). There exist relatively isolated studies (covered well in a chapter, by Bourne, 1966), but for the most part, theories and models of concept learning have not been hospitable to the introduction of individual-differences parameters.

There are, however, exceptions; in the work of Bruner, Goodnow and Austin (1956), there is a rather intensive look at differences in strategy as a function of individual differences and task requirements. Experiments are described which study how changes in task variables, such as the demands of memory, influence the efficiency of the learners using different strategies (see also Chapter 4 in Wallach & Kogan, 1965). Of recent interest is the amenability of information-processing analyses of conceptual learning to the description of individual differences. An example is provided by Gregg's (1967) study on the learning of sequential concepts. The subject was required to learn the correct setting of four switches and the particular rule by which one setting could be transformed into the next serial setting; for example, the settings could be arranged in a series which corresponded to the first sixteen binary numbers. Post-experimental protocol analysis in which the subjects were questioned about their particular performance indicated different "internal representations" by which they learned the task. Some subjects learned the switch settings as a single list of items, and developed a strategy of keeping track of their place in the list so they could move from one correct setting to another; another subject treated the four switch settings as a cycle of two pairs of switches, and learned the list of settings by visualizing the cyclical relationship between the right- and left-hand pairs; other subjects developed compound concepts in which the list was broken up into separate parts and each part treated as a separate concept.

Theories

Concept-learning studies have provided data for the development of some interesting theories of the concept-learning process. However, in the context of this chapter, these theories do not contribute new information about concept learning which might be relevant to research on concept teaching. Theoretical descriptions and formalisms introduced into the concept-formation area have generally been used to flex the muscles of the theories themselves and examine how adequate they are to describe experimental data. The stochastic mathematical models of Bourne and Restle (1959) and of Bower and Trabasso (1964) have handled only the simplest situations. The mathematical models, however, have sensitized us to the issue of incremental versus one-trial learning, a question which may be quite significant to classroom learning (Suppes, 1965; Grier & Bornstein, 1966). The information-processing model descriptions of Simon and Kotovsky (1963), Hunt (1962) and Reitman (1965) are of interest in two ways: (1) providing a description of the characteristics of skillful conceptual performance; and (2) suggesting a methodology which offers rich descriptions of the individual differences among learners in solving concept-learning problems (Gregg, 1967). In general, it would seem that concept learning, standing in a central position between basic and more complex behavioral processes, should be one of the main points of contact between various theories of behavior, as well as between behavior and its theoretical description.

RESEARCH RELEVANT TO CONCEPT TEACHING

If we analyze the nature of concept learning as studied in the laboratory, as I have attempted to do, including the nature of concept tasks as they appear in subject-matter learning, we may be in a position to say how the psychologist's knowledge is applicable to school learning and in what respects it is inapplicable or fails to reflect certain task requirements.

For the most part, the concept-learning task situation about which we have most knowledge can be defined as follows: it consists of physically describable, non-verbal dimensions. These dimensions consist of stimuli which are generally familiar and easily perceived by the learner. The dimensions are combined by simple, logical relations, usually conjunction or disjunction. Irrelevant dimensions and irrelevant combinations of dimensions are present at the beginning of learning and during its course. Generally, concept instances or exemplars are presented one at a time in approximately random order; less frequently, the entire set of concept instances and non-instances is presented at the outset. As the learner responds to a concept instance, he receives continuous feedback of the appropriateness of his response. Usually, the stimulus presentation is not under his control except when the population of instances is presented in full and the learner can select an instance and obtain appropriate feedback. The response required by the learner is an inductive one in which the rule that defines the concept is generated on the basis of experience with instances and noninstances, and the learner indicates his attainment of a concept by placing an exemplar in a correct category, by giving it a correct category label, or by verbalizing the rule that defines the concept.

In light of this description, the question to be asked is: What properties of subject-matter concepts are *not* present in the studies on which our knowledge of concept learning is based? This "not" is pointed to by studies taking place off the mainstream, and by a rough analysis of the kinds of concepts taught in school. Generally, what seems to have been neglected are the following:

1. Concepts that are comprised of dimensions that are verbal, thematic, and meaningful, and involve a mediated response. Stimulus value along these dimensions are more difficult to pinpoint than they are for more direct non-verbal dimensions, and dimensional properties must be predetermined along some behaviorally constructed scale.
2. Concepts with definitional boundaries that change with increased experience and knowledge, like the concepts of mass and weight in physics. Essentially the dimensions making up these concepts form probabilistic concept rules where feedback and reinforcement does not occur on a continuous basis.
3. Concepts in which the salience or preceptibility of different dimensions differ as a function of societal norms, differing preceptual characteristics of the stimuli involved, or individual learner histories.
4. Concept types which are of a relational character, as is true of many scientific concepts dealing with the relationships among dimensions, rather than of a conjunctive character pertaining to the combined presence or absence of certain dimensions.
5. Concepts which should be learned "deductively," that is, the rule first and then exploration of its instances and non-instances. For concepts where the boundaries are reasonably precise, it is probably more efficient to learn the concept deductively. The more precise a concept and greater the ease in verbalizing its dimensions, the easier it is to state the rule. In these cases, it seems reasonable to teach the concept beginning with a formal definition or statement of the rule which identifies the relevant dimensions, their values and relationships. Following this, the student can practice using the rule. This kind of concept teaching has been little studied. On the other hand, where verbalization of the rule is difficult and concept boundaries are unclear, inductive teaching may be required (for example, when teaching the concept of freedom). It seems contradictory that in psychological experiments we have been studying the inductive learning of just the types of concepts that might best be taught by presenting the rule first.
6. Concepts based upon a hierarchy of previously learned concepts, such as the concept of a derivative, which is based upon a structure of prerequisite concepts including slope, change of slope, and algebraic function; or the concept of mass, which requires previous learning of the concepts of the pull of gravity and acceleration. Some of the concepts that Piaget has studied are obviously relational and hierarchical, as in the relationship of

size to the concept of the conservation of volume (Carroll, 1964). The hierarchical structure of subject-matter concept learning makes such learning dependent upon facilitating or inhibiting positive and negative transfer along the hierarchy. This kind of transfer problem in concept learning seems very relevant to school learning and has only recently been experimentally touched upon by Suppes (1965), but has not been extensively investigated at all.

7. Concepts whose acquisition de-emphasizes memory by providing the learner with a strategy which minimizes memory requirements, or by providing him with tools or job aids for memory storage.

I have described the primary kind of concept-learning situations, including the variables involved, that have been studied by psychologists, and have listed the properties of concepts that appear to be relevant to school subject matters, which we have hardly begun to study. It should now be possible to examine the types of concepts that have been studied in order to identify the kinds of subject matter for which they appear to be appropriate task models. Alternatively, it should be possible to describe the kinds of concept situations which may be relevant to school subjects, and to determine what particular kind of research should be encouraged.

My examination of these two categories leads to the following conclusion: We know something about concepts consisting of non-verbal dimensions where the stimulus values are perceptually clear, and where the instance–non-instance boundaries are reasonably clear, and further, where such concepts involve rules that are taught by an inductive procedure. This kind of concept task seems to define the situation that prevails in two cases: (1) in non-verbal tasks and in concept learning with pre-verbal learners, that is, young children; this kind of situation is exemplified by the inductive learning of such concepts as triangle, quadrilateral, and circle; and (2) tasks for verbal learners where a concept to be learned is intricate to verbalize so that it needs to be learned by induction from exemplars, such as in the previous example of the electrocardiograph pattern (Figure 1.2). However, there is a contradictory side to this picture which is also twofold: (1) when verbalization of a concept rule is clear-cut so that the concept can be taught deductively, the inductive paradigms studied in the laboratory seem less relevant to school learning; and (2) it is only when concept boundaries become relatively fuzzy, that it seems best to teach them by the method we have primarily studied in the laboratory, the inductive method.

The conclusions just stated are largely based upon properties of the concept task, that is, the nature of the dimensions, the relations between them, and the definitiveness of class boundaries. These dimensional properties of a concept may exist in the stimulus characteristic of the subject matter, or they may exist by virtue of the entering behavior of individuals who perceive the stimuli in different ways, as clear-cut or fuzzy, as verbal or non-verbal, and so forth.

The procedures used in experiments are also relevant to my conclusions. In most experiments of the traditional concept-learning variety (whether or not the selection or reception strategy is used), the procedure goes as follows: The learner is told to classify a set of instances and that he will be given appropriate feedback; no information is given about the relative perceptibility of the various dimensions to the learner, nor about whether the learner has a good grasp of underlying or more basic concepts out of which the concept to be learned is formed. (This latter point should, however, be tempered by a recognition that the concepts generally studied do not have any very deep conceptual structure.) In the course of learning, the feedback information is usually a binary right-or-wrong. The concept instances are presented randomly so that the learner goes through a classical laboratory discrimination-generalization procedure in which many incorrect responses occur until the appropriate classification response is learned. Little or no attempt is made to adapt to the learner's response history; information about the nature of errors made is not given; and no attempt is made to minimize the occurrence of a long sequence of incorrect responses. In general, the learning conditions are kept quite rigid so that the experimenter can study the learning process under some conventionally accepted conditions.

This rigidity, together with the desire to lengthen the acquisition curve with a procedure which results in slow learning in order that the learning process can be investigated, seems to present a situation in which the acquisition of the concept is actively prevented and delayed. Under these conditions, it is not unusual for an experimenter to say something like the following: "in this concept-formation experiment, the children who failed or had difficulty in learning the concept seemed to show behavior in which they failed to use verbal labels or verbal mediators, and as a result, perseverated on false position cues and on irrelevant cues. This perseveration occurred to the extent that it seems that the children were not adequately sensitive to reinforcement contingencies."

To my way of thinking about instruction and teaching, such a statement implies that the type of learning environment which was utilized to study concept learning was not designed to elicit the appropriate behavior from the child although it involved the appropriate rigidity for certain kinds of experimental study. For the study of instruction, a different strategy is needed. This is particularly obvious in the light of the following requirements for research leading to the design of the instructional process.

The first requirement is analysis of the characteristics of subject-matter competence. Prior to instruction it is necessary to analyze the characteristics of the criterion behavior to be performed by the learner; this is the behavior that signifies that learning has been accomplished and skillful performance achieved. The analysis requires 1 specification of the stimuli which must come to control behavior 2 the conditions under which the performance occurs, and 3 some specification of the nature of the response that indicates competent performance. For research in concept learning we need to specify such considerations as the following: (a) the kinds of dimensions involved and how these dimensions are combined; (b) whether this combination can be verbalized by an explicit rule; (c) whether the stimulus values of the concept are easily perceived or whether they can be made more easily perceptible; (d) whether the dimensions are scalable or whether scaling procedures need to be carried out prior to experimental study; and (e) the memory-storage requirements and amount of information-processing involved. Analyses of this kind may put us in a position to investigate the learning and instructional process relevant to different kinds of concept tasks and to discover the generalities and specificities of each class. As the review of the literature shows, we have been studying a limited set.

The second requirement is embodied in the statement that since instruction is defined as taking the learner from some initial repertoire to a performance repertoire defined as criterion performance, it is necessary to ascertain the pre-instructional behavior of the learner to which subsequent instruction must adapt. This second requirement seems obvious enough, but as has been recently stressed many times in connection with the modern emphasis on individualized education, the fact of individual differences is honored more in the breach than in the observance. Recently, Gagné (1967) has edited a symposium volume devoted to the topic of learning and individual differences. The volume points to what I hope is a future emphasis in learning studies, but also it is a striking testimonial to the paucity of information about the interaction of entering behavior with subsequent learning requirements. At the present time two approaches in the study of learning seem sensitive to individual differences: one is Skinner's approach to the analysis of behavior, and the other is, as I have previously stated, the information-processing model exemplified in the paper by Gregg (1967).

Requirement three is suggested by the following question: Knowing the initial and criterion repertoires, how can the instructional process be implemented? Satisfying this requirement involves setting up new forms of behavior, putting already learned behavior under the control of new stimulus combinations, and supplying the learner with the facility for maintaining and extending skillful behavior once it is learned. The accomplishment of these ends requires, at the very least, knowledge of such factors as the appropriate sequencing of instructional presentations, the interfering and facilitating transfer effects resulting from entering behavior and response histories in the course of learning, and the appropriate application of reinforcing contingencies. In this context, I shall mention some important research areas for concept learning relevant to concept teaching.

1. One area of study can be labeled "concept hierarchies, sequencing, and transfer." This group of phenomena seem to be of special importance, since it appears that a body of subject matter in school consists of successive levels of embedded concepts, each using the previous set of concepts to build deeper ones. If this is the case, it is necessary to lay out the subject-matter structure and to determine the sources of positive and negative transfer and how learning proceeds from one concept level to another. The logic and epistemological structure of the subject matter cannot necessarily be assumed to provide the most efficient learning hierarchy, although it has been suggested (Suppes, 1966) that this correspondence does take place. The transfer of learning from concept to concept seems especially relevant for school subjects and has only been minimally studied in concept-learning experiments. Sequencing and the requirements for it are also related to the teaching of the stimulus and response discriminations and equivalences required in concept acquisition. Although much trial-and-error learning is permitted in the traditional experiment, it seems appropriate to investigate more systematic ways of teaching discriminations through the ordering of perceptual difficulty (Gibson, 1965), and through the employment of the techniques of stimulus fading (Terrace, 1963, 1964; Taber & Glaser, 1962).

2. A second area for research is the use of language and verbalization in concept learning. Here resides a host of implications for instruction, only some of which have been investigated by more than a few experiments. It is obvious that we know little about how students learn from verbal "instructions." As I have indicated, the problem has been avoided in concept learning by neglecting the study of deductive concept learning or rule learning starting with the presentation of the rule followed by a series of exemplars. The influence of "instructions" which define the problem for the learner and provide him with information about the nature of the concept to be learned so that he can establish a pre-learning set also needs to be studied. Research on how rules and "instructions" are presented for teaching is not forthcoming from the concept-learning studies, but it does have relevance for classroom instruction.

3. A third area is concerned with response contingencies and informational feedback. At least two things seem apparent here: one is that the consistent finding that individuals do not learn very well from negative instances might be an artifact of the traditional concept-learning experiment. It seems reasonable to expect, and there is some evidence to show, that with appropriate pretraining and appropriate feedback, individuals can learn efficiently from appropriate responses to negative instances. The second point is that investigation is needed of how the scheduling of reinforcement influences concept formation and its underlying processes of discrimination and generalization; there has been little work on this question. There also has been little investigation of the appropriate reinforcement contingencies in different kinds of subject-matter learning. Is effective reinforcement a matter of providing information about right and wrong classifications, or is it something intrinsic to developing skill in the subject matter, like the increasing ability to manipulate and categorize more and more situations using the concept, or using the concept to learn a new one? As long as such scholarly contingencies are immediate events in the school-learning environment, extrinsic reinforcing stimuli such as used in laboratory experiments may be neither necessary nor applicable.

4. A fourth requirement is the necessity for a strong theory of concept learning and conceptual performance. As an instructional designer, I would require theoretical formulations in my underlying science which will lead me into developmental efforts and which will absorb the phenomena I encounter in making these developments. At the present time, my work is of necessity highly empirical. I

cannot at all deduce very deeply, from what I know, testable requirements for teaching a concept. Although I can, on the basis of experiments, generate hypotheses which will be helpful in improving the instructional process, too much cut-and-try is necessary. With deep, accommodating theories or formalisms, I should be able to operate more satisfactorily as an applied scientist.

A final statement: My review of the psychological studies of concept learning, as they are relevant to research on concept teaching, should lead to the conclusion that I have not said that concept-formation studies are totally inapplicable to teaching, but I have said that they are applicable to a narrow domain of subject matter. The lack of analyses of the nature of competence in different conceptual tasks, the rigidity of the experimental procedures in adapting to response histories, the neglect of individual difference-learning process interactions, and the lack of strong theories, have kept the field much less relevant than it should be to generate research applicable to the problems of how concepts are learned and how they should be taught.

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AUDIENCE QUESTIONS

• *Question:* You seem to imply that instruction might be more efficient if you proceeded deductively. Did you choose to ignore that there might be some characteristics of inductive learning that might be beneficial, even though you could get another kind of efficiency by using deductive approaches?

• *Dr. Glaser:* No, but that's an excellent point. What you get in the long run, if you use inductive and deductive procedures in the same task situation, is an experimental question. I don't know. I would guess that when you have a clear-cut rule, you can tell somebody the rule, and he can then practice with instances of it; but I don't know whether, if you have him induce a clear-cut rule, you come out with any different kind of behavior. On the other hand, as I have said, if the rule is fuzzy, telling him the rule is not as effective as having him induce it. I don't know the answer, but I would expect that there isn't that much efficiency in making a student rediscover everything that has been discovered before.

• *Question:* It seems to me that completed concept-learning studies seem to emphasize the getting of answers; that the end point in the system is getting the answer. One of the difficulties in concept learning or training is getting children to ask the questions which seek information that will lead them to the answer. That seems to be an important part of the process which is neglected in the kinds of unresponsive experiments that have been used. Would you comment on this problem?

• *Dr. Glaser:* Yes. It's like the question of whether you are teaching him to discover, or teaching him *how* to discover. You want the student to discover the process of learning, to be a good inductor. Is that the kind of distinction you are making? I guess I don't know of much work in which the concern is to teach concept formation in a way that forces somebody to be a good concept-inducer.

• *Question:* You made a point about clear-cut rules: What would be a clear-cut rule? Is there such a thing that is a clear-cut rule for everyone? In other words, won't you have to have some sort of reference to the learner before you can say it is a clear-cut rule?

• *Dr. Glaser:* Well, the rules used in the laboratory all concern object properties, you know, such as "yellow" and "round." That's pretty clear-cut, and it's the kind of rule I had in mind. However, if I don't know what "yellow" or "round" means, I am going to get into trouble. So there is some problem there.

• *Dr. Lumsdaine:* In contradistinction, you would like concepts of "short," "brutish," and "nasty"?

• *Dr. Findley:* Would you be good enough to expand on your comments about the inadequacy of research where negative instances have been used in teaching? Is the difficulty that the negative use has been excessive, and not balanced against the positive? Or what is the nature of the inadequacy?

• *Dr. Glaser:* There is a long history of research, as I said. The evidence in concept studies is that when you present a positive instance of a concept and a negative instance of what a concept is not, people do not use the negative instance. They do not trust negative information. If you use the selection paradigms that Bruner used, then subjects hunt around for positive instances, and make the hypothesis on the basis of these. They tend to ignore the negative instances. They don't pay much attention to them. It is almost, I would suspect, a cultural thing.

Hovland and his co-workers equated the information value of positive and negative instances, but their subjects still preferred to use positive instances. There is at least one study in the literature where subjects have been pretrained to use negative instances so that they can absorb information from them. The fact seems to be that laboratory experiments are influenced by a cultural determination which predisposes subjects not to look at the negative points in an argument, and not to use the negative points to make a direct chain of reasoning. We look for all the positive arguments and operate that way.

• *Question:* You indicated that concept-learning investigations did not ordinarily take into account individual differences. Would you care to speak on the variables that you presume to be important?

• *Dr. Glaser:* Let me give some examples. Consider what has been called the "salience" of concept dimensions. Some of the dimensions in a concept are more easily perceivable or more easily detectable than others, because of a learner's past history. It seems to me that some assessment of the perceptibility of the dimensions for different individuals is a reasonable individual difference variable to look at.

Certainly there has been a lot of work on language and concept learning. There have been studies which show that if you get a child to verbalize the irrelevant dimension in a concept study, then that slows him down; whereas, if you get him to verbalize the relevant dimension, then that speeds up his learning. So the acquaintance with a vocabulary which allows the learner to profitably encode the concept rule in some way would be another individual difference variable.

If the concept has an imbedded structure, the existence of a learner's understanding of the prerequisite concept would be another individual difference variable. These are the kinds of things that I think of at the moment.

• *Question:* In your example of stating the rule first, the deductive example, could you explain the difference between that and Type 7 of Dr. Gagné's category? Is that what you would call principle learning?

• *Dr. Glaser:* I guess it is principle learning, that is, principle learning as described in Gagné's book. Bourne has called it rule-using.

• *Dr. Stephens:* But you are distinguishing between the methods by which one learns the principle, by having it presented overtly, or whether one learns it inductively.

• *Dr. Glaser:* I was saying that you can discover the rule through induction, but that it also can be learned by presenting the generalization or rule, initially, and following this with practice in using it in instances.

• *Dr. Stephens:* In either case you would be getting principle learning.

• *Dr. Glaser:* I guess I don't make a distinction between "principle" and "rule." I would say, for instance, that if a concept is defined by, let's say, "short," "round" people, that's a rule as well as a principle.

DISCUSSION OF ROBERT GLASER'S PAPER

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MODERATOR: Dr. William J. Gephart
FIRST DISCUSSANT: Dr. Patrick Suppes

• *Dr. Suppes:* I have nine points I would like to cover.

Point No. 1. I liked Bob Glaser's paper very much. I think it is a very useful and significant review of a very important subject. The indications in the paper of the kind of work we need are those I would agree with. So, many of my remarks are not direct criticisms in any sense of Bob's paper, but are amplifications of remarks he has made about the literature that now exists on the subject, the research that has now been done.

Point No. 2. I agree with Bob very strongly that the definition of concept learning should not in any sense be restricted to verbalized concepts. I agree that we can talk in a meaningful and significant way about concept learning in animals, and certainly a great deal of the concept learning we want to talk about in children is surely primarily non-verbal in character. Jim Gibson is full of examples, I am sure, from the area of perception. I think, however, that this is an important point that has on occasion been ignored in discussions of concept learning, particularly in relation to school subjects, and I would like to simply underscore Bob's point.

Point No. 3. Here is a point of emphasis, an underscoring of a point in Bob's paper that is not sufficiently made. There exists in the literature of concept learning a relatively sharp distinction between concept-identification studies and concept-formation studies. It is fair to say that the bulk of the laboratory studies he refers to and that exist in the literature are studies of concept identification. I mean by this that, if the concept had been described verbally to the subject prior to the experiment, he would have been fully familiar with the concept and what the experimenter was after. This is a rough and ready definition—I won't try to be more exact here—of what one means by concept identification.

Roughly speaking, the subject's problem, as Bob points out in his discussion, is simply to identify the relative dimension. The thing he does not know is what is relevant. The things he does know and is familiar with are the dimensions being investigated.

For subject-matter learning, in many cases—in fact most cases—the precise opposite is true. It is concept formation, not concept identification, that is the critical question. And what we badly need are much more extensive studies of concept formation as opposed to concept identification. Too many of the characterizations one can give of concept learning are restricted to concept identification.

I think it is also as a consequence of this, perhaps, that the formal definition that Bob gives of concepts cannot be taken too seriously. What he says later is, I think, quite correct. That is, we can't simply define concepts in terms of a common response to different stimuli. For example, no one thinks of a paired-associates experiment with twenty items and three responses as a concept experiment.

When we get into the area of concept formation, there is a more difficult problem of defining what we want to mean by a concept. The reason that the definition that Bob gives seems to work is that in concept identification experiments, there is no serious problem about what the concepts are. In the case of concept formation, this can be a difficulty. What is an appropriate definition of concept? How should the objects that fall under this definition be investigated in the sense of concept formation?

I think there are some specific constructive things to be said about it. I think that Bob's remarks about categorizing are appropriate, but I would urge that the definition of concepts, references to category, and references to response to new items, which he mentioned later, are essential and necessary to a correct definition.

Point No. 4. I very much liked and agreed with the emphasis on the importance of more analysis and more study of rule learning. This has already come out in the discussion following the paper. It is highlighted in the paper at a number of points. I would like to underscore my own feeling of the importance of this. I would like to cite one example, not cited by Bob in his paper, where we made a fairly extensive search of the literature. This is of second-language learning.

If there is an area in which teaching occurs to a large extent classically and contrary to popular impressions, neo-classically in audio-visual approaches, it is in the teaching of second language. No one proceeds, with very few exceptions, and certainly not in ordinary school settings, in a purely inductive fashion. The use of rules is continual; the intuitive evidence is that the use of rules is desirable in second-language learning.

If one investigates the literature, however, for experimental studies of what is going on in learning to use a rule, little is to be found. The interesting thing about second-language learning is that it's like a motor skill. It isn't simply a matter of describing a rule. I can give to you, for example, the rule for the conjugation of verbs in Russian, and you will understand those rules. When I put them on the board there is no problem with them. The problem, and this is a special problem in many areas of concept learning, is that it is not easy to bring those rules under command in the appropriate timing sequence required for actual use of a second language.

The list of seven properties of subject-matter learning he gives is certainly a very good list for those who are concerned with experimental studies in this area to consider.

Point No. 5. An important aspect in most parts of subject-matter learning is the learning of abstractions of some kind. I would distinguish, at least as they seem to occur on the surface of subject-matter material, abstraction from generalization and discrimination. I would urge that there needs to be a more serious and more careful intellectual and psychological analysis, particularly a psychologically oriented analysis, of what we mean by abstraction. The psychological literature on abstraction is, again, rather poverty stricken. It would be appropriate to ask for a much more thorough investigation in this area, and here I think that it would be easy to build upon the remarks of Glaser, to consider in a much more serious way what are the problems of defining and studying abstraction.

Point No. 6. Bob mentions problems of memory that we all know are critical in subject-matter learning. I would like to amplify his remarks by saying that there are several aspects of memory that are critical for subject-matter learning, but that have not been studied as yet very successfully by psychologists. The problems I have particularly in mind are problems concerning the logical organization of long-term storage, the problems of cross-references, and the problems of access.

We know very little as yet about the logical structure of memory with respect to these characteristics, but these characteristics are absolutely essential to a wide range of subject-matter learning. A beautiful example is provided in terms of accessing by what I just said a moment ago about second-language learning. Serious psychological theories of accessing memory in learning a second language simply do not exist.

Point No. 7. I think that the paper in certain respects perhaps doesn't emphasize strongly enough the importance of language. One of the things that we have become conscious of in the last few years is that psychological studies of verbal learning have been looking at the mild perturbations in the landscape, and have ignored the mountains and the deep valleys. That is, major characteristics of language have not had a serious impact on much of the verbal-learning and concept-learning studies in psychology. On the other hand, if anything is evident in this world, it is the importance and the richness of the command of a spoken language that a child has when he enters the first grade or kindergarten or a Head Start program. Somehow psychologists have not yet come to grips with the richness, the complexity, the subtlety, the difficulty of that phenomenon. In my own judgment, we shall not have a serious theoretical or experimental analysis of subject-matter learning until a much bigger place is made for the knowledge carried in the language-command of a child or a student. I would put this in the forefront of serious problems for any studies in concept learning with respect to subject matter.

Point No. 8. is a point hinted at in Bob's paper, and one that I simply want to amplify. The theories that we now have in psychology, if they are not simpleminded, are at least far too simple. If we consider the theories referred to in concept learning, for example the work of Bower and Trabasso, the work of Bourne and Restle, the work of Hovland and Shepard, all of these are simple theories of concept identification. None of them touches in any serious way the problems of providing a theory of concept formation, yet, as I have insisted earlier in my remarks, it is problems of concept formation, not concept identification, that are central to subject-matter learning.

There is a great deal to be said about this. There are technical ways of expressing the claim that these theories are too simple, but I think the intuitive arguments are good enough. It is obvious from inspection of the theories I have mentioned, and their near relatives, that no serious account of the learning of a subject like mathematics, or the problems of learning a second language, let alone the subtleties and complexities of discursive subjects like history or English literature, could be given, or even touched, in terms of an analysis of what the student is doing. I think it is to be recognized as a failure of psychology not to have as yet moved in a more substantial way in this direction.

On the other hand, I do want to say I'm not simply trying to make a flat criticism of the work that has been done, because the work of the people I have mentioned is in many respects intellectually at a very high level. It is just, unfortunately, almost entirely irrelevant to the problems of concept formation in subject-matter learning.

Point No. 9, my last, has three subheadings. I would like to briefly mention two topics and methods that supplement suggestions made in Bob's paper. First, I think that a somewhat stronger emphasis to the role of perception could be given in studies of subject-matter learning. I made this remark and noted it before I knew that Jim Gibson was going to be one of the discussants. I won't say more, given the present context.

I think that from an analytical or structural standpoint, the experimental studies, a substantial body of scientific work, have certain very unfortunate tendencies deeply imbedded in their conception. The tools they bring to the analysis of experimental situations, the tools they use for the analysis of concepts, are, given our very great ignorance, wrong sorts of tools. We have put under a microscope in an extraordinarily detailed way certain aspects of concept identification, of concept learning, what I like to call if I had time to amplify, the kinematics of concept learning.

What has not been analyzed in the same way are problems of concept dynamics, much more central if you follow the distinctions of mechanics in concept formation. It seems to me we need a structural analysis of stimulus items, if you want to use the stimulus language; or, if you want to use another language (and I'm quite prepared to work either side of the street) the cognitive analysis of learning items, the cognitive structure of items. Talk about stimulus structure or patterns of stimuli, talk about cognitive structure, I don't care. What we need, though, is the analysis of structure in the items presented to students which will lead to correct prediction of learning difficulties. Such structural analysis exists only in very scant measure. Yet there are techniques available for a general framework in regression models and their relatives within applied statistics that provide extraordinarily good tools if you also have in addition some useful psychological ideas for making this analysis.

I want not to be misunderstood on this point. I am not saying that regression techniques in themselves provide an answer. They no more provide an answer to many psychological problems in the study of learning difficulties than does the knowledge of Markov chains provide a knowledge or analysis of learning processes. But they are a tool for the analysis of structure that has not been very adequately exploited. They provide a framework in the same way that simple stochastic processes provide a framework for the analysis of learning, and I would urge that regression techniques can be applied much more successfully and at a much deeper level than has yet occurred in the study of subject-matter learning.

My final comment is under the heading of "New Topics." Again I certainly concur with Bob Glaser on the importance and urgency of more knowledge about individual differences, and I would add I have been struck in the past year by how very little we know in a systematic way about building curricula for individual differences. The actual substantive scientific information on this topic is practically nil. For example, I don't know of any substantial or integrated body of literature on mathematics learning in the elementary school that gives you any details about individual differences. The same thing can be said of other areas so far as our literature search goes.

Further, the depth of analysis and the conceptualization of analysis in the study of individual differences is as yet very shallow, I would say, in spite of fifty years of testing. What we need is much greater clarity about individual differences. It is important not only for our theories of concept formation; it is practically important in the attempt to apply more systematic analysis to the practical problem of designing and preparing new curricula.

• *Dr. Gephart:* We will first see if Bob Glaser has a comment on the comment, before we open up for further discussion.

• *Dr. Glaser:* It is a source of pleasure to get concordance from Pat. He generally isn't very free with such remarks.

The definition of concept formation which he questioned is of interest. As you know, in the beginning of the paper, I made the case that I was going to derive a definition based on studies in the laboratory, and then point out how it is an awkward definition. I would appreciate your elaboration on how you would like to see the term "abstraction" further defined. Maybe it has to be defined by experimental work. But I seem to get some mileage out of thinking in terms of discrimination and generalization. In teaching things to young children, if I set up my thinking in terms of getting them to learn a series of discriminations, and then getting them to learn a series of stimulus equivalences, I come up with procedures which are very useful for teaching simple concepts like vertical and horizontal, like red and blue, and maybe more elaborate concepts like the electrocardiogram example. It seems to be a useful, practical way to think, even though it may not go far enough.

• *Dr. Suppes.:* I had a note here about theories of generalization, discrimination, and transfer. I certainly agree that you can get a certain mileage out of these ideas. On the other hand, I would make the same charge of failure in any attempt systematically to apply these theories to the complexity of subject-matter learning.

For example, I have found it extraordinarily difficult to apply these notions to mathematics learning in a systematic way.

• *Dr. Glaser:* The other point I have is that when you talk about the analysis of structure, which is of great concern in subject-matter learning, and you talk about regression techniques, are you thinking about a laid-out structure for which a regression procedure gives you some power to predict transfer and non-transfer along this structure? Is that what you are thinking in terms of?

• *Dr. Suppes:* Yes. Let me give you a quick example. We have recently done a study of students learning various aspects of arithmetic. If we look, for example, at addition problems involving two-digit numbers (these will be done in our computer set-up as mental arithmetic, so the student is inputting only the answer). The problems will be of this sort: $A + C =$ ——. A two-digit response goes in the blank. That's a typical problem. It is not the only kind A more complicated one is $AB + CD =$ —— $+ EF$, which is certainly harder.

Now, let P_i be the probability of a correct response to item i. We can analyze item i in terms of a regression model where we have weightings $P_i = \Sigma \hat{\alpha} f_{ij}$ and you have identified the structure in the problem. I want to emphasize this is not factor analysis. The factors f_{ij} are identified objectively, independent of data analysis. We have looked, for example, at three variables and predicted for a large number of items, 80 to 90, the probability of a correct response fairly well, which gives us then predictive power for this whole range of problems beyond the items we have looked at.

Typical factors are these: magnitude of the smallest number in the problem; and the number of steps needed to find the answer. In the case of the second factor, we have a very explicit analysis of an algorithm that probably the student will be using on the basis of what he has been taught.

So now we have an analysis, let's say, of eighty items in terms of two factors and three coefficients, the additive constant α_0, α_1, and α_2. It is this kind of analysis that I had in mind, and I would particularly like to emphasize the kind of process variables involved in conceptualizing the number of steps. I think a great deal can be done with that kind of analysis in the subject-matter area. It is not the same kind of analysis we get in learning theory, but that's a good thing, probably.

• *Dr. Gephart:* If there are further questions from any of the other members of the panel with regard to this topic, we will field them at this point.

• *Dr. Rothkopf:* May I ask a question about Dr. Suppes' last example. I don't quite see what the next step would be after Dr. Suppes has an adequate regression model, say, for difficulty in arithmetic. It certainly would be interesting to understand what causes difficulty in arithmetic problems. How would you apply this to instructional use? Can you give us an idea?

• *Dr. Suppes:* I'll make two remarks. One of the things that come out of this analysis for a large number of items, and not just addition but also subtraction and multiplication, is that the memory steps are the most important in the identification of difficulty, and the direct instructional advice is—and I'll give you something right at ground zero—that in teaching algorithms the teachers do not put enough emphasis, particularly for children in the elementary school grades, on the powers they have of holding a number in memory, short-term memory, in a precise and exact way for a few seconds. This is a very definite and significant piece of information about instruction that comes out of this kind of analysis.

The second remark: theoretical. I am certainly not intellectually satisfied in any final sense with this kind of analysis. For example, if the analysis is in terms of a variable like the magnitude of the smallest number, the way I put it is, "One man's explanation is another man's black box," because the next problem is, "Why is it that the magnitude of the smallest number is a significant variable?" The problem for research is to reach ever deeper for conceptualization of the process variables in a subject. That's why, for example, psychologically I'm much more satisfied with the variable number of steps, because here we are trying to conceptualize the process through which the subject is going in reaching a solution to the problem.

So I would conclude by stating that in regression analysis there is a very big difference in the psychological significance, importance, and fundamental character of the variables you look at. Some factors are very surface-like in terms of the items; others are much more fundamental in terms of one's conceptualization of psychological processes.

SECOND DISCUSSANT: Dr. Evan R. Keislar

• *Dr. Keislar:* The paper by Robert Glaser is an excellent critical summary of the field. It constitutes a very fine indictment of the kind of research which has been going on in this area in terms of the processes of instruction.

It is perhaps appropriate to state, as Bob has, that concept learning lies in between rote learning (example: paired-associate) and the higher complex mental processes. One should recognize, however, that in concept learning, especially in the kinds of experiments quoted in his paper, that complex cognitive processes are usually involved. These include hypothesis testing, the use of problem-solving strategies, and the drawing of inferences. It is necessary to point out that concept learning involves, especially with more mature subjects, even the most complex forms of intellectual processes.

The definition of concept learning which Bob Glaser adopted, and which Pat Suppes underscored, has considerable value. There may be considerable value also in using more restrictive definitions, such as those proposed by Charles Osgood and E. B. Hunt. Osgood's definition, for example, draws a clear distinction between a concept and a label which is not present in Glaser's paper. For Osgood, a mediated generalization is necessary. A parrot can learn "to label" many objects with the word "cracker," but the parrot can't use this label as a way of generalizing other behaviors to objects he labels "crackers." Which definition is best cannot be decided on logical grounds. The ultimate test of which is a better way to define a concept will be in terms of what is more useful in educational research.

I have some reservations about Bob Glaser's use of the term "reinforcement" in concept learning. Reinforcements serve both an informative and a motivational function in human learning. Where we are referring to the informational aspects, there may be little value in stressing the consequences of responses. The ambiguity of the findings from prompting versus confirmation studies suggests that where information is all important, consequences may not be essential; it may be enough to supply the information directly, as through the use of prompts. As Glaser has indicated, we need to know much more about the use of prompting as a way of supplying information in concept learning.

Glaser pointed out many significant and clear implications for educational research. Research in concept learning has all too infrequently been carried out in relation to school subjects; the work of Pat Suppes and his associates is one of the few exceptions. Furthermore, concept learning needs to be studied, as Bob's paper suggested, within the school setting which the student encounters. For example, one very practical problem facing every teacher is to what extent to give negative exemplars in relationship to positive ones. To answer this question we must consider, among other things, the number of alternatives. Where there are only two choices, a negative exemplar may have as much informational value as a positive instance, at least to a sophisticated learner. Positive exemplars may be far more effective when many options are present. The specific instructional conditions under which the student learns must be considered in research.

While we certainly need studies dealing with individual differences in concept learning, we most desperately need those that relate findings to antecedent and consequent conditions. We must bring the work on individual differences in concept learning into a clear relationship with decisions teachers must make regarding instruction by relating the findings on individual differences with instructional variables.

Bob Glaser's suggestions for research on concept hierarchies should be underscored. The learning of concepts may be greatly facilitated by the proper sequencing of previous instruction in which the child first learns those more elemental concepts and which uses these for efficient learning of more advanced concepts. This study of intra-subject transfer present in such hierarchical sequences has been neglected in educational psychology. Most transfer studies have dealt with inter-subject transfer.

I am extremely sympathetic with Bob's plea for more useful theories. We would all probably agree that the most practical tool we can have in educational research is a good theory. Unfortunately, theories available to the educational researcher are not appropriate for studying the rather rich complexities of classroom performance. On the other hand, theories that are extremely precise, like mathematical models, have limited generality; while broad general theories, such as some forms of mediation theory, lack precision for extensive use in education.

Since this problem is not going to be solved in the near future, for the next several years we are going to be involved in little "islands" of research, and overlapping and frequently inconsistent theoretical approaches to the same instructional problems. For this reason, it seems likely that an "engineering" point of view will be prevalent in much of educational program development. We will have to develop instructional programs on the basis of empirical tryout and modification using what meager direction we can get from theoretical work.

An important consideration in concept learning should be the effect of prior training on the part of the child in learning how to learn. Where, through previous experience, children acquire a particular "learning set," the findings regarding concept learning may be tremendously affected. Margaret Jones has called my attention to some work of Michael Cole which suggests the importance of prior cultural experience. In his work with the Kpelle tribe in Liberia, he found that these children learn, apparently, disjunctive concepts more easily than conjunctive, quite opposite to the findings obtained with American children. We may be able to do a good deal about teaching children how to learn concepts, or how to attack problems involving concept learning. For example, children may be taught to use information obtained from negative instances far more effectively than is now done. Many children regard such negative information as a punishment rather than as a valuable kind of feedback.

• *Dr. Gephart:* Dr. Glaser, do you have a comment on this?

• *Dr. Glaser:* I have two comments. One pertains to what you indicated about reinforcement, the other about theories. There is some generalized reaction to reinforcement theories, and we seem to be bending over backward too far to say, "Well, gee, reinforcement as we have been able to define it, you know, doesn't seem to be an important thing." That seemed to be the sense of your remarks.

I would comment that reinforcement for me is defined in terms of a change in the environment that follows a particular behavioral act. The consequence of something you do is a change in the environment. Without this change in the world, you don't really learn. We have been studying how little children learn to discriminate between vertical and horizontal lines in a classical discrimination paradigm. In our setup, the children touch a screen which then proceeds to display the next stimulus. When they make an incorrect response, if we permit them to move on to the next slide, the fact that they are moving on to the next slide is an effective reinforcement from the incorrect response, and they don't learn as well as when an incorrect response results in no immediate change. If we stop the screen from changing by a 10-second holdup, we have provided a very effective way of maximizing reinforcement of the positive response, and minimizing any environmental change from the negative response.

In general, in concept-learning studies, there has been hardly any work with reinforcement contingencies. There has been hardly any concern with what you do when a subject makes an error. I guess Pat Suppes has studied correction, but there is little work on correction procedures following incorrect responses. But such consequences and contingencies are extremely important on subject-matter learning.

The other point was about theories. Certainly it's good to say, "Theories are fine," but what seems to be happening, which is very important, is that people like E. L. Thorndike are beginning to emerge—individuals who can write books on mathematics and at the same time write on cats in a puzzle box. We have two-headed people now who have one head in stochastic processes at the same time they are trying to fight through problems in classroom learning. The interplay between these two enterprises is what is particularly exciting. It will force us to be relevant to real behavior.

• *Dr. Keislar:* Let me clarify my position on the role of reinforcement in concept learning. The consequence of an event can serve a purely informative function, that is, it can act purely as an S^D. For example, Carolyn Stern and I completed a study in which we taught five-year-old kindergarten children to profit from their errors, at least far better than another group given the same opportunity to learn but without special instruction. The children here were able to learn from their mistakes, even though they did not correct their errors, as was done in one of Pat Suppes' studies. I wouldn't call this consequence of the response a non-reinforcement; we have here information, or a stimulus which controls responses on future items. The effectiveness of this stimulus is something that is acquired through prior experience or training.

• *Dr. Suppes:* I feel that I occupy a position somewhere between Bob and Evan on reinforcement versus information. I would agree with Bob on the efficacy and necessity of reinforcers, the necessity in thinking about it explicitly. On the other hand, I think that a fair criticism of the theoretical discussions that have grown up around this concept, and which have quite a long history now in psychology, is that you ask yourself: "Within these theoretical frameworks and discussions, what structural connections are postulated between information, response, and stimuli?" It is precisely around the point that Evan is making that we don't have very much. That is, information is not brought in (usually) explicitly. The relation between the information content of a reinforcement and its information relation or logical relation to the response or to the initial stimulus is not an intrinsic part of classical reinforcement theory, yet it is precisely at that juncture that one of the major failures of classical reinforcement theory occurs.

We need reinforcers, but we need also a very good analysis of this information content.

• *Dr. Lumsdaine:* Before we leave the ambiguities or unrealities or peculiarities with respect to reinforcement, I would just like to note one other prevalent ambiguity. This is often a failure to distinguish clearly between which of two classes of responses are being reinforced. One class is responses that are involved in the actual learning. These are the heart of the matter. The second class consists of the ancillary juristic responses that comprise keeping at the task, keeping working on the machine, and so on. It would appear as if there is a distinction in other aspects of reinforcement, in regard to these two classes of response, both of which are necessary for learning to proceed.

• *Dr. Rothkopf:* In view of my interest in this matter, I find Dr. Lumsdaine's recognition of this problem a very reinforcing statement.

• *Dr. Ellis:* Bob, there is one point in your discussion of the definition of concept formation which I would like you to amplify. As I understand your discussion, you would not like to distinguish between primary stimulus generalization and concept formation because such a distinction may deal with arbitrary differences among the stimulus events involved. I regard concept formation and primary stimulus generalization as distinguishable processes. Moreover, the distinction does not have to do with the *stimulus attributes* themselves, but rather, has to do with the fact that a test for stimulus generalization does not ordinarily provide opportunity for the subject to acquire a concept, since no differential reinforcement is given the test situation. Stimulus generalization is the development of the tendency to respond to stimuli that are similar to some training stimulus; this may occur in the absence of any discrimination training or differential reinforcement, conditions normally regarded as important in concept formation.

• *Dr. Glaser:* First I wanted to make the point that I thought that both verbal secondary generalization, for mediated kinds of stimuli, and generalization for nonmediated stimuli, were similar processes in that they result in learning the stimulus equivalences required in concept formation. I did, however, pick up what Jackie Gibson has said in print about the two processes, and put it in my paper, because I thought I could get her to argue with me.

• *Dr. Eleanor Gibson:* But you left it out.

• *Dr. Glaser:* Her statement—I guess it was in the *Psychological Review*—noted that primary generalization was not the kind of generalization involved in concept learning. However, it is a little difficult for me to see why, if I am learning the concept of red, and conceive of a primary generalization along a continuum of redness, that I cannot consider this as a conceptual response, or as one of the components, the generalization component, of a conceptual response.

So I assert that whether we are generalizing along what I call a direct stimulus dimension, like a wavelength, and you consider that primary generalization, or whether we are generalizing along a symbolic dimension that has perhaps more secondary mediating information, the kind of generalization involved in both cases is relevant to the concept-learning process. I guess that will put the quietus on the thing.

• *Dr. Ellis:* I agree with you when you say they are both relevant. I do not agree with you when you suggest they are the same thing or same process, principally because I believe that primary stimulus generalization does not involve the evocation of responses that are being differentially reinforced in the presence of various test stimuli. You simply present test stimulus and determination if the subject responds, or determine to what extent he responds. In concept formation and attainment studies, additional instances of both positive and negative stimuli are presented, and the subject is differentially reinforced. I do believe that primary stimulus generalization is involved in concept formation; however, I do not view these as identical processes.

• *Dr. Glaser:* "Natural primary generalization" in the young child does lead to errors in concept learning. Because he generalizes, he makes some errors, and then he has to learn what the categories are, even though there is primary generalization.

• *Dr. Ellis:* I agree with that.

• *Dr. Eleanor Gibson:* I wasn't going to answer, since you left it out of your speech. I would agree with Henry that the question is not whether stimulus attributes are relevant to concept formation. The question is, what kind of process is going on? I wouldn't want to say that a pigeon showing a generalization curve to wavelength has a concept of whatever wavelength was presented. But, from what you wrote, I gather that you would.

• *Dr. Glaser:* Yes.

THIRD DISCUSSANT: Dr. James J. Gibson

• *Dr. J. J. Gibson:* This brings up the question of what a concept is, and I would like to ask Bob four or five really fundamental questions, some of which have been touched on by Pat Suppes, but my questions are somewhat less polite. I am speaking, Pat, as a psychologist interested in philosophy: you spoke as a philosopher interested in psychology. I have the following questions to ask:

First, what is a concept? May I question Bob's definition?

Second, how relevant are the existing experimental studies in concept learning? I would really ask whether they are relevant at all.

Third, how adequate is the stimulus-response formula for this inquiry? I will suggest that it is not adequate.

Next, what is the influence of language upon conceptual thinking?

And, if there is time at the end, we can get on to this perennial question of what is a reinforcement.

Well, the first question: what is a concept? According to the paper, it can be stated as a first approximation that concept learning involves learning to make a common response to a set of stimuli; that is, a group of stimuli is assigned to a single response category.

Now, I would like to suggest boldly that this definition misses the essence of what concepts are. Is it not true, don't we all really believe, that the first thing to say about concepts is that they involve *knowledge?* Responses are not essential, are not a necessary feature of the conceptual process. My prejudice would be to say that concepts are extensions of percepts, and that the process of going from the pickup of perceptual information to the pickup of conceptual information is a continuous development.

But to get back to this question of whether a concept can be defined in terms of response, isn't it true that response has no logical opposite? That is, there is no such thing as a non-response. Whereas conception has a clear opposite, misconception. Perception has a clear opposite, misperception; and knowledge has a clear opposite, namely ignorance. I think we ought to face this fact when we try to define concept attainment or concept formation. So that's my first point.

Second point: how relevant are the existing experimental studies of concept learning? They are based on the definition that you gave. In fact, you say you have chosen your definition in the light of the existing experimental procedures and methods. But it seems to me that, as has been pointed out by both previous discussants, in the end this body of experimental literature, with clean-cut measurable criteria of response, simply doesn't touch on the problem of how children learn in school, or, for that matter, how children learn out of school when they are on their own.

That leads to the third question: how adequate is the stimulus-response formula for this inquiry? Of course, that brings up the old controversy between reinforcement theories of learning and cognitive theories of learning, which, I guess, was inconclusive. It is not as fashionable now in psychology as it was ten years ago, but the fact that it was inconclusive doesn't mean that we can give it up. The final development of cognitive theories of learning was unsatisfactory to some of us, but let's take it up again, then. Let's start fresh where Professor Tolman left off, and continue until we can develop some kind of learning theory other than one of S-R connections.

You see, the very first few sentences of Glaser's paper involve certain assumptions that are debatable. I will read the second sentence: "Concepts are built and molded out of simple behavioral acts and form the basic elements that are processed in higher-order behavior." Bob Glaser seems to be taking for granted that stimulation and association are elementary behavior processes at the bottom, and there are complex intricate behavior processes such as problem solving and thinking at the top.

This is, I suggest, a kind of Titchenerian assumption of which thirty or forty years of Gestalt theory have pretty well shown the fallacies. It takes no account of structure.

For example, color! You suggest, Bob, that "red" is a word, and that a child will apply the word "red" to various stimuli. You imply that a single wavelength is a single stimulus. You thereby leave out the whole problem of color constancy and surface color. As a matter of fact, as you perfectly well know, a single wavelength is a rarity and never happens in life. It happens only in rainbows. What you mean by "red," or what we all mean by "red," is a very complex pattern of spectral distribution. Moreover, there are all those difficulties of color mixing. You are really talking about "red" as a surface color, not as a film color, and you are thus begging the whole question of how the visual system detects patterns of constant pigment colors in red objects. That's a completely unsolved problem in perception.

So the problem of color perception is more "conceptual" than I think was suggested in your paper, if you see what I mean. The stimulus for color is not as simple as Titchener believed.

Then, last, what is the influence of language on conceptual thinking? I am very glad that you admitted, and we all here admit, that it is a fallacy to say that concepts are simply verbal responses. This is surely incorrect. Children and animals have, in some sense of the term, concepts. That is clear. But you didn't make much mention of the really fascinating and difficult question of the effect of language on thinking.

How does man differ from pre-verbal infants or animals? As you know, there was a man called Benjamin Whorf, who came out with a radical assumption about the effect of language on thinking some years ago; an assumption that is attractive to a great many philosophers and psychologists. He said the effect of language on perception is to distort it. That seems to me quite mad. If the effect of language on perception is to distort it, then the implications for education would quite clearly be of one sort. Whereas if the effect of language upon perception is generally good, as I believe (and we need to work out *how* it is), then the implications for education would be of an entirely different sort.

I think that Whorf was wrong, and there are experiments inconsistent with the Whorfian hypothesis. Instead of arguing it, however, let us face up to the basic question.

It seems to me that language does something like this: it fixes the achievements of perception and cognition. As soon as the human child manages to distinguish some new invariant common feature of his world, he has language available to *freeze* it. If he freezes this new invariant, this insight, in the right way, he has used language advantageously. But he sometimes freezes it in the wrong way, or prematurely, or wrongly. This, of course, means that he has to keep changing his use of language.

Your suggestion, Bob, that a reinforcement is *any* change in the world, or *any* change in stimulation—that isn't really good enough, is it?

• *Dr. Glaser:* No.

• *Dr. J. J. Gibson:* Well, we need to think more deeply about what a reinforcing state of affairs is. What reinforces learning, especially complicated learning? It is certainly not motivational satisfaction in any simpleminded sense. It may be a little more like what Woodworth years ago talked about in a paper entitled "The Reinforcement of Perception." He suggested that *clarity* is a reinforcing state of affairs, and that getting something clear is what produces learning. The prototype of this process is the accommodating of the eye for a bit of detail.

It seems to me, then, that the isolating of invariant properties in a stimulus flux, the isolation or extraction of those invariants which carry information about the constant, lawful world, constitutes a better definition for conceptual learning than one in terms of stimulus-response association.

• *Dr. Glaser:* I really didn't attempt to say what my definition of a concept is, because I don't think I can define it unless I do experiments for a few more years. I was, however, trying to say what definition emerges from the laboratory studies.

I do agree that concepts involve knowledge; but I tend to think about knowledge in terms of the responses of students. In this way, I can think in terms of conception and misconception. A young child learning to develop a concept has all sorts of misconceptions. I remember my daughter learning, when she was very young, to respond with the word "dog," to a black and white dog, a black and white object. When we met a nun on the street the next day, she said "bow-wow," her response to a black and white object. So you can talk about misconceptions in the learning concept, and I think it can be talked about in terms of responses.

In response to your second point, that the literature doesn't touch on how children learn in school, you were harder on the literature than I was. I said that some of the studies in elementary concept formation seem to me to be relevant to very simple concept learning in either animals or young children. However, when I talk about simple concepts, such as "vertical and horizontal lines" and "redness and blueness" being clear-cut concepts, immediately your knowledge of the intricacies of perception arises which may make them less clear-cut; so one man's explanation is another man's black box, as was said.

So far as S-R formulations are concerned, I don't know if we should get into this argument. I can conceive of something that involves "cognitive structure" as a response, or "cognitive mass" as a stimulus, and talk about it in those terms. So far as my Titchenerian attitude and simple elements are concerned, I may have such an attitude, since it is hard for me to conceive of talking about any sort of scientific subject matter without being able to talk in terms of basic elements. In this regard, I am particularly impressed when I talk to Herb Simon about his work on information processing that he can talk about complex things like chess playing in terms of rather simple information-processing elements. I guess you have to talk for many theoretical and explanatory purposes about some components that make up more complex events, or you have difficulty in dealing with them. At least that is my own predilection. I stand corrected on my simpleminded concept of the stimulus of redness. I am sure it is not correct at all.

So far as the influence of language is concerned, I am certainly impressed with the influence of language and the host of kinds of behavior that language brings up. I was trying to make the point that most of the studies on concept formation are non-language studies. I was also trying to make the point that when we deal with physical dimensions we have scaled continua along which to talk about generalization and discrimination. In the language studies we have the problem of discovering what the underlying continua are. If you think about concept formation the way I do, and do it in terms of a dimension along which you discriminate and generalize, then the scaling of dimensions for language behavior poses a whole new problem.

So far as reinforcement is concerned—well, you know the problems there. But I do think that the most general way to conceive of the process, at present, is in terms of an environmental change, as unspecific as that may be, and I look forward to changes in subject-matter structure in school learning that serve as a reinforcement for the student. I think that intrinsic reinforcement (by M & M's or by pellets) is a very artificial kind of reinforcement which is highly unrelated to the potential reinforcement effects of the subject-matter change.

• *Dr. J. J. Gibson:* Let us first, each of us, ask ourselves what we mean by "structure." You have the first chance, Bob. What do you mean by "structure"?

• *Dr. Glaser:* I defer to Pat.

• *Dr. Suppes:* I would say there is quite a good general theory of structure, and the problem is to get it under control in psychological situations. The mathematical situation is this: The various relations and functions defined on the set of stimuli impose a structure on the set. The general theory of such structures is quite thoroughly developed in what logicians call the theory of models.

We have a good analysis of structure at the general level. It is too general, and therefore too irrelevant to our real problems. The problem is, what structures are relevant to learning difficulties of children in a given subject matter? That, to me at least, is the tough problem.

• *Dr. J. J. Gibson:* We experimental psychologists, you see, have to face the following question: Do we mean, or should we mean by structure, the structure of the stimulus or that of the response? In my terminology is it the structure or the stimulus information we are talking about? Where does the structure come from, from outside or inside? To me this is the basic question.

I think that it is very urgent to put under a common tent of structure stimuli, responses, and reinforcement, so that it is part of the theoretical analysis to say what are the connections that should exist in a formal sense between reinforcement as information carriers and stimuli.

Pat, you are forcing me to be the philosopher, whereas I wanted you to be the philosopher. What I had in mind was the basic question, again, of whether the problems we set in both our experiments and in the schoolroom apply to the real world or not.

For example, you had in your paper, Bob, a quotation from someone who said that the experiments don't always produce a world for the child which is intelligible. Now, it seems to me that we ought to consider that.

Skinner, for example, creates for the pigeons and the rats he puts in his boxes, a world of causation which is arbitrarily selected by Skinner, and his probabilities. His cause and effect laws as to how you get food in this box are at his pleasure, and almost certainly the world faced by an animal in the Skinner box is not like the real world.

This, it seems to me, is the trouble with the concept-attainment experiments. They don't imitate the real world. They don't take into account the problems of epistemology, and I guess I am saying that we need to face up to the problem of epistemology.

• *Dr. Suppes:* Jimmie, I recognize the theme from your writings, yet I must say I am forced to disagree with you. I don't think the problem we are now discussing is restricted to psychology. I can imagine, for example, in the seventeenth century someone who was overly enamored of the real world saying to Galileo, "Well, you know, that's nonsense. Whoever heard of balls falling off towers to the ground? What we're interested in is an analysis of how people walk down the street, or what causes it to rain, or predictions of the weather." Looking at the thing in retrospect, it seems appropriate to say, "Thank God Galileo was not too impressed by the real world," because the real world problems are too complex.

I think we have all criticized, and probably appropriately, the irrelevance to subject-matter learning of many of the concept studies. Yet I don't think we can rush from that criticism to saying, "Well, let us wrap our arms around the real world and study it." The real world is too complex to embrace all at once and we have to take a modest step into it.

• *Dr. Rothkopf:* I have difficulties with some of the arguments that I have heard, and they are of a conceptual character. I wonder whether it isn't an error to place so much emphasis on the notion of concept learning. If we are really interested in making a serious input to educational problems, this emphasis may be misplaced.

You see, the problem with concept learning is that it makes assertions about some state of the student, and we have notorious technical difficulties in determining what the internal states of people are. Perhaps one solution might be to look at this problem in terms of concept teaching rather than concept learning. We might consider defining concept teaching as those cases where the instructional instances that you use are smaller in number than the list of performances that you expect from the student. If the instructional instances are a small proportion of the total domain of performance that you expect from a student, the job is a concept-teaching job.

Perhaps this much-maligned approach which some have called S-R theory can be helpful in that kind of analysis. It would seem to me to be a reasonable expectation that what a theory can accomplish here is to tell us what is the smallest number of steps that are required to produce the desired terminal performances. In other words, what is the smallest proportion of the total instances that you can get away with in a given teaching situation?

If you conceive of the problem in this way, and then ask the question, "Are there findings in the literature that are relevant?" you may be able to provide a much more practicable analysis. Is there something in the literature that tells you what the smallest number of instances is that you can get away with?

Can S-R analysis really be useful here? I am optimistic. The terminal performance you expect from a student has to be described in some way. One way of describing it is by describing a test you wish the student to pass. Test questions lend themselves to specification in stimulus and set response terms.

The point I am trying to make is that problems in concept teaching, as I defined it before, provide a reasonable field for research. It's possible that S-R may turn out to be quite useful in solving problems in concept teaching. It seems to me that this view is somewhat different in emphasis from a research approach that assumes there is something in the head called a concept, and asks how did it grow.

• *Dr. Gephart:* I am going to have to assert the prerogative of the Moderator and close the discussion at this point so that we can keep some proximity to our schedule.

I would like to make one statement, though. We have tended to be quite negative this morning in our comments about prior accomplishments. I wonder if this is perhaps an instance which we might generalize from the prior statements of Dr. Glaser: that negative instances haven't produced much positive suggestion.

CHAPTER TWO

PERCEPTUAL LEARNING IN EDUCATIONAL SITUATIONS

BY

ELEANOR J. GIBSON

CORNELL UNIVERSITY

I am particularly glad to have the opportunity to talk to this symposium about perceptual learning and its role in education, for it seems to me to have been overlooked or treated as an unworthy stepchild, while other kinds of learning have been pampered in the laboratory and offered as models in educational circles. A recent book edited by Melton, *Categories of Human Learning*, provides an example of this neglect. Presumably a taxonomic review of prototypical learning situations and issues deriving from them, the book includes chapters on classical and operant conditioning, rote verbal learning, probability learning, short-term memory and incidental learning, concepts, problem solving and perceptual motor skills. The nearest thing here is "perceptual motor skills." But learning to hit a baseball or perform a tracking task is not the prototype of perceptual learning.

Exploitation of concepts from learning theory in education has not, over the years, been very impressive. What has received plenty of attention is programmed learning, which has theoretical underpinnings derived from operant conditioning. Concept learning has had a certain success in educational circles too, made fashionable by Piaget's influence. As a colleague of mine said after observing him at a conference of educators, "The air seems to part in front of him as he enters the room."

Why should perceptual learning have been so neglected, both by scientists and educators? Perhaps because people think of perception as having to do mostly with pictures—and indeed Gestalt psychology left many students with the impression that the laws of perception are based entirely on two-dimensional drawings. Or on the other hand, perhaps they think it has to do just with space, so the educational implications would be only for mastering new means of locomotion, such as landing a plane. "School learning" is generally thought of as involving neither pictures nor space, but symbols—numbers and words. As a result, verbal association and conceptual rules have seemed the obvious principles to lean on. But, as I hope to show you, symbols must be differentiated before they can be associated with anything, and rules are minimally effective as mere verbalized relations. Structural constraints must be perceived as well as verbalized.

The real reason, however, that principles of perceptual learning have not found wide application in education is that no one has thought much about the principles, or made it very clear what perceptual learning is. I am forced to try, therefore, to tell you what I think it is, and what its useful principles are. Only then can I give you examples of its implications for learning school subjects. The plan of my paper will be to consider first the nature of perceptual learning, and second to introduce some applications to learning in school situations. By applications, I mean psychological analysis of the task and skill to be acquired, rather than a list of recommendations for teachers; but I would hope the latter might eventually follow.

A THEORY OF PERCEPTUAL LEARNING

A decade or so ago, my husband and I published an article which we called "Perceptual Learning: Differentiation or Enrichment?" (Gibson & Gibson, 1955). It was the result of a debate with Postman, in which we took the view that perceptual learning was basically differentiation of the stimulus situation, rather than accrual of contextual associations or cognitive elaboration. Postman (1955) argued that enrichment by images or cognitive context was not the necessary alternative to differentiation, and stressed a view more consistent with modern behaviorism, emphasizing response learning.

There are indeed several alternatives, and in order to make my own position clear I should like to state them. They have in common the fact that they are all *additive* mediation theories, a term which I think characterizes them better than our earlier term, *enrichment*. They can be classified into cognitively-oriented and response-oriented theories, in contrast to one which is stimulus-oriented. Cognitively-oriented theories can themselves be divided into several kinds. One of them considers perception as a kind of submerged problem-solving process, with development occurring as experience piles up assumptions on which inferences may be based. Originally Helmholtzian, this view has many present-day spokesmen such as the transactionalists, and, with his own special touches, Bruner (1957), who thinks of the process as one of trial and check of hypotheses and of categorizing. Another view is that perception develops through the formation of a schema. As experience accrues, the schema is elaborated and the impoverished stimulus can then be filled in by the richer schema to afford a more sophisticated percept. Bartlett's use of the schema in analyzing remembering was borrowed by M. D. Vernon (1955), his student, for her analysis of perceptual development. Piaget, of course, uses the concept of schema in a somewhat similar way—perception involves matching input to a schema and "accommodating" to it, though he stresses the active nature of the process to a somewhat greater extent.

The response-oriented theories can also be divided into two kinds. One is the theory of the Soviet perception psychologists, who refer to it as the "reflection" theory (Leontiev, 1957). I prefer the term "motor copy." Very simply, perception reflects the world by making a motor copy of it. A child palpates an object given him to identify first with gross and later with skillful exploratory movements, his "image" of the object improving and coming closer to the reality as exploratory movements develop. This is another kind of schema theory, similar to Piaget's and also to the theory of Hebb (1949), who proposed that eye movements tracking the contours of objects resulted in the building up of cell assemblies corresponding with visual form perception. The Soviet psychologists go along with the relation of eye movements to form perception, action literally "mirroring" the object. Exploratory eye movements do develop in children, as research in this country as well as in Russia has shown, but I would quarrel with the notion that making a copy is fundamental to perceptual development.

The response-oriented theory most popular among American psychologists is not a copy theory. It goes by the name of "acquired distinctiveness of cues." The idea was first expressed, as far as I know, by William James, who spoke of two "terms," originally confusable, being "dragged apart" by more distinctive associations which they had acquired. Miller and Dollard (1941) recast the theory into behavioristic terms and added the notion of acquired equivalence. The notion of additive mediation can be seen quite literally in this theory. Two stimulus displays, originally confusable, acquire distinctiveness through association with different and distinctive responses that provide "little *s*'s" so that the sum of stimulation is now different. Additive mediation can presumably result in equivalence, too, if identical responses are associated with different stimuli. The typical experiment for investigating this hypothesis consists of teaching subjects in an experimental group verbal labels for unfamiliar visual shapes such as those contrived by Attneave and Arnoult and then showing in an ensuing discrimination test that they are more easily differentiated than in a suitable control condition (Arnoult, 1953). The big difficulty here, of course, is to know whether it is really the added responses that give distinctiveness to the visual stimulation.

In one way this theory of acquired distinctiveness is more like my own theory than the others are, because it emphasizes an increase in differentiation as the fundamental characteristic of perceptual learning. But in another way, it is worlds apart, for it makes the assumption that the stimulus must be *supplemented* by more stimuli, arising from response mediators, to account for learning. I do not accept this assumption and would assert, instead, that stimulation is already full of information and that perceptual learning consists of detecting the relevant information, filtering the distinctive features from the irrelevant or noisy input. This is a drastically different idea—perceptual learning is not addition but reduction. To convince you of its truth, I must explain in more detail four points: (1) what I see as the criterion of perceptual learning; (2) what is learned in perceptual learning; (3) some taxonomy for the area; and (4) the way in which I think the learning occurs.

Definition of Perceptual Learning

The criterion for perceptual learning, as my husband and I argued in the article previously mentioned (1955), is an increase in specificity of discrimination of the stimulus input. I would still accept this—what is originally perceived as homogeneous, random or confusable becomes differentiated.

The experimental example which we gave will still do as a simple prototype, though I shall later provide some needed taxonomy. The experiment made use of a number of pictures of coiled lines (see Figure 2.1) that had been constructed to vary in three ways, the

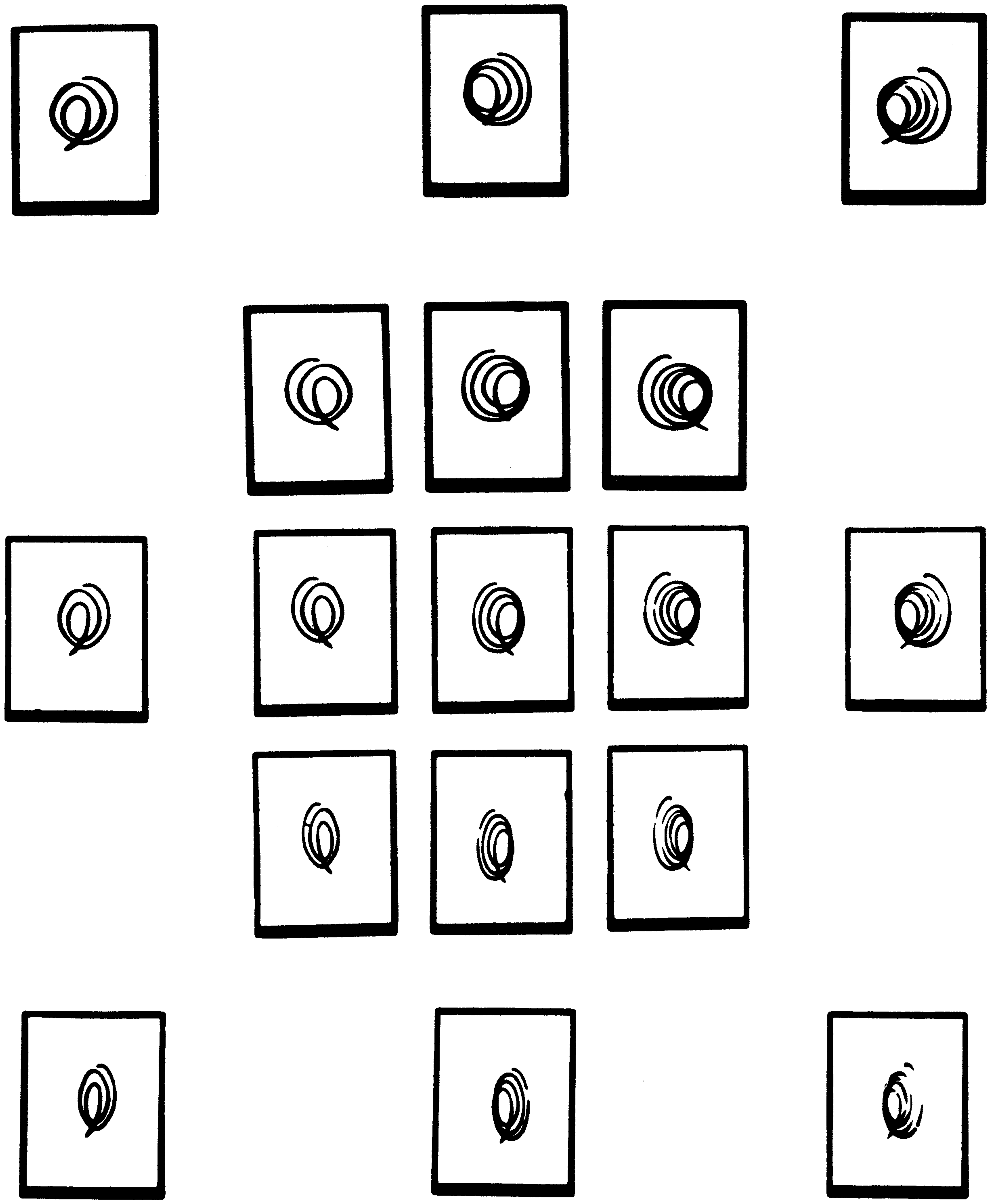

Fig. 2.1. Coiled line figures varying from a standard (center) in number of coils, degree of horizontal compression, and orientation.

number of coils, degree of horizontal compression, and orientation with respect to other members of the set (some were reversals of others). One of the pictures, printed on a card, was shown to the subject with the instruction that he was to examine it carefully so as to recognize it again. Then it was inserted in a deck of cards containing all the pictures, as well as others identical with it. The subject was told to look at each of the cards and to pick out those identical with the standard. After one run through the pack, it was shuffled, the standard shown again with fresh exhortations to examine it carefully, and a new run made.

Our interest in this task was in the subject's errors and their rate of decrease as the runs continued, for they did decrease. A group of adult subjects reduced the number of cards confused with the standard from a mean of 3 in the first run to 0 in an average of 3 runs. Children of an intermediate age (around ten) reduced the confusion errors from 8 to 0 in a mean of 5 runs. Younger children (around seven) had a mean of 13 confusion errors on the first trial. In a mean of 7 trials the errors were reduced to a mean of 4, but most of these children did not achieve perfectly specific discrimination before exhaustion set in. In all the subjects, however, perceptual learning in the sense of an increase in specificity, occurred, without reinforcement or assignment by the experimenter of distinctive responses. Errors were related to the number of distinctive features in common; that is, a card differing by only one feature from the standard, say in degree of compression, was more often confused with it than one differing in two or more features.

The achievement here, I think, is coming to perceive as unique something which was not, in the beginning, readily distinguishable from other members of the set. What is it that has been learned?

What Is Learned in Perceptual Learning?

What is it that is learned as perceptual differentiation increases? I have already said that I do not think it is responses, or a schema, or a representation. Of course I would not deny that responses occur or that representations of objects may develop. But I do not think that they are propaedeutic to, or the basis of, perceptual learning. I think what is learned are *distinctive features* of things, *invariants* of events over time, and *higher-order structures* of both. These three properties have something in common for the perceiver, as I shall show later, but they need to be distinguished, too, for reasons of taxonomy. Let me say a little about each of them.

Distinctive Features. I have borrowed the term "distinctive features" from Roman Jakobson, who with Morris Halle analyzed them for phonemes, the smallest units of speech (Jakobson & Halle, 1956). Phonemes can be differentiated from one another by their bundles of distinctive features, each one being unique. The features are constrasting differences on a dimension or property of the members of a set. In the case of phonemes, examples of contrasts are grave-acute, lax-diffuse, vocalic–non-vocalic, and so on. Jakobson's list of 12 is enough to differentiate all the phonemes of all languages. The features are relational, for they must be invariant over various transformations, such as different speakers and intensities. Only differences, obviously, carry information for discrimination, and it seems reasonable to expect that, for the most part, they must be learned, though it may be, especially with precocial animals, that nature provides a small vocabulary of properties which are attention-getters at birth.

Two psychologists also used the term "distinctive features" more than twenty years ago. They were Robert Gagné and James Gibson, who were investigating during World War II the means of improving training for aircraft recognition (Gagné & Gibson, 1947). In a comparison of two methods over a number of classes of airmen, they found that teaching contrasting distinctive features of the various planes was more effective than a system purported to promote fast recognition of a total "Gestalt." The students themselves emphasized that only the differences should be taught.

Recognition of faces as it develops in infants seems to be a nice case of learning distinctive features in early development, though our knowledge of the process is not yet so refined as to permit definition of contrasts. At one to two months, a cardboard oval with spots, small squares, or angles where eyes would be elicits marked attention and even smiles (Ahrens, 1954). By three to four months, realistic eyes on a dummy or cardboard oval are necessary, and after that a nose portion, but not the mouth. By five to six months, the mouth begins to emerge as a feature commanding attention, especially when in motion. By six months, a widely drawn mouth is more effective in eliciting smiles than a pursed mouth. By six to eight months, the infant begins to differentiate one adult face from another. It can differentiate bundles of distinctive features so as to identify a given face as unique.

Besides distinguishing objects in his world by learning their feature differences, the human child learns distinctive features of printed displays of all kinds; pictures, letters and numbers. I have been particularly interested in analyzing the distinctive features of letters (see Gibson, 1965). Improvement in discriminating letter-like drawn shapes continues up to nine years or so, and I am very concerned with what changes developmentally to render this process more accurate and more economical. One letter may differ from another in ways that are not critical, not invariant distinctive features, as well as in ways that are. A five- or six-year-old has simply not got, as yet, the vocabulary of features that are critical filtered from the noisy ones, and may make a decision on some irrelevant feature such as size. We have been trying to teach a small portion of the alphabet to four-year-olds this summer, and were struck by the remarkable difficulty of the task for them. They had much farther to go than we thought. But the two who finally succeeded were able, in transfer tests, to disregard noisy and partially redundant features that were not distinctive contrasts, though they had not been specifically taught to do so.

Invariants of Events. While one can speak of distinctive features of events, such as a rhythmic property of a musical phrase, it is discovery of an invariance over time that best characterizes perceptual learning for events. The spatial constancies are prime examples. When an automobile approaches me, I do not perceive it as a different vehicle from one microsecond to the next, although its projected image on my retina is in constant change. I perceive it as the same car, coming nearer. It is out of such events, involving time and motion, that perceptual constancy develops, I believe. Continuous change is important, because it permits extraction by the observer of a rule, a mathematical invariant relating properties of stimulation over a temporal change. Events, in short, are units in time. We can only speak of events if there is some invariant giving unity.

Shape constancy, as well, is extracted through observation of events such as the continuous turning of an object in space, or walking around it. An infant can give himself opportunities to discover the invariant relations in events by moving his hands toward himself, or toward each other (in which case size-distance features of the stimulus do not change) or by rotating objects. Piaget (Piaget & Inhelder, 1956) claims that infants actually experiment in this way to learn geometrical invariants and rules for happenings in space and time. His intellectual emphasis I would be cautious in accepting, but that extraction of invariants of events is an important kind of perceptual learning I am sure.

Studies of the development of conservation in children, following Piaget, have emphasized the role of verbalization. Some new intellectual operation comes in, some writers would have it, and in the words of Bruner, "It is plain that if a child is to succeed in the conservation task, he must have some internalized verbal formula that shields him from the overpowering appearance of the visual displays . . ." (1964, p. 7). But if this is true, we must account for the genesis of the verbal formula. Simply telling the child does little good (Wohlwill and Lowe, 1962). It seems to me that the child shifts from judging by the features of a static display to discovery of the invariance in the event. For example, a five-year-old who sees water poured from a tall thin jar to a fat squat jar may judge quantity on the basis of some size aspect of the jars and thus fail to "conserve." He has to see the continuity of the water over shape transformations before he can perceive a unit event in time. The more complex the transformations—e.g., the greater the differences in size and shape of containers—the harder this is. Anything that destroys continuity in the event by calling attention to the stationary display reduces the chance of perceiving invariance in the event, whereas enhancing continuity by centering attention on the pouring facilitates it (Bruner *et al.*, 1966, pp. 193 ff.). One can read about conservation of mass in a book, but I cannot believe that the scientists who first made statements about it did not begin by perceiving invariance of a property over time and despite transformations which they learned to ignore or compensate.

Perception of causality, the least understood of the constancies, is again the extraction of invariance from transformations in the course of an event. A rolling billiard ball bumps another, and the loss of its own velocity is compensated by that gained by the second. Michotte (1954) has convinced most of us that the perception of "cause" here—though I should prefer to say the invariance—is as direct as that of modal qualities of objects, such as color. I would want to see more work on this with children before I would agree that no perceptual learning is involved. But in any case, the observation is the kind that lies at the very base of science education.

Higher-order Structure. My third category of "what is learned" in perceptual learning, higher-order structure, applies both to objects and events. Perceptual development, in the case of objects, begins with detecting distinctive features and unique bundles of them for sets of objects. But the bundle of distinctive features of an object may possess higher-order relations between the features, or there may be redundancy of many orders in the correlation of features. Perception of these relations, and of high-order redundancies, is a sophisticated achievement and one which should be of great concern in studying perceptual learning. It is here, in particular, that one senses a possible contribution to education. Perception of regularity and higher-order constraints obviously frees the information processer to increase enormously the input he can handle.

For a simple example, let us return to development of the infant's perception of faces and facial characteristics. There is evidence that assembly of a vocabulary of features occurs prior to the detection of higher-order relations. Perception of facial expression develops late, and this is a case of noting invariant relations over different faces. Again, discrimination of a pretty face from a not-pretty one is late, not only because cultures have conventions about this, but because the redundancies involved—symmetry, ratio of length to breadth, and so on—are not first-order features.

That second- and third-order constraints are slower to be picked up seems obvious, and there is evidence that this is the case in work on information processing in adults (Oostlander & DeSwart, 1966). It takes longer to process them at first, and in children they may not be utilized at all; but practice in perceiving them can be very effective. Let me remind you of Bryan and Harter's (1899) classic experiment demonstrating progress from receiving lower units to higher units in learning telegraphy.

That young children are slow to perceive and utilize regularities and redundancies which adults find obvious is illustrated by an experiment of Munsinger's (1967). He prepared 8 x 8 matrices of squares, each with four quadrants (4 x 4 submatrices), two of which were redundant with respect to one another and the other two random with respect to the redundant pair, depending on which squares were filled in (see Figure 2.2 for a sample matrix). One kind of redundancy was repetition of a submatrix. Another was reflection of a submatrix, so that the two were symmetrical. Half of the subjects had the filled squares of the redundant matrices colored blue, with the expectation that the redundancy would be enhanced. The number of filled squares in different patterns varied from two to four, providing different levels of difficulty. The subjects were shown a pattern, and immediately afterward asked to put checks in an unfilled matrix so as to duplicate the arrangement of filled squares. Subjects were children (grades two, three, five and six) and adults. There was little difference between the performance of children and adults when the pattern was simple (e.g., only two squares filled in a submatrix), but a big difference when it was more complex. The difference was accounted for by the interaction of complexity with redundancy and age; the adults used the redundancy to complete the more complex patterns. Not even the color helped the children in this case, but it did facilitate performance of adults.

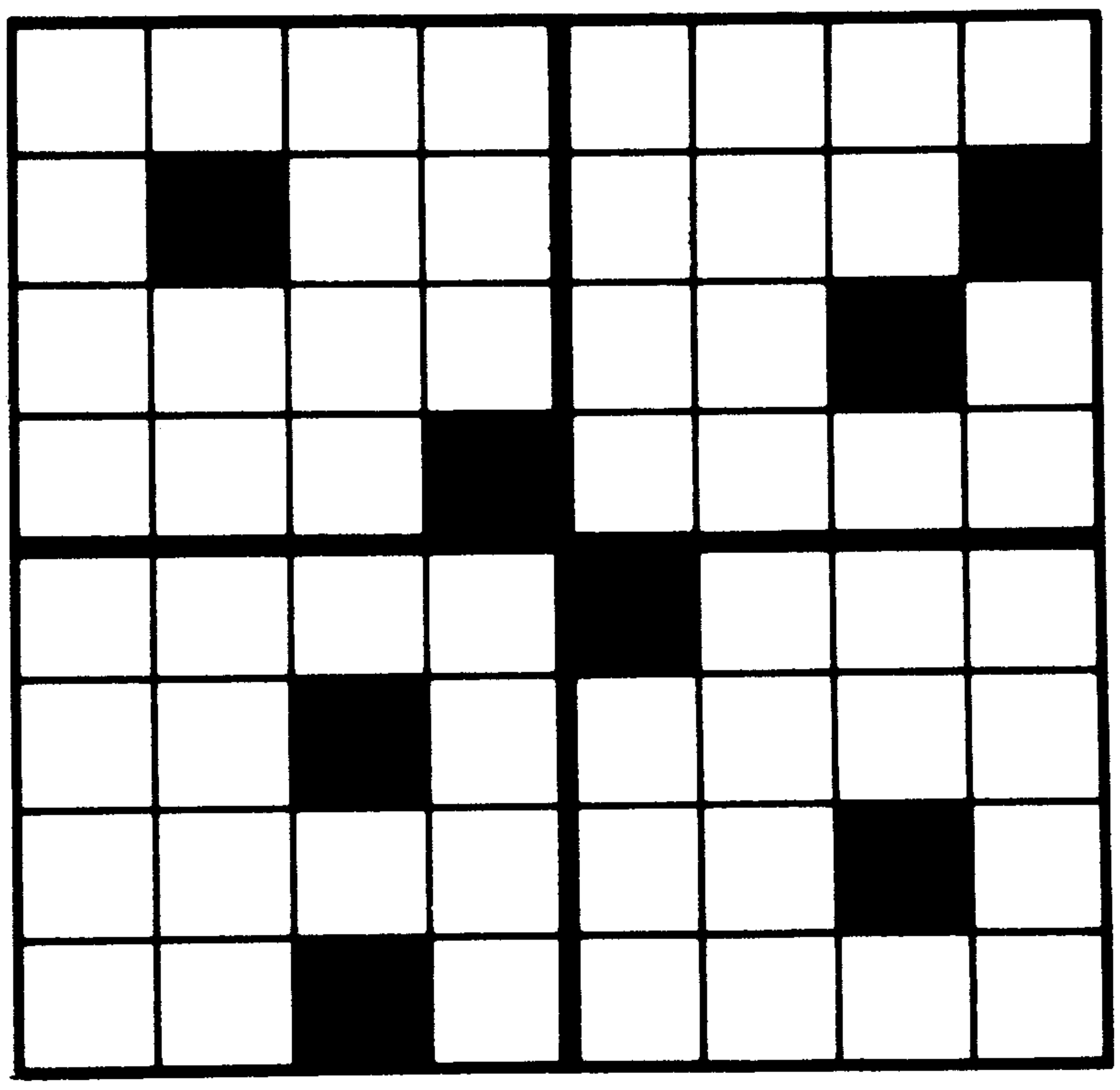

Fig. 2.2. A sample 8 x 8 matrix of the type used by Munsinger. The upper left and lower right quadrants are symmetrical with respect to one another.

An experiment by House (1966) with retarded children presented discrimination learning tasks with dot patterns as stimuli. The patterns varied in redundancy and in form of redundancy (symmetry as opposed to repetition without reflection). Ease of discrimination was related to both. Symmetry, continuous lines, and distinctive clusters of dots were generally utilized by retardates, but order of other kinds, such as repetitive relations between subpatterns, might or might not be effective. She concluded that failure in these children was due to a perceptual deficit, inability to perceive the pattern differences as described by these structural characteristics.

Taxonomy of Perceptual Learning

Distinctive features, invariants, and higher-order structure, the things learned in perceptual learning, can be recognized in several different "media." These media give us a simple and convenient taxonomic classification. The media describe aspects of the world, man's ecology, not differences in process. This is the right basis of classification for a stimulus-oriented theory. Perceptual differentiation occurs within five media: objects, space, events, representations, and symbols. The child's earliest perceptual development is greatly concerned with objects, space (space close by, at least), and events, especially as he achieves locomotor and manipulatory abilities. Learning with pictorial representations and symbols occurs later, and most of what I shall have to say about school learning is concerned with perceptual learning in these media.

Learning Principles

I have said that I am convinced that perceptual learning is a reduction, not an addition. You will not be surprised, therefore, that I do not choose *association* as the ultimate principle of such learning. I think, instead, that *filtering* is the right concept. Distinctive features, invariants, and structure (rules, if you like) are not integrated but rather filtered or extracted from noisy and irrelevant input. They are, in James's sense, dissociated rather than associated.[1]

How does this happen? In terms of factors facilitating learning, anything which enhances distinctive features or impoverishes noisy background in relation to them should help; exaggeration of contrasts in the case of distinctive features, and "showing the bones" in the case of structure. There is a summer course at Andover Academy in "Perceiving" which aspires to teach structure in nature. The methods are "Bauhaus" methods used by Maholy Nagy at the Chicago Institute of Design. One exercise is slicing an onion and printing the cross-section on paper or cloth as a design. Another is taking rubbings of tree bark as illustrations of textural regularity in nature. These seem to me ingenious, because by printing the structure, it becomes dissociated from the objects so as to reveal and enhance it.

Mostly, however, there is no teacher to point out contrasting features and to separate the pattern from the partially correlated but non-critical aspects. The child does it himself, and I think he does it very actively. Perception is active, not in the sense of performance or execution, but in the sense of exploration. Attention

[1] "What is associated now with one thing and now with another tends to become dissociated from either, and to grow into an object of abstract contemplation by the mind. One might call this the law of dissociation by varying concomitants." (James, 1890, Vol. 1, p. 506.)

to aspects of the environment is demonstrable in very young infants, as Fantz (1965), Kesson (Salapatek & Kesson, 1966), and others have shown us. This early attention seems to be a kind of capture by compelling stimulus properties; and the shift to voluntary attention, indicated by directing the regard, turning the head, and canvassing the environment in a more and more systematic search pattern develops only gradually. But perceptual development is characterized by exploratory movements of the sense organs, a search for distinctive features and invariants. It seems to me that the motivation is primarily "curious" or "intrinsic" as Berlyne (1966), Hunt (1965), and others have called it, unlike the motivation of most performatory behavior.

The big question which I am always asked at this point, is what is the selective mechanism in perceptual learning? Is a distinctive feature selected as relevant because it wins a reward, gets confirmation or external reinforcement? Although this *could* happen in a teaching situation, I think that it is not the true selective principle of perceptual learning. So much of it goes on in infancy and is somehow self-regulated; nobody is giving out M and M's, or slaps or even praise. I think the reinforcement is internal—the *reduction of uncertainty.*

Stimulation is not only full of information, it is *too* full of information to the neonate; or to the underprivileged child who lives, I am told by the experts, in a world of confusion and apparent disorder; or to the first-grader presented with a reader; or to a schoolboy given his first algebra text. Reducing this information is the goal and the end of perceptual learning. The invariants are there in the stimulus, but so are a lot of noise and irrelevant information. Distinctive features, invariants and higher-order structure have exactly the function of reducing uncertainty, filtering the gold from the dross, making one out of many and thus allowing *more* information to be handled.

Garner (Garner & Clement, 1963; Royer & Garner, 1966) has demonstrated in clever experiments that the "good" pattern or structure is the unique one. This fits too. The good one is the highly differentiated one that is not confused; it has specificity within the set and it stands out from the noise. Specificity and "uniqueness" are not automatically appreciated by the perceiver. They can be predicted for various displays by the wise experimenter; but perceptual knowledge is required for the detection of univocality in sets of any complexity.

To conclude what I can say in this short time about perceptual learning: It is defined as increase in specificity of stimulation to discrimination; *what* is learned are distinctive features, invariants in events, and structural regularities; and these *are* learned, that is to say extracted from the total stimulus, because they serve the function of reducing uncertainty.

APPLICATIONS TO EDUCATION

Now I want to tell you some ways in which this view of perceptual learning can be usefully applied to education. I have four points to make. I would like to say "principles," but that would be sheer ostentation. I will illustrate my four points with examples from "school learning." I must make one more distinction before presenting them, the distinction between *content* and *strategy* in a task. This is a useful distinction rather than a profound theoretical one, for the two are not easily divorced in actual operation. I am willing, however, to be a structuralist and a functionalist at the same time. I want to talk about units of content of perception and also about active perception, for being a learning theorist, I conceive of perception as active, exploratory, and directed, rather than as passive reception of anything exposed to the sense-organ.

UNITS

My first point is that *tasks can be analyzed into units of perceptual content which form hierarchies*. I think that task analysis is essential for a theory of instruction as well as for a theory of learning. A task varies at different stages of mastery and the optimal skill at the end may bear very little resemblance to the skill at the beginning, but there is an order in the progress. For any task, therefore, I want to talk about *lower-order units* and *higher-order units*. The order of the perceptual unit changes with the stage of learning, progressing from lower to higher. I have a good precedent in making this statement, for Gagné (1962) made such analyses for learning mathematics and showed their usefulness in setting up a program of instruction. It has been demonstrated, furthermore, that the factor structure of a task changes with stage of mastery. Fleishmann and Fruchter (1960) showed this for learning Morse code; in early stages, achievement was best predicted by tests of fine discrimination of details, and later by tests of "Gestalt" perceiving, grasping of "wholes."

Elkind (1965) reported that tactual discrimination of letters correlated with reading achievement in its early stages, but not later; and this makes very good sense.

I would like to take reading as my example, since this is the task I have worked on. I think acquisition of reading skill can be divided into four phases or stages which overlap, but nevertheless have an order in that the earlier ones must precede the later ones. The first phase, which goes on for many years, is learning to speak. This precedes the others both practically and logically, for the writing system is a second-order symbol system decoding to speech, itself symbolic. The second phase begins when the child is first presented with printed letters. He has to learn to discriminate them from one another, for they are the smallest units of the writing system from which words and sentences are built. It may seem to you trivial to distinguish such a stage, but I assure you it is no mean achievement. In fact, progress continues for several years. If you doubt it, try learning to distinguish the graphemes of an unfamiliar alphabet, such as Hindi or Arabic.

The third phase is learning to decode letters to sound. It sounds so easy—just a simple paired-associate learning task. But this is not the case, if you think about it, especially with English where sound-letter correspondences are by no means one-to-one. Letters are the smallest units of the writing system, yes, but the spelling-sound *correspondences* are not reducible to one sound for one letter or vice versa. This does not mean that there are no rules or order in the system; there are, indeed, but they can get fairly complicated. The child, if he is going to be a good reader, cannot learn by a sheer rote memory procedure, for he needs to learn the order in the system, the internal structure given by spelling patterns and syntax. He may start with rote learning, decoding simple words like "oh," "see," "baby" to speech. The kind of reading vocabulary one would achieve by simply learning words with no transfer by rule would be severely limited. This may well be all that some people have. But we know that detection of structural constraints in the spelling system is possible, for it can be shown that skilled readers perceive pseudo-words fitting the rules far better than ones which do not. What we do not know is exactly how this order is picked up, and that is where my research is focused at present.

This takes me to the fourth phase which, as I have shown already, must overlap the third, the learning of higher-order units. A good reader does not read letter by letter, decoding one at a time in sequence from left to right to get the word. Research by Newman (1966) and by Kolers and Katzman (1963) has demonstrated dramatically how much easier it is to read a word presented simultaneously than the same word presented letter by letter. And we know from Cattell's (1885) classic work that with a fast exposure of less than 100 milliseconds, an adult can read about four unconnected letters, a ten-letter string if it makes a word, and twenty or more letters if there are words forming a sentence (The dog barked at the cat). As redundancy is added, the good reader picks up bigger units.

I am sure there are examples of a shift from low-order to high-order units in other subjects than reading. In mathematics a child begins by handling numbers a digit at a time, but a skilled mathematician handles equations or whole theorems as units. Patrick Suppes tells me that when a child is learning the multiplication table, there is not just a slow constant decrease in latency of giving answers, but a sudden dive. As Suppes put it, the child has just acquired a "read-out." I would say that 7 x 9 = 63 has become a unit. I do not believe any tachistoscopic experiments of the kind we have done with reading have been done with arithmetic. I think it would be interesting and provide a method for studying unit-formation in mathematics.

Reading music is another obvious case for feature and unit analysis, and progression to higher units of structure, even to the pattern of a symphony.

Distinctive Features

Now I come back to distinctive features again, because my second point is that *low-order units are distinguished by distinctive features.* This is true for the content of school tasks as well as of any others, but in the former case we have an obligation, as teacher-researchers, to find out what they are. In my research on reading, I have spent a lot of thought and experimental time trying to analyze the distinctive features of letters. The type I chose was simplified Roman capitals. I began by working out simply, intuitively a list of twelve features which would give a different bundle for each letter. This is not easy, as it turns out, but I got one that worked that I liked fairly well. The features were checked off for each letter as either present or absent. The ones included are shown in the chart below (see Figure 2.3). The features are listed down the left-hand side, and some of the letters

Features	A	B	C	E	K	L	N	U	X	Z
Straight segment										
Horizontal	+			+		+				+
Vertical		+		+	+	+	+			
Oblique /	+				+				+	+
Oblique \	+				+		+		+	
Curve										
Closed		+								
Open vertically								+		
Open horizontally			+							
Intersection	+	+		+	+				+	
Redundancy										
Cyclic change		+		+						
Symmetry	+	+	+	+	+			+	+	
Discontinuity										
Vertical	+				+				+	
Horizontal				+		+	+			+

Fig. 2.3. A sample "feature chart" for letter discrimination. (From E. J. Gibson, Learning to Read, *Science*, 1965, *148*, 1066-72. Copyright 1965 by the American Association for the Advancement of Science.)

across the top. A plus is entered when the feature is present in the letter. The pattern for every letter is unique, but some letters share a number of features, while others share few or none.

We did an experiment with four-year-old children, from whom we obtained a confusion matrix for all twenty-six letters. We correlated the results with predictions from the feature list with only fair results. A multidimensional proximity analysis of the matrix was rather encouraging for a straight-curve and a diagonality dimension came out of it. We are getting a new confusion matrix with adults now. There will be less noise in the data, so it may take us farther in discovering what features are actually used.

I have not thought much about features of units of other subjects, but I think reading a map in geography would be a nice one to play with. What characterizes a region as unique? I suppose mountains, rivers, lakes, timber, deposits of ore and so on. I am reminded of a story of Bruner's; he gave a geography class a map of a real region with all these features indicated and asked the pupils to predict the locations of a city, some towns and villages, and a railroad. He promised to tell them afterward where these actually were. It was a very successful maneuver motivationally. Could it not be used for research on critical features too? Research is needed not only in map-reading, but in ways of displaying an area most effectively on a map. What is the optimal critical information?

The Role of Structure

My third point is that *higher-order units are distinguished by structure*. It is perception of structure, that is, internal order, relations, and correspondences, that permits processing of bigger and bigger units. Lenneberg (1962) in speaking of grammatical rules, says that they are rules of correspondence, a formalization of our ability to perceive similarity between physically different stimulus configurations. The child's first lesson in discovering structure and using rules comes in learning language. All the child needs to learn this lesson, apparently, is to hear plenty of it. A child whose environment is impoverished in this respect misses his first lesson in discovering order and never catches up. It seems as if the learning of order in speech transfers to the discovery of order and structural relations in other spheres. As Lashley (1951) made clear to us, the problem of serial order in behavior is a central one for psychology, but we have not gone very far in solving it.

In mathematics, order is obviously all important. Structural principles such as similarity, equality, symmetry, transitivity, and congruence are essential concepts and to grasp them they must be made perceptible. I like especially an example of Wertheimer's from his book on *Productive Thinking* (1945). He taught a young child (only five-and-a-half) how to find the area of a rectangle, drawing it for him, filling it with small squares, counting them in various ways, and so on. Then he presented the child with a parallelogram and asked him if he could find its area. After some moments of puzzled staring at it, the child asked "Do we have a pair of scissors?" They were produced and he proceeded to snip a triangle off one end and fit it neatly on the other. Then he said, "Now I can do it." He literally perceived the solution.

A colleague of mine who is interested in teaching mathematics to quite young children described a venture of his in teaching the concept of moments by having the child draw marbles from a cup holding different numbers of red or green marbles. A listener said, "Why don't you derive it for them?" He answered, "It's a fact of nature. Sure there are axioms for deriving it, but first you have to discover it."

A more elaborate example of the value of making a complex mathematical function or geometric object or physical principle perceptible is the use of computer-produced perspective and stereographic representations, and animated movies to illustrate complex functions.[2] The computer renders, in terms of points and lines, a series of pictures that have been described to it in a more abstract way.

I mentioned earlier the importance of processing high-order units in reading, the units structured by morphological and syntactical rules as well as meaning. Teachers have always, I suppose, encouraged children to use redundancies contributed by meaning to help decode new words and to increase reading speed. In early stages of reading this can be a dangerous thing, not only because it often leads to wild guessing, but because it hinders learning the other important kinds of order, in particular the spelling patterns and spelling-to-sound correspondence rules. They may be hard to formulate (see Venezky & Weir, 1966), but nevertheless they seem to be infinitely valuable for reducing information.

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[2]These techniques are discussed in two articles in *Science* (Knowlson, 1965; and McCue & O'Keefe, 1966). Knowlson gives a fascinating description of a computer-produced movie by F. W. Sinden on "Force, Mass, and Motion."

Our research (the experiments with pseudo-words that I referred to) demonstrated that morphological generalizations are possible and transfer to the reading of new words. This learning has begun to occur by the end of first grade (at least in children from a superior educational background) with short letter-strings, and has advanced to perceiving order in somewhat longer ones by the end of third grade (Gibson, Osser & Pick, 1963). I think that development of *learning sets* for abstracting common patterns has something to do with this. Probably, also, redundancy of sound correspondences with the spelling patterns facilitates the extraction of patterns. How the generalization comes to be made is not an easy question to answer, since discrimination of words on the basis of *differences* of a letter or two is the obvious early accomplishment. A second-order generalization must follow differentiation, when regularities are perceived and used for transfer. We are working on this now, testing the value of a learning-set procedure for promoting abstraction of common patterns.

In experienced readers, still larger units than words are processed, the grouping principles probably including meaning and syntax. The influence of context on speed of reading has long been emphasized (Tinker, 1958; Morton, 1964), but the evidence for pick-up of word chains as units has not been too clear, nor have the reasons for it. Does a skilled reader grasp more in one fixation just because he has increased his "span," which is a constant that has been "stretched" with exercise so that he "takes in more at a glance?" Advocates of practice with a tachistoscope (Renshaw, 1945) often seem to imply this. Or, on the other hand, are structural features of the material correlated with the larger units? Levin and Turner (1966) and Levin and Kaplan (1967) have provided convincing evidence of the latter proposition in new experiments on the eye-voice span, showing the influence of sentence structure in forming super-word units.

The eye-voice span is the distance that the eye is ahead of the voice in reading aloud. It can be measured by turning off the light at some point in the reader's delivery and seeing how far he can continue reading when the print is no longer visible. The reader can be stopped at a phrase boundary, or at any point before or after it which is interesting for syntactical or semantic reasons. Does the eye "reach," so to speak, for the end of the phrase, the structural unit, as a new fixation begins? This is a perfectly plausible hypothesis, since we know that peripheral vision contributes enormously to skilled reading. (If you do not believe it, try reading with occluders on your glasses which leave you only foveal vision.)

Some of the findings of Levin and his collaborators are as follows. The eye-voice span is significantly longer for structured sentences than for unstructured word lists. Eye-voice span increases, in general, with school grade. The eye-voice span tends to be longer *before* the verb in a passive sentence and longer *after* the verb in an active one. The number of times a subject reads to the end of a phrase unit, corrected for his modal eye-voice span, is significantly greater than zero. There is a tendency to read to phrase-boundaries rather than to non-boundary positions, and older subjects do this significantly more often than do second-graders. The eye-voice span is longer in the more highly constrained segment of a passive sentence form than in its less constrained counterpart in the active form.

The evidence thus supports the hypothesis that people tend to read in phrase units and that a person's span is not a constant of so many words, but rather shrinks or expands to accommodate to phrase boundaries, fluctuating with the degree of structural constraint in the sentence. Skilled readers do this more than beginning readers or slow readers. The purist may wish to argue that learning to read in phrase units is not just perceptual learning but involves short-term memory. The eye-voice span technique does not allow us to settle such an argument, but in any case we can emphasize the importance of perceiving structure, however the information is processed in the on-going act of reading. Knowledge of structure, in combination with peripheral "reaching," directs the eye ahead in the sentence so as to complete one structural unit and begin another. More study of the grouping principles involved and of the way they interact with input in perception has highest priority for researchers working in this area.

Perceptual Strategies

My fourth point is that the strategy of exploration and perceptual search develops with age and education. Selective attention, the ability to focus on points of high information and to shut out the irrelevant, is essential for discovering features and structures. It was the conclusion of a recent conference on disadvantaged children that attentiveness is the single most important cognitive attribute. Experiments of mine in collaboration with Yonas (Gibson & Yonas, 1966) studied visual search and scanning in children of different age groups. We found that speed and accuracy in finding a target improves with age, especially when the background is confusing. Experiments on dichotic listening by Maccoby and Konrad (1966) show that hearing one of two voices which are speaking at the same time improves with age. In both cases the person is shutting out the distraction so as to select the wanted information. This kind of perceptual strategy grows from birth to maturity.

Can one "train" attention in any way? In presenting an award for distinguished teaching, President Goheen of Princeton said that "when Louis Agassiz was asked his greatest achievement, he replied that he had taught men to observe."[3] I am sure there are individual differences in this trait; one thinks of Darwin on the voyage of the Beagle noting similarities and the course of differentiation over the world and finding the grand plan, the order over all. The great scientist, we would agree, does this. How do we help a child to sort critical features from noise, to find order and regularity in the world, in particular in the school world?

I am not discouraged by the orthodox psychologist who says you cannot talk about training attention or observation. That's the old faculty psychology, he says, and we were warned by people with famous names that it was wrong. We were, but we have found since that time that transfer is not simply a matter of common elements and individual bonds. Learning sets can be formed that are quite general and that are powerfully facilitating.

How do we help to build a set to observe? For one thing, we try to teach strategies, never answers. I can think of one strategy that comes as a moral from studying the perceptual constancies: How is the invariant discovered? It is discovered by observing motion—approach and withdrawal, rotation, moving projections from different angles. Moving things, or moving yourself in an orderly way is literally a good technique. Looking at something from another angle or from a distance often reveals structure. One of the dramatic examples is the geographer who went for an airplane ride and discovered, looking down on some hillocks in his own cornfield, the outlines of an old Roman fort. We cannot often do this, but any trick that reveals what is invariant over change gives order and reduces information. My husband suggested a very ingenious experiment on this plan. A first-grader is shown the stick-in-water illusion, a rigid stick on a pivot placed in a pan of water. He sees that the

[3] *Princeton Alumni Weekly,* February 22, 1966, p. 11.

stick is apparently bent. Then the stick is twisted for him, as he watches. He perceives, as it twists, that the bend acts in an impossible way. If he is clever, he perceives the straight stick transformed by refraction. A still photograph of the stick in water would never reveal the rule.

Practice in categorizing by perceptual properties, especially graded ones, was a favored method of the Montessori system. Pre-school education in Russia leans heavily on it today. I am not sure how useful it is. We badly need research in this area. I suspect that we want to give practice not so much in *ordering* things as in *finding* order. Habits of easy categorizing are to be avoided, for they threaten us with stereotypy, with blindness to the superstructure of greater value. Perhaps the best thing we can do, in our present state of knowledge, is to give a child plenty of things to observe, sets of things with an inherent order, and leave him in a non-distracting environment to manipulate them and find order. Finding a rule, reducing information, is amazingly rewarding. An educator who can give a child this experience will have him looking forever.

CONCLUSION

I must conclude on a slightly pessimistic note. In the course of studying perceptual development and writing a book about it, I have become more and more convinced that an important part of perceptual learning, grasping the distinctive features of objects and the invariants of events, goes on very early in life. When formal education begins, much of this is done, or if it is not, as in the case of a badly deprived child from an impoverished environment, it may be too late. Nevertheless, there is much for the educator to gain from principles of perceptual development. The young scholar must learn to differentiate the symbols of "book learning," and above all, he must be helped to discover the order in nature and language, in science and art.

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AUDIENCE QUESTIONS

• *Question:* I am curious about *i/t/a*, the initial teaching alphabet. I hold no brief for it, and I don't necessarily understand it, either. It would seem to me that the imposition of a different task for reading than a task that is going to have to be done later, would have to be overcome in some way. The claims are the changeover from *i/t/a* to traditional orthography works effectively. Do you have any comment on that?

• *Dr. Eleanor Gibson:* Yes. I think that basically the idea is wrong, because *i/t/a* makes assumptions that simply aren't true of reading by at least a reasonably skilled reader. For one thing, it assumes that as you read, you are decoding each letter to one sound. It is possible that at the beginning of first grade children do this, but anything that would encourage them to form a habit of doing it seems to me theoretically at fault.

It implies also that one reads sequentially each letter from left to right, and we know from good research that one doesn't read that way. I don't think that even a first-grade child does.

Possibly *i/t/a* doesn't always teach a child to do that. It may be that a bright child will start reading whole words in spite of the fact that you show him how nice it is that you can decode each letter to a sound, and that there are just forty-six of each one. But there is still the difficulty that you are not giving him from the beginning the chance to pick up the structural constraints that are present in a spelling pattern.

You see, promoters of *i/t/a* are not arguing that orthography should be changed in English. Maybe it should, but this is not the argument. They are saying, "This is a good way to start the child reading." But it seems to me that that makes little sense. If the child is going to read English spelling, then he should be learning its rules. There are rules and they are not easy to get.

Furthermore, a child doesn't learn them by just being told what they are. It may be necessary to promote a learning set to observe constraints in orthography for a child to become a really good reader. The children taught with *i/t/a* may find things easier in the beginning. That is perfectly reasonable. But we are not concerned merely with how they read in second grade; we are interested in how they are going to read when they leave school, and I am afraid that easy early success with *i/t/a* is going to deprive them of something they ought to be struggling with during that time.

• *Chairman Gagné:* Are there other questions?

• *Dr. Stolurow:* I want to relate a couple of different things you said, because they lead to implications for construction of learning experiences. The stick problem that you alluded to as an example involved a direction of student behavior around the object, and encouragement to observe from different positions. This could be translated as a structuring of the processing or the examination of the perceptual material. Yet in other remarks you indicated that you thought it desirable for the individual to explore by himself. I was wondering whether there are times when you recommended one or the other of these or whether you have some preference.

• *Dr. Eleanor Gibson:* I don't think it is necessarily desirable for an individual to explore by himself, but I have become convinced that there is a lot of perceptual learning that goes on very early when nobody really is directing the child.

I think, you see, that perception is an active search process, and that it's self-regulated and self-motivated. Furthermore, I think that there is reinforcement in discovering an invariant or a pattern of distinctive features, or especially some structure that reduces the information. I think, therefore, that children search for and select out distinctive features and structures without somebody telling them to or directing the process for them. But I certainly think that in a teaching situation, if there is a way in which you can promote this process, you should do so.

• *Dr. Stolurow:* You weren't advocating complete discovery?

• *Dr. Eleanor Gibson:* No, I would not advocate a complete "discovery method." I think often you can help a child by setting up a situation that enhances the contrasts of features or structure. Sometimes just giving the rule verbally may help, but I think one can try to provide an opportunity for perceptual discovery as well in relation to the rule.

• *Chairman Gagné:* Do we have other questions?

• *Dr. Thompson:* I would like to ask for an evaluation. What is your feeling about the Montessori procedure?

• *Dr. Eleanor Gibson:* My feeling about it is that it's too bad nobody ever did any research on it. We don't really know its value. Offhand, some of it sounds pretty good; for example, giving a child samples of colored paper and letting him make a small-scale Munsell color system all by himself. Probably he learns something about color dimensions and something about grading in general, but I don't think that there has been enough research on it.

• *Dr. Thompson:* My feeling is that it lost its popularity because there was no appropriate curriculum for it.

• *Dr. Eleanor Gibson:* I think arranging things into sets, as a lot of "new math" classes do, may be useful. You needn't say to the child, "Get busy and order this," but let him find some pattern that is there.

• *Dr. Glaser:* Let me speak up. This isn't an aggressive question—I just want to get your response to it.

• *Dr. Eleanor Gibson:* I am suspicious, after the warning.

• *Dr. Glaser:* How do you act to the difference between saying that people have active search processes and look or actively search for structures, compared with the statement that structure is a stimulus to which people respond and differentiate out of the environment? Is there a difference between those two statements?

• *Dr. Eleanor Gibson:* I think that the structure is present in the stimulus. All kinds of structures are present in it, and I think that the reason people look for them is that finding them is so obviously economical for information processing. I can't see that there is anything irreconcilable in saying that people search for it and that it's there to be found. In fact, isn't it necessary to think both things if you are going to talk about either?

• *Dr. Glaser:* I can operate both ways. I just wondered whether there was something else implied to make you say they were absolutely different kinds of statements. You don't say that?

• *Dr. Eleanor Gibson:* No, I don't. I think one fits with the other, and both are necessary.

• *Dr. Hill:* I wanted to raise a very closely related question. Is there any way that you can distinguish, either in experiments or in applications, between saying that somebody distinguishes what is the crucial characteristic in this complicated array (I think of this as your subtractive model) versus adding something such as a labeling response, a mediating response, by which he emphasizes one and not the other? You say there is a great difference between these, and I find it very hard to know how to recognize that difference.

• *Chairman Gagné:* I also wish to comment on this issue. Based on the content of this paper and Dr. Ellis' area of investigation I have asked him to serve as an additional discussant during this session.

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DISCUSSION OF ELEANOR J. GIBSON'S PAPER

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MODERATOR: Dr. Arthur A. Lumsdaine
FIRST DISCUSSANT: Dr. J. M. Stephens

• *Dr. Lumsdaine:* I think that the task of a Moderator is to moderate, and for this reason I asked Dr. Stephens a moment ago to be as immoderate as possible in his comments, so that there would be something for me to do. He has assured me that he is prepared to do this, casting considerations of chivalry to one side. So we will call on Dr. Stephens first, then on Dr. Ellis, and then for other comments from members of the panel.

• *Dr. Stephens:* When the late King George VI of England was still an unemployed duke, he was required as one of his chores to open the Wembley Exposition. At that time microphones were quite new. When he addressed his opening remarks to the microphone, there was dead silence. In some exasperation he turned to the engineers at his side and said, "These G-G-G . . . d-d-d-damned th-th-things d-d-don't work." Of course meanwhile the engineers had remedied the defect, and those were the auspicious opening words.

The thing I am about to do, I'm not supposed to do, but I am going to presume on my geriatric status and do it anyway. I want to revert to the discussion of this morning for a minute or so.

When I listened to the paper this morning, my thoughts immediately ran to the research strategy that might be implied by the various remarks, and envisaged an experiment arising from Dr. Glaser's suggestion. In this experiment we have two teachers, both of whom know a good deal about the subject. One of these teachers, moreover, knows all the intricacies, knows all the steps, and has a grasp of all the intermediate processes specified in a Gagné-like, meticulous analysis. The other teacher is quite unsophisticated and merely goes in and spouts off about a subject.

Would you predict a superior attainment on the part of the sophisticated teacher? As a marked contrast to such a teacher I would like to consider the typical mother (mentioned by Dr. Suppes), the mother who is responsible for the marvelous mastery of the vernacular which her child brings to school. If we were to program this mother, if she were made sophisticated with regard to all the intricacies and all the intermediate steps involved in this terrifically complicated task of learning the vernacular, would you expect her product to be markedly superior? If we took one centipede and gave him complete instructions with regard to what made a centipede run, would he run much better? If this is the expectation—and there's a pool being established—I would like to be in on it. I could use the money.

In anticipation of what I consider the inevitable results of such investigations, let me suggest that we ought to carry on parallel investigations to find out what goes on in the teacher. These are the things which make even the unsophisticated teacher almost impelled to do justice to the subtle necessities which we are laboriously trying to induce. Dr. Suppes, for instance, mentioned that one of the difficulties the young child has to cope with is a rather short, immediate memory. We teachers have a built-in device to take care of that. We repeat ourselves! Most of the intricacies that you worry about, I'll wager (since we are discussing processes that have been evolving for so many years), will automatically be taken care of by built-in devices. I am pessimistic about the efficacy of any great amount of sophistication in promoting classroom learning.

Now let me get back to what I am supposed to be talking about. When you first saw Dr. Gibson on the program, you may have entertained the hypothesis that she was asked to prove that, even in Phi Delta Kappa, coeducation was still possible. But I am sure that, having heard her paper, that suspicion has been completely set to rest.

If you also looked at the program, you will notice that I am the only discussant formally designated to talk about Dr. Gibson's paper. My hypothesis to account for that fact comes from Bob Gagné's association with Harold Schlosberg. Harold Schlosberg is famous for his enunciation of a law showing a clear linear relationship between the age of the speaker and his tendency to exceed the time limits. Anyway, Bob must know that there are precious few things which a sixty-five-year-old man can accomplish in fifteen minutes.

But whatever the reason, I am grateful for it. It is wonderful to have Jackie all to myself. My attention has been directed to her for so many years that I really relish this opportunity.

But I am sad to realize that I must change my attitude. Up until now my attitude has been one of unbounded admiration. Now I am constrained by the adversary system you seem to have adopted to use as a bludgeon. Instead of the congenial approach I would have welcomed, I have to dissect and dismember and call your attention to the fact that perceptual learning will never again be eligible for the neglect it is alleged to have had so far. If it is neglected from now on, and after the completion and publication of Dr. Gibson's book, then it will be a most inexcusable neglect. We are looking forward to the results that she will report and to the results of many other experiments which she has initiated, and the others which she has in one way or another engendered.

In carrying out her work from now on, I hope she will take heed of the injunction that Dr. Suppes put forward in speaking of Galileo—that people who are concerned with theoretical ideas shouldn't feel too constrained, too seriously obligated, to make immediate applications. I hope she doesn't yield to external pressures to emit educational applications. If one strikes her as important, by all means enunciate it with vigor and force, but don't let anybody force her to make applications.

This making educational applications is a mucky business when encountered in the real world. It even besmirches those gallant and pure scientists who come galloping to the rescue of the bemired educator. When Conant and Bruner and Bugelski and Gagné hold forth on theoretical matters pertaining to education, they sound thoughtful, scientific, rigorous, thorough, and seminal. But when they turn to the practicalities of educational application, they sound like us. Even at times like an uninformed "us"!

Don't worry. Don't feel this obligation. If your fancy so directs you, feel free to remain on the mountaintops, and to pursue those glorious invariances that transcend any transformation; and leave the mucking to us.

As I finally take up the cudgel, I find it interesting to see that the points to which I will really argue with Dr. Gibson are peripheral and incidental. When I speak of the essential part of her paper, my comments will be rather lame pleas for clarification; for more information.

The first point I want to take up is clearly peripheral. In her talk about perceptual learning, she has pointed to what I interpret as a disproportionate neglect of perceptual learning on the part of the educator: a disproportionate neglect. Everybody neglects perceptual learning, even psychologists, even Melton. Educators neglect everything in learning except programmed learning which should be neglected. But I detect a feeling of a disproportionate neglect of perceptual learning on the part of educators, and Dr. Gibson advances two hypotheses to account for that.

One is in her opening remarks; namely, that we as educators neglect it because nobody has thought enough about it. The other hypothesis appears in what she calls her pessimistic conclusion. Here she suggests that one reason for the neglect of perceptual learning lies in the fact that perceptual learning might have relatively little relevance for the school system as it's now constituted, for the curriculum that the school now has, and for the age range to which its chief efforts are directed.

If you made an analysis of variance of educational attainments you will find little of that variance attributable to observed differences in perceptual acuity, or perceptual skills. I make an exception—clearly—in the case of learning to speak, or hear, a foreign language; in learning to read; or perhaps in learning art. And I feel sure there would be an even more marked exception if the school gave more attention to such things as wine-tasting or sailing. But with the school curriculum as it is, I wonder if much of the observed variance in attainment could be attributed during the age range utilized to observable differences in perceptual acuity.

Now I turn to a topic which everyone else is going to mention: the role of perceptual extinction. This problem of the extinction of perception which Dr. Gibson stresses is surely one that Mr. Hill and Mr. Ellis are going to hit later on, so maybe I can treat it very generally. I was impressed with the emphasis given to the extinction of perception. The extinction of perception is, of course, an important phenomenon in its own right. We are all familiar with the things we no longer perceive, the sounds we no longer hear, the blue shadows we no longer see, the eidetic imagery which we once had which we no longer experience, and the subtle sounds in foreign languages to which we are now completely oblivious.

The extinction of perception is important in its own right. To one fixated on the classical model of discrimination learning, it would seem that the acquisition of perceptions and the extinction of perceptions should move hand in hand; they would be expected to be coordinate, but equally important, processes. From the interplay of generalization and extinction one would get such things as stable discriminations and also those occasional transpositions which result in overconstancy. One would expect then that these two forces—these two mechanisms or processes—would be complementary.

But I gather from Dr. Gibson's paper that extinction is the primary mechanism. Apparently we start off with an embarrassment of riches in the way of perception. We then have some mechanism for extinguishing all the perceptions that don't matter, and it is on this subtraction process that we must rely if we are to explain perceptual learning. If this extinguishing or subtracting process is the basic mechanism, we should certainly study it further. I am intrigued with the delightful parsimony that would result if we could determine the mechanism needed to explain perceptual extinction, and if we could then use this mechanism to explain the Hebb-like deteriorations that follow from sensory deprivation.

Located as we are within shouting distance of Guy Buswell's office, I find it interesting to note that the eye-voice span has once again become a phenomenon to be reckoned with. Dr. Gibson has mentioned some recent experiments on this eye-voice span. I haven't seen the experiments. But I want to raise a few questions (dangerously, I suppose, being in ignorance of how the experiments were conducted). I want to raise some questions about how such data could be used to give information on the mechanisms of perception.

To illustrate the eye-voice span, we can consider what happens when we turn out the lights while a person is reading. He goes on reading for some time, presumably because he has already perceived the material before the lights went out, and he merely emits vocally what he has already perceived. But this termination of visual input not only permits him to emit what he has already read; it should also permit him to elaborate, to extrapolate, on what he has already read. Suppose there is no eye-voice span. Suppose the reader gives some response immediately upon perceiving each word. Now you stop the sensory input all at once. In this situation you may still get a response even though no stimulus was ever present for that response. I tell you, for instance, "After running through the fire the man was badly———," and I stop. Perhaps some of you were listening as I spoke. Some of you who were may have quite forcefully emitted the unpronounced "burned." This is just Miller and Selfridge stuff. From the very dependencies of speech one might supply words that had never been perceived in any ordinary sense of perception. This is the same technique which Rubenstein and Aborn used to measure readability and to determine redundancy of speech. Because of those redundancies, and because of those dependencies of speech in the language, one might well emit words that had never been perceived at all.

It wouldn't be surprising if some of these extrapolations, or supplied words, were a response to structure. A well-structured sentence might well be expected to elicit a greater number of such extrapolations. It might show the relations which Dr. Gibson mentioned with respect to passive verbs and active verbs. In the passive form that I used, the word "burned" is completely redundant, absolutely unnecessary. If I used an active verb, however, "Because of his annoyance the man burned———," you might have more difficulty in supplying, with confidence, the missing word. Insofar as this extrapolation is present in the experiments on the eye-voice span, it might explain some of the materials that have been mentioned.

In my next remark I am taking on not only Dr. Gibson but Dr. Gagné and Dr. Glaser. I think I must be misreading what they say, because I find myself almost incredulous at what seems to be their suggestion; namely, that we must always learn to deal with an element or a lower-order structure which the elements produce, before we can learn the larger pattern which emerges from the smaller, lower-order structures.

I can't believe the statement. When I watch Elizabeth Taylor emoting on the screen, I know full well that my perception is based on a series of individual, static frames. Change these and you change the picture. These are the things that give rise to, and determine, the ultimate or integrated experience. But I can't believe that in order to deal with the total experience I must be able first to deal successfully with these individual frames. I must sense them, but—to throw one Gibson at another—must I perceive them? Must I see them? Must I be aware of them?

When I read a word, I must know that it is determined by the individual letter elements making it up, but do I have to be able to deal with these individual letters before I can be able to deal effectively with the word they produce? Before I can deal effectively with a tune, must I be able to deal effectively with the individual notes with which the tune has a one-to-one correspondence, and which gives rise to the melody?

These one-to-one correspondences exist, but because they exist, does it necessarily mean that the optimal order of attack is always from the lower order to the higher order? It is quite conceivable that we may be able to deal more effectively at first with the higher-order skill, and then—by individuation and analysis—attack the lower elements on which the higher-order performance was based. It seems cruel to prevent poor old Amblystoma from ever trying to swim until he has proved to our satisfaction that he has mastered every important intercostal muscle twitch.

I wonder, is this true? How much do we lose by concentrating on the words rather than letters? How limited would our vocabulary be if we restrict ourselves to a word-to-sound correspondence? How restricted is the reading vocabulary of the Chinese student? Is he limited to a mere ten thousand, or to a mere twenty thousand? Does a child really need to learn to speak before he can learn to read? Does learning to read on the part of deaf mutes really bear this out?

I am sure I have gone over my time, and that other people are going to be hitting some of the same points I have raised. I will make way.

• *Dr. Lumsdaine:* Thank you very much, Professor Stephens, for your entertaining and illuminating comments. It is quite clear that chivalry is not dead, in that I think despite the threatened wielding of the bludgeon, Professor Stephens has set the stage for some retorts on the part of both Jackie Gibson and one or two of the other people on the panel. In view of this let's depart a little bit from the pre-announced order and first ask Jackie if she would like to respond, and then one or two others before we then turn to the next discussant.

• *Dr. Eleanor Gibson:* Yes, I would. For instance, I have heard Pat Suppes give a lecture on the new math, and he emphasized the importance of giving the child an opportunity to perceive congruence at an early age, before he gets into a school situation where he needs to make use of the word in developing a proof in geometry.

To go on to a point which really has more to do with theory, you talked about "extinction" of perception and implied that I was using this concept. But I haven't. I don't believe this is a word that I want to use in talking about perception because I have become convinced of the notion that external reinforcement is not an important condition for perceptual learning. I think the term "extinction" unequivocally implies external reinforcement and withdrawal of it.

I have moved to another kind of jargon, for better or worse. I find that I prefer the jargon of information processing, and I don't mean "extinction" when I say that learning to filter out the irrelevant is an important part of perceptual learning. It simply isn't accomplished by withdrawal of external reinforcement.

About the eye-voice span, I think the question you are asking is whether the kind of chunking revealed by the eye-voice span is really perceptual. You say that the subject can just invent the rest, and I must admit that this particular technique of experimentation is not one that will allow a very purist psychologist to make a fine distinction between where perception stops and short-term memory begins.

But in order to see whether or not it is sheer invention due to some redundancy of meaning, it is necessary to put a lot of controls in the experiments that I did not explain. You must, for instance—when you consider the difference between an active sentence and a passive one—have a sentence that has exactly the same meaning, or as nearly the same as you can get in changing the voice. I doubt in that case that you can ascribe the difference to better guessing in one case than in the other; but I had better leave that to the people who are doing these experiments.

One more thing: Elizabeth Taylor and the individual frames. I don't believe that learning distinctive features, which I consider perceptual learning at a lower order than perceiving structure, implies that one must first perceive something analogous to single frames, one at a time. In fact, I don't think that a frame is a lower-order structure at all, and I would like to emphasize—as I thought I had—the importance of motion and continuity in perceptual learning. I think it is essential, and I think that some of the earliest accomplishments of perceptual learning—such as spatial constancy—depend upon perceiving motion rather than looking at the individual frame.

Just one more thing about reading and whether or not there is some sort of hierarchical structure of skill. Dr. Stephens said that we don't know, really, that learning to speak is an important antecedent of learning to read. I will pick up this point, because I happen to have some information about it.

In the case of deaf mutes, which you mentioned, it is in fact true that it is very hard for them to learn to read without having speech to start with. I have just had occasion to do experiments at Gallaudet College with deaf readers who are probably the most gifted of any in the country or even in the world, because it is an international college. Although these people were very highly selected and intelligent compared with hearing subjects of the same year in college, their reading was inferior. In spite of the struggle they made and the fact that they learned, there were very serious problems for them in learning, and they still have some of them. I don't suppose that this cinches the argument for designing progressive programs of training, but it suggests to me that certain kinds of learning should precede others. Even if they don't necessarily have to. It makes sense to consider the possibility if one is going to design a teaching program. But then I'm not supposed to do that, so I'll stop.

• *Dr. Lumsdaine:* I think it is safe to say that under the bludgeoning of J. M. Stephens, Jackie's head is unbowed.

Since aspersions were cast on several other people by your comments, I think with full intent, I might ask for comment from at least one of the other targets, Jim Gibson, Bob Gagné, or Bob Glaser. Since Bob Glaser is closest at hand, he might wish to offer a rejoinder, either with respect to the negative status of programmed learning or other aspects which appear to be under attack here.

• *Dr. Glaser:* The only thing I will react to is the nature of going from research to applications in schools. I am very aware of the centipede phenomenon; when a centipede tries to analyze how he walks, he can hardly walk at all. The point to be made is that I think most people around these tables don't move directly from research to applications. Between research and application there is a stage called "development." I don't think we move from research to applications without a period of development and engineering which moves closer to applications.

• *Dr. Lumsdaine:* I might, since I have the floor, just make a comment following up Bob's. This may relate to some comments which I think both Dr. Hill and Dr. Ellis want to make about this common question they seem to have in mind.

It might be said that although one may properly refrain from or be disinclined to move immediately into applications, some of the differences in the power of alternative theories—for example, the subtractive versus additive ones—may really only be resolved when the implications of these are translated through development into alternative sets of instructional programs or materials.

One sometimes wonders if the differences in conceptualization which you prefer, as against those which Bob Glaser might prefer, go beyond the level of, "In what frame of reference is it most convenient for me to think?" Do these different frames of reference really have action implications that will make a difference in how one implements instruction subsequently?

This point will perhaps be returned to, but it was relevant to the notion of translating these "ideas" into developmental exemplars.

SECOND DISCUSSANT: Dr. Henry C. Ellis

• *Dr. Ellis:* Jackie Gibson is still unbowed, and I have no desire to change the situation, since she is much too delightful a person. Nevertheless, I want to comment on two or three aspects of her paper.

Since Jackie indicates that she is not ready to embark on a program of application, I should like to return to an examination of the mechanisms of perceptual learning. This point that I want to make is related to one that Dr. Lumsdaine raised; namely, whether or not her conception of perceptual learning differs in some way from alternative conceptions.

Let us take a closer look at what I believe she is saying about perceptual learning. Her view is that perceptual learning, in part, involves responding to finer and finer aspects of stimuli that already exist in the world; that is, the events of stimulation exist inherent in the physical world, and they are to be discriminated ultimately by the learner. When a person is learning to discriminate, say, from one wine to another, which initially he may not be able to do, what he ultimately does is to detect aspects of stimulation that are already inherent in the two wines he is comparing. So stimulation is there in the world. It is a rich and varied world, and the learner must be brought in contact with this stimulation; the process by which this occurs is perceptual learning.

Perceptual learning, then, according to Jackie Gibson's position, is a *discriminative* as well as a subtractive process. She distinguishes this conception of perceptual learning from that of additive, mediational, and associative theories of perceptual learning. I will treat additive, mediational, and associative approaches together, since they have overlapping conceptual properties.

A mediational or additive theory that is probably the most widely referred to is one proposed some years ago by Miller and Dollard. They proposed the mechanism of acquired distinctiveness of cues, a mechanism by which stimuli become more distinctive or discriminable, and a corollary mechanism by which stimuli become more similar, that of acquired equivalence of cues.

The Miller-Dollard position would view perceptual learning as the attaching of distinctive response-produced cues to stimuli. These response-produced cues may become added to the original stimuli through verbal labeling, and it is this addition that makes the originally confusable stimuli more distinctive. In contrast, Jackie contends that there isn't any addition of response-produced cues; or, if there is, that this is not the central or core mechanism in perceptual learning. In contrast, what is central is that the organism does come to discriminate these already existing aspects of the stimulation.

I propose that until the empirical implications that follow from these two theoretical positions can themselves be clearly distinguished, it will be difficult to determine which of these two alternative conceptions best accounts for the data of perceptual learning. Jackie Gibson believes that her view best accounts for the data, and she may very well be correct. When it is difficult to determine if there are *alternative empirical implications* that follow from the two positions, how does one know which of these alternatives best accounts for the data?

In order to test a theory in science, as I understand theory testing, you do not begin by arguing about the descriptive properties per se of the theoretical concept; that is, one does not argue about the content of the intervening variable or mechanism that presumably explain the data. Rather one determines if there are systematic differences in the predicted outcomes of the alternative explanations, and attempts to test these alternative predictions in some common-test situation. If, in fact, two theories predict the same thing, then they can be regarded as the same theory; until we can distinguish between predictions, they remain functionally identified. So my first question to Jackie is, does she see any hope of being able to identify empirical implications that follow from her theory of perceptual learning which differ from implications that might follow from a mediational or an acquired-distinctiveness-of-cues position?

Let me add one point here by saying that with respect to the educational implications involved in perceptual learning, such a distinction may or may not have any differential implications.

The second question that I want to raise has to do with the mechanism that she proposes to account for perceptual learning, that of uncertainty reduction. As she says, the reinforcement involved is the reduction in uncertainty. That, I think, may be reasonable. However, to assert that uncertainty reduction is reinforcing raises the question: How does reduction in uncertainty itself become reinforcing? Suppose that one were placed in an environment in which, from an early age, no one externally reinforced reduction in uncertainty. Would this make a difference? It might. Is it possible that the reduction in uncertainty is itself contingent upon sources of external reinforcement from the parent at a very early age? For instance, is it possible that we like to have uncertainty reduced, we like to get clarity, because at a very early age we receive external reinforcement for this? If not, then how does this mechanism arise? Is it perhaps an innate biological mechanism? Is this characteristic of many species? I don't propose to know, but I raise the question to Jackie Gibson.

• *Dr. Eleanor Gibson:* I can't say that I know, either, but I see no reason for supposing that reduction in uncertainty as a reinforcer is any less primary than some kind of metabolic need reduction which has been happily taken for granted by all of you for many years, or than M&M's or pellets, or "Yes" and "No," "Right" and "Wrong."

I think, myself, that it probably is a built-in thing. I think it is characteristic of the way the species has evolved. But obviously that is not a question that I can answer any more than you can answer why people give pellets or M&M's. I guess we know why they do it—it's handy, and it gives the experimenter control; but why does one assume that that is a primary reinforcer any more than something (reduction of uncertainty) that seems to me to answer a lot more questions?

I would like to go back to one of the first things you said. You said that I had said perceptual learning is responding to finer and finer distinctions between stimuli. I think I should clear that up. The way I am using "stimulation" is probably not the way you are, because you imply that I think the epitome of perceptual learning is something like reducing differential pitch thresholds to something finer and finer. But I don't at all, because I don't think that that is relevant to anybody except perhaps a violin player.

Let me give as an example learning to discriminate distinctive features of phonemes, which I do think is a case of perceptual learning that occurs very, very early and without specific reinforcement by parents. It is not a case of responding to finer and finer distinctions along a graded dimension, because it looks as if the distinctive features are "either-or" matters. They are also relevant. So I am talking, when I talk about greater specificity as a criterion of perceptual learning, not about making finer discriminations in the sense of telling the higher from the lower pitch. Perceiving finely graded differences along a dimension is not relevant to most of the tasks of perceptual learning that people are concerned with, and I should say once again—if I didn't make it clear before—that I think perceptual learning is an adaptive self-regulative process. I think that the task is an exceedingly important aspect of it. In answering your question, "How can we tell the difference between a theory of response-produced cues and the kind I have suggested?" I have an experiment which makes a clear distinction between these two theories.

This was a disjunctive reaction-time experiment. What we were trying to demonstrate was that a subject would progress in the course of practice to an increasing economy or optimization of processing features of letters that had to be discriminated. There were three conditions in the experiment. In one of them there was one letter which was a positive target, if I may call it that, and eight others which were negative ones. The subject pushed a button and then—on a little screen in front of him, a letter was projected for twenty milliseconds. If it belonged to the positive set, he pressed a lever as fast as he could to the side assigned to it.

In another condition he had three letters in the positive set and six in the negative. They were the same total set of letters in all conditions, of course. They were divided in this case so that the three letters in the positive set had no common features by which they could be differentiated from the others. Then there was a third condition in which the three letters in the positive set were chosen from the total set of nine, so that there was a single distinctive feature (in terms of my analysis of the distinctive features of letters) which would differentiate them from the negative set.

Now we ask the question: Will the subject early in training show, for the two conditions in which there are three letters in the positive set, an approximately equivalent latency, and one which is longer than in the single target condition? This was in fact the case. The next question is whether, with practice, the subject will begin to optimize in the sense that some regulatory process will cause him to select the single distinctive feature and cause this latency to fall to the same point as the latency for the condition with a single letter target instead of three.

We found that over even a rather short practice time there were learning curves for all three conditions, but for the condition where it was possible to make a decision on the basis of a single distinctive feature the curve fell way below the other condition with three members in the positive set. It didn't fall to the level of the curve for a single member. That is because, I think, the practice was not very long (less than twenty minutes for each condition).

Then we ran a subject for thirty-five days. Over this time we found beautiful learning curves, and the one for the set with the single distinctive feature met the curve for the condition with a single member in the positive set.

This seems to me a clear case of perceptual learning. The subject cannot tell you he is doing this. If you ask afterward, "Was there any difference between those two?" "Is one easier than the other?" they simply say, "No." But the difference is there and it develops gradually. It is not a matter, so far as I can see, of a response difference of any kind. The response is always the same. Nor does it seem to be an intellectual difference in the sense that he solves a problem intellectually. It seems to me that this is an example of perceptual learning that cannot be explained by mediated cues.

• *Dr. Lumsdaine:* Were time not so short, I'd like to ask Dr. Ellis if he feels that the very interesting experiment described by Jackie Gibson answers appropriately the challenge of whether there are real implications for the differing kinds of conceptions that have been identified here. I think instead, since Dr. Winfred Hill wanted essentially to ask the same or a closely similar question, I will ask him if he is satisfied with this reply. Then, if time permits, there are at least two other people on the panel who would like to comment.

• *Dr. Hill:* Yes, this sort of experiment is exactly the sort of thing that I had in mind as an attempt to see whether these two approaches are clearly discriminable interpretations. Of course it is the nature of theory of the additive sort to assume mediative responses, to try always to hedge a little and find something that is not obvious.

As Dr. Gibson spoke, I was thinking, "What happens when the apples upset?" She answered that. I might go on to say, "What happens if you make very careful measurements of their eye movements? Are they looking at exactly that part of whatever they discriminate?" If it turns out that all our ingenuity fails and we are not able to find anything indicating a mediating response or one that the subject can report, I would be prepared to say, so far as that experiment is concerned, the assumption that there is an additive or a mediating process is of no use. It is only as we can see differences in their possible applications that it makes a difference.

But I suspect if we turn to another aspect of this concept-formation problem, the example Dr. Glaser gave this morning about the similar electrocardiograms, I would want to have some kind of verbal mediation. I may not be able to say precisely what it is, but there will be something that I can perceive and state in terms of the heights, the distance between the peaks, or something. I suspect that in this kind of situation—where there is something in the way you fixate with your eyes—there is something that you can report verbally that explains how you were able to make the necessary distinctions. It is probably a much more typical situation than the one he gives, even though I am prepared to concede in that case that very likely the additive interpretation has nothing to add.

• *Dr. Lumsdaine:* Dr. Glaser?

• *Dr. Glaser:* Jackie, you said you didn't think that discrimination of a dimension involved gets finer and finer, and—

• *Dr. Eleanor Gibson:* No. I said that is not my definition of perceptual learning. Maybe with a violin player it would.

• *Dr. Glaser:* But in learning a perceptual structure that you differentiate out of the rest of the world so that you respond to it, you pull out a structure and you recognize a structure and you use this. Certainly that's a discrimination process which gets finer and finer, discriminating a structure from the rest of the environment.

• *Dr. Eleanor Gibson:* Except that I wouldn't describe it as a dimension that gets finer and finer.

• *Dr. Glaser:* What I mean is that it is a discrimination which is very rough in the beginning. It is a perception in which you make errors, and as experience accumulates in some way or other you make fewer and fewer errors. I suspect there are some dimensions which get finer and finer.

• *Dr. Eleanor Gibson:* Dimensions of structure are very difficult to define, as you have probably considered.

• *Dr. Lumsdaine:* I am sure there are a number of other comments that we would like to make. I think, in view of the time, I will presume that these will be pursued during the coming recess in our deliberations. I would like to evoke discussion on one more item before I return the gavel to Dr. Gagné. As prefatory comment, I think that the relationship between basic research and application has come out implicitly at a number of points in these discussions. This is an exceedingly important and pervasive issue, not only as a philosophic and scientific one but as a matter of fiscal policy in the funding of research programs. I might note at this point that the Phi Delta Kappa Research Advisory Committee and the American Educational Research Association are sponsoring a study group on this issue.

There are several of us here at this table who have been involved in programmatic research in which there was a constant interplay between the application of basic research inquiries and techniques in the context of practical application. This includes, for example, Dr. Stolurow, Dr. Gagné, Dr. Rothkopf, and myself. I would like to ask Ernie Rothkopf to comment on this, with particular reference to the near identity of a technique which he used in the course of a practical inquiry in Morse code learning. In this case a stimulus confusability matrix similar to that to which Jackie Gibson alluded a little while ago was part of his attack on the training problem. Ernie?

• *Dr. Rothkopf:* I don't know what to say, Art.

• *Dr. Lumsdaine:* You can make some other comment. I thought you had another one, so you can have that one in reserve.

• *Dr. Rothkopf:* There is something that I wish Jackie would clear up. I just find it difficult to accept a characterization of man that decouples distinctive features of stimuli from environmental contingencies. I understood this to be what you were saying. It seems to me that this is just contrary to the adaptive evolutionary argument that you had made earlier. As a matter of fact, distinctive features that depend only on the stimulus strike me as a highly specialized trait in a species and not at all like the more flexible system that man has evolved.

I would think that there are many potential distinctive features possible for any set of stimuli. There is a good deal of anecdotal evidence that would seem to point in that direction. I am told, for example, that the bushmen of the Kalahari Desert make fantastic distinctions in the way grass is distorted by tracks. They have evolved a system of distinctive features for grass that depends very critically on environmental contingencies. If the uncertainty principle that you propose were the only factor that was operating, all men who grew up in the presence of grass would tend to develop the same set of distinctive features. I just don't believe that is true. Did I just misunderstand your point completely?

• *Dr. Eleanor Gibson:* I think you may have, because I think different distinctive features may be relevant, depending on the task.

The reason I consider this an adaptive process is because the task makes a difference. Consider what happens when the task is varied. In the experiment I described, the task we set up for the subject was not the same task as reading, although letters served as stimuli. We set up one that went counter to the effects of long-time training if the subject was a good reader. We forced him to change his habitual means of processing if he were to adopt an optimal process for the task, because some features had become irrelevant.

• *Dr. Rothkopf:* Well, does that change your view as to uncertainty as the strong principle in the development of distinctive features?

• *Dr. Eleanor Gibson:* No.

• *Dr. Rothkopf:* This is the difficulty I can't resolve. I don't understand the relationship between—

• *Dr. Eleanor Gibson:* He is reducing the information he needs to process in this case, and that is a reduction of uncertainty.

• *Dr. Rothkopf:* Then this reduction must be in some way still coupled to some sort of environmental contingency, which is beginning to sound an awful lot like the reinforcement notion you rejected.

• *Dr. Eleanor Gibson:* I think it is a kind of reinforcement, but not a metabolic reduction.

• *Dr. Lumsdaine:* You know, a policeman's lot is not a happy one, but I am supposed to run this railroad on time. To the next to the last question Jackie replied "No." I think it's appropriate that the lady have the last word, and I am pleased her last reply was, in effect, "Yes."

CHAPTER THREE

TWO SCIENTIFIC APPROACHES TO THE MANAGEMENT OF INSTRUCTION

BY

ERNST Z. ROTHKOPF

BELL TELEPHONE LABORATORIES, INCORPORATED

Instructing is a very old human enterprise. Western culture has well-established traditions about how it should be done. We have traditions about the mechanics of teaching, about the proper atmosphere for learning, the formats of instructional materials, and about the signs of educational success and failure. Also part of our cultural tradition is the belief that scientific technology and the growth of scientific understanding will increase our capability to carry out any human activity, including teaching.

The dramatic and apparently quantal progression of the scientific bases for our electromechanical and chemical technologies has been the most appealing popular model for interaction between fundamental knowledge and practical pursuits. However, it does not seem realistic to expect the progress of instructional technology to follow the physical science pattern. Matters are not that simple.

Agriculture is probably a better model for the growth of the rational bases for instructional technology. The historical lesson of scientific agriculture is encouraging because it shows that the impact of science on practical affairs may be gradual and diffuse and yet effective. The impact of the learning laboratory on the schoolroom has also been diffuse. The facts and methods of experimental science have influenced instructional practices in diverse and fragmented ways. The purpose of this paper is to examine some of the scientific attempts to improve our capability to teach and to discuss certain selected aspects of how our understanding of learning phenomena has influenced our understanding of the instructional process.[1] I shall pay special attention to two approaches: one that I have called the calculus of practice (Rothkopf, 1965, p. 216), and another that deals with a class of activities called mathemagenic behavior (*Ibid.*, pp. 198-202).

PRACTICAL ELEMENTS IN EARLY LEARNING RESEARCH

Hermann Ebbinghaus started a line of inquiry into the human learning process that many consider excessively remote from practical concerns. Yet Ebbinghaus was deeply interested in educational problems. It is well known, for example, that he served on a committee for the investigation of educational practices in the city of Breslau. In this connection, Ebbinghaus attempted to study the effects of fatigue on children, in order to help authorities decide on length of the school day. This led to the invention of the "Kombinations methode" (Ebbinghaus, 1897) which has been translated in English as the *completion method.* (For a review of early use of this method, see MacGinitie, 1960). This research technique is in common use today, although Ebbinghaus is rarely credited for its development. We generally know it as the CLOZE procedure.

It may be said that the beginnings of fundamental learning research were markedly entwined with educational interest, although I will not attempt to document this statement here. Practical concerns in early work on human learning were reflected in the choice of both subject matter and experimental measurements. For one thing, up to and including the last century, rote memorization of school material such as poems, royal family trees, or the procession of saints, appear to have been common in schools. The emphasis of early experimentation on rote learning probably reflected the pedagogic Zeitgeist.

The roots of early fundamental learning research in applied

[1] By instructional process I mean the activities, arrangements, and interactions which promote the achievement of specified educational objectives. The critical feature of this broad definition is that the instructional process is goal-directed.

problems is also revealed by a certain preoccupation with efficiency or economy of learning. Study or practice was broken down into discrete, countable events such as trials and stimulus presentations. Likewise, learned performances were characterized in terms of simple unitary measures such as number of correct anticipations or proportion of correct reponses. There was a tendency to conceive of what happens in a learning experiment as growth along one dimension, a usage that is not free of logical dangers.

Experiments were conducted to find out which of several practice conditions produced a given performance in the shortest time period, or after the smallest number of practice events. It is interesting to note that we speak of the "savings" technique in studying retroactive inhibition or transfer. The concept of economy of practice is very much with us today in the learning laboratory, and a very large portion of experimental and theoretical papers are concerned with it. The notion of economy of practice provides much of the language with which the laboratory speaks to the schoolroom. Bookkeeping on discrete, countable events such as trials provides a basis for decision for those who wish to bring the results of studies of the learning process to bear on the conduct of instruction. The assumption is made that if a treatment produced results with small amounts of practice (trials) in the laboratory, a like or derived instructional treatment will also produce quicker results in the school, and is therefore educationally desirable. I will return to this point later.

THE CALCULUS OF PRACTICE

The strong concern with economy of practice, which has been influential in learning research from the time of Ebbinghaus to the present, may have led to similar lines of development for different learning problem areas. The several domains of inquiry in learning started with brute-force empirical comparison of the economy of practice of various procedures. The next step usually took place much later. It was the analysis of discrete key elements of the experimental task or treatment in terms of discrete events which had been identified in simpler-appearing learning situations, most typically in classical conditioning. Finally, there emerged an attempt to derive economical practice strategies from conceptual analysis. This type of theoretical synthesis or practical calculation I have called the calculus of practice (Rothkopf, 1965, p. 216).

An Illustration: The Whole-Part Problem

Research on the whole-part problem provides a concrete illustration of historical development from gross empiricism to the calculus of practice at the laboratory level.

It is difficult to avoid the conclusion that early experimental interest in the whole-part problem may have been almost entirely motivated by the emphasis on rote memorization, especially poetry in the schools. Grace McGeoch (1931) listed some thirty studies concerned with the whole-part question between 1900 and 1920. Several of these used poetry as the experimental task. Nearly all involved uncomplicated empirical comparison of alternative methods for dividing the learning task. During this period and in the subsequent two decades, a special methodology for comparing the economy of various methods was evolved. Choice of task was quite broad. One suspects the main determinant of choice was whether part-organization of the task was logically possible. There also appears to have developed a preference for learning activities with easily countable subunits such as syllables, words, or stanzas. While the methods became more sophisticated, the approach remained empirical. The cumulative results of almost four decades of research were highly ambiguous.

At the end of the thirties, a marked change occurred in the general conception of the learning process. The Lepley (1934) hypothesis brought the work of Pavlov and instrumental learning closer to each other. The works of Guthrie, Tolman, Hull, and Skinner set the stage for widespread interest in theoretical synthesis.

The interest in theory came quickly for human learning. Among the first of such approaches was the *Mathematico-Deductive Theory of Rote Learning* (Hull *et al.*, 1940), an impressive *tour de force* that did not quite do the trick. But another work which appeared in that year was more successful. Eleanor Gibson's (1940) remarkably influential paper "A Systematic Application of the Concepts of Generalization and Differentiation to Verbal Learning" provided a theoretical basis for a calculus of practice in verbal learning. Its implications were that each practice event, such as the presentation of a word-pair, should be weighed in terms of its effects on the relevant word-pair association, as well as on the spread of this effect (generalization) to other relevant habits. Similar allowances had to be made for inhibitory, extinction-like processes. Such a calculus of habit interactions promised to make the calculation of economical practice strategies a possibility.

In terms of the whole-part problem, these developments resulted in at least one systematic practice calculus. This is a very fine, but unfortunately unpublished, Yale dissertation by Orbison (1944). Orbison assumed that the time required to learn a given paired-associate is directly proportional to the number of other pairs from which it must be discriminated. This assumption was based on Gibson's notion of within-list interference as the main source of list difficulty. In a list of fairly homogeneous inter-item interference, the practice required to reach a criterion should be proportional to the square of the list length, or more specifically $N^2 - N$, where N is the list length. Orbison arrived at this by reasoning that each of N items is interfered with by the remaining $N-1$ items in the list. Hence total within-list interference is $N(N-1)$ or $N^2 - N$.

Orbison further assumed, on the basis of Woodworth's finding (1938, pp. 19–20), that interpair discrimination increases disproportionally with increases in list length. He deduced from these two assumptions that as the list grows longer, the part method should become increasingly more economical than the whole method. Orbison concluded that his experiment supported this theoretical prediction. Orbison also predicted that, as list length increases, it will become more economical to divide the learning task into a larger number of parts. This second prediction, as far as I know, has never been tested.

The calculus of practice is a very general class of experiments or of theoretical constructions. It is distinguished by the tendency to divide practice and other aspects of the experimental environment into relatively small, discrete events. The assumption is made that in general any instructional subject matter can be divided into an enumerable set of subunits such as trials, stimulus exposures, and the like. Subunit sequence, or amount of practice time for various subunits can be experimentally varied and, in this way, instructional configurations can be discovered which are more economical than others.

There appear to be at least two varieties of the calculus of practice. In one, calculations are based on the nature or number of the practice events. The other calculus depends on performance measures. In the first form, an effort is made to produce economical sequences by manipulating the amount of time spent at various practice tasks and the sequence of these practice maneuvers. In the second form, progression through the acquisition sequence is made contingent on the achievement of certain performance levels, and calculations are made on the basis of these criterion levels. The second approach aims toward deciding on what performance criteria must be reached and with what learning sequence this should be accomplished.

This is the gross empirical form of the approach. In its higher order or theoretical forms, *psychological state* is substituted for practice unit. *Psychological states* are habits, discriminations, and the like. The assumption is that certain sequences in which the required psychological states are attained, and certain configurations of their strengths when new practice maneuvers are undertaken, represent more efficient instructional strategies than do others.

Some Difficulties

Laboratory experiments that are aimed at a calculus of practice represent a very ambitious attempt to provide a scientific basis for instructional decisions. I would like to comment on two difficulties which arise in connection with this approach. The first of these is a logical problem that is connected with attempts to provide a theoretical substrate for any given calculus of practice. This logical problem may place some limits on the power that any theory can have in this domain. The second difficulty arises in connection with decisions to translate laboratory results into instructional practices. I mean specifically difficulties connected with decisions that are based on economies in practice or study time.

A Logical Problem. Logical problems result from attempts to provide some observational basis for the psychological states on which any theoretical calculation of practice economy must depend. Any theoretical calculus must mediate between at least two classes of observables, either between (a) practice events and performances, or between (b) performances and other performances. If the calculus is to be based on interaction between psychological states then it would be convenient to be able to infer the psychological states that result from various practice events in a reasonable unambiguous manner. Yet such inference is seriously complicated by two principles that I will for convenience call the *practice* and the *performance indeterminacy principles.* The two principles are as follows:

1. A particular, objectively described practice event can result in several different psychological states.
2. A given, objectively measured performance may be supported by several different psychological states.

The implications of these two principles are that (a) terminal performance cannot be simply and specifically forecast from training events; and (b) the specific character of the disposing training events cannot be simply and unambiguously inferred from terminal performance.

The indeterminacies would appear to limit the power of a theoretical calculus of practice. It does not matter whether the practice calculus is built on control of discrete practice events, i.e., trials, presentations, units; or whether it is calculated on measurement of discrete responses, i.e., performances, such as achieving some criterion score before going on to the next unit. *A priori* calculation of training outcomes from various configurations of practice events contain the indeterminacies described above, and therefore can be computed only in a very approximate sense. This may be reflected in the fact that the experimental literature of applied learning research has been characterized by experimental effects of depressingly small magnitude.

The *a priori* (or practice) indeterminacy principle is to an important degree due to a discrepancy between nominal and effective stimulation. The nominal stimulus is an objective description or characterization of the instructional situation. The effective stimulus, on the other hand, is the psychological consequence of this stimulation. A printed page in a textbook is an example of a nominal stimulus. It is clear that exposure to a page of text may result in a great range of effective stimulation, depending on the actions of the student. The student may read with care, daydream, skip about the page, or count the number of words that include the letters *b*, *a*, and *t*. He may not be able to read. He may misread or think the word "catamaran" to mean a cross between an orangutan and a cat. We can only make a very approximate guess as to the specific character of effective stimuli in the instructional situation. This is to a large degree due to the fact that the instructor does not determine effective stimulation. Realistically speaking, he can only see to it that instructionally desirable effective stimuli are a possibility. Whether this possibility is realized depends critically on the actions of the student.

Practice Economy. I have pointed out earlier that bookkeeping on the discrete, countable practice events of learning experiments, such as trials or number of repetitions to a criterion, has been one of the bases for communication between laboratory and schoolroom. The assumption is made that if a method or treatment produced results with small amounts of practice (e.g., trials) in the laboratory, a like or derived instructional treatment will also produce quicker results in the school and is *therefore educationally desirable.*[2] It seems appealing and sensible to prefer those experimental procedures that produce the required performance after the least amount of practice. Therefore it would also appear desirable to find theoretical models that would forecast economical practice arrangements in instructional experiments.

There are grounds for believing that economy of practice is sometimes not a sensible criterion for generalizing laboratory findings to the management of instruction. One of the reasons for this belief is that amount of practice as measured by time or trials, is not necessarily *cost* in the instructional sense. This is particularly true in self-instructional situations when the student works by himself, no teacher or other school personnel is required, and no expensive instructional hardware such as computers are tied up. Substantial portions of instruction have this character and involve no one else besides the student.

Under these circumstances increased study time does not necessarily mean increased cost to either school or student. There are clearly no burdens on the budget of the fostering school. The student's study time need not be viewed as cost either. The equation of cost with study time really depends on the assumption that if the student did not spend so much time on study task A, he would spend his time in some other useful way, e.g., on another study task or otherwise in some more pleasant manner. Serious doubts are possible about both of these assumptions. Learning could be a pleasant activity that students would seek to prolong. We have gotten stuck to the notion that learning is a chore. Questioning this view weakens the equation of practice with cost. And there seems to be plentiful encouragement to raise such a question. Among the more dramatic forms of this, for me, are the self-instructional activities engendered by the statistical tables in back of the baseball player pictures found in bubble gum packages.

The second assumption hidden in the conception of study time as cost, is that there is a fixed available amount of study time that will always be filled by study activities, i.e., if the student has to

[2] It should be noted that comparison among treatments in terms of relative performance after a fixed amount of practice is basically also an economy argument. The assumption is usually made that the educational objective is mastery. It is implied that if treatment X performance lags behind treatment Y after N trials, X would require greater number of trials to mastery than Y, hence X is less economical.

spend less time on task A, he will spend more time on task B that day, or that he will complete a greater number of tasks. It does not seem reasonable to believe that this assumption holds all the time either in private study or in the classroom. If the assumption is not correct that students will make good use of the study time saved by economic practice, then the view of practice as cost is again weakened.

One more consideration troubles the view that economy of practice is a strong basis for deciding whether laboratory findings have implications for instructional practices. Woodworth (1938, pp. 19–20) and others (see McGeoch, 1942, pp. 374–75) have shown that a long list learned to a criterion is much better retained than a shorter list. There are some indications that this is due to better discrimination among items. This in turn is a result of greater exposure, that is, the practice difficulty of the longer list. It is noteworthy that in paired-associate lists the relevant discriminations are innumerable. This is not always true for instructional situations. There increased study or greater teaching time often involves increases in samplings from the instructional subject matter. Hence more instruction time often implies wider experience for the student, and therefore a greater number of discriminations. The possibility cannot be dismissed that a seemingly "less economical" instructional treatment may produce discriminations which the criterion test instrument does not measure.[3] It is conceivable that these unmeasured discriminations may be quite educationally desirable and thus may compensate for the increased practice time requirements.

MATHEMAGENIC ACTIVITIES

As has been discussed earlier, one of the problems in the management of instructional events is that only limited control of the effective stimulus is possible. The instructor is in control of the nominal stimuli. In this way he can arrange for potential stimulation of the student. Whether these potential stimuli become actual ones depends critically on some actions by the student. Such actions have frequently been called *attention*, *study*, *effort*, and so on. It is a concern with such actions that forms the basis for an alternative rational approach to the management of the instructional process. This approach depends on understanding of a class of activities that I have called mathemagenic behaviors.

[3] This is just the kind of logical difficulty which has made the interpretation of instructional experiments very difficult.

The term "mathemagenic" is derived from two Greek roots. These are the words *mathema* which means "that which is learned" and the word *gignesthai* ("to be born"). Mathemagenic behaviors are activities which give birth to learning (see Rothkopf, 1965). The phrase refers to a rather broad class of activities. Some of these are directly observable, while some are not. The concept of mathemagenic activities is rather loosely defined and still somewhat plastic. However, despite this regrettable lack of rigor, the concept has been useful in analysis of how humans learn from written instructional materials. I believe that mathemagenic activity is a useful construct in the analysis of many other instructional situations.

Mathemagenic activities refer to activities of the student in the instructional situation. They cannot be exhaustively enumerated, nor does any great logical need exist to do so. They include such activities as reading, asking questions, inspecting an object, keeping the face oriented toward the teacher, and mentally reviewing a recently seen motion picture. Mathemagenic activities also include looking out of the classroom window, yawning, turning the pages of a textbook without reading, writing notes to a student in a neighboring seat, and sleeping, either in class or in a library carrel.

The need for such a concept as mathemagenic behavior is clear if one tries to understand how humans learn from written material. In that situation the gap between the nominal and the desired effective stimuli is wide. The subject must engage in prolonged and skilled activity—namely, reading—if the desired instructional objectives are to be achieved. Of course, the ability to translate the alphabetic symbols into speech sounds is not enough. This can easily be verified by brief observations of a student confronting a difficult and boring textbook.

We have found it expedient to endow mathemagenic behaviors with certain potential functions. These are translation, segmentation, and processing. Each of these functions has topography, including some attention-like characteristics, rate, and persistence.

Translation involves scanning the page and translating the alphabetic display into the sound of words or their subvocal surrogates. Segmentation refers to activities that break the stimulus string into syntactic and other unit components. The rationale for the segmentation function of mathemagenic behavior is discussed in somewhat greater detail elsewhere (Rothkopf, 1965). Its theoretical need stems from attempts to account for the data about sentence learning with which Thorndike (1932) was trying to deal in his *Law of Belonging*. One way of conceptualizing the segmentation function is to think of it as comprised of activities that have their overt vocal accompaniment in speech intonation and pauses.

The third important aspect of mathemagenic behavior is processing. I mean to include here a variety of activities including review, the invention of mnemonic devices, problem solving, as well as other activities that resemble thinking.

The three functions that I have just described do not exhaust all mathemagenic behaviors. On the contrary, they are but a small part. They have been singled out for discussion here because these functions play a critically important role in determining whether instructional objectives are approximated through the study of written documents. Actually the great majority of mathemagenic behaviors produce learning states that are unrelated or inconsistent with instructional objectives, and which may be quite undesirable from the viewpoint of the instructor. Examples of such are some cases of skipping and page turning, yawning, daydreaming, and improperly focused attention. Mathemagenic behavior is often observed to deteriorate (from the instructional point of view) during the course of study. Such deterioration bears some resemblance to the phenomenon of experimental extinction.

Ordinarily the deterioration of mathemagenic activities is not very visible in conventional classrooms, particularly if courses are taught by a lecture-discussion method. Deterioration can be quite dramatic, however, in correspondence-format courses or other self-instructional arrangements. I understand that sellers of commercial correspondence courses sometimes depend on the deterioration of mathemagenic behavior for profit. Figure 3.1 shows data on deterioration for a long self-instructional program. The graph shows the proportion of students remaining in the course at the end of each of fourteen units. Ordinarily, however, students cannot physically leave the instructional situation. What often happens instead can be described by an expression the Germans coined after World War II. They called the tendency of people to dissociate themselves mentally from the Nazi regime without overt rebellion *internal emigration*. This strikes me as a good description of what happens when mathemagenic behavior deteriorates in the classroom. In general, mathemagenic activities appear to deteriorate in reverse order from their nearness to the human periphery. Processing becomes instruc-

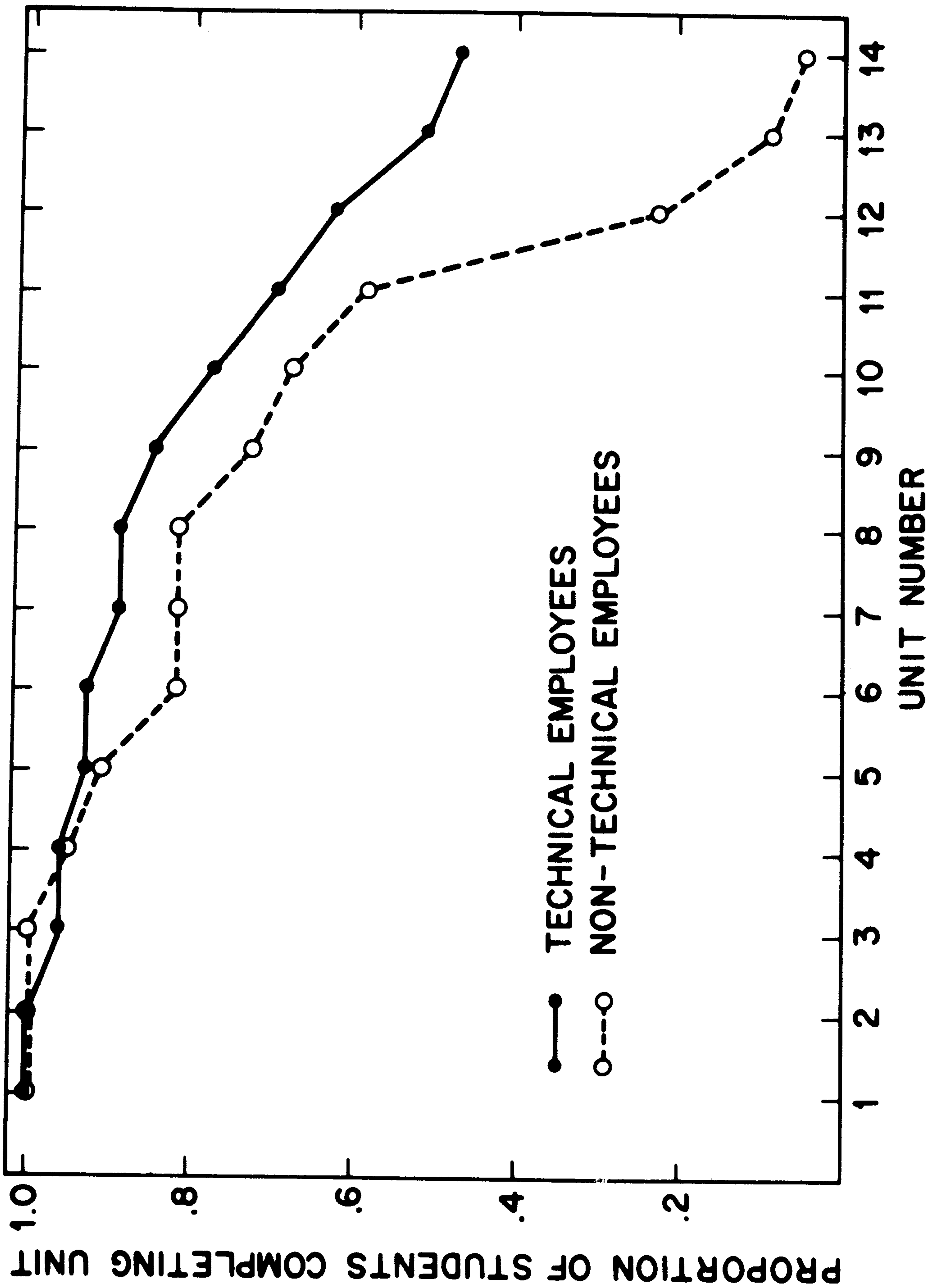

FIG. 3.1. Proportion of students completing each unit of a self-instructional course on computer programming. The course was conducted in an industrial setting. Data are plotted separately for students holding technical and non-technical jobs.

tionally ineffective first, then segmentation deteriorates, and finally the translation and scanning process. Many of you have, I am sure, observed aspects of the extinction-like deterioration of mathemagenic behavior. One of the more dramatic forms is finding that one has reached the bottom of a printed page without having "read" anything.

We can classify observable mathemagenic behavior on an *a priori* basis in accordance with whether these activities are consistent with instructional objectives or whether they are not. The classifications can then be empirically validated. Many very interesting mathemagenic activities are not observable, however. These include what we usually characterize as attending, listening, processing, and so on. Some indirect physiological measurement of such hypothetical states is possible. But in the main we make inferences about these covert activities on the basis of their consequences. It is possible to measure the performance of students after instruction. From these measurements we can again infer two possible stages of the covert mathemagenic activities—they were either consistent or inconsistent with the attainment of instructional objectives. Inferences about covert mathemagenic activities, however, pose some special technical problems which I shall discuss later.

Mathemagenic behaviors, to an extremely important degree, determine the effective instructional stimuli. Mathemagenic behaviors and instructional content are therefore not unrelated. But it is nevertheless clear that if other conditions dispose toward inappropriate mathemagenic activities, even carefully prepared instructional materials or sequences will fail to produce the intended results.

EXPERIMENTS ON THE CONTROL OF MATHEMAGENIC ACTIVITIES

Most of the initial work on mathemagenic behavior grew out of experimental interest in written instructive material. Much of it involved trying to understand how questions and other test-like events affect mathemagenic activities. But there are reasons to believe that these concepts and experimental results apply to a broad variety of instructional situations. Questions and other similar test-like events are particularly interesting because they are a very important form of instructional intercourse. They have been so from at least the times of Pericles to the computer-assisted present. We wanted to find out how questions determine both the characteristics and the persistence of mathemagenic behavior. As part of this work we have discovered that because mathemagenic behavior is adaptive, the teacher can at times betray the student and interfere with the attainment of instructional objectives. In general, however, the adaptive character of mathemagenic behavior serves the student well.

I should like to discuss one of these experiments in greater detail. My purpose is to illustrate how the character of mathemagenic activity is inferred. The point of departure for this series of experiments was the notion that test-like events are a potent force in shaping how people study. I was trying to make a point about programmed instruction (Rothkopf, 1963). The aim was to show that making some overt response to a frame affected chiefly how students studied and used the programmed material, and not directly the growth of the relevant subject matter skills or associations.

Subjects were asked to read a very long selection from a book with relatively high factual content. The multilithed experimental passages typically were 36 pages, or about 9,000 words. Two experimental questions were inserted in the passage every three pages. The student was directed to answer each question. The correct answer could be supplied to the student following his response. After the student had worked his way through the 36 pages, he was given a 48-question test, in order to find out how much he had learned.

Final test performance should have been high, if the experimental questions tended to shape mathemagenic behaviors that were consistent with the test objectives. However, it is well known (e.g., Estes, Hopkins & Crothers, 1960; Levine, Leitenberg & Richter, 1964) that questions have direct instructive effects which might also elevate final test performance. A technical trick was used to avoid confounding the direct instructive effects of questions from their effects on mathemagenic behavior. This is illustrated in Figure 3.2. A large pool of questions from all parts of the text was collected. This pool was divided into two subsets such that there was no transfer of training from the knowledges required for subset *A* to those required to answer subset *B* correctly. This meant that a student who knew enough to answer all subset *A* questions correctly, would be performing at chance level on subset *B*. This was experimentally verified, and subset *A* was used to constitute the experimental questions, while subset *B* became the final examination.

Suppose it could now be shown that some sort of exposure to the experimental questions improved performance on the final examination! It could then be reasoned that this result must be due to changes in mathemagenic behavior produced by the question and not due to their direct instructive effect.

We have been able to demonstrate just this outcome in a number of experiments (Rothkopf, 1966; Rothkopf & Bisbicos, 1967). Our

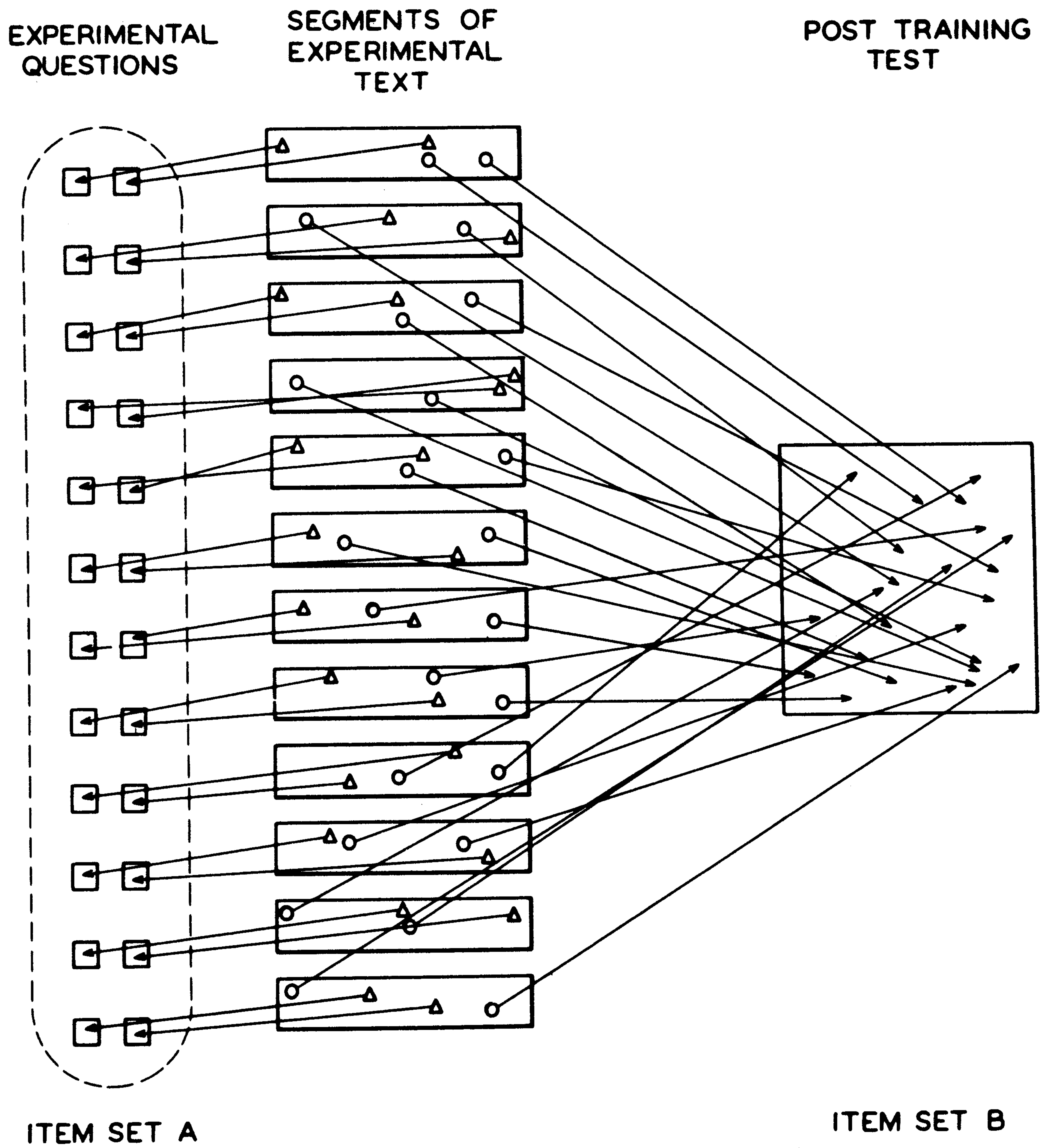

FIG. 3.2. Scheme for selecting experimental questions that were based on knowledges with no direct transfer to the retention test question. For details see text.

results indicate that shaping of mathemagenic activities takes place only if the questions follow the relevant text segment and only if they are administered fairly frequently.

Such findings are shown in Figure 3.3. Two experimental questions after each text segment produced distinct facilitation of test performance which could be attributed to mathemagenic behavior. Questions administered before the text segment, on the other hand, produce no different effect than the no-question control treatment. The direct instructive effects of questions (see Rothkopf, 1966 for details) were the same whether the experimental questions were administered before or after the relevant text segment. This finding that only questions administered *after* the relevant text segments shape facilitating mathemagenic behaviors has led us to believe that mathemagenic activities have adaptive character. They are shaped by a process which resembles the principle of natural selection as it has been proposed for organic evolution. The student starts with some set of study activities. If this is immediately followed by successful performance on experimental questions, the same topography of mathemagenic activity tends to be maintained in the subsequent instructional segment. If, however, the student fails to answer experimental questions to his satisfaction, then he will modify his mathemagenic activities in the next study segment. This process of selective modification continues until the student consistently answers the experimental questions in a satisfactory manner.

Using the technique described earlier, we found several interesting facts about how questions produce mathemagenic behaviors that are consistent with high criterion test performance. As I indicated before, short-answer questions shape the topography of the mathemagenic activities and can result in general facilitation of learning. We have also observed that by biasing the set of questions to which the subjects are exposed, we can produce mathemagenic behavior that facilitates test performance in specific classes of knowledge.

For example, if the experimental questions uniformly require quantitative answers or proper names, more knowledge about quantities and names are acquired during the course of study. Such results from a recent study (Rothkopf and Bisbicos, 1967) are illustrated in Figure 3.4. The plot shows the proportion of correct responses on retention test questions that required measures (*M*) or names (*N*) for answers. Performance on test items derived from the first eighteen pages of the experimental text has been plotted

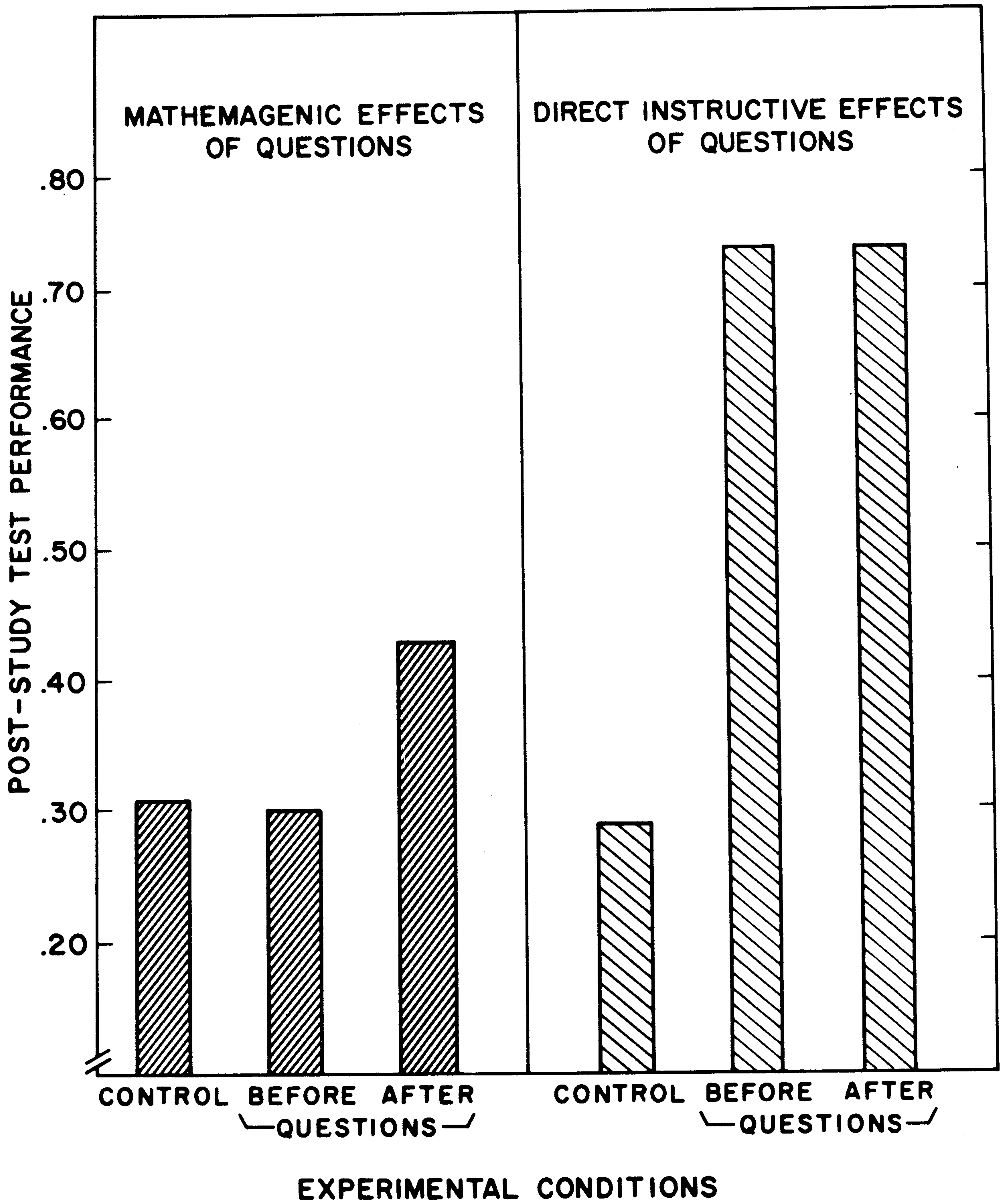

FIG. 3.3. Retention test performance as function of the location of questions with respect to the text segment from which the questions were drawn. The control group saw no question during study.

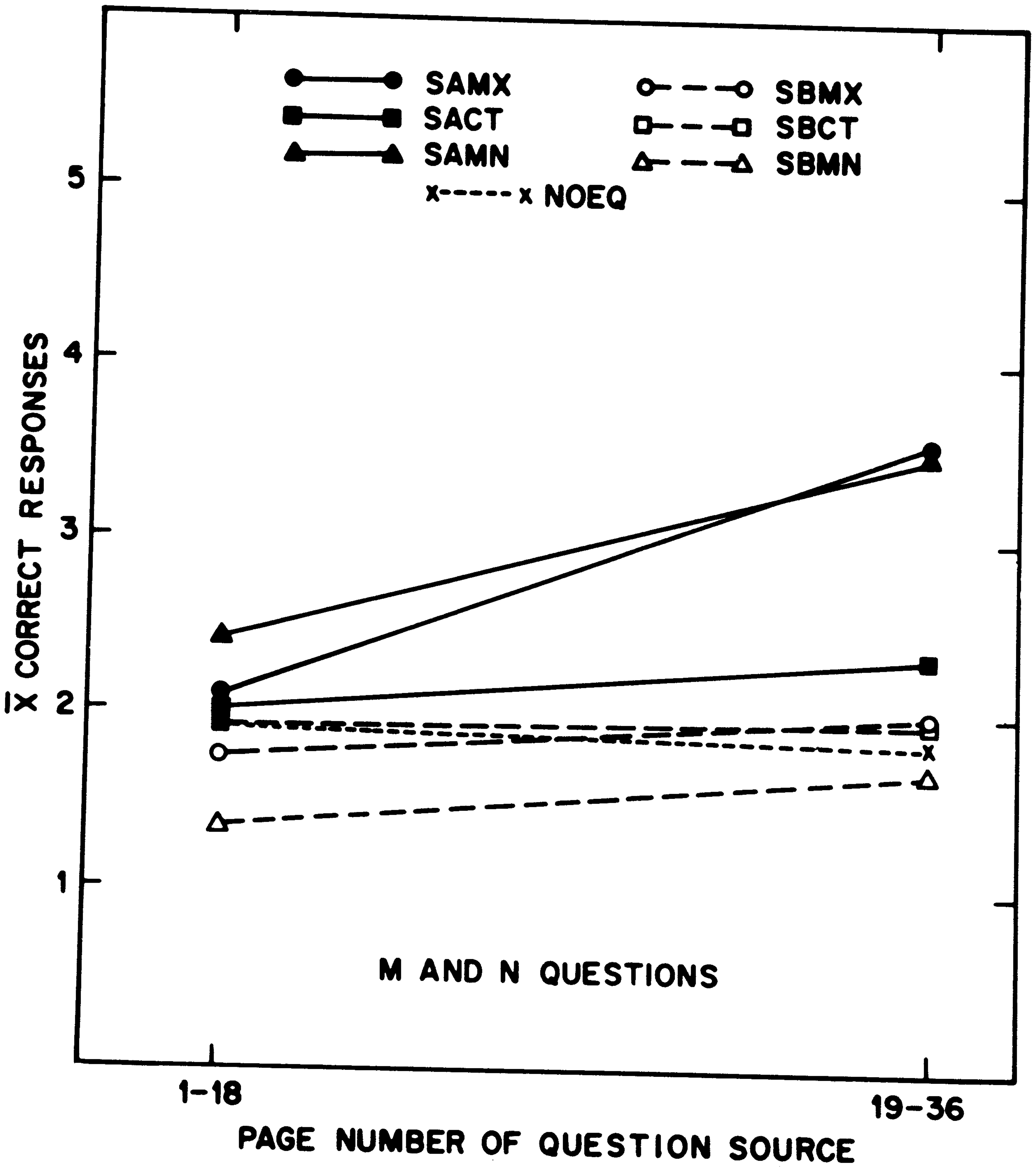

FIG. 3.4. Performance on measure (*M*) and name (*N*) questions on a retention test as function of the location of the question source in the experimental passage. The various treatments are explained in the text.

separately from performance on items derived from the second eighteen pages. The treatments in which questions followed the relevant text segments and that involved some questions requiring *M* or *N* type answers during study (*SAMX* and *SAMN* in Figure 3.4) produced better performance on *M* or *N* items in the test. This advantage increases as study progresses.

Facilitated *M* or *N* learning that was attributable to mathemagenic effects was not observed in connection with treatments involving *M* or *N* questions embedded in the text just before the relevant segments (*SBMX*, *SBMN*) or in treatments without *M* or *N* questions (i.e., *SACT*, *SBCT*, or *NOEQ*). Facilitated learning of special subsets of the instructional material appears to take place without deterioration in the learning of other classes of material within the text.

These results do not lend themselves readily to glib interpretations such as: "Well you told the subject what you wanted of him, and he looked for that kind of information." It should be noted that the experimental effect which I have just described was obtained only when questions followed the relevant text segment. It was not observed when questions preceded the portions of the text from which they are drawn. We have replicated these results in two experiments (Rothkopf, 1966; Rothkopf & Bisbicos, 1967).

The effects of questions on mathemagenic behavior persist for some time after the practice of questioning the subject while reading is halted. In one experiment we exposed *S*'s to either two or four experimental questions after each three-page zone. After six zones (eighteen pages) the questions were stopped. Continued study following the cessation of questions was accompanied by a gradual but fairly rapid deterioration of mathemagenic activities. Results of this nature from a recent study (Rothkopf & Bisbicos, in preparation) are shown in Figure 3.5. This deterioration was accompanied by a decrease in inspection time per page, an effect which is frequently associated with deterioration of mathemagenic behavior.

While our results do not exclude some other interpretations, they very nicely fit a selective adaption model. The subjects engage in an unknown variety of mathemagenic activities during study. If performance on the interposed experimental questions is satisfactory, similar mathemagenic behaviors will tend to take place again during the study of the next segment of the written instructional text. If performance on the interposed experimental questions is not satisfactory, mathemagenic behavior is likely to be modified, and this

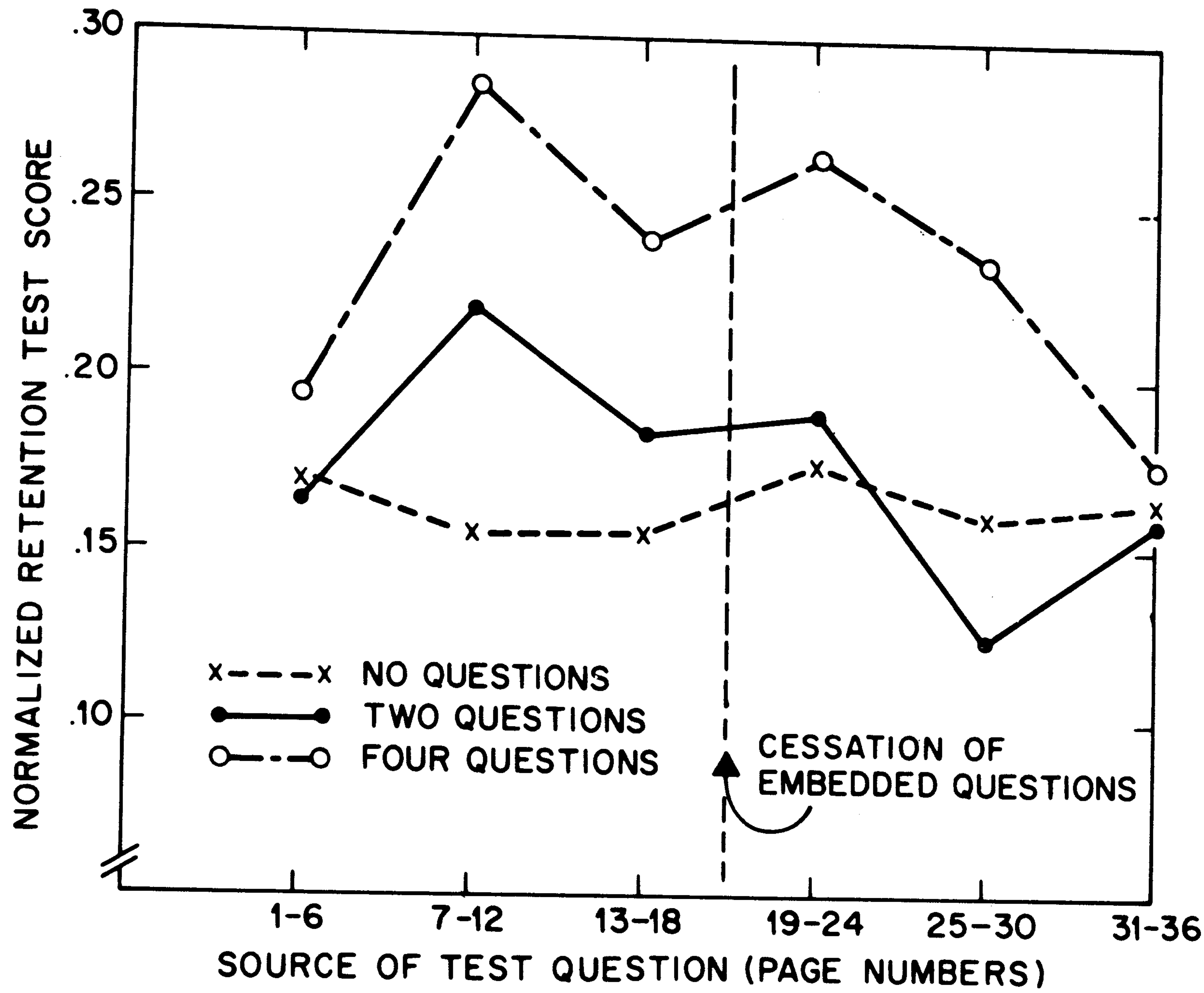

Fig. 3.5. Retention test performance as function of the location of the source of the test question in the experimental passage. Test performance after the cessation of embedded questions reflect the persistence of effective mathemagenic behaviors. The treatments are explained in the text.

process is likely to continue until mathemagenic behaviors have evolved which produce the knowledges that are required to support satisfactory performance on the experimental questions.

There is some evidence that questions sometimes shape mathemagenic behaviors which are not consistent with instructional objectives. Rothkopf and Coke (1963, 1966) have found that the learning of relevant equivalences was depressed when subjects were interrogated in a *predictable* manner *immediately* after the initial presentation of information. Subjects were exposed to factual, paired-associate-like equivalences about a fictional primitive tribe. Information was included about distinctive foodstuffs sacrificed to each of a series of ghosts. Some students were asked to write down the food that was suitable for a given ghost immediately after this information was presented. These students could not supply as many ghost-food facts on a post-training test as students who had not always been questioned immediately, or as students who had been questioned in a more varied manner during the written instructional sequence. We have interpreted these findings to mean that the experimental questioning shaped selective attention in reading, and that the material to which attention was paid was "instructionally" inappropriate, that is, not consistent with the criterion test objectives. The *immediate question-predictable response* group probably learned to pay attention only to those parts of the information-bearing sentence that contain the response term. Similar findings have been reported recently by Faust and Anderson (1966), who have shown that fast readers learn less from programs with predictable, formal prompt pattern. Modifications in visual inspection of written instructional materials due to questioning and overprompting have also been reported by Holland (1964), by Holland and Kemp (1965), and by Resnick (1962).

MATHEMAGENIC BEHAVIOR AND THE INSTRUCTIONAL PROCESS

The most intriguing single result from our work is that the character of questioning tends to shape the character of the knowledges which are acquired. This result was obtained even when experimental questions were related in only a very general or topical sense to the measured criterion skills.

These findings, interpreted in a strong sense, suggest that the pattern set by the instructor in his dialogue with the student determines, to some degree, the processes of inquiry by which the students acquire knowledge. I am not proposing that present evidence warrants strong interpretations. But I am advocating that instructors purge themselves of any cavalier feelings which they might have about class questions, quizzes, and tests.

Questions appear to be fairly potent, and learners modify their learning maneuvers in order to respond to questions in a satisfactory manner. Given the adaptive character of mathemagenic behaviors, it may be expected that they will tend to match the instructional relevance of the questions. There is some suggestion in our data that something like a *law of minimum learning* operates here. Subjects will tend to adopt those mathemagenic strategies that yield the minimum amount of learning necessary to respond to a particular class of questions or other test-like events. Unfortunately it has been so far impossible to define satisfactorily what constitutes a *minimum*. As a consequence we have not yet been able to devise a suitable direct test of this interesting hypothesis.

Humans appear able to detect fairly small statistical biases in their environment. Results from the so-called *probability* learning experiments (see Estes, 1962, p. 132) provide fairly convincing evidence of this. My guess is that we need to understand the statistical biases of the large collection of test questions to which students are exposed each year. I should like to see, for example, if two courses that differ only in the character of their test questions produce different end results. The interesting outcome of this type of research would be to find out how robust the phenomenon is. It would also be intriguing and useful to discover those attributes of questions which exercise measurable controls over mathemagenic behavior.

The gross morphology of mathemagenic behavior in the classroom, such as postural set, proper orientation, participation in class activities, also appears manageable by manipulating suitable classroom contingencies. The work of Baer and Wolf (1966) appears to be a step in that direction. One caution, however, should be considered in this connection. Some gross postural adjustments that teachers might judge desirable may produce undesirable results. The most obvious example is reducing the student's mobility, causing him to look fixedly in one direction. This often brings about deterioration of mathemagenic processing and light sleep.

CALCULUS OF PRACTICE VERSUS MANAGEMENT OF MATHEMAGENIC ACTIVITIES

The calculus of practice and the management of mathemagenic behavior are markedly different conceptions of the process of instruction. The differences between the idealized form of two conceptions can be characterized in several ways. The calculus of practice is more comprehensive in scope, and represents a more ambitious technological undertaking. Its achievement may seriously tax our current scientific resources. The calculus tends toward instructional designs that determine the exact occurrence of each of many discrete instructional events. Its ideal technological goal would be to program instruction through the design of all materials and events, and through the systematic allotment of student effort. The ideal implies, to give a concrete but absurdly extreme illustration, something like rewriting or editing the library, and controlling how the rewritten material is doled out to the reader. This is the kind of instructional Valhalla of which the bards sing at symposia such as this. Approximations to such an ideal are likely to be very expensive (e.g., Rothkopf, 1964) even for very modest instructional objectives such as, for example, the mastery of the physical and economic geography of California. The power that such a system can attain is largely unknown.

The management of mathemagenic behavior, on the other hand, is a much less highly controlled instructional system. It is likely to be carried out largely by managing contingencies, in a statistical sense, between large collections of instructional events and student actions, and also by controlling the aftermath of students' activities. Such an approach depends on the discovery of factors that will shape instructionally useful mathemagenic behavior and that will prevent the deterioration of these behaviors. It also seeks the material means for controlling these factors in the instructional situation.

In the management of mathemagenic behavior, instructional materials are accepted as *givens*. They are, more often than not, unpleasant facts of life produced by an educational publishing industry that persistently chooses to misunderstand what is meant by educational technology. These unpleasant facts of life are accepted and the instructor concentrates on how to maintain processing activities in his students so that they may learn available instructional materials. In the library illustration that has been mentioned earlier this would imply assigning books to read, and bolstering the likelihood that students will read the assignments in an effective manner.

The calculus of practice and the management of mathemagenic behavior are two approaches toward a scientific basis for instructional practice. Neither the research programs nor the instructional systems implied by these two approaches are mutually exclusive. The calculus of practice tends to emphasize detailed design of substantive instructional material and the close programming of instructional episodes. The management of mathemagenic behavior, on the other hand, accepts available instructional materials with a certain degree of resignation. This approach concentrates on maintaining those student behaviors that produce learning. In doing so, mathemagenic management attempts to capitalize on the adaptive, error-correcting characteristics of man.

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AUDIENCE QUESTIONS

• *Question:* I wonder if Dr. Rothkopf could expand on the deficiencies of the publishing industry in generating the kind of behavior in students that we all seek.

• *Dr. Rothkopf:* I have to be careful in answering this question because the role of the publishing industry in the production of instructional materials is one of my pet peeves.

I am convinced that a large proportion of printed material that is used in school has been developed in a way that is far behind the most advanced state of the instructional art. There is very little serious effort to maximize instructional effectiveness. A lot more could be done by trained people who are clearly aiming at instructional effectiveness and who are willing to look at educational publishing as a technological enterprise on levels besides the mechanics of printing and distribution.

Many difficulties stem from economics and from the traditions that exist in this area. With some exceptions such as encyclopedia production, the publishing industry does not create instructional materials. It's my impression that this industry does not routinely invest substantial capital in the development of their instructional product. They select and process the works of individual entrepreneurs. Those entrepreneurs, the authors, almost never invest any other capital besides their labor in their work. The publishers invest their resources in manufacture, promotion, and distribution. They limit themselves to selecting, book design, preparing copy suitable for printing, contracting for printing services, and distribution. The way these functions are carried out are governed by considerations of profit. This is as it should be. But profits and instructional effectiveness are not necessarily correlated. This is lamentable but not the fault of the publishing industry. The responsibility is clearly that of the educational community which has not yet developed good criteria for selecting text material. If such criteria were widely employed to forecast instructional effectiveness, the publishing industry might find that the manufacture of instructionally effective material was good business.

• *Dr. Gagné:* It is true, isn't it, Ernie, that one thinks of textbooks as being written with a literary style criterion, as one writes a novel. Literary standards are not the same as pedagogical or instructional standards.

• *Dr. Rothkopf:* Yes, I agree. Take as an example the criterion of clarity. Most editors that I have spoken to agree that sentences should be clear, and will make editorial changes to bring the sentence in line with their conceptions of clarity. When one probes this conception of clarity a bit one can discover at least two components: absence of ambiguity and how well the sentence reads. It's this last criterion that interests me particularly. It's widely accepted, so widely in fact, that most of you probably think it strange that I should question it. But the relationship between how well a sentence reads and how effective it is in teaching has to my knowledge never been tested. Personally, I am very suspicious of very smooth, beautiful prose as a carrier of denotative meaning.

• *Question:* When I try to direct my students' study in multiple texts, I excerpt parts from the books, and present them on typewritten ditto sheets. In this I have noticed recently a stronger response on the students' part for this type of material. Would this be an example of the mathemagenic shaping process?

• *Dr. Rothkopf:* You may be right. A book is really a pedagogically absurd instructional unit, though it is an economical way of packaging paper. Licklider in his book *Libraries of the Future* made a similar observation. He felt a book was an inefficient package for information while a page was just about the right size.

One of my students at Rutgers University has made some observations similar to yours. She found in a pilot study that what students learned from an experimental passage of a given length depended on the number of text pages in which the passage was embedded. This was found despite the fact that the students never had to read anything else besides the experimental passage.

DISCUSSION OF ERNST Z. ROTHKOPF'S PAPER

MODERATOR: Dr. Asahel Woodruff
FIRST DISCUSSANT: Dr. Arthur R. Jensen

• *Dr. Jensen:* I find Dr. Rothkopf's paper stimulating, not only for the major themes that he explicates so well, but also for all of the richly interesting asides and tidbits that can be found in it. It has also increased my professional vocabulary by a number of new terms—at least they are new to me. And I was happy to find some support in the paper for a notion I have long held: that Pavlov is quite relevant to the psychology of instruction as well as to the experimental psychology of learning.

Rothkopf seems to deplore the practice of speaking about *how much* is learned. He implies that this kind of measure is an unfortunate artifact of our traditional methods of studying learning in the laboratory. I am not at all sure of what the alternatives to this practice would consist. When we study the learning process, we think in terms of discrete subunits, such as stimulus exposures, trials, numbers of correct responses, and the like, mainly as a means of gaining some degree of quantitative control over our independent and dependent variables. I see nothing inherently wrong with this; it is an inherent aspect of an analytical approach to the study of learning. But this does not mean I would not be willing to entertain some other basis for analyzing the learning process. It is the possibility of quantification that I would *not* be willing to give up, rather than the particular method by which it is achieved.

The whole discussion of what Rothkopf calls the "calculus of practice" is most interesting in terms both of historical and recent developments in the psychology of learning. The calculus of practice essentially has been concerned with understanding variability in speed of learning, goodness of retention, and the like, as a function of the conditions of learning. The question of *economy* of practice comes into the picture when attempts are made to manipulate the conditions of learning in ways that will speed up learning, so that some given criterion can be attained in fewer trials or with less practice. The problem has been that of discovering the kinds of manipulations that will make for greater efficiency of learning. In the realm of verbal learning, at least, successes have not been impressive. There have been two main reasons for this relative lack of success in discovering ways to make learning more efficient; the first is that, in most cases, the wrong variables have been manipulated; the second is that experimental psychologists have ignored—guess what?—individual differences; that is, the Subjects × Independent Variable interaction.

(I wish to thank Dr. Rothkopf at this point for giving me a legitimate excuse for singing my favorite old theme song!)

But first, a word about the choice of experimental variables. The favorites of experimental psychologists have been distribution of practice and some variation of the part-whole method of learning. In terms of economy of performance these variables, it turns out, are extremely puny in their effects. (The one exception is distribution of practice on strictly motor-learning tasks, like the pursuit rotor. Here distribution seems to make a real difference, not only in speed of learning, but also in final asymptote of performance.) The vast research literature on whole-part learning, as Rothkopf points out, adds up to little. The best and most recent evidence we now have on the subject of whole-part learning—the work of Leo Postman and his colleagues in the Institute of Human Learning—has produced results that are fairly consistent with the Bugelski hypothesis, which states that a given amount of material requires a certain amount of time to be learned up to a given criterion, and no matter how you manipulate such variables as distribution of practice, rate of stimulus pacing, or various combinations of part-whole learning, the total time required for learning will remain the same. This rule that total learning time for a given task equals a constant for a given individual has held up in many kinds of data. A serial or paired-associate list presented at a rate of one stimulus every two seconds will be learned in, say, twenty trials. If the rate of presentation is slowed down to four seconds per item, it will be learned to the same degree in only ten trials; but the total learning time remains the same. This is a fascinating finding, but the limits of its generality have not been adequately explored. It is a reasonably safe bet, however, that with the usual methods of experimentation on learning, this time constancy will hold up across a wide variety of experimental manipulations of the conditions of learning—particularly those conditions that Rothkopf has in mind in referring to the "calculus of practice." This may mean that we are looking at the wrong kinds of variables if what we are interested in is economy of practice.

Fortunately, we are beginning to see that there is another class of variables (Rothkopf might include them under the heading of mathemagenic) which do, in fact, increase the efficiency of learning, and they do this without contradicting the Bugelski hypothesis. It is accomplished by somehow changing what is actually learned. To analogize with the vocabulary of genetics, we can say that the input remains phenotypically the same, and the output, i.e., the subject's final performance, is phenotypically the same; but the genotype, that is, what the subject actually learns, is somehow made quite different by the subject's method of processing the input. In some cases, we know that the material to be learned is transformed or coded by the subject in some way that—in effect—markedly reduces the amount of material that must actually be learned. In this way the subject circumvents the Bugelski constancy. The now classic example is the one given by George Miller in his well-known paper on "The Magic Number Seven Plus or Minus Two." A long string of some twenty-odd binary digits can be recalled by an average subject after one presentation if he has first learned to encode binary digits in groups of three. Without this encoding mechanism the subject could recall only about seven digits.

Another striking example is the work begun by my colleague William Rohwer and me a few years ago, in which we induced persons to think of ordinary paired-associates as subjects and objects of sentences, which the learners supplied themselves. Learning rates could be boosted to more than five times as fast as normal by this method—an increase in learning efficiency that far outstrips any effects anyone has ever been able to produce by manipulating variables such as distribution of practice or part-whole learning.

In the case I am discussing, of course, the crucial variables lie *within* the subject; the experimenter's influence over these processes is only quite indirect. This transformation of what the subject has to learn apparently is not limited only to verbal forms of learning, but can play a role even in what may appear to be a motor skill. As a clear-cut example, permit me the indulgence of an anecdote rather than a scientific datum. A few years ago Pablo Casals was a visiting professor here at Berkeley. I recall watching him coach one of his talented students in a passage which gave the student particular technical difficulty; her performance of this tricky phrase fell far short of the clean, crisp precision of Casals' performance of it. Casals stopped the student after her second unsuccessful attempt and told her not to think of the phrase as being written in 2/4 time, which it was, but instead to think of 8/16 time while playing it. She then tried again, and this time played the passage with almost the same needle-sharp accuracy we had heard when the Master himself played it. The student looked amazed, as did the spectators; it was a revelation—to all but Casals. Thus, an important task for experimental psychologists interested in the economy of practice is to discover the kinds of cognitive transformations subjects can be taught to perform on the material to be learned in order to increase their efficiency of learning.

Now about individual differences: the simple fact is that we really don't know how important such variables as distribution of practice or part-whole procedures are to learning efficiency. Experimental psychologists have insisted on looking only at group means when they compare various experimental conditions. If there are large Subjects × Conditions interactions, as we know there often are, the actual strength of the experimental variable can be completely hidden if all we look at are mean differences between experimental and control groups. Many experimental variables which have been thus dismissed as unimportant may have, and probably *do* have, quite large but opposite effects on different individuals. Our traditional experimental paradigms, however, will never permit us to discover this.

The simple hypothetical experiment below, showing the learning scores of two individuals under the Experimental and Control conditions, illustrates what I mean.

	Conditions	
	Experimental	*Control*
Bill	75	25
Jack	25	75
Mean	50	50
$t = 0$		

When we come to Rothkopf's discussion of practice or study time regarded as a form of *cost*, I find I am unable to agree with his thesis, that is, assuming I have correctly interpreted what he has said. Practice or learning time is cost, not only to the school and the teachers, but also to the pupil—*especially* to the pupil. The idea that since study can be a pleasurable activity, it cannot be regarded as involving cost to the student, simply does not stand up under analysis. Study may be a pleasure, but taking longer to learn the same material that under better study conditions could be learned in less time doesn't increase the pleasure. When learning progresses too slowly, the subject "drops out" or in some way leaves the situation. One of the most important reasons for improving the efficiency of learning is not to cut instructional costs, but to cut the cost in time and effort expended by the student. Cost in time and effort cannot exceed motivation and reinforcement (either intrinsic or extrinsic), or the individual will stop learning altogether. Much of the reinforcement for learning, I believe, comes from the individual's own perception of his gains. When these are too slow in coming, he bogs down.

Now, if we can speed up the time it takes for a student to learn something, this would seem desirable, and it is irrelevant to the issue whether the student wastes the time we have saved him or puts it to good use. What he does with this extra time will depend on his interests, values, ambitions, etc. If my hunch is correct, that learning is self-reinforcing to the degree that progress is perceptible to the learner, then more efficient practice or study techniques will make him want to study more, not less. After all, bright students don't study only half as much as dull students simply because they can learn the same assignment in half the time!

Mathemagenic Behavior. The most important part of this paper, for me, is the discussion of mathemagenic activities. It is a wonderful conception, and one which has not yet gained the attention it deserves in our psychology of learning and instruction. It is in this realm of mathemagenic activity that I think we can best understand and possibly remedy the most crucial deficiencies of so-called culturally disadvantaged children. The self-directed activities the child brings to the learning situation appear not to be the most crucial variables in the whole picture. I note especially Rothkopf's statement that "The pattern set by the instructor in his dialogue with the student determines, to some degree, the processes of inquiry by which the student acquires knowledge." I think the implications of this can be fruitfully extended to our thinking about the parent-child relationship and how the styles of learning that have their roots in the early parent-child interaction can affect later educability.

Rothkopf points out that there are also *negative* mathemagenic activities, and he conceptualizes some of these activities in ways that should arouse the interest of learning psychologists. I am delighted with the analogy between certain forms of negative mathemagenic behavior and *experimental extinction*, as described by Pavlov. I think this is more than just an analogy; I think these behaviors really *are* experimental extinction; and in some students, I believe, we can see evidence that the stimuli of the classroom—blackboards, papers, pencils, books, and especially the teacher—have become conditioned inhibitors. How can we prevent the build-up of experimental extinction and conditioned inhibition in the instructional situation? The Pavlovian answer probably would be to insure adequate reinforcement, to adequately space periods of intensive effort, and to avoid monotony in practice. The finding that deterioration of positive mathemagenic activity and the inducement of light sleep can result from requiring students to maintain a particular posture and look fixedly in one direction (usually at the teacher) could have been predicted from Pavlovian theory, provided one also assumed that the students were not getting much reinforcement or feedback from their own mental activity. Under similar conditions Pavlov's dogs, too, dozed off in the conditioning stand, and human subjects in conditioning experiments have been observed to do the same thing, usually during the extinction phase of the procedure.

I have observed an even more extreme and undesirable form of negative mathemagenic behavior in culturally disadvantaged first-graders, probably because tasks were forced on them that were too far beyond their immediate capacities. This, too, has its Pavlovian counterpart: the children's behavior had a remarkably close resemblance to Pavlov's descriptions of experimental neurosis. It consisted of aimless hyperactivity, active inattentiveness, willful avoidance of the learning task, and increased aggressiveness and emotionality. These forms of behavior, which I believe often develop to a large extent in the classroom itself, seem to defy practically all normal instructional efforts once the undesirable behavior has developed. Certainly we need much more systematic study of all these negative mathemagenic behaviors, as well as of the positive.

Rothkopf's intriguing findings on the mathemagenic consequences of inserting questions in instructional texts speak for themselves, though of course this research raises many questions to which one would like to know the answers. But these must be gotten empirically. I wonder why, for example, questions presented *before* a passage do not have the same facilitating effect on retention of the relevant textual material as questions presented *after* the passage. This phenomenon is in need of further exploration and explanation.

Finally, let me say that I hope investigation of the mathemagenics of learning, both in and out of the classroom, soon becomes a popular area of research. From Rothkopf's introduction to the subject in the paper we have just heard, it seems to me a most worthwhile approach to the understanding and improvement of the instructional process.

• *Dr. Woodruff:* I think before we go on to the next discussant it would be appropriate to ask Dr. Rothkopf if he wants to comment on what has been said thus far, or just sit there and purr.

• *Dr. Rothkopf:* I would like to make two comments. I am very appreciative of Dr. Jensen's remarks. He has brought out some interesting relationships that never occurred to me. He has caught me up very properly on two points, and I would just like to comment on these.

I stressed the matter of economy of practice and the matter of how much is learned in part for dramatic effect. I don't mean to imply that a statement of how much is learned is always meaningless. Sometimes it is. People often compare two experimental treatments in laboratory studies of instructional methods and act as if their criterion test has exhaustively sampled all the performance the subject might have acquired in the course of practice. As a matter of fact, it can be demonstrated at times that there are a number of transfer tests that would reveal that the group that proved to be less effective on a given criterion test actually acquired not less but different knowledge. This "different" knowledge might be instructionally relevant or not. This is the source of my objection to describing the results of the training experiment in terms of one treatment producing more learning than another.

I find it disturbing to read one report after another, and you see them all the time, that end up by saying, "group so and so learned more than some other group," even though the domain of learned performances that resulted from the treatment probably vastly exceeds what was actually tested. My objections were primarily a reaction to this practice. Dr. Jensen's point is certainly well taken that we must have some kind of measurable aspect of instruction. I didn't mean to imply that I want to retreat into the qualitative forest.

The second point concerns itself with Dr. Jensen's comments on the economy criterion. I am not trying to imply that more economical practice is always a bad criterion for the selection of instructional methods. However, I think that there are many, many cases in which it is a bad criterion.

I think economy is probably a good criterion in many drill-like situations. If the treatment produces results in less practice time, it is the treatment you would wish to choose. In many cases, however, more practice or more study really means an expansion in scope of what the student is studying. He reads in more books in order to master some particular subject matter. He works with more problems. It may be the case that with respect to the particular narrow criterion instrument that was used the effect of this greater amount of practice isn't detectable with a given narrow criterion instrument. If transfer tests increase one might discover that the extra practice has actually broadened the student's competence in some way.

SECOND DISCUSSANT: Dr. Stanford C. Ericksen

• *Dr. Ericksen:* I have studied Dr. Rothkopf's paper and during recent weeks have also reviewed several of his earlier reports. I will talk less about the particulars of this paper and place greater emphasis on the two or three main implications for educational research and practice. My enthusiasm runs high and I may sound like someone giving a seconding speech for a candidate at a political convention. I am confident that I am backing a winner.

"The calculus of practice" is a useful handle to carry ideas back and forth between the learning research laboratory and the natural learning setting of the classroom. In the first place we are reminded that the early research on rote learning was based on "... the assumption that if a treatment produces results with small amounts of practice (trials) in the laboratory, a like treatment will also produce results in an economical way in the schools" (pp. 3-4). These historical notes help to clarify the foundations on which subsequent investigators have tried to build a superstructure regarding the nature of human learning and the instructional process. However, I am not particularly impressed with either the form or the function of this superstructure which, as Dr. Rothkopf emphasized, has long displayed disproportionate attention to "the economy of practice."

The persistence of the "calculus of practice" in laboratory-based research is, in part, the self-conscious effort of investigators to meet the quantitative requirements of the scientific method. In consequence, these psychologists are biased toward the use of clean, measurable variables that help to establish the rigor of their experiments.

"Economy of practice" has also served as the model for the educational researcher, the teacher, and the school administrator. However, at the applied level, its justification has been made in terms of efficient, business-like management of our schools—which has long been a strong demand and a popular criterion for "progress" in education. The question of how to lower instructional costs is asked more often than the question of how to improve the acquisition of knowledge by our students; and the reason, I suspect, is because the former is a "get-at-able" and can be measured more easily than the latter. However, I would quickly add that the economic self-interest of taxpayers and tuition-payers should never be minimized as a "cause" for educational reform.

It should be pointed out that applied educational researchers and teachers must not circumscribe the freedom of the basic researcher to make his laboratory-based probes in whatever direction and with whatever methodology he deems most appropriate to achieve his purpose. The gap between learning theory and educational practice is a no-man's land, true enough, but the responsibility to bridge this gap rests more directly on the back of the practitioner and his research advisors than it does with the theoretician.

I agree with Dr. Rothkopf that the calculus-of-practice mode of laboratory research has not been a particularly exciting source of ideas for improving the instructional process. We must realize, however, that not all learning-theory research is aimed toward the schoolhouse door. The generalized, nomothetic phenomenon of learning is a legitimate and self-sufficient object of study, and it is to be preferred that changes and improvements in the design strategies by the basic researcher should come about rather because he himself recognizes the limitations of traditional and conventional experimental paradigms, than as a consequence of complaints and criticisms by teachers or applied researchers. More would be lost than gained if the utilizers of research knowledge began to apply pragmatic pressures and constraints on the research freedoms of the basic investigator. On the other hand, the learning theorist is denying himself a valuable source of data if he does not extend the boundary of his laboratory to include the classroom, the library, and the many other sites where real people learn about real things, acquire concepts, and learn how to attack concrete and abstract problems.

Let me add a further note of caution. The utilization of laboratory-based knowledge by the classroom teacher is, itself, a complex conceptual problem. We must dissuade ourselves of any notion that the learning theorist is a Knight on a White Horse riding in with his special package of "scientific" ideas that will save education. I hope that by now we have learned that the application of "off-the-shelf" ideas, procedures, and techniques from the learning theorist to the desk of the classroom teacher does not happen in a straightforward, linear fashion.

In this connection I was particularly interested in Dr. Rothkopf's reference to the whole-part problem to illustrate the calculus of practice type of research. Every once in a while my colleagues and I receive requests from faculty members, department chairmen, and students that show they have read and still remember the "management of learning" chapter in a psychology text. A typical question might be "What data do you have that will show that a student will learn more from courses given in five-hour blocks versus the 'fragmented' two- or three-hour courses?" To a certain extent educational psychologists are now beginning to reap the harvest from planting the wrong seeds, namely, that the management of learning can be improved by the straightforward extrapolation from laboratory data to the classroom. I am afraid we are faced with a rather serious and momentous task of upgrading our own conception of how research relating to instruction should be designed and carried out. And this is why I appreciate the greater emphasis Dr. Rothkopf has given to the "mathemagenic activities." The calculus of practice typifies the behavior-in-general orientation of the classical learning theorist; but the transition from nomothetics to ideographies is, itself, a process demanding a great deal of rigorous and ingenious research, and I think Dr. Rothkopf has given us an excellent exemplar.

The teaching profession should, I believe, become far more aggressive in attacking the tradition that a classroom of students is and must remain the basic instructional unit. The lowering of instructional costs is the pedagogical counterpart of the calculus of practice, and carries with it the same limitations for the improvement of the instructional process. Professor Jensen's point is well taken, namely that conventional educational research involving a comparison of group means is a thoroughly inefficient way to analyze the primary, student-linked variables that are involved in the student's education. As psychologists and as teachers we have no alternative but to pound the table and insist that the individual student rather than the class become our primary unit of instruction. It is high time we moved beyond our lip-service tradition of recognizing individual differences to implement the procedures that would release and utilize the student-linked variables as the most powerful single set of resources to upgrade the quality of education. The study of mathemagenic behavior is a significant step leading us beyond the generalized smooth functions that so often tend to mask out the zigzag curve of learning that marks the acquisition of knowledge by the individual student.

In some respects mathemagenics represent the educational version of in-the-field research that is being revived and re-emphasized by comparative psychologists. I am intrigued, for instance, by the parallel between mathemagenics and ethology. I realize I am moving somewhat beyond the boundaries of Dr. Rothkopf's field of view, but he has made a significant move, and in such a critically important new direction, that I simply want to encourage him to continue on and to seek new and better ways to obtain information about how the individual student acquires, retains, and utilizes new knowledge.

Most of us needed this demonstration by Dr. Rothkopf that not all the educational data need be based on group tests, average group performance, or with performance data obtained from the "empty organism." The ethologist has already established the value of detailed longitudinal observations of the individual organism and it may be appropriate for the educational researcher to adapt and to continue these techniques. I would like to see Dr. Rothkopf and his associates apply to the study of the individual pupil the same diligence and persistence and discriminating power demonstrated by Konrad Lorenz in his study of the goose. Dr. Rothkopf has already made giant strides with his primary concept and his tentative subtopics: translation, segmentation, and processing. Fortunately, Dr. Rothkopf recognizes that to construct a taxonomy is not enough, and he has given us references to an unusually interesting and valuable pattern of experimental studies which, in effect, turn the high-powered microscope (telescope) on the "human error," otherwise known as subject variance. And this matter, the individuality of students, is what teachers are paid to handle and one to which we as educational researchers should make a stronger contribution in the immediate years ahead.

• *Dr. Rothkopf:* I don't believe that the philosophy I called the calculus of practice is restricted to learning theory or to laboratory studies. A good many of the assumptions about curriculum sequence, for example, are very reminiscent of a calculus of practice. Some of this is changing now, but for a long time it was thought that you had to have analytical geometry before calculus, algebra before analytical geometry, and so on. Reading readiness training before reading instructions is another example of sequence assumption that resembles the practice calculus. So is the ubiquitous practice of extensive review of grade G topics in grade $G + 1$, particularly in the lower primary grades.

Designers of curriculum sequences commonly look for the optimum path of progression, whether they say so or not. So do many of the mathematics or physics curriculum reformers. They may speak about the "natural" development of ideas, but they may have converged empirically on an optimum path.

• *Dr. Woodruff:* Dr. Gibson, would you like to raise some questions now?

• *Dr. Eleanor Gibson:* Well, I would just as soon pass, but I'll ask a question. It has to do with the slogan of shaping behavior, and I must confess that I dislike the slogan very much, because I think it means just exactly what it says. It is nothing but behavior in the "response bias" sense that you shape. Why do you want to shape behavior when you say you know that removing the conditions of shaping causes the behavior to disappear?

• *Dr. Rothkopf:* Well, I think this is pretty straightforward. I wouldn't call a practice educationally useless even if you have to use it constantly to keep things going in school situations. The fact that mathemagenic activities might deteriorate if you remove the conditions that support them doesn't necessarily mean that you want to avoid incorporating support procedures into educational practice. It simply means that you want to engineer the instructional system so you can maintain mathemagenic behavior. I see no reason, for example, to conclude that paying workers is an undesirable industrial practice because people stop working when you stop paying them.

• *Dr. Eleanor Gibson:* Is it the behavior that you want to change or manipulate, or is it knowledge that you want to be gained?

• *Dr. Rothkopf:* My formulation grows out of a deep sense of pessimism about what is possible to do by detailed design of instructional programs. I am really proposing that we take advantage of the fact that the human being is a very adaptable error-correcting organism. The important thing is to keep him going. That may even be enough. In our present state of ignorance it is not practicable to spell out what is optimum for him. One alternative solution is to throw him into an instructional situation and to keep him going, in some way that is presumably instructionally effective.

In a certain sense practically all of human knowledge is deposited in libraries. Yet every day millions of students pass the libraries on the way to school. If you could somehow get the kid into the library and keep him going there, maybe we could dispense with the schools altogether. Maybe we can have a new kind of instructional library especially designed to maintain the mathemagenic activities of student users.

• *Dr. Eleanor Gibson:* Of course I don't think so, or I wouldn't have been talking the way I did today. But let me just ask one more question. You talked about selective adaptation models. What do you think of as the selective principle? You asked me that. I'll ask you.

• *Dr. Rothkopf:* Well, let me speak quite frankly. I can think of at least one other model for how testlike events maintain appropriate mathemagenic activities which probably works as well as the selective adaptation principle. I cannot go into the details of this alternative model here. Implicit in the selective adaptation interpretation of testlike events is the assumption that there is such a thing as a satisfactory outcome to a question. Those study activities that produce these satisfactory outcomes are selected from the study repertoire by the outcomes.

• *Dr. Eleanor Gibson:* I agree with that, but it doesn't sound like what you have been saying.

• *Dr. Rothkopf:* I guess, then, my initial presentation must have lacked the kind of clarity it should have had. I was trying to say that the behaviors of the students are shaped to meet the demands of the test situation. The adaptive principle is that study strategies will converge on that study pattern which meets the demand of the instructional questions.

• *Dr. Eleanor Gibson:* Where they can pass the test?

• *Dr. Rothkopf:* Where they can consistently pass the test. I have some suspicions that these testlike events don't work the same way for students of all socioeconomic backgrounds. It's also possible that they don't work as well when the source from which the questions are issued is a low-prestige person or situation. I am speculating. Lots of these problems are completely up in the air and need exploring.

Nor do I rule out the possibility that other factors—self-reinforcement factors, uncertainty-reducing factors—might possibly shape up these behaviors. It seems to me what is necessary is some fairly concrete way—more concrete than I have been able to identify so far—of spelling out the general problem so that we can check out some of these possibilities in a systematic way.

My notion of mathemagenic behavior is related to Berlyne's notion of epistemic curiosity, although he perceives it as a drivelike state, whereas I look at it as more of a habitlike state.

• *Dr. Glaser:* Ernie, by trying to maximize test performance through testlike events aren't you caught in your own trap of the calculus of practice: trying to maximize the final behavior, which is sort of the accusation you made of "calculus of practice" procedures?

• *Dr. Rothkopf:* I don't know. I don't think of it that way. I think of calculus of practice as either empirical or theoretical arrangements of substantive instructional events, rather than the manipulation of study activities.

• *Dr. Woodruff:* Isn't this the calculus of practice of the teacher?

• *Dr. Rothkopf:* I guess in some sense. You mean of the teacher's maneuver? Perhaps.

• *Dr. Lumsdaine:* I would like to make one protest with respect to throwing out economy as an appropriate criterion in learning. I may have limitations to my logical capacity at this hour, but I see no alternative other than economy; that is, amount learned per unit of time in some sense as a practical measure of learning.

What I suspect—and your remarks seemed to reinforce this—you are really saying is that we frequently don't have adequate comprehensive measures of the dependent variables. Thus, a great deal is being learned which is not recorded in the particular measure that we use. I would ask to clarify the difference between us—suppose we do have adequate measures of all the things we are interested in having learned, would you still feel that there is a reasonable alternative to the economy measure? If so, why?

• *Dr. Rothkopf:* I don't know whether there is a reasonable alternative, but I can think of one instance when economy of practice would still be a bad criterion. I think Dr. Jensen has mentioned it. It would be if the most economical practice mode is one that would discourage the student from ever studying again.

• *Dr. Lumsdaine:* But then the student's tendency to study further is a criterion. It is one of the relevant dependent consequences of the training schedule, and must be considered along with what content he learned in the particular study.

• *Dr. Gephart:* At that point economy is not the objective, but a factor in measurement. Isn't what you were saying that when we use economy as our end we are trying to achieve the economy versus using this as one of many measures of achieving other ends?

• *Dr. Rothkopf:* I merely meant to imply that economy was nowhere nearly as efficient, perhaps not even a very good criterion for adopting a particular training maneuver.

Let me talk about a very concrete example. Suppose we put students in a room and expose them to a highly informative message, one with very little redundancy. Just pack the channels with information, and arrange some condition such that we make sure the student can and will attend to the message during this period. Perhaps we will tell him that we will throw him out of school if he doesn't make it!

• *Dr. Lumsdaine:* Put them in the infantry!

• *Dr. Rothkopf:* Put them in the infantry; right. One particular case is they won't get a medical degree. So now they have to cram, really get the stuff mastered in a minimum period of time. This sort of thing undoubtedly takes place in medical school, particularly in anatomy. I think the record of many physicians shows that they never study again when they are through.

• *Dr. Lumsdaine:* I don't think this is just an argument over words. Let me simply say that again what you are failing to do is to include relevant outcomes in your dependent measures, such as whether they study again. If you were to include as the relevant outcomes of instruction the motivational effects, as manifested in what they do subsequently in other learning activities, what they retain and what they apply, and not just what they may have learned in this particular session, then I would ask again, would you still feel that the total accomplishment isn't properly viewed as a function of some kind of input investment?

• *Dr. Rothkopf:* I can't resist that. You are absolutely right. I mean, if one goes about things in a sensible way, one is very likely to get sensible results. But do we really know how to do this sensible thing?

• *Dr. J. J. Gibson:* May I suggest, Dr. Rothkopf, that you are not standing up to the courage of your implicit convictions, which if I see their direction, suggest that you contradict all these experimental psychologists—I am one—and say that learning cannot be measured. It seems to me implicit in what you are saying. It would be a very bold assertion, but I think it is implied in what you have been arguing.

• *Dr. Rothkopf:* I am in absolute agreement with you, and your wise encouragement has given me the strength of my convictions.

• *Dr. J. J. Gibson:* Good! Good!

• *Dr. Rothkopf:* I think that is the implication of the indeterminacy principle that I proposed. It is a nasty state of affairs, because when you try to determine what learning does take place through a series of complicated transfer tests, then you run into another indeterminacy principle. Namely, that the operations that you use to measure what learning states exist in themselves are maneuvers that produce learning. So you are in a real bind. For this reason I have always been perplexed by people like Simon and Newell, for example, who attempt to infer something about internal mental activities by testlike events such as saying to people, "Tell me what you're doing." They are really affecting these internal activities. It's a psychological Heisenberg principle. You cannot really infer what is learned.

• *Dr. Lumsdaine:* One more quick comment. It seems to me most remarkable that a person can be as productive both of ideas and of useful research data as Ernie Rothkopf is, and still deny one, that you can't control conditions of learning effectively, and two, that you can't measure the effect of what you have done.

• *Dr. Rothkopf:* I'm just a damned fool, that's all!

• *Dr. Ellis:* If you can't measure learning, Ernie, can we measure anything in psychology? Are we equally helpless in other areas?

• *Dr. Rothkopf:* It is my real conviction that we are overestimating what we can measure.

• *Dr. Ellis:* If you had the courage of your convictions, you would say "Yes" to the question too.

• *Dr. Rothkopf:* I am sure that your comment must be a very thoughtful one, and I think I ought to think about it for a while.

CHAPTER FOUR

THREE CONCEPTUAL APPROACHES TO RESEARCH ON TRANSFER OF TRAINING

BY

M. C. WITTROCK

UNIVERSITY OF CALIFORNIA, LOS ANGELES

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This conference has a theme important to educational researchers, emphasizing as it does basic approaches to research useful to the study of change of behavior in schools. Educational researchers do not often use models and theories to guide their research, although they frequently extrapolate results from experiments relevant to their problems. In education, we would do well to discover the transfer value of conceptual approaches appropriate to research on behavior change in schools. Perhaps this conference will help in this discovery, and perhaps it will also encourage the development of theory and models from the problems of learning in schools.

The position taken in this chapter is that transfer is one of the most important products of education. The identification of the conditions of transfer is an important area for basic research on school learning. The next and most needed step in current research on the identification of the conditions of transfer involves the use of models, including the development of new ones, to indicate some of the fundamental conditions of transfer. A series of questions follows from this position. What effects upon research can be expected when educational researchers interested in identifying the conditions of transfer adopt a given model of transfer? Which problems of transfer are they likely to study, and which problems are they likely to ignore? Which models of transfer seem especially useful to identify the conditions of transfer in school learning? What does each of these models lead us to expect about the conditions of transfer in school learning? In brief, the chapter argues for the importance of basic, theoretical research in the study of the conditions of transfer.

APPROACHES TO THE STUDY OF TRANSFER

In this chapter we will consider three conceptual approaches to the study of verbal transfer: (1) simplistic *S-R* models; (2) mediated generalization models (*S-r-s-R*); and (3) Gagné's model of hierarchical learning sets. These approaches were chosen primarily because they have demonstrated their utility for educational research. They also represent different degrees of specificity to the problems of transfer in school learning. The broad S-R approach is concerned not only with verbal transfer, but also with non-verbal behavior. The equally broad mediated generalization model next discussed was developed largely from the problems of verbally mediated transfer. The Gagné (1962, 1965) model was developed to explain productive human learning and transfer, especially of organized subject matter. It is expressly designed for the study of the problems of transfer in school learning.

The Meanings of Transfer

Before a discussion of transfer of training is undertaken, several meanings of the term should be made clear. Transfer of training is the effect of prior learning upon subsequent learning. Beyond this commonly accepted definition, transfer has different meanings in different contexts.

To many classroom teachers, transfer of training means the achievement of an important set of objectives, i.e., a dependent variable. No student can practice all responses to all stimuli. He can acquire knowledge useful in two ways. His knowledge should help him learn more advanced subjects in the same or related fields; and it should help him to solve a broad range of practical problems. These two different types of transfer, savings and applicational transfer, need to be distinguished from each other.

Transfer is a phenomenon of interest to the learning psychologist.

Beginning his chapter on transfer in his text on the psychology of learning, Deese writes, "There is no more important topic in the whole of the psychology of learning than transfer of training." (Deese, 1958, p. 213.) To learning psychologists, research on transfer involves the study of how classes of behavior, rather than specific behavior, are acquired. Transfer is indexed by performance on tasks different from the learning tasks. Transfer is considered either a process, an intervening variable, or a product, a dependent variable. To some *S-R* psychologists, transfer is primarily a result of the processes of stimulus generalization and discrimination and response generalization and differentiation. Mediation theory emphasizes acquired verbal generalizations and discriminations as well as primary generalizations and discriminations. In the Gagné model, lateral and vertical transfer are mediated by hierarchically ordered learning sets.

In this chapter, transfer is considered a product of learning processes such as operant conditioning, verbal mediation, discrimination learning, concept learning, etc. Major consideration will be given to verbal transfer, which is mediated by verbal associations, often hierarchically ordered verbal associations.

Verbal transfer is assumed to be a product of learning, not a process different from learning. By examining the products of learning and the conditions which produced the learning, we should be able to predict much about verbal transfer. We assume that such transfer occurs primarily because learned associations, mediators, rules, and verbal hierarchies are being applied to the classes of stimuli present during the transfer task. Transfer is not "glued on" after learning. It is a product of learning and of the conditions of learning.

In educational research, one meaning of transfer has often been overlooked. The study of transfer should include not only transfer from earlier instruction to later instruction, but also the transfer to the instructional situation mediated by the learner's history, general ability, and cognitive style. Research on individual differences is an important, but usually neglected, area in the study of transfer in school learning. In this chapter we will not discuss in detail these important individual difference variables. Instead, we will hope that educational research will begin intensively to investigate them. Gagné's model of hierarchical learning sets gives some explicit attention to them, as we shall see later. His model provides a basis for studying individual differences in learning sets and in cognitive styles. But the basic research upon individual differences in response to educational treatments has yet to be performed.

Designs and Formulas

Formulas. The designs and formulas commonly used in transfer studies are themselves valuable in studies on education. But a detailed discussion of designs and formulas is not central to this chapter. A summary of methods of measurements of transfer, including a discussion of transfer formulas, is given by Ellis (1965, pp. 9-14). A discussion of measurement of transfer of training, particularly of the formulas to describe percent of transfer, may be found in Gagné, Foster, and Crowley (1948), in Murdock (1957), and in Woodworth and Schlosberg (1954, pp. 735-38).

Designs. Perhaps the most common of the non-analytic designs (those which do not analyze experimental conditions into stimuli and responses) in studies of transfer is Design 1 in Table 4.1. In this design the experimental group learns the original task, task A, while the control group rests. Then both groups learn the transfer task, task B. The differences between the two groups in the learning of task B can include non-specific transfer such as warm-up and learning to learn.

TABLE 4.1

Non-Analytic Transfer Designs

Design	Group	Learning	Test
1	Experimental	A	B
	Control	rest	B
2	Experimental	A	B_1
	Control	A	B
3	Experimental	A	B
	Control	B	A

To control for these differences, one can use Design 2 of Table 4.1, in which both experimental and control groups learn task A first. The experimental group then learns task B_1 while the control group learns B. Assuming that B and B_1 are equivalent to each other, the difference between the experimental group score and the control group score on the *B* tasks should represent differences in transfer due to features of task *A*, but not to warm-up or learning to learn. Design 3 is useful when one wishes to determine how transfer from *A* to *B* might differ from transfer from *B* to *A*. In this design, the experimental group learns *A* followed by *B*, while the control group learns *B* followed by *A*.

Although they are not usually considered transfer designs, the three-stage and four-stage mediational paradigms (e.g., learn *A-B*, learn *B-C*, and test *A-C*) are properly analytic designs for studying transfer. In each paradigm, transfer to subsequent learning is predicted as a function of prior learning, even though the earlier learning may involve two or three separate stages. See Jenkins (1963) for a discussion of three-stage and four-stage mediational paradigms.

A SIMPLISTIC *S-R* APPROACH TO TRANSFER

The influence of simplistic *S-R* models upon the study of transfer in school learning will be considered here. No attempt is made to review the literature. Instead, studies are described to characterize the utility and limitations of a simplistic *S-R* approach, and to summarize its principal implications for the study of the conditions of transfer in school learning.

The faculty psychologists' belief in formal discipline was rightly questioned by William James, who believed that retentiveness was not susceptible to training. But it was neither James's work on the effects of practice upon memorizing poetry, nor Judd's (1908) work on preliminary training with principles that devastated the theory of formal discipline. Instead, the utility of that theory was destroyed by the analytic approach of the *S-R* psychologists, who insisted upon specifying the stimulus and response variables involved in the study of transfer.

In a series of experiments, Thorndike and Woodworth (1901) used an *S-R* approach to question the position of the faculty psychologists. Later Yum (1931), McGeoch (1931), McKinney (1933), and Bruce (1933) advanced knowledge about the fundamentals of transfer. The classic study by Bruce (1933) on the conditions of transfer of training exemplifies how an *S-R* approach to the paired-associates learning of nonsense syllables can be used to identify the conditions under which positive or negative transfer occurs.

In Bruce's study, each of the subjects learned an initial list of paired-associates, nonsense syllables (S_1 - R_1) followed by a transfer list of nonsense syllables (S_2 - R_2). The nine experimental conditions were designed to vary the relations among the S and R terms from the first list to the second list. For example, in condition 1, S_1 and S_2 were identical, but R_1 and R_2 differed from each other and also from S_1 and S_2. One of the S_1 - R_1 pairs was "req-kiv," and the S_2 - R_2 pair was "req-zam." In addition to varying stimulus similarity and response similarity, Bruce also varied degree of learning of the original list from zero trials to twelve trials. He found that learning to pair an old response term with a new stimulus term regularly produces positive transfer, but that learning to pair a new response term with an old stimulus term usually produces some negative transfer. Both these statements were true across the different degrees of learning of the original list. He also found that the learning of the second list was usually easier in direct proportion to the degree of learning of the original list. This was true whether one was learning to make new responses to an old stimulus or an old response to a new stimulus.

In this study of paired-associates learning, Bruce established fundamental S-R relationships of stimulus similarity and response similarity still largely accepted today in research on transfer. Others after him gave a more complete picture of how transfer was related to stimulus similarity.

Eleanor Gibson (1941) worked with nonsense figures as stimuli and nonsense syllables as responses. Her results showed that when the responses remain the same, positive transfer increases as the stimuli become more and more alike. However, increasing the stimulus similarity was found to reduce transfer when the responses are different from each other. When the stimuli are identical and the responses are different, negative transfer was obtained.

Theory relevant to transfer was developing also. Eleanor Gibson introduced her famous discrimination hypothesis, asserting that, "A major necessity of verbal learning is the establishment of *discrimination* among the items to be learned, and that this process of discriminating is actually a fundamental part of what is called generally the learning process" (Gibson, 1940, p. 197). According to this hypothesis, when different responses are to be learned to different stimuli, the stimuli must be differentiated from each other. The more similar the stimuli are to each other, the greater the need for differentiation among them. As will be seen from the studies on stimulus predifferentiation to be described next, this hypothesis stimulated an impressive amount of productive research on transfer.

The effect of response similarity upon transfer is a complex and not thoroughly understood issue. Bruce's earlier finding, that negative transfer is produced when different responses must be made to the same stimuli, was incorporated by Osgood into his diagram of transfer and retroaction. He assumed that an increasing amount of negative transfer should be produced when subjects learn to make increasingly different responses to the same stimuli. However, this has not always been shown to be the case (Bugelski & Cadwallader, 1956). As we shall see when we consider mediation theory, when the stimuli are identical, a shift from a response to its opposite often produces positive transfer. Perhaps in verbal transfer the concept of opposite responses is deceptive. Antonyms are usually strong associates of each other and are not "opposite" in a simple sense of the term.

Stimulus Predifferentiation

Stimulus predifferentiation is one of the areas in which the *S-R* approach has been productive of knowledge about the conditions that increase transfer. Although a few studies on stimulus predifferentiation indicate no transfer or even negative transfer, an overwhelming number indicate positive transfer. In these studies, the usual procedure is to provide the experimental groups with preliminary activity with the stimuli, without giving them practice with responses identical with or similar to those used during the transfer task. The four major types of stimulus predifferentiation training may be categorized as *Relevant S-R*, *Relevant S*, *Irrelevant S*, and *Attention*. In the area of *Relevant S* pretraining, there are interesting differences in theoretical interpretations of the results. A number of these studies have been inspired by Gibson's discrimination hypothesis.

In *Relevant S* pretraining, the subject is exposed to the same types of stimuli he will encounter on the transfer task. But the type of responses he makes during the pretraining will neither be symbolically nor physically like the responses of the transfer task. For example, if the transfer task involves his moving a lever to the right or to the left, the *Relevant S* pretraining task would involve a verbal response on an irrelevant dimension such as animal-vegetable. *Relevant S-R* pretraining is like the *Relevant S* pretraining except that the verbal response is directly relevant to the motor response on the transfer task. In the above example, the verbal response would have been right-left. In *Irrelevant S* pretraining, not only the response is irrelevant but also the stimulus discrimination is irrelevant. In *Attention* pretraining, although the stimulus dimension is relevant, no overt response is required from the subject.

Gagné and his collaborators conducted a series of studies on stimulus predifferentiation (Gagné & Baker, 1950; Gagné, Baker & Foster, 1950a; Gagné, Baker & Foster, 1950b; and Gagné & Foster, 1949). In one study (Gagné & Baker, 1950), he used Gibson's discrimination hypothesis to predict the transfer produced by different numbers of trials of predifferentiation training. Four groups of college students were given, respectively, 0, 8, 16, or 32 trials of associating four letters of the alphabet with one of four different lights on a panel. The transfer task involved a manual response, operating switches, to these same four light stimuli. On both response-time scores and error scores, the group given thirty-two trials of preliminary training on stimulus predifferentiation showed by far the greatest positive transfer. Gagné explained the results in terms of Gibson's discrimination hypothesis, proposing that predifferentiation training reduced the amount of generalization across the stimuli on the transfer task and thereby increased transfer. In another study (Gagné and Foster, 1949), transfer was investigated as a function of *Relevant S-R* pretraining upon a component of the transfer task. During preliminary training, the experimental groups received different numbers of trials on two of the four switches which would appear on the transfer task. The four stimuli were red and green, upper and lower lights. During preliminary training the subjects learned to respond only to one of these two dimensions. Positive transfer to the total task was found to increase with the number of preliminary training trials. Transfer was also increased in accordance with the degree of learning of one of the components of the original task.

A second study on component pretraining (Gagné, Baker & Foster, 1950b) investigated the specificity of the component training. The question was posed, would the component training on one part of the task produce a reduction in errors and also a reduction in response time for the parts of the total transfer task not included in the preliminary training? The interesting finding was that with color and position discriminations, training on the more difficult discrimination, *color*, generalized to reduce the errors and time scores on the less difficult discrimination, *position*. However, training on the less difficult discrimination, *position*, produced no reduction in errors or time scores on the more difficult discrimination, *color*. Transfer occurred from practice on one type of problem to a more complicated problem. It would be interesting to follow up this study to find other conditions which will produce positive transfer to untrained dimensions in discrimination problems.

In a study with children, Cantor (1955) compared the effects of *Relevant S-R*, *Relevant S*, and *Attention* predifferentiation training. In the *Relevant S* condition, during the preliminary training, the children learned the names "Jean" and "Peg" for the two drawings of girls' faces. In the *Relevant S* group, the children learned the names "Jack" and "Pete" for the two pictures of the girls' faces. In the *Attention* group, the children looked at the pictures of the girls' faces and pointed to features of the faces. The transfer task involved choosing which of the two wooden cars, each with one of the two girls' pictures on it, was the correct car to roll down a track. The *Relevant S-R* pretraining group was superior to either the *Relevant S* pretraining group or the *Attention* pretraining group. The difference between the means of the *Attention* group and the *Relevant* group was not statistically significant. Cantor did not attempt to account for his results in terms of either Gibson's differentiation hypothesis, or of the hypothesis of acquired distinctiveness of cues. Whatever the reason, the subjects in the *Relevant S-R* pretraining group did exhibit transfer in learning to differentiate between the two pictures of girls' faces.

Studies on stimulus predifferentiation have not always produced positive effects. Ellis and Muller (1964) noted that often when the transfer task is a perceptual one, involving judgmental responses such as "same-different" rather than a learning task requiring the association of new stimuli and responses, no positive transfer occurs. However, in most of the studies involving transfer to a learning task, positive transfer from labeling does occur. Ellis and Muller gave their subjects training at either labeling random shapes or at simple exposure to the shapes with instructions to observe and to discriminate among them. For one transfer task, some of the subjects were shown a variety of random shapes and were asked to recognize those they had seen. Other subjects were given a transfer task involving learning to press one of eight switches for each of the shapes used during pretraining. The observation group did better than the labeling group on the recognition task, but the labeling group out-performed the observation group on the learning task. As these investigators hypothesized, the type of task and the type of training interact. Training in labeling apparently will transfer to tasks requiring motor responses. But observation training transfers better than labeling training to a recognition task.

Ellis has conducted additional studies on stimulus predifferentiation (Ellis & Feuge, 1966; and Ellis, Muller & Tosti, 1966). The latter study, on stimulus meaning and complexity in transfer of stimulus predifferentiation, is directly relevant to this discussion. The subjects were given paired-associates practice during predifferentiation training. They labeled shapes and then took a shape recognition test. In this perceptual task, meaning facilitated recognition, at least when meaning was defined as associative value or associative constancy. Associative value and associative constancy refer to the strength of only a single association. When meaning was defined as an associative frequency, however, it was not found to affect the recognition of shape. Associative frequency was measured by averaging the number of available associations to the stimuli. If stimulus predifferentiation training is effective because it enhances the meaningfulness of a stimulus by adding associations to that stimulus, it is important to know the pre-experimental associative properties as well as the experimentally introduced properties of the stimulus.

Johnson (1964 and 1965) studied problems of gathering data on the associative meaning of words and concepts used in high school physics. Studies on transfer and learning in the schools need to give attention to this individual-difference variable of associative meaning.

The implications of these studies for educational research on transfer are several. First, the type of differentiation training interacts with the transfer task, as discussed above. Second, stimulus predifferentiation treatments are often effective in increasing positive transfer. Third, the *S-R* conceptual approach can be applied to at least some complex instructional problems. This third point is discussed further below.

Response Differentiation

The *S-R* approach to transfer of training has not been as thoroughly or as extensively applied to the study of response differentiation as it has to the study of stimulus predifferentiation. Suffice it to say that in contrast to Bruce's finding previously mentioned, one does not always obtain negative transfer when a second and different response is learned to the same stimulus (Underwood, 1951; Arnoult, 1957). When the transfer list re-pairs the same responses to the same stimuli used in the first list, negative transfer usually does result.

Learning Set

Learning set may be distinguished from learning to learn, or discrimination set. Learning set involves transfer more specific than the transfer observed in learning to learn. Learning set has been found after the learners have solved a large number of simultaneous discrimination problems involving two dimensions of stimuli (Harlow, 1949). When in one trial the subject can solve problems involving these same dimensions of stimuli, he is said to have acquired a learning set.

The literature on discrimination learning set is reviewed in a chapter by Reese (1963). Learning set has been established with animals and with children, e. g., Harlow's (1949) work with monkeys and with humans.

Learning to Learn

Learning to learn involves a broader type of transfer to dimensions outside the specific ones used in the training stimuli. A subject with a discrimination set, or one who has "learned to learn," will not necessarily solve each new problem in one trial. Learning to learn generally includes observation and attention to verbal instructions. For example, learning to learn might produce transfer from a paired-associates task to a serial learning task, a broad type of transfer not characteristic of a learning set.

Although not as well understood as learning set, learning to learn is a highly interesting part of the study of transfer of training. As a part of a series of studies on learning to learn, Keppel and Postman (1966) report one of the most informative studies in this area. In an earlier study, Postman (1964) had found that with transfer paradigms, such as "A — B, first, then A — C"; or "A — B first and then A — B_1," subjects improved their performance on subsequent tasks involving these same paradigms. In the later study Keppel and Postman (1966) wanted to find if the learned skills were specific to each paradigm or if the skill generalized to more than one paradigm. They trained subjects on one paradigm, and yet found transfer to a second paradigm. The acquired skill was general. This is an important finding, and it helps to put learning to learn on substantial ground.

One wonders if practice improves not only transfer to different paired-associates paradigms, but perhaps also improves applicational transfer to concept learning involving the same basic paradigms. At any rate, Keppel and Postman were able to obtain broad transfer without priming verbal mediators.

The *S-R* Approach Applied to Classroom Instruction

Can a simplistic *S-R* approach, developed largely in the psychology laboratories, be effectively applied to the study of transfer produced by classroom instruction? A simple *S-R* model has been so applied in some instances. In a series of five experiments, Keislar and Mace (1965, pp. 163-91) studied two different sequences of speaking training and listening training in the teaching of beginning French. They described the training of listening and the training of speaking respectively as stimulus discrimination and response differentiation.

In the first two of the five experiments they found that one type of *Relevant S* pretraining transferred to reading a list of French words. During this pretraining, the subjects read the printed French words and circled them when they heard them spoken aloud in French. A different kind of *Relevant S* pretraining required the subjects only to distinguish French and English words. This pretraining was not effective on the transfer task. These findings are consonant with the Ellis and Muller (1964) results mentioned above. Recognition training usually doesn't transfer to later learning tasks, whereas labeling training does transfer to learning tasks.

In three of the five experiments, Keislar and Mace studied the effects of variations in order of listening and speaking training. They found that response differentiation training, that is, speaking training followed by listening training, transferred to listening comprehension. This finding agrees with the notion, in paired-associates learning, that the subject's task consists first of differentiating responses and then of associating responses to stimuli. In these studies an analytic *S-R* approach was effectively applied to the study of problems of transfer in school learning.

In reference to a more complex classroom situation (Stolurow & Bergum, 1958; Stolurow, Hodgson & Silva, 1956), the analysis of a military training course has been described. The subject matter of a course on the mechanics of aircraft engines was divided into stimuli and responses. These were the cause and symptom relationships in the malfunctions of aircraft engines. Three association paradigms were used to describe the stimulus-response relationships encountered by the trainees: reponse sharing, where several responses are associated to one stimulus; stimulus sharing, where several stimuli are associated to one response; and stimulus-response sharing, where many stimuli and many responses are associated in either or both forward and backward directions. For example, the symptom "low power" might be caused by either a broken valve spring or a loose intake pipe coupling. When viewed in the other direction, a cause, such as a broken valve spring, could lead to several symptoms such as surging, low power, and detonation. The effects of stimulus learning, response learning, and association reversal were then investigated. Association reversal involved learning causes first and symptoms later, followed in turn by transfer to the practical problems of engine malfunctions, whose diagnosis involves beginning with the symptoms and responding with the probable causes.

Stolurow and Bergum (1958) used an analytic version of transfer design No. 3 (Table 4.1). One group of subjects learned cause-symptom relationships first, followed by symptom-cause relationships. A second group learned the same two tasks in reverse order. These investigators also studied prose versus paired-associates styles of presenting information. They predicted and found the direction of association to vary with the complexity of terms. Learning was faster when the more complex terms were in the stimulus position, rather than in the response position. This meant that when the causes, the more complex terms, were in the stimulus position, cause-to-symptom learning was easier than symptom-to-cause learning.

Secondly, the paired-associates style produced learning better than did the prose style. But these two primary effects are difficult to interpret, because the experimenters also found an interaction between them. Paired-associates learning combined with symptom-to-cause learning, and prose learning combined with cause-to-symptom learning, were the best two combinations. This interaction was attributed to the pre-experimental history of the subjects, who had been exposed to months of training on aircraft engines. Their habits probably determined the most effective instructional sequences of the experiment. From the data on association reversal, it was also shown that the amount of positive transfer was related to the degree of learning of the first task.

This study by Stolurow and Bergum indicates that even in lengthy classroom instructional sequences, an *S-R* analysis can be applied to advantage. When the analysis includes "many-to-one" and "many-to-many" associations, it may apply to a variety of complex instructional sequences.

Comments on the *S-R* Model

This sample of studies on transfer of training indicates how a simplistic *S-R* approach can be used to identify conditions of transfer. The analysis of events into operationally defined stimuli and responses is a strength of this type of *S-R* model. The emphases on stimulus generalization and response differentiation have been productive of knowledge in paired-associates learning, discrimination learning, and in complex classroom learning and transfer. Research on classroom learning and transfer has profited from careful analyses of the stimulus-response relationships in complicated instructional sequences. Following the leads of Stolurow and Keislar, we should encourage further analyses of classroom learning into the stimuli and responses which affect transfer. Especially do the many-to-one, one-to-many, and many-to-many associational paradigms used by Stolurow seem well suited for *S-R* analyses of problems of transfer in classroom learning.

A simplistic *S-R* approach has shown some limitations, too. The definition of stimulus similarity and the meaning of "opposite" responses are not clear. These concepts probably have little value for educational research.

If a researcher in education chooses to adopt a simple *S-R* approach to the study of transfer, and if he follows the interests described above, one can expect his work to center on certain types of problems. He is likely to study specific transfer. To a lesser degree, he is likely to study learning set and learning to learn. Learning to learn has usually been approached as an event to control or to counterbalance, rather than as an interesting phenomenon in its own right. One notable exception is the recent work by Keppel and Postman (1966).

It may also be expected that some researchers will not be particularly interested in studying methods of priming verbally mediated transfer. One wonders what might have happened if Keppel and Postman's subjects had been given instructions about different types of paradigms; or if they had been given reinforcement for discovering and verbalizing the paradigmatic relationships used in the study.

If we may generalize from the past, our hypothetical *S-R* researchers studying transfer in education are not likely to try to increase transfer by use of verbal cues and verbal sequences that make explicit the higher order relationships, such as the paradigms used in paired-associates studies. We can probably expect these same hypothetical researchers to be less interested than, let us say, a mediation theorist would be, in the study of hierarchical relationships among concepts and rules. We can expect some of the *S-R* researchers on transfer in education to be more concerned about the learning of one-to-one associations than they are about the learning of many-to-many associations as ways to produce transfer.

But we can expect a simplistic *S-R* model to lead to rigorous work upon transfer, with a high probability that some of the conditions important for producing transfer will be identified. The *S-R* model of transfer is broad, and allows one opportunity to do creative research upon the complex processes of verbal hierarchies, concepts, observing and orienting behavior, learning to learn, and learning set. Even if it has not often been used to study them, the model applies to these complex processes and to the phenomena of stimulus generalization and response differentiation. Educational research on transfer could greatly profit from an analytic, simplistic *S-R* approach, provided we remember that the complex phenomena of education may not always be reducible to simple *S*'s and simple *R*'s.

A MEDIATED GENERALIZATION APPROACH TO TRANSFER

There is no one simplistic *S-R* model, nor is there any one mediated generalization model. By listing mediated generalization separately from the *S-R* approach described above, there is no intention to imply that mediated generalization is not an *S-R* approach itself, nor that many of the researchers listed in the above section do not at times use a mediated generalization model. Instead, the implication is that mediated generalization *S-R* models offer a distinctiveness and a utility that warrant their separate consideration here.

One can frequently read articles in the field of human learning suggesting that mediated generalization approaches are either completely reducible to simple *S-R* models, or that the mediation phenomenon is not firmly established or "real." Whether or not the phenomenon is "real," the hypothesis leads one to study problems of transfer often given scant attention by researchers adopting a one-step, *S-R* model. These important problems include verbally mediated transfer and secondary or acquired generalizations and discriminations. The mediated generalization approaches also allow one to apply such important concepts as reinforcement and contiguity to the learning of verbal mediators.

Although not limited to verbally mediated transfer, mediated generalization models are useful ways to conceptualize verbally mediated transfer. The terms "mediated generalization" and "mediated verbal transfer" are frequently used synonymously in the literature. One of the influential thinkers in the field of mediated generalization, Charles Osgood, has written (1961, p. 94), "A significant experimental literature is rapidly accumulating on what is usually called 'mediated generalization' but which, . . . I think should be termed *mediated verbal transfer*."

The Meaning of the Term "Mediated Generalization"

Words and other verbal stimuli are used to make distinctions and generalizations not readily apparent to the senses. For example, with the proper words one can teach a child to treat as equivalent a stack of four quarters and a dollar bill. Words can mediate a transfer from familiar objects to unfamiliar objects.

Verbal distinctions, such as the distinction between liberal and conservatives, can mediate discriminations among people who might otherwise be grouped together on bases such as physical similarities. Although their difference in number might not be distinguishable to the eye, a pile of 49 pennies and a separate pile of 50 pennies will be responded to differently in some situations after the different meanings of the words 49¢ and 50¢ are each attached to one of the two piles of pennies. The verbal mediator affects the response made to the stimulus.

In the laboratory, the phenomena of verbally mediated transfer have been observed in several ways. In a typical study on semantic generalization, a subject is taught to make an arbitrary response to a familiar word. He might learn to respond with the nonsense syllable "kev" to the English word "happy." After he had learned this association, he might then be given the word "joy," a transfer item synonymous with "happy." Assuming he had no association between "joy" and "kev" before the experiment began, if he now tends to respond to the word "joy" as he was taught to respond to the word "happy," this constitutes an example of a verbally mediated generalization; in this case, a semantic generalization.

A four-stage paradigm of mediated generalization summarizes the above example. The acquired stimulus equivalence is predicted on a test of *C-D* given after the subject has learned *A-B*, *C-B*, and *A-D*. In the above example, the subject pre-experimentally learns *A-B* and *C-B*, and then is taught *A-D*. On a later transfer test over *C-D*, a mediation effect should occur because the *B* term gives the *A* and *C* stimuli an acquired equivalence.

As we shall see below, mediated generalization frequently does seem to occur. But sometimes it does not occur, perhaps because the experimental situation does not provide the opportunity for the subject to generate or to use verbal mediators.

Research on Verbally Mediated Transfer

In describing how the mediated generalization *S-R* approach to research differs from the simplistic *S-R* approach discussed above, it will not be necessary to discuss again the types of studies listed above under the simplistic *S-R* approach. Some of these same studies could have been categorized under mediated generalization. For example, the Gagné and Baker (1950) study, which involved associating letters to four different stimuli, is an example of verbally mediated discrimination. Since these authors did not explicitly use a mediated discrimination model, their studies were described under the simplistic *S-R* approach to transfer.

To show how a mediated generalization approach has been used to study transfer, two lines of research on child learning will be described. In the first series, the Kendlers emphasize the development of verbal mediators with age. In the second series, Wittrock and others study conditions for teaching verbal mediators and verbal rules, especially hierarchically ordered sets of verbal mediators.

Development of Verbal Mediators. In the well-known studies on reversal and non-reversal shifts (e.g., Kendler & Kendler, 1959; Kendler & Kendler, 1962), rats, children, and adults have all been exposed to problems involving discriminating stimuli that vary in brightness, black or white, and in size, large or small. The fundamental issue is as follows: What happens after a subject has learned to respond regularly to one of the qualities of the stimuli, for example, black, if the experimenter then shifts the reinforcement from black to the other instances of brightness, in this case white? Will the subject make the shift on the dimension of brightness (from black to white), a reversal shift, more readily than he will make a shift across dimensions (from the response black to the response large), a non-reversal shift? The interesting answer is that rats and young children make the non-reversal shifts more readily than they make the reversal shifts. On the other hand, older children and adults make the reversal shifts more readily than they make the non-reversal shifts. Although other experimenters explain this phenomenon in different ways, the Kendlers interpret their data in the light of the fact that the adults and older children have learned verbal concepts such as brightness and size. These concepts mediate transfer to the discrimination problems. For adults and older children, a non-reversal shift should be more difficult than a reversal shift because it involves not only a change of a particular response, such as from black to large, but also the change in the mediated response from brightness to size. These subjects can make use of the concept relating white to black as the "opposite" of what has been learned. But young children and rats have not learned such concepts. For the younger children and for the rats, the non-reversal shift should be easier than the reversal shift because the change from black to large does not involve the extinction and relearning characteristic of the change from black to white. The studies by the Kendlers exemplify how the concepts of practice, contiguity, and reinforcement are applied to hypothetical mediating stimuli and responses to make useful predictions about transfer.

The Conditions of Verbal Mediated Transfer. Wittrock has studied some of the fundamental types of conditions to induce subjects, usually young children, to transfer. In these studies, the subjects were randomly assigned to the treatments, and in each study with young children, an extensive period of preliminary training in associating words into hierarchies was given. This was done to gain some control over the associations to the words and sentences the experimenter would use to prime the children's verbal mediation assumed to occur during the learning task. The studies apply but do not test a mediated generalization model.

In one investigation (Wittrock, 1963a), instructions to generalize or to discriminate between Christianity and Buddhism were given to college subjects before they learned a passage on Buddhism written by Ausubel, and used in several of his studies (e.g., Ausubel & Blake, 1958). In line with Gibson's discrimination hypothesis, it was predicted that instructions which emphasized discriminating between the two religions would produce better learning. Further, the better the subject's score on Christianity, the better would be his ability to discriminate between these two positions. Compared with a control group told simply to learn and understand Buddhism, the groups given the instructions to discriminate or to learn both the similarities and differences between the two positions performed the best on the test of Buddhism. Transfer to the learning of Buddhism was affected by knowledge of Christianity and by the instructions. Positive transfer from Christianity to Buddhism was obtained by instructing the subjects to develop their own rules for discriminating between the concepts of the two religions.

In a series of studies on deciphering cryptograms, Wittrock investigated how rules could be used by an experimenter to influence transfer of problem solving. The rules, called the verbal cues, emphasized either the particular answers to cryptograms, the rules appropriate to solving a class of cryptograms, or the distinctions between transpositional and substitutional cryptograms. There was a hierarchy of cues or rules from *most general*, those having to do with the two classes of cryptograms, to *less general* sentences, those having to do with the particular rules for subclasses of transpositional or substitutional cryptograms, and to the *least general* sentences, those having to do with the particular answers to each problem. A "generality hypothesis" was introduced to predict the results. The generality of verbal transfer was predicted to be directly proportional to the generality of the cue or rule used by the experimenter, provided the necessary prerequisite associations to the rule were learned by the subjects. Incidentally, this hypothesis was also used to explain the often conflicting results of studies of learning by discovery. That is, it was hypothesized that in those instances in which discovery was mediated by verbal transfer, the more general the rules and concepts the greater would be the breadth of transfer.

In one study in this series (Wittrock, 1963b), college subjects learned to solve transpositional cryptograms. Some of these subjects were given the rule relevant to solving a class of transpositional cryptograms. Other subjects were given only the answer to each cryptogram. Another group of subjects was given both the rules and the answers relevant to the solution of each cryptogram. A fourth group was given the same problems with neither the rules nor the answers for any problem. The transfer task involved two types of problems: (1) cryptogram problems involving applying the same rules used during learning and (2) transfer problems involving transpositional rules not encountered during learning periods. By hypothesis, the prediction was made that the groups given the particular answers would do best on the simple learning test involving the same types of problems deciphered earlier; but the groups given the rules were predicted to perform best later on transfer problems involving applying those same rules. On remote transfer, problems involving new rules, the groups given neither rules nor answers, but reinforcement for deriving their own rules were hypothesized to perform the best. With one major exception, the data were in close agreement with the hypotheses. The learning and near-transfer test results were as predicted. The learned rules mediated transfer to new examples of the rules given on a transfer test. However, on transfer to new rules, the group given neither the rules nor the answers during learning did not perform the best of any of the four groups. Instead, the groups given the rules during learning continued to perform best on the remote transfer test. Perhaps this was the case because the group given neither rules nor answers achieved little; they obtained very few correct answers during learning. However, their mean score on the remote transfer test was considerably higher than their mean score on the learning and near-transfer tests. The results of this study emphasize the transfer value of priming verbal mediators. By making the rules explicit and by providing practice for applying these rules, transfer was mediated to other problems exemplifying the rules.

In another series of experiments (Wittrock, Keislar & Stern, 1964; Wittrock & Keislar, 1965; Wittrock, 1967), the conditions of verbally mediated transfer have been investigated with young children in the learning of French vocabulary, English grammar, and discrimination problems involving color, size, shape, and number. In the first study (Wittrock, Keislar & Stern, 1964), the object was to find if the transfer mediated by the hierarchically ordered verbal cues would be as predicted by the generality hypothesis.

For example, to mediate transfer to problems involving matching pictures of common objects, but where the matching was done on the basis of the gender of the French names for the objects, the question posed was which type of verbal cue would be most effective—a general rule, a cue for the relevant concept, or a more specific one. Each child was asked to match one of two lower pictures to the top picture on a systematic basis that did not change throughout the entire series of fifteen sets of pictures which comprised a problem. Reinforcement was always given for choosing the lower picture with a French name of the same gender as the gender of the French name of the top picture.

From the generality hypothesis, the prediction was made that the most general cue, the word "article," should mediate transfer across a greater variety of problems than would the more particular cues. However, the more particular cues should produce the greatest learning. The cue most effective on the transfer test should be the one which agrees most closely with the concepts sampled on that test.

Experimental training began after two months of preliminary training with the difficult discriminations betweeen the words "*la*" and "*le*," and with the learning of the associations in the hierarchy. During the experimental training, one group of children was repeatedly given the most general cue, "article," while the second group of children was repeatedly given the cue "*la*" or the cue "*le*," and the third group of children was given each correct answer, such as "*la clef*," as they solved each problem. A fourth group was given none of these cues, but was told that there was a basis for finding the correct answers. They were asked to press one of the two buttons before them to tell them which of the two bottom pictures was the correct match.

The transfer tests were then given without any verbal cues from the experimenter. The transfer tests used objects not previously presented during the experimental training. These objects and their French names had been associated in preliminary training. As we see in Table 4.2, on four different tests—two transfer tests, a retention test, and a learning test—the group given the "*la*" or "*le*" cues during experimental training performed the best ($p < .01$) by Newman-Keuls multiple comparison tests. Differences among the other means were not statistically significant on any test. These results supported one part of the generality hypothesis. The verbal cue most appropriate for the breadth of transfer measured on the transfer test was effective for inducing transfer. The most specific cue did not produce the highest mean score on the learning test. In this experiment, there was no way to test remote transfer.

In two later experiments with young children, remote transfer was investigated. Although the results on learning and transfer within the previously learned concept agreed closely in these latter two experiments with the results described previously, no effect on

TABLE 4.2

MEANS AND STANDARD DEVIATIONS OF THE TREATMENT GROUP SCORES

Treatment Groups	Preliminary Score					Post-test Score			
						Immediate		Three Weeks Later	
	N (39)	IQ	MA	CA	Preliminary Training (48 items)	Problem Solving[a] (30 items)	Transfer[b] (42 items)	Retention[b] (24 items)	Transfer[b] (24 items)
Press a button	10	103.0	69.5	65.4	39.4	17.8	24.4	15.9	14.5
		7.6	8.5	3.5	4.3	5.1	5.7	5.0	4.0
Article (general cue)	11	103.2	69.3	64.2	42.5	19.8	24.9	14.0	12.8
		13.2	12.0	3.8	4.9	5.2	6.5	4.5	5.6
La (*le*) (class cue)	9	107.9	73.3	64.1	42.9	24.1	33.7[c]	19.1[c]	18.7[c]
		11.4	14.2	4.1	5.8	3.7	4.0	3.4	4.0
French name (specific cue)	9	112.0	76.8	65.6	39.4	19.1	23.0	12.1	11.7
		15.5	11.3	4.7	5.3	4.9	3.8	3.1	3.1

NOTE—Adapted from M. C. Wittrock, E. R. Keislar, & Carolyn Stern, 1964.

[a]Differences among the means significant ($p < .05$) by analysis of variance.

[b]Differences among the means significant ($p < .01$) by analysis of variance.

[c]By specific comparison test, greater than the average of the other three means ($p < .01$).

remote transfer was obtained by any of the cues, including the specific and highly general cues.

In a recently completed study (Wittrock, 1967) remote transfer was obtained. To obtain the remote transfer, something more than a series of repetitions of single words such as "*la*" or "*le*" or other names for classes of responses was required. Remote transfer was obtained after second-graders had been taught to discriminate abstract objects on the basis of their color, size, shape, and number. A type of rule was taught to prime each child. One type of rule was called a non-replacement strategy. The child was instructed to try, as the basis for matching top and bottom objects, first the concept color, then size, then shape, etc. until he had exhausted the four possibilities for each problem or until he found the correct basis for matching. In the non-replacement strategy, each child was given four cards, one for color, one for size, etc. He was instructed to turn face down each card found not to be a correct basis for matching top and bottom pictures. In the replacement strategy, the procedure was identical to that described above for the non-replacement strategy, except that children were not taught to turn face down the cards representing the incorrect concepts.

The tests measured learning, near and remote transfer. The items of the test of learning sampled the same colors, sizes, shapes, and numbers used during the experimental training. The items of the two tests of near transfer were also made from the concepts *color*, *size*, *shape*, and *number;* but the colors and shapes were different from those used during the experimental training. The difficult remote transfer test involved matching top and bottom pictures on the basis of concepts other than color, size, shape, and number. The concepts were borders, dotted or solid; position of the apex of a triangle in relation to a bar; direction of the apex of a triangle, up or down; or texture of the interior of a triangle, dotted or striped.

Compared with the replacement procedure or the control procedure, the non-replacement procedure not only produced better performance ($p < .01$) on the near transfer tests, but also produced better transfer ($p < .01$) to concepts other than color, size, shape, and number. The difficult test of remote transfer had been used to measure remote transfer in several other studies (e.g. Wittrock & Keislar, 1965). But in none of these other studies did any treatment ever produce an effect on this test of transfer to new concepts.

Apparently, children may be able to use rules and strategies to transfer to problems which do not involve the particular concepts

TABLE 4.3
MEANS AND STANDARD DEVIATIONS OF THE POST-TEST SCORES

Treatment Groups		Learning (with cards) 40 Items	Transfer to New Instances (with cards) 40 Items	Transfer to New Instances (without cards) 40 Items	Transfer to New Concepts (without cards) 25 Items
School I					
Non-replacement Group		29.4	29.6	30.1	17.8
	$N = 20$	4.4	5.2	4.0	3.5
Replacement Group		25.1	24.9	26.4	16.9
	$N = 20$	6.3	7.1	7.6	5.5
Control Group		22.4	20.6	20.9	14.2
	$N = 19$	5.2	4.7	6.5	3.6
School II					
Non-replacement Group		25.7	25.0	27.7	16.1
	$N = 20$	8.5	8.5	6.5	5.0
Replacement Group		22.9	24.0	23.3	14.4
	$N = 20$	6.5	6.1	6.2	5.2
Control Group		21.7	20.9	20.1	12.8
	$N = 20$	7.2	8.0	6.4	4.7

NOTE—Adapted from M. C. Wittrock, 1967.

associated to them during training. Practice and reinforcement for applying verbal rules spoken by the experimenter is one way to obtain verbal transfer. There is some support for the notion that breadth of transfer and generality of the verbal cue are positively correlated. (See Table 4.3.)

In contrast to the views of the faculty psychologists, transfer is to be expected when a logical or a psychological basis is provided to mediate it. The faculty psychologists asked an important question when they wondered what conditions could mediate broad transfer. It is their answer to the question that is highly tenuous. If there are conditions which control remote transfer, they probably will involve something more than simply practicing the total behavior. One way to discover these conditions is to carefully analyze the components needed to mediate the transfer, and by a careful construction of hierarchically arranged sequences and treatments to teach the bases which the theory suggests are important for the transfer.

Comments on Mediated Generalization

The studies which have been described provide a brief sample of how mediated generalization models reflect a distinctive approach to the study of the conditions of transfer. Mediated generalization approaches differ from simplistic *S-R* approaches in several ways. They make the many-to-one, one-to-many, and many-to-many associational paradigms the basic models of learning and transfer. Mediated generalization models emphasize acquired generalizations and distinctions common in the study of transfer in schools. The models are useful for developing sequences of instruction that will progress in hierarchical fashion from the learner's history.

If our hypothetical young researchers in education were to entertain mediated generalization approaches to transfer, we might expect several effects upon their behavior. First, they are likely to view verbally mediated transfer as one of the most important problems in education. They are not likely to feel that if one wanted to test students on something else he should have taught them the something else at the beginning. Second, we can expect some of them to be interested in knowing or controlling individual differences in the subjects' histories of verbal associations. Third, they are likely to be interested in studying many-to-one relationships and many-to-many relationships in the problems of concept learning and problem solving.

Unfortunately, we cannot be sure that they will be as interested as their counterparts from simplistic *S-R* theory in establishing the conditions of verbal transfer. In its relatively brief history, mediation theory appears to be weak where its potential is strong, namely, in the study of the conditions of transfer.

To date, mediated generalization approaches have produced a disappointingly small number of studies upon the conditions of transfer. This is true especially if one remembers that the position is explicitly designed to handle the problems of verbally mediated transfer. Mediation theorists have devoted much energy to self-defense of their hypothesis and to justification of the loss of parsimony necessitated by introducing the concept of mediation into an otherwise simple, uncluttered *S-R* theory.

In the last several years, research on mediated generalization has begun to focus on the problems of identifying the conditions for producing verbally mediated transfer. In an excellent paper on "Mediated Association; Paradigms and Situation," Jenkins (1963) urges further interest in the conditions producing mediation rather than in the mediation paradigms themselves. Jenkins discusses the following type of problem. Why was Mink (1957), in an instrumental conditioning situation, unable to obtain transfer to an *A-C* test after giving the subject *A-B* and *B-C* training? And why was Razran (1949), in a respondent conditioning situation, able to obtain successful transfer with essentially the same paradigm? The situation was further compounded because Mink, again in an instrumental conditioning situation, was able to obtain successful transfer on a *B-C* test after giving *A-B* and *A-C* training while Razran, with the same paradigm but with a respondent task, was not able to obtain this sort of mediational effect.

After considering the differences between the respondent and operant tasks, Jenkins concludes that these differences probably are not the reason for the discrepancy between the findings of the two investigators. Instead, the more important factors include the instructions used by the experimenters, the reinforcement each gave to his subjects, the opportunity each provided to the subjects for engaging in verbal mediation during learning or testing, and also the opportunity each subject had to engage in associative arousal.

Jenkins argues for having subjects engage overtly and repeatedly in mediating behavior. He proposes that one should study the effects of providing reinforcement for this mediating behavior, including priming the mediators, using verbal cues, and instructing subjects to use responses as mediators In general, he argues for practicing, reinforcing, and making the mediating behavior explicit. His suggestions stand in marked contrast to the early attempts of mediation researchers interested in devising crucial tests of the theory to insure the exact opposite, namely that their subjects were not aware of the mediation process.

If one is willing to move beyond questions about the nature of the mediating process itself to the investigation of conditions for inducing and improving mediational processes, Jenkins' suggestions are well taken. For researchers in education interested in the fundamental conditions for inducing verbal transfer, Jenkins' suggestions appear to have considerable importance.

THE MODEL OF HIERARCHICAL LEARNING SETS

It has previously been stated that especially in school learning, a model of transfer should involve general abilities, general intelligence, and knowledge and information relevant to the transfer task. With the term "learning sets," Gagné's (1965) model involves these variables in predictions about rate and achievement of subject matter in schools. His model is well suited to the study of transfer in school learning.

The present discussion is based primarily upon Gagné's article on the acquisition of knowledge (Gagné, 1962) and an experimental study (Gagné & Paradise, 1961) on abilities and learning sets in knowledge acquisition. The attempt will not be made to describe the systematic treatment of human learning and the conditions of learning presented in the recent book, *The Conditions of Learning* (Gagné, 1965).

Gagné's theory of hierarchical learning sets is designed to explain phenomena which occur in productive learning. The phrase "productive learning" refers to transfer across a class of behavior such as "solving linear equations." These classes of behavior, or learning sets, are considered to be the components of knowledge. Thus knowledge can be operationally defined as successful performance on tests which sample an entire class of tasks. Knowledge then becomes operationally defined by transfer tests, and Gagné's theory becomes, in substantial part, a theory of transfer.

Knowledge consists of a set of subordinate capabilities called learning sets which are arranged in a hierarchy. Each learning set may have several other learning sets subordinate to it. Together the subordinate learning sets mediate positive transfer to the learning set of the next higher order in the hierarchy. If one or more of the subordinate learning sets is not present or cannot be recalled, transfer to the next higher order of learning set is predicted to be zero. The learning sets at the bottom of the hierarchy are basic human abilities relevant to the superordinate learning sets. The learning sets higher in the hierarchy are the sets of behavior particularly related to the problems and tasks to be learned in a sequence of instruction.

Learning sets, along with instructions, comprise the two fundamental variables of the theory. Together these two variables are used to predict transfer as it operates in instruction.

To define the hierarchy of learning sets relevant to any given learning task, Gagné suggests the researcher begin with the following question: "What would the individual have to know how to do in order to be able to achieve this (new) task, when given only instructions?" (Gagné & Paradise, 1961, p. 2.) By answering this question, the experimenter begins the first cycle of the analysis of the final task. Each of the subordinate learning sets obtained by answering the above question is in turn investigated with the same question which leads to a definition of the next level of subordinate learning sets. This procedure is reiterated until the entire hierarchy is defined. Individual differences in learners may be related to this learning set hierarchy. By using achievement tests, general intelligence tests, and tests of basic abilities, one can make predictions about a learner's rate of progress and his level of achievement through the learning sets in the hierarchy. In a more specific sense than either of the other models presented above, Gagné's model relates ability and achievement measures in predicting transfer from, in this case, lower-order sets to higher-order sets.

In particular, the model predicts correlations between individual differences, general intelligence, relevant basic abilities, number and pattern of relevant learning sets, and measures of rate and achievement of transfer to the higher levels of a learning set hierarchy. Let us assume that positive transfer is measured by rate of learning of a relevant learning set. As the learner progresses upward through a learning set hierarchy, his rate of learning should depend increasingly on the newly learned subordinate sets of the hierarchy. Conversely, his rate of learning should depend less and less upon relevant basic abilities. In other words, positive transfer, measured by rate of learning, should correlate to an increasing extent with the learning of the immediately subordinate learning sets, but decreasingly so with relevant basic abilities.

However, the correlation between general intelligence and rate of learning should remain constant as the learner progresses up through the hierarchy of learning sets. So should the correlation between irrelevant basic abilities and learning rate remain constant. Both sample general ability and, as such, both should constantly mediate a small amount of general transfer.

The relationship is complex between achievement as a measure of positive transfer of training and the individual difference variables given above. For example, Gagné predicts that achievement of a given learning set should correlate increasingly positively with basic ability scores as the learner moves up the hierarchy. At least, this is the case in a moderately ineffective learning program where not all learners achieve each learning set. In this case, basic abilities enter into the determination of success and failure and increasingly should contribute to the variance in achievement of a given learning set.

Predictions such as these are uncommon but highly useful in the study of transfer of school learning. They emphasize utility of models specifically developed to study school learning.

So far we have discussed only one of the two primary variables in Gagné's model, learning sets. We have not discussed instructions, the other primary variable in the model. Gagné states (1962, pp. 357-58) that instructions, the content of communication, mediate positive transfer in one or more of the following four ways: They (1) identify required terminal performances; (2) identify parts of the stimulus situation; (3) help to establish high recallability of learning sets; and (4) serve to guide the thinking of a learner, as in establishing a set to put ideas together in solving a problem. Since these four functions are not difficult principles to understand, they will not be further discussed here. Research is needed upon the effects of instructions or instructional variables in the mediation of transfer. So far, Gagné's own research has been primarily concerned with the first of his two major variables, knowledge of learning sets.

Research on Hierarchical Learning Sets

In the study by Gagné and Paradise (1961), virtually all of Gagné's predictions were given support. The subjects were seventh-grade children and the learning set involved the task of solving linear algebraic equations. Achievement of higher order sets was found to be dependent upon positive transfer from all lower order component learning sets. These investigators found a considerable consistency between the mastery of each prerequisite set and positive transfer to the next higher ordered set. The model appears to provide a creative approach to the study of verbally mediated transfer in a hierarchically arranged set of learning tasks.

The Conditions of Transfer

What are the conditions for inducing transfer that we can infer from this model? In his recent book, Gagné (1965, pp. 231-35) discusses the conditions of transfer. He distinguishes between lateral transfer and vertical transfer. Lateral transfer is the broad application of previous learning sets. Vertical transfer is essentially the type of transfer discussed previously, in which subordinate learning sets mediate positive transfer to superordinate learning sets.

For lateral transfer, Gagné maintains that the most important conditions are individual differences among the learners, including relevant learning sets and basic intellectual abilities. Practice in a wide variety of situations is one external variable predicted to contribute to transfer. Reinforcement for application of principles and knowledge to a broad variety of instances is the other external variable. Gagné seems a little reluctant to take from his model all the mileage there is for predicting conditions to produce lateral transfer. For example, he might have predicted that lateral transfer would be improved by the mastery of each higher order of learning set in a hierarchy, because positive transfer across all subordinate components of the learning set is mediated by the higher-ordered member of the hierarchy. Lateral transfer might also be increased by associating a name or label to a higher-order learning set. That is, simple practice may not be as effective as practicing the addition of verbal labels.

Vertical transfer, which can be indexed by rate of learning, is mediated by mastery of subordinate learning sets. Basic abilities and variety of previous knowledge are also important conditions for mediating at least a small amount of vertical transfer. The external conditions are the same as those for lateral transfer and include practice, variety, verbal cues, and applications of previously learned rules in novel combinations.

Comments on the Learning Set Hierarchy Theory

The theory of hierarchical learning sets applies to the study of transfer in school situation, knowledge about the individual differences among learners, concepts from verbally mediated transfer, and also the fundamentally important idea of analysis of complex tasks into components. But most importantly, the theory combines abilities and learning into the notions of hierarchically ordered sets. The model accounts for individual differences in response to transfer problems better than does either of the two above models.

Gagné does not hold himself strictly to the language of simplistic *S-R* theory, but he has developed a sophisticated *S-R* model for the study of transfer in school learning. The model seems particularly relevant for someone who wishes to evaluate instruction for its logical and hierarchical arrangement of simple to complex learning tasks.

We can expect that the hierarchy model will be widely used by our hypothetical educational researchers, because it is carefully and thoroughly discussed in terms of classroom instructional problems, and because the problem of transfer is considered from the point of view of an educational researcher. The conditions for transfer include the instructional variables present during learning, but also the individual difference variables.

However, our hypothetical researchers may be disappointed when they realize how little is known about the conditions of transfer. Gagné's model, as well as the simplistic *S-R* models and the mediated generalization models, all offer ideas about the control of transfer of instruction. But a staggering amount of empirical work is needed to learn about the conditions of transfer. At the present time, there would be no better way to begin this work than by the adoption of a conceptual model useful to the study of transfer of instruction. For the study of such transfer, Gagné's model is currently a leading contender. For many problems of transfer in classroom instruction, the model of learning set hierarchies has no serious rival for predicting the transfer which will be produced by instruction and individual differences in interaction.

CONCLUDING STATEMENT

Models and theories can have value for educational researchers interested in identifying the conditions of learning and transfer. In the literature on human learning, a few such useful models specifically designed for applications to education are beginning to appear. Arthur Jensen (1967) has developed mediational models useful to understand and explain phenomena of verbal learning and social class, especially as they may interact in school learning. David Ausubel (1963) has created a cognitive theory of meaningful verbal learning. These two fundamentally important theoretical developments are examples of model building which we should encourage in education.

It is also time for educational researchers to make frequent use of conceptual models to guide their research and thinking about transfer. In the study of the conditions of transfer, whether one chooses to use one of the three models discussed in this chapter or chooses to develop another one is not the major point of this chapter. Individual differences among educational researchers must also be respected.

The major point is that in the study of the conditions of transfer, one should create or apply models relevant to the problems under study. These models can help to build findings of empirical studies into a systematic body of knowledge.

Especially to be encouraged is the development of models from the specific problems of transfer under study. The model of hierarchical learning sets is a good example of the type of specific model to be developed in educational research on transfer. Gagné has led in the development of a conceptual approach appropriate for the study of the conditions of transfer in school learning. Where are the other specific models to lead to other predictions about transfer from instruction in school? Where is the competition to excite fundamental, theoretical research on the conditions of transfer in school learning?

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AUDIENCE QUESTIONS

• *Question:* I would like to return our attention to your verbally mediated model and particularly to the Kendler experiment, where no reversal was found for animals and for children who hadn't developed verbal capacity. There is some evidence to show that rats, in particular, can develop this verbal transfer, or this reversal transfer. I am particularly referring to the overlearning situation and the research reviewed by Mandler in *The Psychological Review,* I believe in about '56.

The evidence shows, apropos of what we talked about yesterday, that perhaps by conceptual mediation, instead of verbal mediation, rats are able to perform this reversal transfer. I would like to ask, is there justification for insisting on verbally mediated generalization as opposed to conceptually mediated transfer?

• *Dr. Wittrock:* No. It seems to me that these are all interesting phenomena to study. I simply restricted the paper to verbal transfer because it is a large area to try to encompass. In no way did I intend to imply that verbal processes are the only basis for mediation. If rats can mediate on a conceptual basis, if they can work their way up from associations to structures, more power to them.

• *Question:* Is there any prediction about the results of self-discovery versus non-discovery in your study on presenting the rules versus having the subjects generate their own rules, when you study their transfer in new situations? I wonder if there is any reason to hypothesize that one group would have performed better than the other, or some of the others, on the basis of this model?

• *Dr. Wittrock:* Yes. The model makes one prediction difficult to support empirically. We assume that by building in higher-order mediators, the breadth of transfer is increased. In that study (Wittrock, 1963b) we tried to produce transfer by building in associations low in the hierarchy. The model predicts that with naive subjects you get verbal transfer better when you build higher-order associations into the subject's repertoire. Keppel and Postman (1966) find that you get transfer without building in the higher-order verbal associations relevant to the problems. This is why I think their findings are exciting.

My response to the Keppel and Postman study is as follows: What have they actually said to their subjects while they are learning the associations? From instructions to mediate, I predict they would get an even greater transfer to a third paradigm. If

Keppel and Postman would verbalize and explain the paradigms to their subjects, I predict they would also increase transfer. These predictions follow from the model. Does that answer your question?

• *Questioner:* Yes. Thank you.

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DISCUSSION OF M. C. WITTROCK'S PAPER

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MODERATOR: Dr. Warren S. Findley
FIRST DISCUSSANT: Dr. Lawrence M. Stolurow

• *Dr. Findley:* In preparation for my duties this morning as Moderator, I thought I should come prepared to tell a story to put the session in some sort of perspective. Yesterday I was prompted by Dr. Gibson's incidental mention of M&M's, and then last night, when Dr. Rothkopf was speaking, my thought was reinforced by his reference to the fact that sometimes the saving of time by more efficient methods results in saving time also from some of the unintended and incidental outcomes, and therefore might not be a complete gain.

The story comes from the Early Education Center at Peabody College, where they are attempting to help youngsters from disadvantaged backgrounds come better prepared to take advantage of school at age six. They start with youngsters at age three. The subject of this story was pointed out to us when we came to observe one day. She was a little three-year-old who evidently had been subjected to some of the conditioning that is typical. They said they used raisins and M&M's considerably, and we could be quite sure they were using the M&M's because we could hear them clicking in the pockets of the smocks of the teachers.

One day two members of the staff were conversing quite incidentally about what they should do the next day without attention to who might be nearby. Finally, one said, "Fine. We'll do it that way." Whereupon, a voice from below was heard to say, "Good. Right. Give yourself an M&M."

After some of the terminology that has been used this morning I am working on a concept of a "reversal non-shift" to explain behavior. But with that much introduction, I will turn the program over at this moment to Dr. Stolurow for discussion of Dr. Wittrock's paper.

• *Dr. Stolurow:* I can't appeal to either age or sex, so I guess I am left to cope with the problems of the paper on its merits as I see them. There are many points on which I agree with Merl. Certainly, I agree with his basic reaction to the charge he was given, namely, that the studies of the objective behaviors from an *S-R* point of view are the most useful ones we have. Both of us, however, are quite aware of the inadequacies of this approach as it has been used. This is clear in his paper, and I hope that my remarks will amplify this point in ways that are provocative.

In delineating his position three different approaches are distinguished. These may be thought of as varieties of a single approach. While agreeing with this in some respects, I have problems with the interpretation of the distinctions he developed. It seems to me that a theory is usefully considered as a language which the scientist is trying to evolve to account for certain phenomena. From this point of view, the trichotomy developed in Merl's paper takes on a different cast. From the point of view that I am expressing, Merl is describing different levels not different types, of effort to cope with data from laboratory approaches to the study of transfer phenomena.

In looking at the problem of theory construction as an effort to develop a language to describe natural phenomena, I see implied in Merl's paper the Whorfian hypothesis, namely, that the scientist is influenced by the language he uses, and in fact, is restricted in his conception of phenomena by his language. I think, however, that despite this inference, Merl's paper gives evidence contrary to Whorf. Many theorists who are represented in the lower level of Merl's analysis also are represented in the upper levels. Clearly they are not limited in their level of thinking by the simple language they started with. This suggests that what we're dealing with might be better put in reverse; the theorist does have the ability to think beyond the language he uses, and when he does he adapts the language to what he now sees. As he sees more complexity, he develops the language he uses to express what he sees to others as well as to himself. In other words, as he recognized the need, the psychologist has added to the language system (his theory) hoping by doing so to make the language more adequate—more accurate and reliable—and therefore more useful.

One of the major voids in the *S-R* language as we currently see it being used is its lack of operators. We see *A*'s joined by lines, or possibly arrows, to *B*'s. The line, or arrow, is a pun, however. There are many different relationships between an *A* and a *B*, or an *S* and an *R*, not just one, yet all are currently being represented by the same symbol, a straight line. For a long time now, the symbolic world of the psychologist in verbal learning and transfer has been limited to an *A-B* sequence or an *S-R* chain. Either the *A-B*'s, or *S-R*'s, were ordered when there were sets, or they were unordered. When ordered the task was serial learning and when unordered it was paired-associate learning. Obviously, this limited language is too incomplete to represent the problems that confront an educator or educational psychologist concerned with classroom learning and transfer. The *S-R* language as a device for abstract representations has been considered adequate for the limited types of laboratory situations which have been studied because the culture in which the researcher was working was a closed one. The linguistic shorthand devices the members of this culture use were adequate descriptions for the purpose because the members of the group were properly schooled in this cryptography and in the meta-language behind it.

Problems occur when one moves out of the laboratory and attempts to talk *S-R*-ese. The first problem is that one does not have rules for assigning symbols, so that semantics is a problem. The real world events which we observe have to be coded. In the use of the *S-R* language for laboratory reporting, the code is used according to some conventions that are relatively well established though seldom treated explicity. The second problem is the lack of a grammar that is adequate for the problems that we are trying to study in the school situation. The grossly oversimplified *S-R* language which we are using for our laboratory studies has not been seriously developed to cope with all the complexities we now recognize and hope to understand. To this end, it seems useful to describe some associative paradigms that were identified (Stolurow, Hodgson & Silva, 1956) and used in a series of unpublished studies of transfer conducted in my laboratory at the University of Illinois under an Air Force contract. When we asked what basic associative paradigms were needed to represent the learning tasks imposed by the instructional materials we examined (task analysis), we identified the following in addition to the one-to-one relationship described earlier for paired-associate and serial learning sets. These new paradigms are the many-to-one, the one-to-many, and the many-to-many relationships. Fig. 4.1 illustrates these.

Thus far, I have focused on the inadequacies of the *S-R* approach as a language that is used to express theories and in which the theories are the grammars. These theories are more, or less, adequate, not only in terms of their ability to suggest predictions that are confirmed by research, but also in terms of their explicitness with respect to the use of the language in unambiguous ways. Right now, the major ambiguity in the use of this type of language results from: (a) The lack of rules for assigning language symbols to different types of events; (b) the limited set of symbols available as operators or symbols for the various relationships that can exist; and (c) the limited rules for constructing legal sentences (associative paradigms). Thus, while I agree with Merl's position that the *S-R* approach has been the most useful one in coping with problems of transfer, there are definite and serious shortcomings in it as it stands. These need to be dealt with if we are to make progress in our understanding of transfer. Data from unpublished studies have revealed that the paradigms or structures that are not now an established part of the *S-R* language, but could be, are important determiners of transfer effects.

To develop this point further, I want first to discuss research strategy. While pointing out deficiencies in the current state of the *S-R* approach, I have indicated that a critical stage in our efforts to cope with transfer in school settings requires a task analysis. This step is not so critical for the laboratory investigator as it is for the applied researcher. The task analysis is essentially a process of identifying variables and relationships. Once this is done then paradigms can be characterized. These are represented in terms of a language which provides a consistent form of representation. The next step is to develop the language where it is found to be inadequate. Once the representations can be made, then research is possible. To accomplish the research, a design must be formed by controlling some variables and systematically manipulating others.

In the research we accomplished, the structural features of associative paradigms were the independent variables and the percentage of transfer was a dependent variable. Of the conditions to be held constant, we chose association reversal because of data obtained

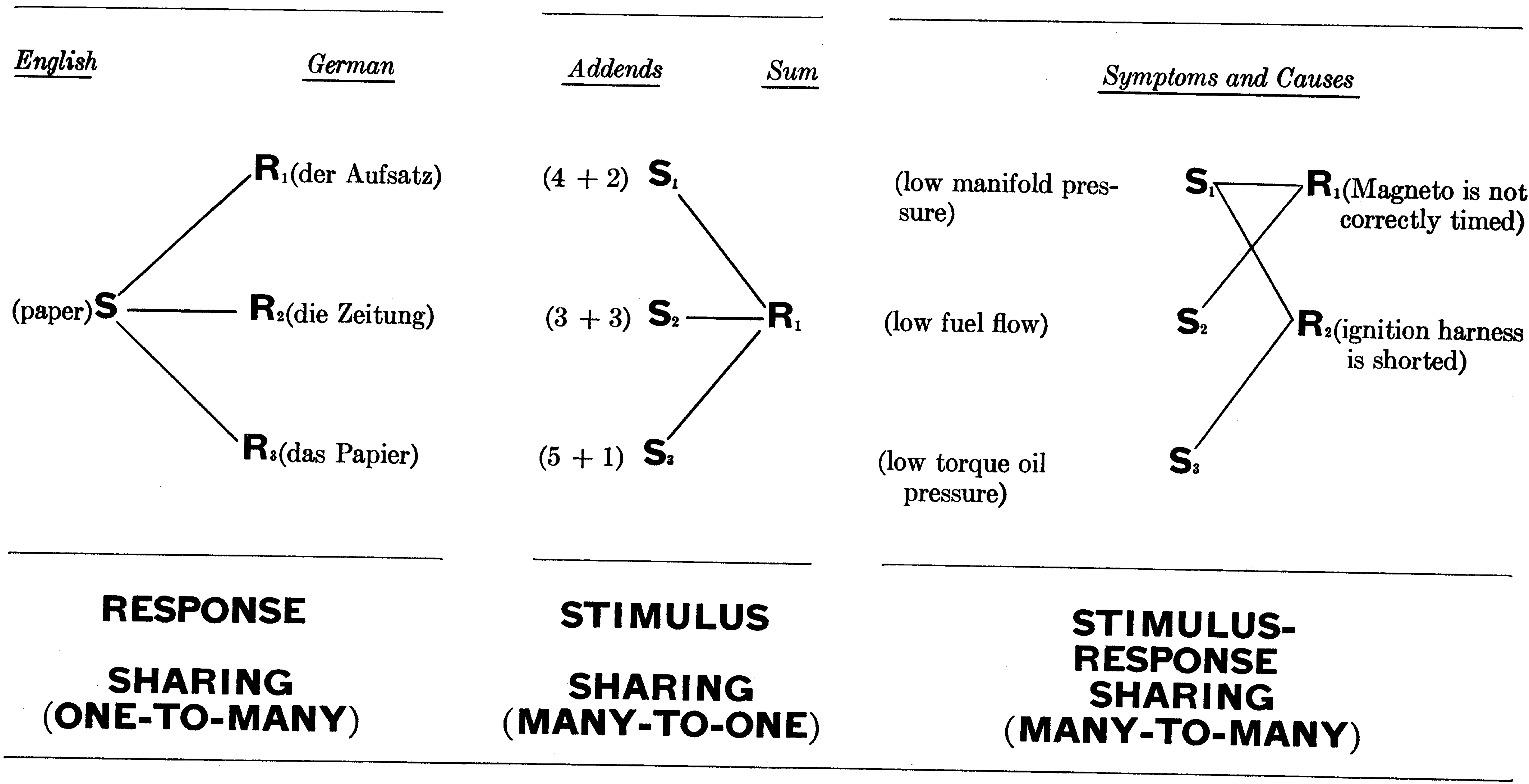


FIG. 4.1. Three association paradigms.

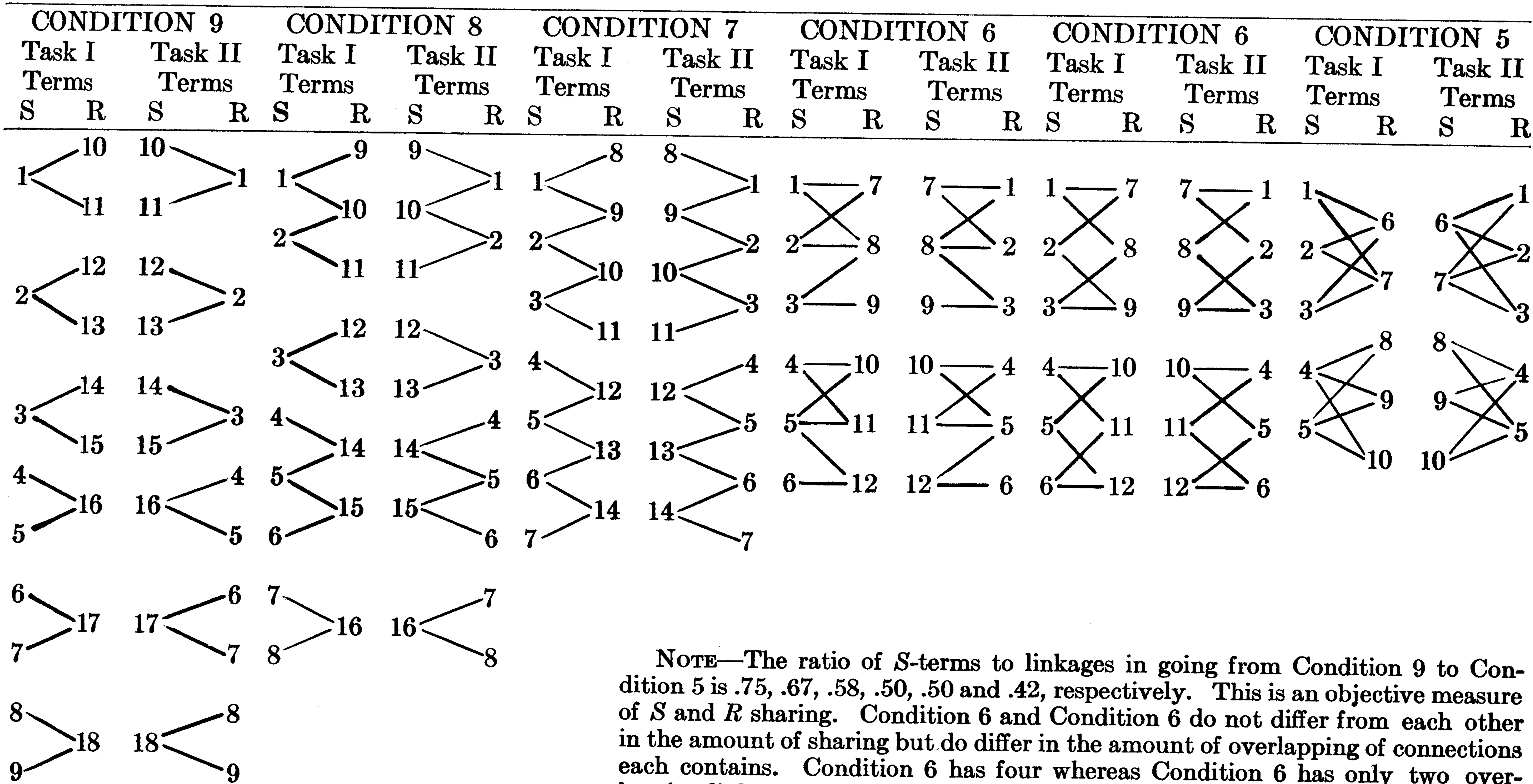
in our non-laboratory studies (Stolurow, Hodgson & Silva, 1956).* Association reversal is a special case of transfer in which terms that are stimuli and those that are responses in one task are reversed in learning the second task, e.g., English-French, then French-English vocabulary.

The non-laboratory analyses permitted us to define the conditions of a laboratory, or more highly controlled, study. Two operations, simplification and regularization, were employed in rendering the problem to a manageable experimental form. The number of terms used was reduced to allow the learners to master the task in a short time. Second, the terms to be associated were chosen from the Thorndike-Lorge frequency count as high frequency words. Thus the terms could be assumed to be very familiar to the learners. Consequently, neither stimulus learning nor response learning as related to the stimulus and response terms, was an important source of variance. The idea was to make the associative processes imposed by the paradigms the significant source of variance in learning and transfer scores. To eliminate bias, the high frequency *T-L* words were assigned randomly to positions in the paradigms. These positions represented places in associative structures where terms could appear (see Figure 4.2).

It is important to keep in mind that these studies relate to the special case where the learner has to master two directions of association, e.g., symptom-cause, cause-symptom. We wanted to know what effective differences in associative structure would have upon learning the reverse direction of association. We found clear and systematic changes from positive transfer to negative transfer as the structural complexity of the material was increased. Structural complexity was measured in terms of the number of "shared" and "overlapping" associative relationships to be learned among the terms. It was found that increased sharing decreased the amount of positive transfer. (See Figure 4.2, Conditions 9, 8, 7.) With increased overlapping there was increased negative transfer. (See Figure 4.2, Conditions 6, 6′ and 5.)

In another study, subjects learned symbolic logic using either a Polish notation system or a Peano-Russell notation system. In other respects the two programs were alike. Our interests here were in the

* L. M. Stolurow, T. F. Hodgson, and J. Silva, Transfer and retroaction effects of "association reversal" and "familiarization" training in trouble-shooting. *Psychol. Monogr.*, 1956, 70, No. 12 (Whole No. 419).



NOTE—The ratio of *S*-terms to linkages in going from Condition 9 to Condition 5 is .75, .67, .58, .50, .50 and .42, respectively. This is an objective measure of *S* and *R* sharing. Condition 6 and Condition 6 do not differ from each other in the amount of sharing but do differ in the amount of overlapping of connections each contains. Condition 6 has four whereas Condition 6 has only two overlapping linkages.

FIG. 4.2. Paradigms of interrelationships of S-terms and R-terms for the six conditions employed in two experiments

transfer value of those systems. (See Stolurow, 1966.)* In other words, after completing each program would the students who studied with the Polish notation learn to apply the concepts of symbolic logic to new problems more rapidly than the students who studied with the Peano-Russell notation system? Figure 4.3 summarizes these results. In three out of the four measures used the group who studied the Peano-Russell notation showed greater transfer. The exception occurred when they learned Polish notation. They learned this less rapidly than the other group learned Peano-Russell notation.

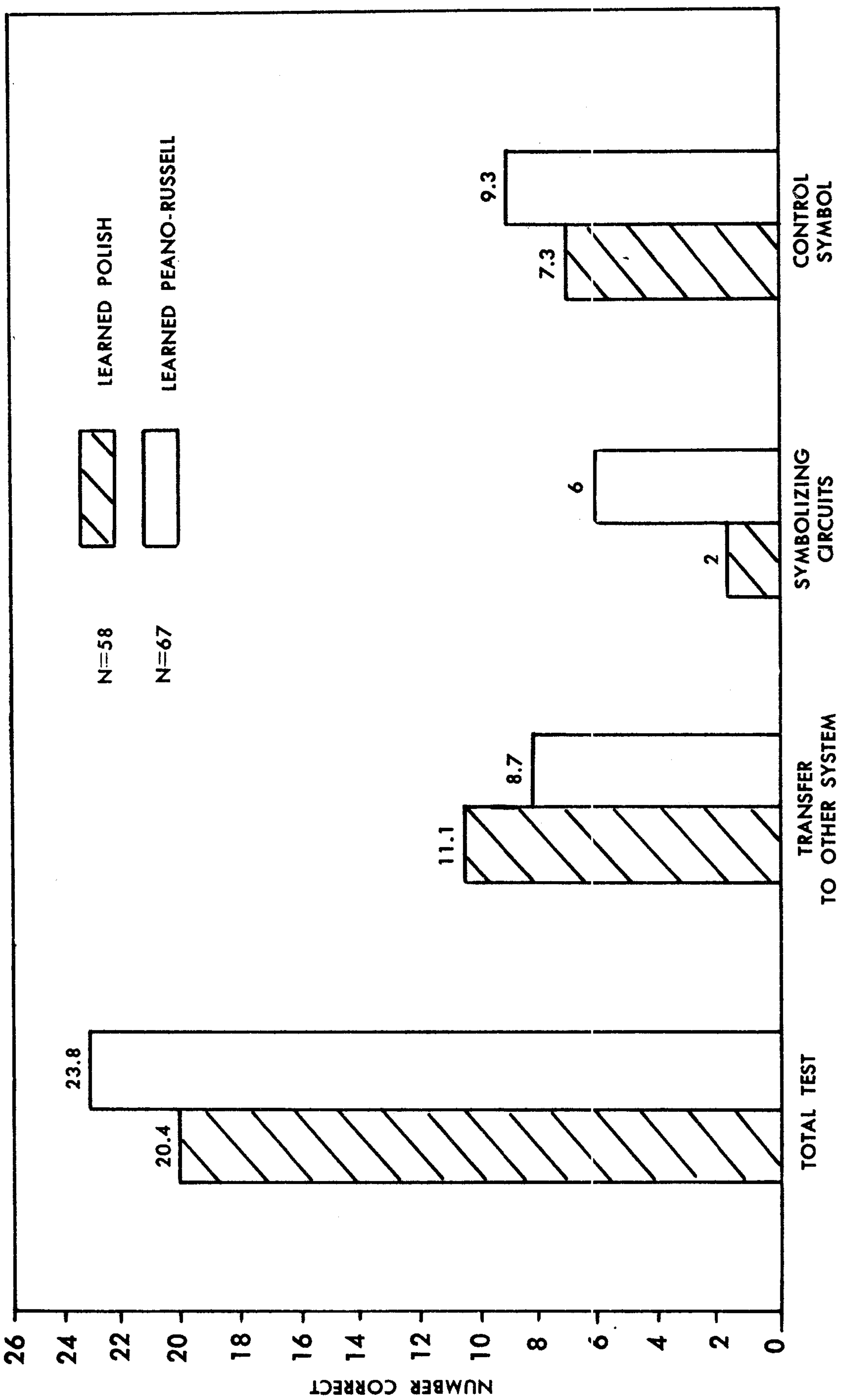


FIG. 4.3. Mean number correct on total transfer test and subtests for students who learned the Peano-Russell or Polish notational system.

* L. M. Stolurow, A computer-assisted instructional system in theory and research. In D. Unwin and J. Leedham (eds.), *Aspects of Educational Technology* (London: Methuen, 1966), Ch. 24, 257-73.

TABLE 4.4

CORRELATIONS OF ABILITIES WITH PERFORMANCE ON A TEST COVERING IDENTICAL CONTENT TAKEN AFTER LEARNING THE POLISH OR PEANO-RUSSELL NOTATION (WITH MEANS AND STANDARD DEVIATIONS)

Group	N	Inference			Advanced Vocabulary			Letter Sets			Math		
		r	X	S. D.	r	X	S. D.	r	X	S. D.	r	X	S. D.
Polish	58	.01	15.8	2.8	.09	22.0	5.6	−.01	22.0	4.0	.18	19.7	6.2
Peano-Russell	67	.01	13.9	3.4	.27[a]	19.5	5.5	.31[b]	19.8	4.6	.31[b]	13.5	5.0

NOTE—Means scores on a post-test (in notation appropriate to groups) were 40.74 (Polish); 49.36 (Peano-Russell); $t = 10.2$, $P < .001$.

[a]Significant at .05 level (two-tailed test).

[b]Significant at .01 level (two-tailed test).

Of even greater interest are the data revealing the correlations between three aptitude tests with the learning scores. We chose the four aptitude tests reported in Table 4.4 because it was assumed that they measured skills used by students when they learned symbolic logic. The results suggest that three of these sets of skills are involved but only if the students are learning with the Peano-Russell notation system. Vocabulary, letter sets, and math skills correlated significantly for those students, but did not correlate with the achievement scores of the students who learned with the Polish notation. Since both groups learned the same logical concepts, the differences appear to represent differences in the processing skills which the notation system requires of the learner. It is conceivable that the English speaker who looks at a Polish expression such as "Kpq" transforms this to *p* and *q*. The Peano-Russell expression *p.q.*, however, is already cast into this form. The English language speaker does not have to develop the transformation skills that the student learning Polish notation has to learn. This suggests the following hypothesis: It is the symbol-manipulation rules which one must learn to use in mastering a language that account for both prediction from aptitude tests and transfer. In the Peano-Russell notation system the translation of an expression into English is minimal, and vice versa. Therefore, the student who learns symbolic logic with this notation system can readily apply it wherever he can define the problem in English. The symbol-manipulation rules that the Polish notation requires of the individual are different. They require him to go through, or engage in, transformation whether he codes English in Polish notation or vice versa.

While both groups learned symbolic logic, each learned it by means of a different set of skills for processing symbols. It presumably is this difference which accounts for both the presence and absence of aptitude correlations and differences in transfer effects. The transformation skills learned by the Polish group are not the same as those involved in performance on the aptitude tests. Similarly, these processing skills are not involved in the transfer to learning the new tasks. It appears that it is symbol-processing skills, and not conceptual content, that appears to be critical in transfer in this situation. This hypothesis raises many questions among them being the utility of the identical elements conception of transfer. Clearly, this idea does not help very much in accounting for these data. The identical elements notion focuses on content. It is not the content per se, but rather what the content requires the learner to do (information processing) that is the critical thing. The study referred to reveals evidence for the information-processing-skills conception of transfer. This is not an *S-R* mediation conception, but it is not inconsistent with a mediation theory. The important difference is that it goes beyond a mediational position.

I would like to finish with six points. My first point is that the *S-R* approach as a language system is in need of development to permit it to account for learning and transfer phenomena more unambiguously and usefully. Particularly we need operators to be specified so that we can differentiate between, let's say, the *either/or* condition and the *both/and* condition, which are not represented by the usual *S-R* diagrams that we draw in our reports.

Secondly, a psychological language is badly needed to map educational tasks into sets and relationships that define psychologically relevant dimensions, permit us both to see dimensions and problems more clearly, and to cope with them in terms of our laboratory research approaches.

Third, the aspects of the language we develop for learning and transfer should be able to deal with associative structures of tasks in an unambiguous way since these aspects seem to be important in transfer particularly under association reversal conditions.

My fourth point is that an adequate description of structure should include both the associative relationships and the symbol manipulation skills. Both of these have to be represented in studying language.

Five, the major unsolved problem from the information-processing-skills point of view of transfer is the delineation of the particular symbol manipulation skills which we require of learners when we present them with a task to learn. The representation of the task can come from the analysis of school subjects by taking into account the transformations, transpositions, algorithms and heuristics, required of the learner who has mastered the task. The "learning sets" need to be analyzed in these terms so as to specify what it is that the individual has to do at a criterion level of performance. Once we identify the information-processing skills of each learning set, then we see how they interrelate with one another, e.g., association reversal.

The sixth and last point is, if we have an adequate language for describing tasks in terms of associative relationships and symbol manipulation skills we can provide the much desired link between the learning laboratory, aptitude testing, and school learning.

• *Dr. Findley:* Thank you, Dr. Stolurow. Before proceeding with any other comments, would you care to respond, Dr. Wittrock, to these remarks?

• *Dr. Wittrock:* I think the work Larry is referring to is precisely the kind that is needed in the study of conditions for transfer. He is quite right when he emphasizes focusing upon particular responses and behaviors in addition to focusing upon particular subject-matter content. We need to attend to behavior in any model of transfer. I encourage more of the kind of work he has been doing.

• *Dr. Findley:* I wonder if there are others in the panel who should care to comment on either this or Dr. Wittrock's presentation before we proceed to the second discussant.

• *Dr. Lumsdaine:* Just one very brief comment which I am sure was meant implicitly. In the strategy which you propose, Larry, of identifying the problem in the field or operational context and then abstracting it into the laboratory, I presume you recognize that there is an incomplete step. The abstracted laboratory result must be retranslated into the operational setting, with the hope that the process of having abstracted it and purified it in the laboratory will enable you to do better in the field setting than you would have been able to do it if you confined all your attention there.

• *Dr. Stolurow:* Yes. I think that is an important point for completing the cycle. Of course, it is really a kind of a spiral. You go back and do the retranslation and that reveals, then, some of these kinds of ecological and indigenous variables that I alluded to. So you work back and forth, cleaning up the situation, so to speak, in the sense of your ability to isolate the variables that are affecting performance.

• *Dr. Glaser:* Merl, you mentioned individual difference factors in the transfer, and that you think about facilitating and inhibitory phases going through a hierarchy, and you think possibly of different learning rates to master something before you go on to learning the next step. But what other individual-difference variables affecting transfer have impressed you in going through this literature?

• *Dr. Wittrock:* Well, quite a few of them. Let me talk about several. Maybe these just reiterate some you have mentioned, but first of all are some of the ones Bob Gagné tests: (1) general intellectual ability, (2) aptitude, and (3) achievement.

• *Dr. Glaser:* How do these influence the transfer sequence? Does this mean going through it faster, or do you mean what is a sequence for Mr. A is not a sequence for Mr. B?

• *Dr. Wittrock:* Let me come to that, if I may.

There is another group of individual difference variables I don't think Bob Gagné has in his model of transfer. I don't know of anybody else who has them in any other model of transfer. They have to do with cognitive style, particularly problem solving. Jerome Kagan studies these variables. In learning we often ignore cognitive style because we look upon these variables as personality variables. I don't really think they are only personality variables. But let's say "O.K. So they are personality variables." So what? It still seems to me that they may be quite relevant to the study of transfer. For example, whether an individual solves problems via a replacement stategy or a non-replacement strategy, or an impulsive or a reflective strategy, may determine the kind of hierarchy he encounters in the study.

Now, to come to your question, which I take it is "What about the rate of progression through different hierarchies; and are there different kinds of hierarchies for different people?"—is that it, Bob?

• *Dr. Glaser:* The answer is yes. I was just trying to get your reaction to the kinds of individual transfer interactions you see, other than just rate of learning different steps in the hierarchy.

• *Dr. Wittrock:* They would be the ones that I have mentioned. They stand out as most salient today.

About predictions of rate of progress, I refer you to Gagné's work. One or two problems may attenuate the relationships he predicts. For example, one has a sampling problem introduced by dropping out people as the hierarchy is learned. As you go through a hierarchy some of the subjects are not going to succeed. As a result, you are not always going to have an increased correlation between intellectual ability and achievement. Attenuation of the variability among learners may reduce the correlation.

It is too early to make predictions about cognitive style variables and rates of progress through different hierarchies. We simply don't know very much about how these variables are related to each other. I believe there are different hierarchies for different people, and that these hierarchies are partly determined by the strategies and cognitive styles employed by the learner.

• *Dr. Hill:* In connection with these three models referred to by Wittrock, I would like to ask to what extent would it be appropriate to think of them as forming a hierarchy in which the first two could be conveniently subsumed under the third?

• *Dr. Wittrock:* I deliberately drew distinctions that weren't completely warranted. I wanted to see people get involved in the intellectual dialectics that ought to be going on in research on transfer. I think Larry was right when he criticized me for drawing sharp distinctions without emphasizing the relatedness of these approaches.

You do have a good point. One of these approaches can subsume the others. The three approaches are on a continuum. But I don't think we should meld them all into one approach. I tried to look at those models that tend to analyze complex transfer situations. I think the research in education needs analysis at this point. Research on transfer needs the kind of work that Larry and Bob Gagné are doing, and I tried to draw particular attention to their work.

There is enough distinctiveness among models of transfer to warrant separate attention to several of them. I singled out Gagné's work because he is building a limited model to handle particular problems that exist in educational research. His model can be subsumed by a simplistic *S-R* model. However, I still think his model is different enough to deserve special attention.

By organizing the paper the way I did, I was forced to categorize research in unfortunate ways. Eleanor Gibson's work was placed in the simplistic *S-R* school. I am sure she would rather not be there. But the paper was organized largely in a quasi-historical fashion. I started with simplistic *S-R* model; then I went to mediated generalization approaches; and then to more specific kinds of models used in educational research. I used a quasi-historical organization for the paper to show that your point, Dr. Hill may be the case.

• *Dr. Stolurow:* I do feel there is a point to be talked about here. We want to distinguish between the language as an evolving device for conceptualizing problems and the specific applications to which it is being addressed. I think the example of Bob Gagné's work is a case where the language is being related to a much more comprehensive section of learning than historically was the case with the two-stage transfer conditions in the laboratory. This is not a difference in kind so much as a difference in an attempt to relate the theoretical language and its constructs to a more comprehensive problem. In order to cope with it, one generates some particular devices that permit you to describe all of that information.

We want to distinguish, it seems to me, between the rules of the game, or the syntax of the language, so to speak, and the applications. If we looked at the slice of the English language which represented the utterances of two-year-old children, they would appear very simple; or even younger, they might be one-word responses. When you look at the complex writings of a person who is trying to describe a scientific process, the language is going to be much more complex. The language has not changed, but you are seeing it addressed to very different tasks. It is that kind of difference that I was trying to address myself to.

• *Dr. Findley:* Since his name has been taken in vain several times, perhaps we ought to let Dr. Gagné comment.

• *Dr. Gagné:* I would like to respond just briefly to what Dr. Hill suggested, namely, that these three approaches are perhaps not different so much as representing a kind of continuum from somewhat simpler theoretical notions to somewhat more complex ones.

I tend to agree with this, that they do represent this kind of continuum. My own work, particularly the studies that Dr. Wittrock has cited, began looking at some relatively complex learning situations such as in the learning of algebra. In this I think students are learning what I call principles, or rules. This is one kind of mediation. One can get a little more simple about it and talk about the learning of concepts or of multiple discriminations, which I would think are mediational also.

Actually, I think that if you go down in this scale of simple kinds of learning very far and get to the strict *S-R* non-mediated learning, you are talking about something very, very special, something which doesn't have much application to school learning. So I want to make it clear that my own work, which began with this relatively complex kind of learning, leads me to the hypothesis that hierarchies are present throughout the domain of learning, even in the relatively simple situation that one finds in the perceptual learning Dr. Gibson mentioned. I do not think of these as being discontinuous at all. I do think that all important learning is mediated.

• *Dr. Findley:* We must move on, but I would like to allow Dr. Gibson to comment. I saw her hand go up a little earlier.

• *Dr. Eleanor Gibson:* It is really apropos right now, too.

Dr. Wittrock said something about my probably not liking an *S-R* model. It is true. One can talk in *S-R* language, and I have done it. But, as Dr. Stolurow pointed out, the stimulus term has got to be discriminated. He gave an example of a three-word chain. I would like to say that you can't stop there, either, because I can memorize that three-word chain but I still would not have learned to detect the symptom that it identifies. As my husband can tell you, I seem to be incredibly bad at doing the perceptual differentiation when engines make strange noises. He often gives me some of these three-word chains and then asks, "Is it that symptom, or that, or that?" I can't possibly say.

It isn't that I haven't memorized those three-word chains, but I have not differentiated the noises perceptually so I can use the right label. That's the kind of learning that I think nobody has been talking about this morning.

• *Dr. Glaser:* May I ask one question? When you say "all important learning is mediated," what do you mean by "mediated," Bob?

• *Dr. Gagné:* Learning in which the learner contributes something from his own intellectual store. I think the only learning in which this doesn't occur, and I'm not sure of this either, is classical conditioned reflexes. I therefore consider that a very poor model for the study of any other learning. I think that even the simplest learning that may be described in animals, the acquisition of an operant response, as it is sometimes called, is not a simple *S-R*. I think it is mediated by kinesthetic feedback, and even in that case requires the contribution of the organism itself.

Now, you work your way up from that to concept learning and other kinds, and the mediators of course become more and more complex. This is what I meant.

SECOND DISCUSSANT: Dr. Henry C. Ellis

• *Dr. Ellis:* I would like to endorse the central thesis of Merl Wittrock's paper; what is needed in transfer is more conceptually oriented research. I think that in chatting with Merl yesterday I discovered a bit of tongue-in-cheek in his paper, and he admitted this slightly today when he indicated that what he would like to see is more controversy among various theoretical positions. Indeed, Merl indicated that he would like to see more theoretical controversy among educational psychologists similar to that among learning theorists. I am not sure I would entirely agree with Merl in this respect, because I think that there have been several kinds of theoretical controversy; some seem to have been fruitful and to have generated important research, whereas others have seemed to be less fruitful. As an example of the latter category, I would not like to see a debate among transfer researchers of the order of magnitude that we saw in the forties between Hull and his students on the one hand and Tolman and his students on the other. I don't believe that kind of theoretical controversy is going to be very useful as a model for educational researchers. This is not to imply that I think that theory, on the other hand, is unimportant. I think theory is very important, but for reasons of a different nature.

Consider an example of controversy in learning theory which ended quite discouragingly. It was the long controversy over whether one learns responses or cognitions. The argument over what has been learned, as developed between Hull and Tolman, revolved around the issue of whether we learn responses, reactions, peripheral events, or whether we learn ideas or central events. The answer today, as we know, is that the data allow us to make both interpretations. Whether we learn one or the other, or both, depends very much upon the arrangement of the experimental conditions. This was, I think, brilliantly shown by Frank Restle in 1957, in his analysis of the "place- versus response-learning" controversy. He indicated that what an animal learns in a maze task depends very much upon the placement of the extra-maze cues and upon other environmental stimuli; animals will, in fact, learn to go to a place, or they will learn to make a response, depending upon how you arrange the environment.

I would caution educational researchers against getting involved in what I call two-valued controversy; namely, controversy which raises the questions, "Is it this or is it that?" "Is it place or is it response?" because I think that developments over the last twenty-five years suggest that this kind of question is likely to yield a similar type of answer that Frank Restle has given us; namely, that it is not "either/or," but "both," depending upon the conditions of the experimental environment. So my first point, to summarize, is that I would not like to see educational researchers become engaged in the kind of theoretical controversy that we had in the forties and even up until the mid-fifties.

The second point that I want to make is that Merl's discussion of three conceptual approaches to transfer can be viewed as approaches that fall along a complex continuum of molarity, complexity, and abstractness. Each of these approaches should be employed to the extent that one can attain mileage from them. If one is working in the laboratory with relatively simple kinds of tasks, it may well be that an *S-R* model is quite adequate to account for the kinds of data that are generated. In contrast, if one is concerned with more complex forms of learning, I believe that one must begin to deal with mediational phenomena and mediational theory. If one goes further and attempts to deal with larger units of behavior, and perhaps more molar conceptual units, I believe that Bob Gagné has given us an effective start in accounting for phenomena at this more molar level with his notion of hierarchical learning sets.

From the point of view of education, when you are trying to determine what kind of theoretical approach may be useful to you, I think you have to look at this dimension of conceptualization and decide which of these approaches best approximates the kinds of events that are of interest. If one is interested, for example, in developing principles of transfer of training in mathematics and asks questions at that level, then I think that one has to deal conceptually at a level of complexity which involves hierarchical learning sets. On the other hand, I would point out that I don't see a fundamental discontinuity between *S-R* principles and hierarchical learning sets. I believe that one can analyze the concept of a hierarchical learning set into simpler components. I don't like to think of learning set as something independent of any prior learning; nor do I like to think of the concept as one that cannot be analyzed into subprocesses or components.

Finally, I want to identify what I regard to be some needed areas of research in transfer. Although we know something about the conditions which influence transfer, there are several areas which may yield considerable payoff. It is clear that one of the important conditions of transfer is that of stimulus variation; namely, that in a learning situation the learner should be exposed to a variety of stimulus conditions that are instances of the event to which he must ultimately respond. I think we need a theory or strategy which would allow us to say something about the selection of the stimulus events to be placed in the instructional sequence. Consider a situation in which one is going to teach particular concepts in algebra, such as those related to solving linear equations. What is the minimal number of events that one would want to sample from this class of problems to place in an instructional package? When one writes a program for teaching algebra, technically what is done is to make some kind of task analysis of algebra and then sample instances from this problem class. We need some kind of stimulus sampling theory, or some sort of strategy that will enable us to be more efficient in sampling the instructional environment.

In addition, we should recognize that the very conditions which maximize transfer, on the one hand, are typically the conditions that minimize initial acquisition of knowledge and skills on the other. If we want to maximize acquisition or initial learning, we know that the best way to do that is to make the environment as relatively noise-free as possible with a minimum of stimulus variation. Yet, when we decrease stimulus variation, we reduce the likelihood of transfer. This relationship between initial learning and transfer suggests that we need research which deals with an optimizing strategy; namely, how can we be sure that initial learning is well-established, and yet be sure that there is sufficient stimulus variation in original learning so that we can ensure desired transfer effects?

Another point in transfer research concerns the dimension of task difficulty. This dimension has not always been clearly defined; however, its meaning is quite clear in the work of Douglas Lawrence in which animals learn a discrimination task with stimuli that vary in initial difficulty. Lawrence finds that if one trains animals on a very simple discrimination task in which the stimuli are quite distinctive, one gets more efficient transfer to a difficult discrimination task than if the animals are trained on an initially more difficult discrimination task. In this case the stimuli lie along the same dimension. Lawrence's work, and that of others, including Pavlov and the more recent work of Frank Logan, suggest that in trying to optimize transfer we should train individuals on initially easy discriminations and move progressively to more difficult discriminations. But what we lack, other than this general principle, is a theory which would permit us to say something about how rapidly we ought to move along this dimension. From operant conditioning and programmed learning we are told that we should make the discriminations easy at first and then progress to more difficult discriminations. Although this is true in a general sense we still don't have a theory which would permit us to make precise statements about how fast we should move or about the difficulty-level of the stimuli at various stages of progression.

As far as I can tell, an unexplored topic in transfer is the effect of allowing an individual to spend part of his time teaching a task, as well as learning a task, on subsequent transfer. At the University of New Mexico I have tentatively explored this question by having college freshmen learn a very simple task. They are also given an opportunity to teach other individuals. The task I am talking about involves a simple operant response like touching your hand on top of your head, and the "reinforcement" is clapping your hands as in the old game "You're getting warm." The learner has to discover what particular response is going to be reinforced. I have arranged the situation so that part of the time the subjects serve as subjects per se, and part of the time they serve as experimenters teaching other subjects. My view is that part of what is important is learning how difficult it is to learn. We have discovered that, if we give subjects practice in teaching others this operant task, transfer to another class of operant tasks is moderately enhanced. Presumably, the subject who is serving as a teacher is learning something about the complexities of the stimulus environment; he is engaged in perceptual learning. I believe that he is learning how to attend; he is thinking about how difficult this task is, and what he can do to assist his learner.

Finally, I would like to see a systematic description of the basic knowledge of the field; we need to identify and describe the major empirical generalizations of learning, transfer, and retention. Twenty years age, so I am told, almost everyone in the field of learning knew whose laboratory was active, and he knew the basic findings of the various fields of learning. Today that is very difficult, and I think therefore that what we need are efforts which are directed toward trying to state our basic empirical generalizations. This was one of the things I had in mind when I wrote *The Transfer of Learning*. At that time I thought I saw seventeen empirical generalizations of transfer. I would like to see this kind of effort carried out as a part of an informational project for not only our students but for people directly involved in teaching.

• *Dr. Findley:* Thank you, Dr. Ellis. Dr. Wittrock, would you like the first response?

• *Dr. Wittrock:* I would like to make just one comment.

He and I want the same thing, in the sense that we both want some conceptual approach toward identifying what I call the conditions of transfer and what he calls a theory of how to sample the environment. My whole paper was focused on the conditions of transfer. What can we say to somebody, such as Bob Glaser, when he is writing a program and finds a child doesn't learn from his written materials? Bob needs to know something rather concrete about what stimuli (words, sequences, and variety) he might give that individual child.

When I look at the implications of different learning theories for this problem, I find that the theories are useful. A simplistic *S-R* theory implies that you might try reinforcing the behavior discriminatively and repeatedly. I can see Bob trying these implications.

But what else is there he might do? What could we offer him at different levels? We need something more specific in terms of models relevant to that kind of problem.

My response to you, Henry, is that we both want the same useful models. The question becomes, "How do we get them?" You seem to be saying that you don't know that having people generate new models will produce useful approaches. We might wind up with pointless controversies such as place learning versus response learning. As a result we could waste much time and energy.

I think that's a problem, but if we don't try to build particular models, such as those of Gagné, what would you suggest we do?

• *Dr. Ellis:* I would strongly suggest that we do just that. My only caution is that I would not like to see educational researchers placed in the position where they engage in disputes about the descriptive properties of their explanatory constructs. Rather I would like to see them get on with the business of trying an array of theoretical approaches. I would prefer to see people working at all levels conceptually, trying out a number of different approaches. I certainly want to see theory. I do not, however, want to see us make the mistakes that I think were made in the forties about theory. I would prefer to see theory being used as an effective tool in generating research and as a device for accounting for a large amount of data, rather than as a basis for what I would call chess-game-like disputes among psychologists.

• *Dr. Rothkopf:* It seems to me no one is speaking here for the devil, and I am willing to undertake this task.

It seems clear that much of what we call both positive and negative transfer of training in the laboratory also takes place in instructional situations. We see such all the time. But what isn't at all clear to me is how an understanding of this phenomenon, and I am really raising this as an honest question, contributes toward instructional decisions.

I think that Dr. Ellis' view is a wonderful example of the kind of theoretical romanticism that has characterized the calculus of practice approach. Basically he is saying, "Let's have a theory that would allow us to deductively elaborate what are essentially optimum sequences in a particular task." You mention such terms as "What's the minimum sample that we need in a particular instructional package in order to induce a particular concept?" You want to generate a general principle that can be generalized beyond the laboratory tasks from which they were obtained, and that can be applied to a great variety of tasks.

Well, one of the problems that one has to solve in order to make this conception work is how do we take the variables that you use in the laboratory experiments and dimensionalize and measure them in the instructional situation? Because in order to perform the same kind of theoretical calculations with the instructional material, we are going to have to identify the same types and classes of variables in the classroom as in the laboratory. This is where one study after another goes afoul.

For example, you can talk about stimulus similarity very nicely if you have neatly ordered stimuli in the laboratory, but you have a heck of a time doing this with some of the amorphous material used in school learning. While I understand that transfer of training is a fact of life in school learning, it is not at all clear to me that understanding this phenomenon necessarily helps us make instructional decisions. We need more than this simple understanding.

I think there prevails among those interested in putting psychology to work, particularly those interested in applying psychology to instructional problems, a kind of illusion, a grand illusion, about how theory is elaborated into practical decisions. We think that we are going to be like physicists and we are going to have an elegant way of elaborating some wonderfully simple principles into decisions about tremendously complicated problems. On the face of it, that view is doomed to failure.

First of all, school situations just don't lend themselves readily to prepackaging of solutions. You don't have the good control over activities most of the time. Programmed instruction was an attempt at creating a training package that would incorporate instructional decisions that presumably have come out of theory. Most of the time, however, it just isn't possible to do this. I believe that the stance of instructional theory, and probably of learning theory in general, should not be the stance of physics; it ought to be the stance of agriculture. That's roughly where we are. We are just getting out of the wooden-plow stage. We have only put a little metal edge on that thing, and we are going to make a little bigger scratch in the soil.

What we need, in instructional research, it seems to me, is an agricultural experimental station. We need something like Rothams', and not the Lawrence Radiation Laboratory, because we just don't have the kind of conceptual equipment in psychology right now that will permit us to generalize from the laboratory study into the theory involved with contralateral phenomena similar to the instruction set.

• *Dr. Ericksen:* I have read enough books and reports on learning, transfer, and teaching to have known better, but nevertheless I did make the mistake of attempting a classroom experiment as a direct extention and a test of concepts derived from place-response learning by rats. My deductive logical chain was sound enough, but the insignificant data reminded me once again that the transition from learning theory to teaching practice is, per se, a research-dependent problem. We have not conceptualized and attacked directly the problem of how education can utilize learning theory.

I am simply underlining the fact that as learning psychologists, we can and should make our analysis of theories at any point on the molecular-molar continuum. But, as educational psychologists we must accept the pragmatic purpose of finding how these theories can be used to support, for example, the arrangements we make to help students transfer information from site *A* to site *B*. Our new research programs should include some rather complicated experimental plans and will probably require us to sacrifice some of the purist rigor of the laboratory that we have always found so safe, secure, and comfortable.

On the other hand we must be equally wary of running away from data and evidence. Whenever I hear an educational psychologist say, "We need more theory," I think of the last sentence of many master's theses, namely, "More research is needed in this field"—a completely empty phrase. In essence, what I am saying is that I think we have a fairly substantial backlog of facts, concepts, procedures, and theories, etc., relating to how humans learn, and we also know a great deal about teaching and how to keep students striving to acquire knowledge. What we do not have is the know-how, the transitional procedures, that permit us to validate learning theory in the natural learning setting.

• *Dr. Ellis:* I note that Ernie's pessimism has not basically changed since yesterday. Yesterday we were told we cannot measure learning; today we are told that theories will not take us very far.

There are two points that Ernie makes and I shall comment on the first. This concerns the problem of how one generalizes from laboratory research to more complex situations. I think that I would be the first to admit that this seems like an overwhelmingly complicated problem, and if one is concerned with molar problems of education, then one may have to conceptualize at a very molar level. The point that I would like to emphasize is that the level at which one conceptualizes depends very much on the kind of learning task in which one is interested. If you want to work with the learning of language then you have to conceptualize at a complex level, and *S-R* theory may be of limited value. On the other hand, if one is going to deal with very simple kinds of phenomena, such as simple one-to-one associations that Merl talks about, then I suggest one see how much mileage he can get out of *S-R* theory before assuming that a more complex kind of theory is needed.

• *Dr. Findley:* I have to catch the 1:04 helicopter, and I need to bow out of this very fruitful discussion. I want to thank all for the privilege of being here, and simply say if I had been able to stay to the end of this session and to give you my parting benediction, I would have used Voltaire's final words in *Candide,* in which he says, after listening to Professor Pangloss, "Let us go out and cultivate the garden."

• *Dr. Gagné:* Thank you very much, Dr. Findley. I am sorry you have to run off from one of the hottest discussions that we have had.

• *Dr. Wittrock:* Ernie has set up a straw man, and has shown us that it is a straw man. I was not arguing for the kind of theory that he attributes either to Henry or to myself. As a matter of fact, I want quite a different kind of approach.

If you recall, in the paper I said there is something useful to educational research in simplistic *S-R* theory, and there is something useful in mediated generalization theory, but these approaches don't tell us many answers to complicated and specific problems. At this time we need some more specific conceptualizations of the particular problems we face, including identifying conditions, orders, sequences of stimuli to teach rules and generalizations. I think we need theory and conceptualization to ward off the dangers of a dust-bowl empiricism.

I am talking about an approach quite different from the straw man that Ernie has raised. I don't think we need any additional molar—let's not use that term—any additional theories growing from problems remotely related to the problems of instruction. I want us to start with the data and problems of research in instruction.

• *Dr. Gagné.* I should like to make a general observation that I think may be apparent to everyone, and with which I think everyone here would agree. It seems to me that most people on the panel do fundamentally believe that models and theories which attempt to predict economy in learning will work. At the same time I think it is also true that most people have been very impressed with Ernie Rothkopf's work on mathemagenic models, or a theory of how people learn to want to learn, wherein are very general strategies and techniques of learning. I just wanted to point out, although Ernie may disagree, that there really isn't any inherent conflict in these points of view. At least I don't think so.

CHAPTER FIVE

THE QUEST FOR PRESCRIPTIVE VALUES IN OUR EDUCATIONAL PROGRAMS[1]

BY

GEORGE G. THOMPSON

THE OHIO STATE UNIVERSITY

Planning and conducting research on the acquisition of attitudes and values is plagued by a series of complex problems. It is my conviction that these problems must be recognized and at least partially resolved before our research efforts can have any significant impact on educational policies and procedures. It is for this reason that my comments are directed toward what still needs to be done rather than toward a review of our extremely modest achievements to date.

Civilized man has always been anxious and uncertain about his choice of prescriptive guides for rearing and educating his children. Changing philosophical commitments and varying curricular emphases in our modern schools continue to document such uncertainties. Education in home and school has typically been a blend of what has gone before and what is anticipated for the future. Such eclectic mixtures of value positions are likely to have the sorry disadvantage of being internally inconsistent, but the considerable advantages of being somewhat easier to correct and of not being completely useless or outdated at any given time. History has

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taught us over and over again that child training and education must change with the needs and demands of the times. In other words, the search for *universal* values to further man's civilized state has become increasingly hopeless in the judgement of a succession of reflective scholars.

Only one value appears to be the necessary prerequisite for the human adventure—survival of homo sapiens with his unusual talents intact and relatively unfettered. All other values must change upon demand to make this possible, as aptly noted in the following observation by Glass (1965, p. 1255):

> Those who distrust science as a guide to conduct, whether individual or social, seem to overlook its pragmatic nature, or perhaps they scorn it for that very reason. Rightly understood, science can point out to us only probabilities of varying degrees of certainty. So, of course, do our eyes and ears, and so does our reason. What science can do for us that otherwise we may be too blind or self-willed to recognize is to help us to see that what is right enough for the individual may be wrong for him as a member of a social group, such as a family; that what is right for the family may be wrong for the nation; and that what is right for the nation may be wrong for the great brotherhood of man. Nor should one stop at that point. Man as a species is a member—only one of many members—of a terrestrial community and an even greater totality of life upon earth. Ultimately, what is right for man is what is right for the entire community of life on earth. If he wrecks that community, he destroys his own livelihood. In this sense, coëxistence is not only necessary but also right, and science can reveal to us the best ways to harbor our resources and to exploit our opportunities wisely.

To me these views highlight the basic value that must necessarily undergird all of our educational efforts. Glass further offers a belief, and one which I share, that science must point the way toward making all other values consistent with the fundamental prerequisite of man's survival in an environment favorable to his continuous emergence as a creatively resourceful intellect. And since science is in a continuous state of change, offering new advantages but with each advance posing new problems for other dimensions of civilized living, it seems reasonable to believe that our ancillary values must also be adjusted and corrected accordingly.

Philosophically and theoretically oriented psychologists have offered extremely divergent views as they attempt to depict man confronted with this very real threat to his survival in an era of almost unbelievably rapid scientific and technological change. Since our available knowledge about values and attitudes is closely related to these major philosophical positions, it may be desirable to review a sampling of the most extreme viewpoints and show how their inherent values have influenced our psychological research and, perhaps to a lesser extent, our pedagogical practices.

THE PESSIMISTIC VIEW OF MAN'S NATURE

Although there were many philosophical precursors of the pessimistic appraisal of man's basic nature, the most influential advocate was undoubtedly Sigmund Freud, whose writings have influenced the direction and scope of so much of our research on values. Indeed, psychoanalytic principles are by now inextricably interwoven into the whole fabric of Western civilization.

THE FREUDIAN VIEW OF VALUES

Freud's view of the origins of man's values has been clearly stated by his biographer Ernest Jones (1957, p. 307) as follows:

> Of one thing Freud was quite certain in this field [biology]: namely, that he could find no evidence of any instinct impelling man toward higher moral, ethical or spiritual aims, an idea which he termed a "benevolent illusion." "The present development of human beings requires, as it seems to me [Freud speaking], no different explanation from that of animals. What appears in a minority of human individuals as an untiring impulsion towards further perfection can easily be understood as a result of the instinctual repression upon which is based all that is most precious in human civilization."

Freud's presuppositions about the basic nature of man place a heavy burden on parents and educators who must tread the narrow and always uncertain path between too much restraint-coercion and too little affection, concern, and provision of resources for reality testing. Freud's constructs of identification and the superego have been most influential in guiding research studies related to the acquisition of human values.

Identification

Identification is a powerful conceptual and explanatory principle. If it did not exist, it would certainly have to be invented, because it is needed to explain the many apparent shortcuts in the child-rearing process, the means by which our young can learn from human example and implicit precepts and thus avoid the necessity of reality testing all of life's alternatives. The concept of identification has been variously interpreted by psychologists—Bronfenbrenner (1960), Kagan (1958), Sanford (1955), Sears (1965), and many others—but its dynamic significance remains elusive (cf. Kohlberg, 1963). Studies of social modeling by Bandura and Walters (1963) have shown that there are many variables that can influence children's tendencies to imitate the behavior patterns of others. The conceptual properties of identification will not be easy to partition from the complex of human relationships, but their potential significance for educational influence is manifold and difficult to overestimate. A thoughtful observer of the current scene cannot help but wonder whether the average teacher in our large and sometimes impersonal schools has not lost most of his, or her, effectiveness as a social model for the identification processes. It may well be that the most significant fruits of methods for automating parts of the instructional programs in our schools will be the freeing of the master teacher's time for more personal discussion with pupils, time to explore the troublesome value questions of our times.

Development of Conscience

Now a few comments are needed on the functions of the superego, mediated through conscience and the ego ideal. The agonizing pains of conscience have long been recognized in human experience, but are perhaps nowhere more poetically voiced than in Shakespeare's *Macbeth* in which tortured Macbeth, bowed down with the guilt of his monstrous crime, cries out in anguish (Act II, Scene 1):

> Whence is that knocking?
> How is't with me, when every noise appals me?
> What hands are here! Ha! They pluck out mine eyes.
> Will all great Neptune's ocean wash this blood
> Clean from my hand? No, this my hand will rather
> The multitudinous seas incarnadine,
> Making the green one red.

Freud needed such a conscience to act as a counter force to the base instinctive impulses which he attributed to the unsocialized infant and child. Again centuries of human experience have verified the existence of man's inner controls, restraints that operate independently of social forces, moral dictates that threaten their own forms of punishment for real or imagined transgressions.

What do we know about the development of conscience that might have implications for educational planning in the twentieth century? The extremes of control by conscience functions are not difficult to recognize. They range from the morally rudderless "psychopath" who for unknown reasons has failed to acquire the necessary inner controls, all the way to the guilt-ridden neurotic who is effectively immobilized by the expiative demands of his overly punitive conscience. Parents and educators would, of course, like to have their efforts foster conscience development somewhere between these socially undesirable extremes. The guidelines for such good intentions remain thin in psychological theory and empirical fact. The one recognizable and seemingly valid strand, drawn from the studies of such investigators as Redl (1952), the Gluecks (1950), and others, seems to have at its core the quality of the social interactions experienced by the growing child. All of the variables related to effective identification and social modeling seem necessary for the interiorization of a reasonable conscience: genuine affection that surpasses selfish concern, demonstrable love without possession, sharing of both triumphs and failures, and so on through the galaxy of human compassions known to reflective man.

Many educational philosophers have described education as, above all else, a morally oriented enterprise. If education should fail in this dimension of its mission, then the entire venture would come to a disastrous end. Children must become cognizant of the ends toward which their acquired skills may permit them to progress, and these ends must necessarily be defined as the common good. Since there seems no reasonable alternative to this philosophical dictum, the assignment for modern education seems obvious. We must find means for persuading our teachers that compassion, concern, and understanding take precedence over all other qualities in the educational process. Then we must find the means for helping society develop the type of teaching environment in which these sentiments can be effectively projected to pupils. Only under such conditions can education lead modern man from the wilderness, now made more dense and impenetrable by his own ingenious talents.

Science has provided, and promises to extend, some of the many skills necessary for effective and economical control of all natural phenomena. Now the critical question of our times is whether the scientific method can be used to help man select those goals consistent with the common good and thereby permit him to survive in a favorable environment. The horror of the alternatives demands a positive answer to this question.

The Ego Ideal

Equally important to conscience in superego functions, but much less discussed in theory and less frequently investigated by empirical methods, is the ego ideal. By this construct Freud formalized another commonly observed human experience, namely that man learns to set ideal goals for himself which serve as inner controls for continuously orienting him toward his own interiorized standards of excellence. Progress toward these goals yields feelings of self-fulfillment, self-worth, and occasionally what Maslow (1959) has described as peak experiences. If conscience could be described as the bit of restraint, then the ego ideal might be called the spur toward achievement.

In very broad terms, the "golden mean" of the ancient Greek philosophers is again relevant in considering the educational significance of nurturing the growth of ego-ideal functions. To foster ego ideals inconsistent with the pupil's psychological resources is wasteful. Relatively low ego ideals for the highly talented pupil leave him satisfied with achievements well below his capabilities; and society, as well as the individual, is the loser. High ego ideals for the pupil of limited resources often leads to demoralization and personal despair, expressed through resignation or continuous frustration, with again society, as well as the individual, being the loser.

Perhaps it is not surprising that American education makes its best showing in fostering reasonable ego ideals, since our culture is highly achievement-oriented and still dominated by a strong pragmatic spirit. We have been the world's leader in developing procedures for the assessment of individual talents. We have allocated a large share of our educational resources to the establishment of special curricula for pupils of varying levels of ability. And we are currently engaged in a much expanded program of training for school counselors whose major concern will be helping pupils to work within the range of their abilities.

Questions about Current Practices

Despite these favorable indicators, it is always within the American tradition to question whether we are doing enough of the right things according to the best procedures, thereby yielding a maximum of positive benefits with a minimum of deleterious effects. Only history can provide an unbiased answer to this question, but a review of some of the current doubts may be useful. The following are only a few of the debatable issues related to the ego ideals now being so strenuously encouraged in our schools:

1. In this age of science are we seducing too many of our youths into the sciences, which provide such obvious, tangible payoffs in technological advantages for a materialistically dedicated culture? Some critics charge that subsequent generations (if such there be) will view our time in history as one in which the arts and humanities languished and altruistically inspired social services were held in low esteem.
2. By our heavy stress on individual achievement and materialistic indicators of success are we choking off the personal satisfactions to be derived from nurturing the less fortunate, an activity that some humanists regard as the signal quality of civilized man? By institutionalizing our charity do we thereby lose the primary benefits of this activity, and so further dehumanize man whom many critics already regard as hanging by his fingernails to the cliff of human dignity to which automation and other technical advances have pushed him?
3. Are the testing procedures used by educators in their efforts to individualize curricula and instructional procedures a serious breach of the pupils' (and parents') privacy, an invasion of privacy so serious as to negate the benefits to be derived from such information? (Cf. *American Psychologist*, May, 1966.)
4. Do the ego ideals of individual achievement, which our modern schools admittedly attempt to instill in pupils, lead to a form of class snobbery, an intellectual elite among those who are able to play the academic game successfully, an elite that forces changes too rapidly for the successful adjustment of less intellectually advantaged individuals?

The foregoing are only a few of the questions that critics have raised about current educational policies related to their potential influence on pupils' ego ideals. Although it must be admitted that schools reflect to a large degree the commitments and aspirations of the supporting social-political culture, it is also easy to document the influence of educational leadership on a culture. The power for cultural change inherent in a series of interlocking ideals is almost unlimited if they can be presented in such a way as to gain popular support. History has shown us repeatedly that an idea can topple kingdoms, lead man to the heights, or hasten his decline to barbarism and self-destruction. So it seems to me that the basic question being raised by the critics is whether competitive achievement, defined according to nineteenth-century pragmatic standards, is still the most useful and valid guideline for our present educational policies. It would be foolish to take a strong position on either side of this issue, considering our relative ignorance of how man best relates to his fellow-men, but perhaps it would be equally foolhardy to plunge ahead without considering the consequences of what appears to be an increasingly expedited journey. It might well be that corrections and emphases of other values would be desirable during this period of our cultural evolvement.

THE OPTIMISTIC VIEW OF MAN'S NATURE

Having viewed briefly some of the value issues for education that seem to be inherent in the psychoanalytic view of the nature of man, now let us take a look, again necessarily incomplete, at these same issues from what I shall call "the optimistic view of man's nature."

Although this philosophical perspective is certainly not the most popular in the American culture, it appears to be gaining influence, perhaps as a cultural counteractive to the extreme polarity of pessimism. The eighteenth-century philosopher Rousseau was one of the most vigorous propounders of the optimistic view. The following short quotations give something of the flavor of Rousseau's (1911 translation) convictions:

> God makes all things good; man meddles with them and they become evil. He forces one soil to yield the products of another; one tree to bear another's fruit. (p. 5)

> Give your scholar no verbal lessons; he should be taught by experience alone; never punish him, for he does not know what it is to do wrong; never make him say, "Forgive me," for he does not know how to do you

wrong. Wholly unmoral in his actions, he can do nothing morally wrong, and he deserves neither punishment nor reproof. (p. 56)

May I venture at this point to state the greatest, the most important, the most useful rule of education? It is: Do not save time, but lose it . . . the education of the earliest years should be merely negative. It consists, not in teaching virtue or truth, but in preserving the heart from vice and from the spirit of error. If only you could let well enough alone, and get others to follow your example: if you could bring your scholar to the age of twelve strong and healthy, but unable to tell his right hand from his left, the eyes of his understanding would be open to reason as soon as you began to teach him. Free from prejudice and free from habits, there would be nothing in him to counteract the effects of your labours. In your hands he would soon become the wisest of man; by doing nothing to begin with, you would end with a prodigy of education. (p. 58)

A Contemporary Viewpoint

As a contemporary representative of the optimist's view, I have selected the American psychologist Carl Rogers as the most influential spokesman. Rogers (1964) proposes "the possibility of universal human value directions *emerging* from the experiencing of the human organism." He believes that evidence from therapy indicates "that both personal and social values emerge as natural, and experienced, when the individual is close to his own organismic valuing process." He suggests that "though modern man no longer trusts religion or science or philosophy nor any system of beliefs to *give* him values, he may find an organismic valuing base within himself which, if he can learn again to be in touch with it, will prove to be an organized, adaptive, and social approach to the perplexing value issues which face all of us."

Rogers proposes that the human infant's values are based on the hedonistic outcomes of his sensory experiences, but from his acculturation "he gradually learns that what 'feels good' is often 'bad' in the eyes of significant others. Then the next step occurs, in which he comes to take the same attitude toward himself which these others have taken. . . . He (introjects) the value judgement of another, taking it as his own. To that degree he loses touch with his own organismic valuing process. He has deserted the wisdom of his organism, giving up the locus of evaluation, and is trying to behave in terms of values set by another, in order to hold love."

The distorting and often deleterious effects of cultural intervention in a natural organismic growth process is forcefully proposed in the following statement by Rogers in a 1964 publication (p. 163):

> I believe that this picture of the individual, with values most introjected, held as fixed concepts, rarely examined or tested, is the picture of most of us. By taking over the conceptions of others as our own, we lose contact with the potential wisdom of our own functioning, and lose confidence in ourselves. Since these value constructs are often sharply at variance with what is going on in our own experiencing, we have in a very basic way divorced ourselves from ourselves, and this accounts for much of modern strain and insecurity. This fundamental discrepancy between the individual's concept and what he is actually experiencing, between the intellectual structure of his values and the valuing process going on unrecognized within—this is a part of the fundamental estrangement of modern man from himself.

From the results of therapy with adults in cultures as divergent as the United States, Holland, France, and Japan, Rogers believes there is good and ample evidence to support the following position (1964, p. 166):

> I believe that when the human being is inwardly free to choose whatever he deeply values, he tends to value those objects, experiences, and goals which make for his own survival, growth, and development, and for the survival and development of others. I hypothesize that it is *characteristic* of the human organism to prefer such actualizing and socialized goals when he is exposed to a growth promoting climate.

Some Implications of the Optimistic View

The educational implications of this and other variant optimistic views of the basic nature of man seem very similar to the philosophical suggestions of Rousseau. I interpret them as a directive for more emphasis on *education* (a leading from the learner) and less emphasis on *instruction* (a building into the learner). This, of course, is a plea for return to something like the Socratic, or individual-experiencing, approach to education. Critics of this point of view would undoubtedly say that education according to this philosophy would be too expensive and too time consuming, that training by instructional procedures is a necessity in a modern technological society, that some individuals, at least, simply *have* to be told what to do and how to think so that we can maintain our thrust toward tomorrow's brave, new world. I suppose that the optimistic viewer would reply that little is gained from our rapid strides into the vast universe of knowledge and technical skills if man is estranged from his human qualities during the journey. Or wouldn't it be better and more humane, to pay the price now and take the necessary time during the formative years of childhood rather than simply to delay the payment until the later years of man's allotted span when it must be made anyway through the construction and maintenance of institutions and services for the incorrigible and the mentally ill?

Extreme philosophical positions are infrequently all correct or true, but then they are also seldom all wrong. Usually they call for a compromise, or a corrective, in customary cultural conditions. The optimistic view of man's nature and its inherent educational values, proposed as long ago as the ancient Greeks (cf. Jowett, 1892) and persisting as a recurring theme for 2,500 years, has had only a negligible influence on modern education. Whether or not this is desirable or undesirable probably should be re-examined by each generation with its available methods of inquiry. Although the social sciences have as yet only a modest array of scientific procedures, perhaps they are adequate for a first appraisal where the findings support useful reforms in some of our educational policies.

There are some interesting parallels and differences between Rogers' theses and those of Jean Piaget (1932), whose writings on the moral and intellectual development of children have become in recent years extremely influential on both developmental psychology and educational experimentation in this country. In his early investigations of the development of moral judgement Piaget defended the thesis that the shift from contralateral restraints to autonomous control was a natural organismic process, entirely consistent with other dimensions of the child's psychological growth. It seems explicit in Piaget's theorizing, and it may be implicit in Rogers' position, that the young child must introject the values of significant others, albeit in distorted and rigid form, before he can move on to the decentered abstractions that permit autonomous value decisions. Kohlberg's (1958) evidence for the sequential patterning of moral development—from Type I (punishment and obedience orientation) through Type VI (morality of individual principles of conscience)—provides support for Piaget's position, although one must as yet reserve judgment about the universality of this developmental progression across all cultures. Perhaps parents and teachers must be dogmatic in answering children's questions about values in order that they may acquire the psychological skills for making their own choices and decisions in later life, or perhaps this hypothesis is only a rationalization of our unwillingness, or inability, to test the limits of human potentiality in home and school.

THE SUSPENDED JUDGMENT VIEW OF MAN'S NATURE

Adherents to this point of view regard man as neither inherently social or asocial, nurturant or aggressively destructive, or any other extreme of possible antitheses. Some proponents of the suspended-judgment view of man's nature regard such sweeping propositions as scientifically meaningless and of only metaphysical interest. Other members believe that man is merely an evolving organism who happened to be favored in the evolutionary process by the development of an exceedingly complex central nervous system and precise thumb-forefinger dexterity for the fashioning and using of tools. To others, inquiry or speculation about the basic nature of man is nothing more than a pseudo-intellectual activity, an interesting but pragmatically useless game played by philosophers. Others might even be called a "no-judgment group" because of a lack of interest. Whatever their particular inclination, members of the suspended-judgment group are inclined toward the empirical, pragmatic, experimental point of view with respect to values and their proper role in the educational-instructional-training process through which the culture transmits what is known to succeeding generations.

The activities of this group, which incidentally includes the vast majority of American psychologists and educators, may be described as a continuous cycle of hypothesize-experiment-theorize→ hypothesize-experiment-theorize, etc. A basic assumption is that no final decisions on the values most favorably related to man's welfare can ever be made, or even conceptualized, because an ever expanding knowledge and an ever increasing control over the variables related to man's many roles in a changing universe bring new problems and the need for different value decisions to each generation.

A Behavioral Science View

The members of this group are largely disinterested in values. They are preoccupied with the search for reliable and useful information about antecedent-consequent relationships in nature of any and all types. They assume the universality of those values that are required for diligent and precise observation and an "honest" reporting of findings, but even here they are not optimistic as indicated by the frequent replication of others' investigations. This is the "pure" science approach that has paid off so handsomely in the physical sciences. An early spokesman for a parallel view in the social sciences related to child rearing was the American psychologist John B. Watson (1928) who wrote as follows:

> After this brief survey of the psychological care of infant and child the behaviorist hastens to admit that he has no "ideals" for bringing up children. He does not know how the ideal child should be brought up.
>
> As a matter of fact there are as many ways of bringing up the child as there are civilizations. (p. 184)

> There is no ideal system of civilization—there are only actual civilizations, hence the child must be brought up along practical lines to fit a given civilization. (p. 185)

> Above all, we have tried to create a problem-solving child. We believe that a problem-solving technique (which can be trained) plus boundless absorption in activity (which can also be trained) are behavioristic factors which have worked in many civilizations of the past and which, as far as we can judge, will work equally well in most types of civilizations that are likely to confront us in the future. (pp. 186-87)

An influential contemporary spokesman for this pragmatic view is B. F. Skinner who has interpreted the influences of the behavioral sciences on cultural values in the following manner (1953):

> Perhaps the greatest contribution which a science of behavior may make to the evaluation of cultural practices is an insistence upon experimentation. We have no reason to suppose that any cultural practice is always right or wrong according to some principle or value regardless of the circumstances or that anyone can at any given time make an absolute evaluation of its survival value. So long as this is recognized, we are less likely to seize upon the hard and fast answer as an escape from indecision, and we are more likely to continue to modify cultural design in order to test the consequences.
>
> Science helps us in deciding between alternative courses of action by making past consequences effective in determining future conduct. Although no one course of action may be exclusively dictated by scientific experience, the existence of any scientific parallel, no matter how sketchy, will make it somewhat more likely that the more profitable of two courses will be taken. To those who are accustomed to evaluating a culture in terms of absolute principles, this may seem inadequate. But it appears to be the best we can do. The formalized experience of science, added to the practical ex-

perience of the individual in a complex set of circumstances, offers the best basis for effective action. What is left is not the realm of the value judgment; it is the realm of *guessing*. When we do not know, we guess. Science does not eliminate guessing, but by narrowing the field of alternative courses of action, it helps us to guess more effectively. (p. 436)

Scientists and professionals who fall into this group (and may I repeat, they constitute a majority in the American culture) are not valueless. They admire and pursue knowledge and individual competence. They gladly offer scientific findings and technical inventions to their fellow-men for whatever use may be made of them, but in their personal roles as scientists they are reluctant to make prescriptive statements about how things ought to be or what should be done. They often consent to work on problems related to the control of nature, but they largely leave the identification and definition of what constitutes a "problem" to non-scientists.

Whether or not scientists in this group should be more interested in human destiny and the ultimate goals of civilized man is a prescriptive question, hence unanswerable as a "pure" science proposition. Perhaps the question will only become important if the many attractions of science siphon off the best of intellectually talented individuals so that the value decisions are left solely in the hands of less gifted citizens. Under these conditions the privileges of scientists might be seriously threatened. Some well-known social scientists of today are beginning to wonder if we aren't overselling the benefits of a life in science to our pupils at the expense of other important contributions to human welfare. At the present time it would appear that public service, including education, is gradually losing in the competition for the most talented recruits. Whether or not automation and other more economical methods of conducting human affairs can compensate for this loss may not become known in the time of our generation.

HUMAN VALUES AND EDUCATIONAL ISSUES

Having reviewed these major philosophical viewpoints, I should like now to make some bold though hypothetical assertions about valuing behavior as it is influenced by our American schools and as it is described in our social sciences.

1. Our culture is currently confused and uncertain about what values should be nurtured in its schools. (It may be noted

that lack of clarity in educational goals proved to be a serious stumbling block to the development of a taxonomy of educational objectives in the affective, or values, domain. See Krathwohl, Bloom, & Masia, 1964.)

2. Our publicly supported schools reflect this confusion by frequently making drastic revisions in their values orientation when confronted with lay criticism.
3. Many of the values stressed in our schools (both public and parochial) are confusing to the pupils because they are antithetical, or at least unrelated to, the values stressed in other parts of the cultural environment.
4. We do not have the necessary instrumentation, or any very promising prospects for securing the same, to evaluate the schools' influence on human values (cf. Thurstone, 1959; Gardner & Thompson, 1963).
5. Although we assume that values are acquired according to the learning principles that describe other dimensions of human behavior, we have scanty data to support the degree of their relevance or adequacy (cf. Hill, 1960; Tolman, 1951).
6. Although civilized man has developed an extremely useful logic for interrelating and rationalizing concepts about his physical environment, he has as yet failed to devise a comparably useful logic for symbolizing and rationalizing human values. The latter appear to be dominated by disjunctive, as contrasted with conjunctive, concepts (Thompson, 1963).
7. Last, and perhaps of greatest importance: the majority in our culture has centered on pragmatism with its corollaries of economy, efficiency, and expediency, to such a strong degree that there is only mild interest among our scholars and statesmen in examining, or experimenting with, alternative interpretations of the human adventure.

An additional comment may be in order at this point as to how research on the learning of values may be most fruitfully pursued. First of all, I believe that the learning of values and their related logic is a distinctly social phenomenon which can most profitably be studied in social situations in which the learner relates to highly significant others, i.e., persons with whom he has interacted frequently and "affectively" in the past. This belief persuades me toward the following research approaches as most promising at the present time: (a) naturalistic studies—of which we have had surprisingly few in view of the almost universally adjudged importance of the topic, and (b) more creatively designed experimental studies that would involve parents, teachers, and selected peers of the learner and would extend over longer periods of time, i.e., weeks and months instead of hours and days.

In the conduct of such studies, I would especially direct any investigator's attention to the potential significance of the variant conceptual properties of identification and vicarious reinforcement. The latter, it seems to me, has been particularly neglected in studies of social learning. We know very little more about this concept than did Mark Twain when he wrote the engaging story of how Tom Sawyer got his friends to do his work for him by feigning enjoyment in the task. I believe that research and theoretical constructions related to vicarious reinforcement could bring us considerably closer to an understanding of the more subtle and psychologically important aspects of social influence and the related acquisition of values. Although I intuitively favor experimental procedures of social simulation for investigating the effects of the presently obvious variables, I recommend that equal attention be given to naturalistic studies whose primary aim would be the search for hitherto unrecognized variables and the development of new hypotheses.

What Is Needed

It seems to me that the following actions might be helpful in bringing values back to the fore of human concern:

1. We should educate our parents and citizens to accept sustained programs of experimentation related to the transmission of different patterns of human values.
2. We should encourage the social sciences to develop new instruments for evaluating the immediate and long-range effects of different educational programs on the development of valuing behavior.
3. We should encourage psychologists to identify those variables most significantly related to the acquisition of valuing behavior and prescriptive judgments.
4. We should seek new educational procedures for helping boys and girls to deal more effectively with the disjunctive concepts and the probabilistic logic that characterize valuing behavior.

I have phrased my recommendations for what is needed in the imperative form, because I believe that the hour is late and that man has no visible alternative but an earnest appeal to the social sciences and to the professional disciplines concerned with human welfare. It seems to me that events which take place now have their effects too rapidly for man to rely solely on solving social problems as they arise. If he is to survive, he must learn to anticipate the consequences of his value judgments and decisions well in advance of their execution. The future of man lies, as of course it always has, in the appropriateness and effectiveness of his educational resources.

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AUDIENCE QUESTIONS

• *Question:* By your last statement are you implying that we do need prescriptive values to anticipate the consequences of value judgments and decisions?

• *Dr. Thompson:* I think we need to recognize those prescriptive values by which we are now living, which I don't think we now do, speaking for adults as well as for children. Then, if we did recognize these values, we could make modifications in them as the culture shifts.

I was impressed the other day by a story in a British paper in which the Anglican Church was reporting on a meeting on what attitudes they should have about certain modern sex practices. They decided that they should hold generally to prescriptive guides that the history of man has shown to be most useful, but they would give a little, too, here and there. In other words, the only way that a prescriptive guide can remain viable is that it must give a little.

Now there is the old question: When are you giving so much that you give up? I don't know the answer to that. I don't think anyone does. That is the reason, I suppose, that civilizations rise and fall, as people fail to make a distinction between when they are giving a little and when they are giving up what is necessary for them to survive.

• *Question:* You are saying the criterion measure would be somewhere in survival values?

• *Dr. Thompson:* Survival is the only criterion. I have talked with many philosophers on that, having been interested in the topic for a good many years. It is the only one you can come back to. It is sort of like children playing a game, you know. As long as you keep the game going, that's one thing. But when one youngster gets the football, puts it under his arm and goes home, that's the end of it. The game is over—at least that particular game. As far as I can see, that's the only criterion value we can fall back on; however, there may be others that we will discover. There are some people in other disciplines besides psychology who courageously postulate some of these other values that may be characteristic of man, such as mother love. On the latter we get shaken up in psychology, you know, when we show that we can manipulate the mother rat's litter environment so that she won't nurture her newborn pups. So what is built into man and what isn't, I don't know. I am convinced that man does have a strong need to survive.

• *Dr. Gagné:* I am sure, regardless of differences in opinion about values, how they are derived as well as how they may be established, we all tend to agree that how values are established in the individual is an important educational problem. I am sure you can see also it contrasts somewhat with the kinds of intellectual skills and knowledges discussed earlier in this symposium. This contrast raises the question as to how relevant are various conceptions of the learning process to the whole problem of the learning of values. If relevance is established or accepted, another series of questions is indicated. I believe a number of these questions will come out as the discussion proceeds.

DISCUSSION OF GEORGE G. THOMPSON'S PAPER

MODERATOR: Dr. Eric F. Gardner
FIRST DISCUSSANT: Dr. Winfred F. Hill

• *Dr. Gardner:* Since the conference is drawing to a close and you have already heard words of wisdom from a variety of sources and, furthermore, since I am the sort of person who can tell the most humorous joke imaginable and not even draw a smile, I propose that we continue with the business of the afternoon. Dr. Hill.

• *Dr. Hill:* First I want to say what an impressive job Dr. Thompson has done in highlighting the conflicts in the area of values that characterize not only our educational system but our whole society. Because parents are uncertain about the proper values to instill in their children, more and more of the burden of this aspect of education falls on the schools. The schools, however, are just as uncertain as the parents, and so are confused in their approach and vulnerable to pressures and fads. Ideal values compete with practical working values, traditional values with emerging ones, and specific values within each category compete with one another. Moreover, as Dr. Thompson so well explains, these conflicts are present in our psychological theories as much as in the general culture, thus weakening our ability as supposed experts to advise the parents and teachers who seek our aid.

I am reminded of what Jane Loevinger wrote a few years ago, at most only partly in jest, about the futility of using learning theory to raise children. The theories make conflicting predictions, and since all the theories are partly right, when you do what is best in terms of one learning process, you are doing something wrong according to another. Thus, as Dr. Thompson indicates, we are unable to speak with a clear voice about the problems of values.

Given this difficulty, and given that it is not likely to be solved in the near future, if indeed ever, given the rate at which the world is changing, carrying our values with it, Dr. Thompson gives two excellent pieces of advice: that our education should be pragmatic rather than absolutist, geared to a world of probabilities rather than certainties, and that more research should be done on the problem of values.

However, it appears to me that Dr. Thompson is a bit ambivalent about both of these recommendations, and I would like to explore *this* a little further. On one hand, he began his paper by approvingly quoting Bentley Glass on the value of science and its pragmatic approach, and he ended the paper with a call for more research and for more education in probabilistic logic. On the other hand, throughout the paper he expressed fears that the recruitment of scientists is draining off too many of the best brains from non-scientific pursuits, and that the pragmatic approach is overwhelming other approaches, to the general detriment.

I am curious to what extent there is a true ambivalence here, and to what extent we merely have a problem in semantics. When Dr. Thompson worries that we have too many scientists but too little science, perhaps he means that we have too many narrow specialists in the physical sciences but too little scientific research and scientific attitude in education. Likewise, perhaps the kind of pragmatism he wants less of is that which seeks narrow, selfish goals without regard to the general welfare, and the kind of pragmatism he wants more of is that which uses any available means to the general welfare. Yet I cannot help wondering if he does not feel, as do many other people, that the whole scientific pragmatic approach is a mixed blessing, a gigantic and unruly servant that we can neither get along well with nor without. If I have caught the spirit of his treatment correctly, I wonder if some of our discussion ought not to focus on this ambivalence. I realize that this may be a suggestion to open Pandora's box, letting out all the difficult issues of the relations between values and facts, between means and ends, between science and humanities and other such philosophical dichotomies. Nevertheless, I think it is too basic an issue in our culture today, both among educators and social scientists and among the lay public, not to bring it at least clearly into the open.

While we are on the subject of ambivalence, I should like to discuss another issue that he raised: the positive or negative value of competitive achievement. Although I have never systematically studied the history of education, and I feel rather out of place here because I know so little about education, my guess would be that if you were writing a history of American education you could take this ambivalence about competitive achievement about as well as any other theme as a central one upon which to build your history. I see repeated swings of the pendulum: from the traditional education of the gentleman with a low emphasis on competitive achievement—he was already where he wanted to be—to a high emphasis on competitive achievement for the upward-mobile bourgeois or would-be bourgeois; then back again with progressive education to a low emphasis on competitive achievement—the important thing being social adjustment and individual satisfaction in special forms of creativity—back again, as mentioned, with Sputnik, to the emphasis again on high achievement and "We can't afford to indulge our own satisfactions. We have to produce to save America from the Russians." Right now we can cite McClelland as somebody who is putting a lot of emphasis on the pro-achievement side of things. On the other hand, we don't have to go any farther away than this big campus here to find those who are claiming that it is all achievement and nothing else, and that is what is dehumanizing education.

As with the problem of science and pragmatism, this conflict is a reflection of the conflicts within the entire culture. Perhaps the lesson that we as educators are maybe even more troubled about than other people is that we seem to have reached a point at which school achievement is the main road, perhaps the only road, to vocational success and social status. At one moment we as educators are frightened by the awesome responsibility of determining which students will make the grade in life and which will not, and we wonder whether the things we teach and the examinations we give deserve to be so influential. Then, at the next moment, we look at it from another point of view and begin to suspect, much more humbly, that in our gatekeeper-role we are merely rubber-stamping selections that have already been made by the status of the children's homes, both by their intellectual push and by their economic and social pull. Then we begin to feel humiliated and we wonder what we can do to make our teaching more influential.

However much we may worry about whether we can afford to have or not have an ideal of competitive achievement, the question is not likely to be answered primarily by either educators or psychologists. If the present affluence of our society continues and if international tensions cool, we may find the single criterion of academic excellence being more and more replaced by a multicentric achievement system, in which success at flower-gardening, bridge-playing, or ping-pong is as highly valued as success on the academic-vocational ladder. I suspect competitive achievement of one form or another will always be with us, but we may perhaps see it in a less emotionally taxing form than it seems now to be.

Although I feel that these philosophical issues of science and pragmatism and of competitive achievement deserve consideration, they are not the areas in which we are likely to make the greatest contributions at this conference. So, to counteract these abstractions, let me now turn to more specific matters of teaching and research.

Dr. Thompson's presentation focused mainly on the larger theoretical issues, reflecting his conviction that at the level of specific applications we have as yet little to offer. He did, however, offer some specific suggestions, and I feel that it is in connection with them that we can probably make the greatest contribution today.

I second heartily Dr. Thompson's suggestion for research guided by learning theory on the learning of values. Both naturalistic studies and long-term experiments should be of great value in determining to what extent our various ideas about the learning of values are correct. In this connection I would like to call attention to the fine contribution of methodology that Campbell and Stanley have made in bringing together a variety of experimental and quasi-experimental designs for use in hard-to-control research situations and in evaluating the strengths and weaknesses of each design. As we get away from the neat designs of conventional experimentation into less artificial situations, the need for well-informed use of these other designs will become greater, if our high research aspirations are not to flounder on methodological flaws.

I also second Dr. Thompson's emphasis on vicarious reinforcement as a particularly important topic for investigation. Not only do we need to know much more about the conditions under which vicarious reinforcement operates (as well as modeling without any known reinforcement), but also how vicarious reinforcement, direct reinforcement, and verbal instructions interact as determinants of behavior. The importance of studying the results of conflict among influences is highlighted by those competitions among values that Dr. Thompson has discussed.

I am a little more—I am not sure I am much more—optimistic than Dr. Thompson, but at least I am more willing to say that the kind of research that is going on in the experiments of Aronfreed, Bandura, Mischal, and other people represents substantial progress toward an understanding of modeling, even though it certainly is not answering the really complex questions about what we mean by identification in the highest human sense. They, at least, are giving us a start in the direction of seeing some of the mechanisms that are involved in that sort of learning. I think we need more research of this sort, and I agree with Dr. Thompson that it becomes better as it is extended into longer-range studies.

There are two fairly minor points of Dr. Thompson that I would like to quibble about. One of these is the point that values are primarily concerned with disjunctive concepts. A little of my reaction to this may be my prejudice that any real, honest-to-goodness concepts should be capable of being stated in conjunctive form. There is something a little peculiar about a disjunctive concept anyway. I would like him to clarify what it is about values that makes them more a matter of disjunctive concepts than otherwise.

The other minor point is his suggestion that the teacher may have lost whatever importance he once had as a model for identification. Here I have mixed reactions. His concern with the teacher as a model is well taken. As our culture comes to speak less and less with one voice, and as the home becomes less and less a neatly simple focus of life, the importance of role models outside the home becomes greater and greater. In principle teachers have a great opportunity to serve as models, and whether in fact they do so or not is a very important question. I have some doubt whether they do so less now than they used to. Once we get beyond the one-room schoolhouse, I don't see why the teacher's opportunity to serve as a model should depend on whether the school is large or small or whether it keeps its records on IBM cards or not. And, if we do go back to that proverbial little old red schoolhouse, do we find that the Hoosier schoolmaster or the prissy "schoolmarm" really was an acceptable model for more than a very small proportion of the students? I rather doubt it.

There may be many reasons why teachers are less effective as models than either we or they would like them to be. I question to what extent the size or impersonality of the modern school is among them. I hasten to point out I am talking about the school as distinguished from the university for the sake of anyone from Berkeley or other campuses. However, I speak from considerable ignorance about the history and sociology of education. Perhaps I am blissfully ignorant of just how impersonal a modern school can be, so I will welcome clarification on this point.

It is impossible, of course, to do justice in a brief discussion to the many issues Dr. Thompson has raised, but I hope I have succeeded in providing his values about values both with reinforcement and with a few suggestions toward possible cognitive reconstructing.

• *Dr. Gardner:* Thank you very much, Professor Hill. Now, George, would you care to react to some of the comments and questions that Win has raised about your paper?

• *Dr. Thompson:* I think this observation about my ambivalence over so many people going into science is a true one. Maybe most of us feel this way, sort of caught, that we should go into science. For example, medical sciences may save lives so people should be encouraged to go into them. My ambivalence comes in because I further feel that if a man doesn't have the arts and humanities then there is very little for him to be living for. So I guess that my ambivalence shows up in a value sense. I have a feeling that science is getting the better of the humanities at the present time.

I think guidance counselors and parents are influencing this trend, because it is so easy to go to a good-paying position in the sciences or engineering, whereas to go into one of the arts makes the future a lot less certain. I personally wouldn't want to live in a world in which the arts didn't flourish. I guess it is a true ambivalence in that sense.

On the disjunctive concept, as I cited in a footnote, Spinoza tried to use conjunctive concepts to handle moral and ethical ideas, and his work shows that it just doesn't work. In my many hours of interviewing groups of adolescent youngsters disjunctive concepts always come in for a great deal of play. Suppose you present a problem: "Your seatmate asks you to cheat for him on an examination. What is the right thing to do?" And adolescents will use disjunctive concepts which are perfectly internally consistent, at least for the moment, with which they support their particular decisions. All the necessary conditions for conjunctive concepts aren't there, such as we must have for defining a triangle, but part of them can be at one time and another part of them can be at another time.

We need training in the use of disjunctive concepts; almost all of our training goes in the other direction. We need some kind of training to serve as guides for when our children should tell the truth, because they see plenty of evidence that there are times in which it is inhumane to tell the truth, the so-called brutal truth, or instances in which their parents don't quite tell the whole truth. Some experience in handling these kinds of disjunctive concepts seems to me important.

Now, on the size of the school and identification, I'm an old Kansas farmboy, and like Roger Barker, am enamored with the small community and the small school. It seems to me that my feeling is related to the sort of thing that Leon Festinger showed in his housing research; when things get big you don't see certain other persons so often. In a very large school you don't see certain students so often. You may know almost as much about them, but you lack a kind of casual information you pick up in a smaller group. I am not a Goldwaterite in the sense of wanting to return to the one-room school, or anything like that. We need to break down the large school into smaller functioning units so that there is some part of it with which pupils and parents can identify, and where the probability of the principal, the supervisor, and the teacher just running casually into one of the pupils might be increased.

Life is complex, and it's going to get more complex. I have no doubt about that. Maybe I am just nostalgic about the times when it was less complex, but I won't accept what appears inevitable until man uses his ingenuity to set up school buildings and school programs that can make them a lot more personal than they are now.

For example, take the matter of shifting populations in city school districts. At first they may be heavily populated and then residences are torn down, factories built, and so forth. We should build less stately mausoleums so we could shift our schools and make them more flexible, all designed so that we can have more of this personal contact. I think this is one of the things Carl Rogers is talking about: Man doesn't feel that he belongs; he does feel estranged from his fellow-man, and even from himself. This may be one of the necessary prices for large population concentrations, but as a scientist I am not willing to accept it yet.

SECOND DISCUSSANT: Dr. Paul H. Mussen

• *Dr. Mussen:* I find Professor Thompson's paper most interesting, and addressed more to the basic question, what is the human experience all about, than to the question of approaches to research aimed at understanding the processes of learning in the schools. I make this point because I want to come back to it very shortly. I think that he is expressing some dissatisfaction that we as educators and psychologists aren't taking a lead in establishing values. Most of us would agree that we don't want the responsibility of establishing values, dissatisfied as we may be with some of what seem to be values of the society.

On the other hand it isn't entirely a bleak picture. I think most of us would agree that we would like to foster the values associated with what Professor Thompson calls the optimistic view of human nature, that is the surviving, making optimal use of each individual's talents, allowing for maximum amounts of self-expression consistent with the common good, greater emphasis on the richer and more fully satisfying personal experience as we go through this vale of tears. But I don't think we want to go much further than this in establishing values and then acting in ways to inculcate these values. We are no better qualified as educators and psychologists to do this than any other intelligent person is. We have no unique training qualifying us, so far as I can see, to establish values for others.

But if we could, even if we could, do this, what should the role of the school be in inculcating values? Let's say that we accept, that we want, positive values of the sort I have just mentioned. Professor Thompson seems to be afraid, and perhaps properly, that the emphasis on the learning of skills and the mastery of domains of knowledge may result in an underemphasis on the acquisition of positive and important values, particularly those related to interpersonal relations and to an inner sense of fulfillment.

I don't think he is right in this fear. At least I don't have it as much as he does. First, I think that we have to admit the limitations of the school in this area. The early, very complex interactions the child has at home are much more important for the learning of values than they are for learning of intellectual skills. By the time the child is enrolled in school he already has a pretty potent backlog of attitudes toward others, toward himself, toward acting out or controlling, toward generosity and consideration of others, et cetera. There is a real question about how much school experiences can change the values already established.

If the child's home situation, if his early experiences, are such that he has acquired, or is on the road to acquiring, many of the values that we would like him to have, then the school has only to act in ways that reinforce that which he acquires at home. That is, the school then has to avoid presenting the child with situations and experiences that counteract the influences of the home.

If he fails to acquire these values at home, then what role can the school play to develop the values we would like the child to have? It seems to me that the new work in educational psychology that we have been discussing here actually provides a basis for creating a school situation which is much more likely to result in the acquisition of positive values, to reinforce them where they have been acquired, and to foster acquisition where they haven't been acquired at all.

First of all, the recent advances in our understanding of learning in the schools seem to me to lead to greater efficiency in the educational process. Implied in this efficiency is making the school experience a more rewarding one for many, many children. If the school experience is a better one, that is, less fraught with frustration for the pupil and the teacher, if it is more pleasant, more relaxed, then you have created a basis for better interactions between the teacher and the child. You have created a situation that makes for the reduction of tension and aggression between teacher and pupil and between pupil and pupil. This reduction of tension, if one accepts a Rogers' notion of positive growth force, should then stimulate the actualizing of socialized goals and values which we see as desirable. Thus, the new systems that are being developed on the basis of research such as we have been discussing here the last two days have some built-in ways of promoting positive values. For example, to be quite specific, automated programmed learning may have an emphasis on discovery, an emphasis on achievement based on personal satisfaction rather than a dog-eat-dog sort of competitive achievement. There may be satisfactions from this kind of learning resulting from recent research that make for a much more positive set of values.

I think that the recent research is as much geared to seeing the limitations of what we can do as it is to push, push, push! Professor Thompson is concerned with the possible pushing and competitive achievement aspects of modern education. I think it is a relevant concern, but when you get research showing at what ages children are most capable of achieving the various objectives the school holds, then you can build programs which cut down the pushiness. You can gear curricular programs to the scientific data on when it is best to teach what. I think that the recent research helps us to achieve positive goals better, through learning of content, and at the same time a learning of ways of approaching problems without undermining the child's morale and adjustment.

Certainly there is need for help for children who do not learn, who cannot learn easily. Professor Thompson is concerned with the overautomation, the lack of individuality. I would like here to call attention to a recent thrust in what is being called community psychology, work such as that of Sarason at Yale and Cowan and a co-worker here at California, in which without much disruption of curriculum teachers are taught to be, in a minor way, diagnosticians of learning problems. The results of Sarason's work I think are quite compelling. Teachers can do things in the course of their day-to-day activity which makes the situation somewhat therapeutic, and thus free the children for the kind of self-fulfillment and growth-promoting experiences with which we may be concerned.

We have been talking about the school as a place which simply allows the child to express himself and, if one accepts an optimistic view, thus to behave in more socially desirable ways. Can the school do much to change values if the proper values aren't acquired elsewhere? I think that the answer can be "Yes," although the evidence here isn't strong. I want again to state that I think what it has been shown to be able to do is limited. Hopefully it can do more. Here the concern with the emphasis on science and disciplines related to science is an important concern.

It is true that the programs, the experimental programs with respect to science and mathematics, have been better worked out than programs in social science and the humanities. Nevertheless the score is not zero. There are projects designed for better teaching of the social sciences and the humanities. The kinds of values we would want to inculcate come better through teaching in the social sciences and humanities than they do in teaching in the sciences. These curricula can be built up to emphasize human values.

I had occasion to sit in on some meetings of a group designing a curriculum in social sciences. The emphasis there really was not on the human values and I felt very discouraged by this. The emphasis was on getting at principles of how cities were built up, how one chooses locations for cities, how economic factors determine the course of history, and so on. It would be possible, with a good deal of experimentation, to build into these courses more discussion, more questioning with respect to human values. It seems to me that this is the place in which it should be done. But again, in doing this we now have some models. We know something about successful programs in other domains. We can use these as models for working out programs in humanities and social sciences. Programs which can very directly serve to inculcate values.

On this question of the teacher as an identification model, I merely want to throw out the question, is this realistic? How important a model can the teacher be? We hope that we can be an important model, but so far as I can see in the literature on identification the teacher is the model in very unusual cases, that he or she becomes a model for a particular child under certain specific circumstances. As a generalized model for identification, for formation of ego ideals, I don't think that the teacher is generally effective. Maybe we should learn more about how the teacher may be effective as a model, what he can do to make himself such a model. At the moment the best data we have are from studies such as Bower's, and I agree they are fairly far from real life although they are suggestive of what might occur.

Certainly I second Professors Thompson and Hill on the need for more naturalistic research on values and better planned programs in the acquisition of values, but I really am very much concerned with how much we can expect the schools to do in this area. We need more information about how values may be taught in the schools, but it is a vast program of research we are talking about, and there won't be easy answers. Before we can do anything we have to have some means of evaluating the value structures of children and of assessing change. This is in itself a most difficult problem.

• *Dr. Gardner:* George, do you have any reactions to Paul's comments?

• *Dr. Thompson:* I do believe that psychologists are better qualified than many people to try to work out the values that should undergird a surviving culture. I don't say psychologists are the only ones, but I think they are within a class of people who could do better than people typically do, and I would like to have Dr. Mussen's reaction to that.

In other words—I guess I might as well be quite specific. I was quite pleased with President Kennedy's administration because he started bringing some people in, and apparently was listening to them, who I thought might be helpful in changing values. I think they are. I believe art got a little upsurge because of his interest. But I think that usually the groups who make these kinds of decisions don't include the most qualified people. For some kinds of value judgments I think psychologists are among the best qualified people. I would like to hear some other reactions to this. I do not feel we should hide our light under a bushel when we do know something about people.

• *Dr. Mussen:* I don't deny that we don't know anything about people. I just don't think we know more than anybody else.

• *Dr. Thompson:* That's what we are disagreeing on. I think we do; maybe not more than anthropologists or sociologists, but I think the usual thing we say in the social sciences is what it is so darned easy to say, for example, "I don't know what's going to happen to my findings." To me, from a moral-ethical point of view, this is the coward's way out, a way I take pretty often, but one I feel guilty about.

• *Dr. Mussen:* I think that we are more likely to spend time thinking about the issues than testing them, but I am not sure that that means we have any better answers.

• *Dr. Thompson:* Of course, if people with these talents would get involved, it might require some generations to know whether it makes a difference or not. But if I were putting my money on a promising hypothesis, I would say it might very well make a big difference.

• *Dr. Lumsdaine:* I think, Paul, it's not only a question of whether we have any better answers to provide but also whether we have the perspectives and the techniques which, if applied to this purpose, might be more likely to generate better answers than some alternatives.

• *Dr. Mussen:* This is why I think much of what we have been talking about is highly relevant to this.

• *Dr. Gardner:* Are there other comments or questions members of the panel would like to address to Professor Thompson or Professor Hill?

• *Dr. Ericksen:* Dr. Thompson has made a valuable contribution and his concern about the research bypass of the problem of student attitudes and values is, unfortunately, quite legitimate. We may find some reason for optimism in the changing scene in higher education. Universities are spending millions of dollars to establish cluster (residential) colleges, curricula are being modified, counseling programs are being upgraded and, by other means, the larger institutions are trying to give more explicit attention to students as individual persons.

I do slap the wrist of education for being so docile with respect to mass education and mass communication. I have been surprised and disappointed at the almost complete quiet about, for example, the Ford Foundation proposal for the satellite television network whereby millions of people can be taught the same thing and in the same way. To me this would be a negative blessing since the greater need is to adapt educational procedures to the motivational base, the aspirations and the intellectual abilities of the individually different students.

We must, of course, look to the future and the inevitable changes resulting from automation and technology. In the same breath, most teachers say something about "cold, mechanical impersonality." I suspect that the here-and-now values of computer technology to education may have been oversold, but eventually computer technology will have a tremendous impact on education, and we should anticipate some of the changes that may result. As I see computer-assisted instruction (CAI) today and for the immediate future, it may be possible for the student to acquire sequential and descriptive information about as well as he can from reading a book or listening to the teacher give a lecture. Insofar as this comes about, the role of the teacher should change. More often he will be the leader of a discussion group and, at the college level, he and his students will face the value judgments and review the new trends and developments that add up to the knowledge explosion. In other words I think that automation can be used in a manner that will greatly increase the total attention to the educational requirement of the individual student. But to do this we must certainly turn away from the mass communication model.

• *Dr. Gardner:* Are there other comments or questions?

• *Dr. Gagné:* There are just a few things, perhaps, that I should say in closing. I will not attempt a summary. This I think would be quite difficult, if not impossible, in view of the diversity of issues and ideas which have been discussed.

One point upon which I would like to comment is the question of the seduction of experimental scientists away from the laboratory in asking them to address the problems which appear in instruction and education. This was by no means an intention of this symposium. Nor is it the direction that the discussions of the symposium have led us to believe will be a fruitful one. It is not a matter of teasing anyone away from the very productive kind of theoretical and experimental work. It is rather an indication of the value of both of these modes of activity—basic and applied research.

It seems to me that there is a very impressive clarification of much that occurs in the educational sphere when Robert Glaser speaks about the learning of concepts in a way that is derived from a consideration of what happens to the child in school. A similar kind of clarification may be attributed to the other speakers: to Eleanor Gibson when she speaks of the reduction of uncertainty in perceptual learning; to Ernie Rothkopf when he talks about the kinds of behaviors that are involved in getting students to learn by themselves; to Merle Wittrock when he discusses the conditions needed for transfer and the models which underlie these; and to George Thompson when he speaks about the kinds of values that we have to deal with and their acquisition in the school situation.

I think two kinds of clarification can result from these discussions. In the first place, the problems needing experimental analysis obtain a sharper focus. We learn how to investigate the problems of conceptual learning, as I think our speakers will agree, when we look at how children actually learn to discriminate letters and to read. And, we learn to ask the right experimental questions when we look at how children actually learn concepts like verticality and horizontality.

We need again to ask these questions when we look at what is happening when students are staring fixedly at the teacher, or when we examine how they transfer the knowledge they have gained to other educational or practical situations, or when we examine the question of how is it that they learn to be good citizens. That's one side of the coin.

On the other side there is a clarification, I believe, in the issues in instruction itself. They are more sharply defined when they are subjected to the glare of incisive theory or model building and experimental findings. The variables that are important in the instructional situation are again put into sharper focus.

There was a question as we went along about how on earth do we get the results of research done by the learning psychologists into use in the schools? Two things that were said during the Symposium I would like to re-emphasize. I believe they are in contrast to the view that we might as well give up, because we can't make this kind of translation and utilization.

One is the point made by Glaser. I am sure there is a stage of development needed to get from the findings of experimental work and the theory that it generates to the practical instructional situation. One does not proceed directly because, of course, the practical situation has many, many requirements, all of which must be put together in useful packages, if you will. In order to do this, one doesn't think of going directly from a study finding to the schoolroom, but rather one must think of going through a process of development, whether it is of a curriculum or an instructional method or a larger system of education.

The second important point pertains to this development phase itself, to the application of the results of learning studies to education. It seems to be true that these findings can be applied to a system which deals with the learning of the individual student. Now that is very important because, as many recognize, the school doesn't always deal with the individual student. We are inclined to think of the school in terms of classes or classrooms. I think it is true, nevertheless, that the findings that we have obtained in learning, transfer, and related matters—and I would emphasize also, as indicated by Professor Thompson in the field of values—really have to be translated into a practical development which must attend to the individual student.

I don't know how that's done. But I would say that it is a mistake to think that it can be done through the vehicle of what is thought of currently as the class. I am trying not to be unrealistic here. Presumably education has to deal with groups of children. But what kinds of groups? How one puts the instructional techniques together in such a way that they really affect and deal with the individual student seems to me to be a very important problem.

In closing I would like to mention a couple of points. Most of us would subscribe to the notion that research in the traditions of the economy of learning is going to be important to educational practice. We also agree that research on mathemagenic behavior is important because we do need to learn how to get students to learn by themselves. I think that we also agree that we can learn a good deal about the acquiring of intellectual skills and desirable attitudes.

I might say on the latter point that I hold, I suppose, what must be a far-right view. I think that values, the abstractions that we use to characterize the social behavior of children and adults, like competencies, are based upon capabilities. I know that the common tendency is to view attitudes and values as being based upon tendencies. Does an individual tend to be honest or tend to be upright in some way, or tend to be polite? I think the far-right view here is that these things are just as dependent upon capabilities of the individual as are what usually are called the cognitive aspects of his performance. I think it is at least a good hypothesis and worthy of investigation, that what makes somebody capable of being discriminatingly honest is whether he in fact has acquired the complex rules which enable him to do that. In other words, whether he is capable of doing it may be the proper question to ask here. I realize that many people don't agree with that, but that's my view.

• *Dr. J. J. Gibson:* That's far left, not far right.

• *Dr. Gagné:* Thank you, Jim. I'm relieved to know that.

At any rate, these are the things we have talked about. We all agree we are not going to find the answers overnight. We have listened to some very intelligent, capable people talk about these things in terms that are useful to education. I have no doubt that they will continue to think about these problems which are so vitally in need of resolution.

With that I declare the conference closed.

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