Transfer For Learning

Foreword

By the Editors of the Series

In the field of psychology we believe that the student ought to get the "feel" of experimentation by reading original source materials. In this way he can acquire a better understanding of the discipline by seeing scientific ideas grow and change. However, one of the main problems in teaching is the limited availability of these sources, which communicate most effectively the personality of the author and the excitement of ongoing research.

For these reasons we have decided to edit several books,* each devoted to a particular problem in psychology. In every case we attempt to select problems that have been and are controversial—that have been and still are alive. We intend to present these problems as a set of selected original articles, arranged in historical order and in order of progress in the field. We believe that it is important for the student to see that theories and researches build on what has gone before; that one study leads to another, that theory leads to research and then to revision of theory. We believe that *telling* the student this does not make the same kind of impression as letting him see it happen in actuality. The idea is for the student to read and build ideas for himself.

Suggestions for Use—These readings books can be used by the student in either of two ways. They are organized so that, with the help of the instructor (or of the students if used in seminars), a topic can be covered at length and in depth. This would necessitate lectures or discussions on articles not covered in the series to fill in the gaps. On the other hand, each book taken alone will give a student a good idea of the problem being covered and its historical background as well as its present state and the direction it seems to be taking.

* (Pub. note: a sub-series within the Insight Book Series.)

Contents

Introduction

How does previous learning affect subsequent learning? The selections included in this volume introduce the reader to the ways in which research workers, both in psychology and in education, have attempted to answer this question over the last half century. Ever since William James discovered in 1890 that practice in memorizing Milton's "Paradise Lost" did not produce any improvement in his memorization of French poetry, transfer of training has been one of the most popular topics for educational-psychological research. At first, there was a rush of concern as to whether in fact transfer was present in the school curriculum, whether it was positive or negative, and to what degree. As is often found in other fields of scientific inquiry, however, increasing attention began to be shown to the exact conditions under which transfer occurred. Both the characteristics of the learning situations and the characteristics of the learners came under study. The concept of transfer itself became broader as it was realized that all learning involves transfer through time, even from learning trial to learning trial of the same task.

Impetus for research stemmed both from the practical needs of the classroom teacher and the developing learning theories in the 1930's and 1940's. We have attempted to include studies both from the psychological laboratory and from the classroom. We have limited our selections to those dealing with human subjects and those having interest both for students of general psychology and for students in education. In its developmental history, the idea of transfer of learning has become related to a variety of allied concepts such as: cross-education, generalization, learning sets, discrimination reversals, verbal mediation, transposition, psychoanalytic transference and displacement, retroactive inhibition or facilitation, and many others. Space has prevented the inclusion of readings on all of these topics, but the inquiring student will find it possible through the bibliographies of the sample of articles presented here to probe further into his own area of interest.

The first two articles of this book fill in the early history of the work on transfer. To contrast with his own point of view, Thorndike presents an intriguing collection of statements supporting the formal discipline point of view that endured until the beginning of this century. The now classic studies by Thorndike and Woodworth in the 1900's on the estimation of length of lines and the size of geometric figures not only led to the theory of identical elements, but also had a tremendous impact on education. Sandiford summarizes the experimental research over the first twenty-five years with its emphasis on which school subjects show transfer effects. Here is also presented the early theories of both Thorndike and Judd attempting to explain how transfer occurs.

The next three articles represent the more analytic experimentation on the variables affecting transfer that made its appearance in the 1930's. Yum's investigation of the dimension of stimulus similarity was the forerunner of a number of studies to follow. Both the Barlow and the Hendrickson and Schroeder experiments were concerned with what could be done by the teacher using verbal means to maximize positive transfer. Also, the intelligence of the students had appeared as a factor involved in the amount of transfer, as can be seen in Barlow's work.

As various learning theories were born and developed, attempts were made to incorporate the data of transfer. There was increasing interest in the relationships between the stimuli and the response of the first task with those represented in the second task. The outstanding example of a synthesis of these relationships by means of a theoretical model is represented by Osgood's well-known "Transfer-Retroaction Surface." Not only did the nature of the stimuli and responses come under examination, but questions were asked about the possible discriminations made by learners among the stimuli prior to the transfer to a second task; and we, therefore, include next Arnoult's review of stimulus pre-differentiation. The problem of relating advancing theory to classroom practice is ever present. Stephens then reviews for us the more recent work on transfer of school subjects and presents suggestions for teaching to obtain maximum transfer.

The remainder of the selections sample, if briefly, the more contemporary research and conceptualizations of transfer of learning. In a rigorous multivariable design, Goss and Greenfeld examine a variety of stimulus prediscrimination procedures while co-varying the degree of first-task learning. The next two articles are concerned with examples of how a teacher might maximize transfer. One study investigates the initiation of a guiding principle by the student as opposed to one presented by the teacher, while the other examines what happens when one varies the number of problems practiced prior to transfer.

The last two articles extend the application of the transfer concept into new areas. Schulz develops problem solving as an example of the paradigm for negative transfer, while Ferguson describes a general transfer function and relates it to our conceptualization of human abilities. It is perhaps too early to tell whether the merits of the concept of transfer as a broad integrative term outweigh its potential defect of being too inclusive. At any rate, there seems at present to be a new wave of studies relating to transfer on the horizon; and it is our hope that the reader of this limited sample of materials will, by his reading, be better equipped to interpret the many new findings to come.

I

The Influence of Improvement in One Mental Function upon the Efficiency of Other Functions

EDWARD L. THORNDIKE

From Educational Psychology, Volume II: The Psychology of Learning, *Chapter XII, excerpts from pp. 357-364 (New York: Bureau of Publications, Teachers College, Columbia University, 1913). Here Thorndike used his earlier book on* Educational Psychology *to express the opinions about transfer held a decade before.*

The application of experimental and quantitative methods to the dynamics of human nature has made rapid changes in educational theories of the general value of special practice. The condition of opinion a decade ago is fairly represented by the following pages from the 1903 edition of the author's *Educational Psychology.*

"One of the quarrels of the educational theorists concerns the extent to which special forms of training improve the general capacities of the mind. Does the study of Latin or of mathematics improve one's general reasoning powers? Does laboratory work in science train the power of observation for all sorts of facts? Does matching colored sticks educate the senses for all sorts of discriminations?

The problem, which is clearly one of psychological fact, may be best stated in psychological terms as follows: How far does the training of any mental function improve other mental functions? In less technical phrase, How far does an ability, say to reason, acquired with data A, extend also to data *B*, *C*, *D*, etc.?

No one can doubt that all of the ordinary forms of home or school training have some influence upon mental traits in addition to the specific changes which they make in the particular function the improvement of which is their direct object. On the other hand, no careful observer would assert that the influence upon the other mental traits is comparable in amount to that upon the direct object of training. By doubling a boy's reasoning power in arithmetical problems we do not double it for formal grammar or chess or economic history or theories of evolution. By tripling the accuracy of movement in fingering exercises we do not triple it for typewriting, playing billiards or painting. The gain of courage in the game of football is never equaled by the gain in moral courage or resistance to intellectual obstacles. The real question is not, 'Does improvement of one function alter others?' but, 'To what extent, and how, does it?'

The answer which I shall try to defend is that a change in one function alters any other only in so far as the two functions have as factors identical elements. The change in the second function is in amount that due to the change in the elements common to it and the first. The change is simply the necessary result upon the second function of the alteration of those of its factors which were elements of the first function, and so were altered by its training. To take a concrete example, improvement in addition will alter one's ability in multiplication because addition is absolutely identical with a part of multiplication and because certain other processes,—*e. g.*, eye movements and the inhibition of all save arithmetical impulses,—are in part common to the two functions.

Chief amongst such identical elements of practical importance in education are associations including ideas about aims and ideas of method and general principles, and associations involving elementary facts of experience such as length, color, number, which are repeated again and again in differing combinations.

By identical elements are meant mental processes which have the same cell action in the brain as their physical correlate. It is of course often not possible to tell just what features of two mental abilities are thus identical. But, as we shall see, there is rarely much trouble in reaching an approximate decision in those cases where training is of practical importance.

The standard psychology and pedagogy books, with few exceptions, answer our questions in a manner very different from this. They extend the influence of any special form of discipline much farther, and describe its manner of operation only by vague and, I think, meaningless phrases.*

In place of any descriptive account I shall give a number of quotations picked almost at random from all the statements about the influence of special training on general ability made in some fifty representative books on psychology and pedagogy. These will represent fairly the prevailing attitude.

Systematic treatises on psychology, with two or three exceptions, neglect the functional aspect of mental life and so do not furnish any apt quotations. Their implied point of view, however, is, again, with one or two exceptions, that alterations in mental powers are alterations in the general facility of attention, reasoning, etc.

Books on applied psychology express this implication outright, and books on education carry it to an amazing extreme. The following quotations represent accurately a wide-spread opinion:

Since the mind is a unit and the faculties are simply phases or manifestations of its activity, whatever strengthens one faculty indirectly strengthens all the others. The *verbal* memory seems to be an exception to this statement, however, for it may be abnormally cultivated without involving to any profitable extent the other faculties. But only things that are rightly perceived and rightly understood can be *rightly* remembered. Hence whatever develops the acquisitive and assimilative powers will also

* The leading exception in psychology is James' 'Principles of Psychology,' and in pedagogy is the Herbartian literature. The often correct conclusions of the latter are, however, based upon defective principles.

strengthen memory; and, conversely, rightly strengthening the memory necessitates the developing and training of the other powers. [R. N. Roark, 'Method in Education,' p. 27]

It is as a means of training the faculties of perception and generalization that the study of such a language as Latin in comparison with English is so valuable. [C. L. Morgan, 'Psychology for Teachers,' p. 186]

Arithmetic, if judiciously taught, forms in the pupil habits of mental attention, argumentative sequence, absolute accuracy, and satisfaction in truth as a result, that do not seem to spring equally from the study of any other subject suitable to this elementary stage of instruction. [Joseph Payne, 'Lectures on Education,' Vol. I., p. 260]

By means of experimental and observational work in science, not only will his attention be excited, the power of observation, previously awakened, much strengthened, and the senses exercised and disciplined, but the very important habit of doing homage to the authority of facts rather than to the authority of men, be initiated. . . . [*Ibid.*, p. 261]

I wish to understand by mental discipline the exercise of some faculty of the mind, which results in increasing the power or readiness of that faculty. [E. H. Babbitt, p. 126 of 'Methods of Teaching the Modern Languages']

The faculty which is by far the most important of the human mind, and which we most earnestly strive to develop and perfect in our pupils, is the faculty of judgment, or the reasoning faculty (I am not trying to be psychologically exact), the faculty whose perfection gives what we call a logical mind—a mind which has a ready perception of the relations of things and is not likely to be misled by false reasoning. [*Ibid.*, p. 127]

The most valuable thing in the way of discipline which comes from a study of a foreign language is its influence in improving the pupil's command of his own. Of course this means the improvement in general judgment and discrimination which is evinced by a finer linguistic sense. . . . [*Ibid.*, p. 129]

. . .

The value of the study of German 'lies in the scientific study of the language itself, in the consequent training of the reason, of the powers of observation, comparison and synthesis; in short, in the upbuilding and strengthening of the scientific intellect.' . . . [Calvin Thomas, 'Methods of Teaching Modern Languages,' p. 27]

. . .

As regards the first point, it may be noted that the pursuit of mathematics gives command of the attention. A successful study increases or creates the power of concentrating the thoughts on a given subject and of separating mixed and tangled ideas. The habits of mind formed by means of this one set of studies soon extend their influence to other studies and to the ordinary pursuits of life. The man or woman who has been drilled by means of mathematics is the better able to select from a number of possible lines which may be suggested that which is easiest or most direct to attain a desired end.

The second purpose of this study is the one which has been most universally acknowledged in all ages, namely, the strengthening and training of the reasoning powers. [R. Wormell, 'Teaching and Organization' (edited by P. A. Barnett), p. 78]

. . .

It is clear that the common view is that the words accuracy, quickness, discrimination, memory, observation, attention, concentration, judgment, reasoning, etc., stand for some real and elemental abilities which are the same no matter what material they work upon; that these elemental abilities are altered by special disciplines to a large extent; that they retain those alterations when turned to other fields; that thus in a more or less mysterious way learning to do one thing well will make one do better things that in concrete appearance have absolutely no community with it.

The mind is regarded as a machine of which the different faculties are parts. Experiences being thrown in at one end, perception perceives them, discrimination tells them apart, memory retains them, and so on. By training, the machine is made to work more quickly, efficiently and economically with all sorts of experiences. Or, in a still cruder type of thinking, the mind is a storage battery which can be loaded with will power or intellect or judgment, giving the individual 'a surplus of mind to expend.' General names for a host of individual processes—such as judgment, precision, concentration—are falsely taken to refer to pieces of mental machinery which we can once for all get into working order, or, still worse, to amounts of something which can be stored up in bank to be drawn on at leisure." [Thorndike, '03, pp. 80-85]

Such an account would today entirely misrepresent the standard view. When the three papers by Woodworth and Thorndike appeared in 1901, describing the limited extent to which practice in sensory discrimination, the observation of small details, and the like, spread beyond the specific abilities trained, they aroused surprise and incredulity. At the present time, such a limited spread of training would be taken almost for granted.

The notions of mental machinery which, being improved for one sort of data, held the improvement equally for all sorts; of magic powers which, being trained by exercise of one sort to a high efficiency, held that efficiency whatever they might be exercised upon; and of the mind as a reservoir for potential energy which could be filled by any one activity and drawn on for any other—have now disappeared from expert writings on psychology.

2

Reciprocal Improvement in Learning

Peter Sandiford

This article is excerpted from Chapter XI in Sandiford's book, Educational Psychology—An Objective Study *(New York: Longmans Green, 1928, pp. 284-300), published while Professor Sandiford was at the University of Toronto.*

The number of experiments on transfer of training is very great. Before giving a general summary, a typical research in a selection of the fields which have been investigated will be described.

(*a*) *Cross Education.* The term "cross education" was given by Scripture to the special transfer of skill from one side of the body to a symmetrical part on the other side. Training the left hand to grip more strongly, for example, leads to an increase in the strength of grip of the right. The subject has been investigated by Volkmann (1858, discrimination of two-point threshold); Fechner (1858, writing); Scripture (1894, speed, force, and accuracy of movement); Woodworth (1899, hitting a dot with pencil); Swift (1903, tossing balls); Starch (1910, tracing six-pointed star); and others.

Woodworth [1899] practised his left hand in hitting a dot with a pencil and studied the effect it had upon the right. In terms of the errors made, the results were as follows:

	Hitting at a rate per minute of		
	40	120	200
Left Hand: Before training	3.5	4.2	8.3
After "	0.4	3.8	7.2
Right Hand: Early test	3.1	3.9	7.1
Late "	0.7	3.8	6.6

There is an improvement of the right hand which is equal to about 50 per cent. of that of the left.

In cross education the transfer is not a general transfer to all parts of the body but a specific transfer to a symmetrical part of the body. Such transfer effects, of course, cannot be used in support of the doctrine of formal discipline, although the transfer from one side of the body to the other is always positive and always high, running in some cases to as much as 80 per cent. Woodworth was inclined to believe that the chief reason for his improvement was the training given to the eyes in fixating the point.

But it should be remembered that the same portion of the cerebral hemisphere (left for right-handed persons) is concerned with skilled movements from either hand. Since some of the neural connections from the left and right sides are different, the transfer is never perfect. If training of the right hand transferred to the right or left foot the argument for transfer would be strengthened, but even this result would have to be interpreted with caution, since there are so many avenues by means of which the transfer could take place.

(*b*) *Memory*. The chief experiments on transfer of memory training have been those of James [1890, memorizing poetry]; Ebert and Meumann (1904, effect of training in memorizing nonsense syllables on other forms of memorization); Dearborn (1906, repetition of Ebert and Meumann's series without the intervening practice); Fracker (1908, memorizing the order of four different intensities of the same tuning-fork and finding amount of transfer in memorizing poetry, order of four greys, geometrical figures, two-place numbers and the like); Winch (1908, effect of memorizing poetry upon memorization of history and descriptions of places; 1910, effect of memorization of meaningful and meaningless materials upon meaningful material); and Sleight (1911). As Sleight's experiments are the most extensive and careful that have been made, we select them for a more extended mention.

Sleight directed his attention to the problem of the effect of memorizing poetry, tables, and prose substance upon other forms of memorizing.

The first set of experiments had as subjects 84 girls in Standard VI (about Grade VII) of average age 12 yrs. 8 mos. These subjects were divided into four approximately equivalent groups. Group 1 was untrained, taking only the ten tests; Group 2 was trained in learning poetry such as *Hiawatha*, *Skylark*, and *The Cloud;* Group 3 was trained by learning multiplication, pence, and metric tables of all kinds, squares, vulgar fractions with their decimal equivalents, etc.; and Group 4 was trained with "prose substance"—scientific, geographical, and historical —by reading selections to them and having them write out the gist as well as they could remember it.

The series of ten tests included:

(1) Points in circles. Children reproduced the positions of points in a circle drawn on a card and exposed six times for one second. Each child, therefore, made six attempts.

(2) Dates. Dates and corresponding events were learned orally, then children wrote date when event was announced.

(3) Nonsense syllables. Eight syllables exposed singly in succession five times.

(4) Poetry. Stanzas of 8-12 lines were used.

(5) Prose. Short literary extract. Verbatim learning.

(6) Prose substance. Children wrote substance of a piece of prose read twice to them.

(7) Map test. Trained in location of names on a large mercator map and then marked places on their own smaller ones.

(8) Dictation. Continuous prose dictated in portions gradually increasing from 8 to 19 words.

(9) Letters. Sixteen series of consonants gradually increasing from 4 to 8 letters were dictated.

(10) Names. Forty-four common Christian and surnames in combinations of 2, 3, and 4 were used.

These ten tests were given at the beginning, middle, and end of a period of training, which lasted for 720 minutes spread over six weeks.

The results are shown in summary form in Table XXVII.

The second set of experiments was conducted with a group of women normal school students of average age 18-19. Six of the ten tests were given before and after the period of training. The summarized results are given in Table XXVIII.

TABLE XXVII

Superiority of the practised groups (2, 3, and 4) over the unpractised group (1). (Subjects, children, after Sleight.)

Tests	Superiority of Group 2 (practised in poetry) over Group 1 (unpractised)	Superiority of Group 3 (practised in tables) over Group 1	Superiority of Group 4 (practised in prose substance) over Group 1
Points	21	48	23
Dates	19	−12	−12
Nonsense	66	85	8
Poetry	−31	−9	0
Prose verbatim	−14	−5	21
Prose substance	−22	7	31
Map	50	1	13
Dictation	−31	−10	0
Letters	−5	1	−2
Names	−3	−14	17
Average	9	5	10

Tables XXVII and XXVIII show conclusively: (1) that training in one kind of memorizing may affect other kinds either favourably or unfavourably. Practice with prose substance, for example, greatly improved the learning of prose substance but affected adversely the learning of nonsense syllables; and (2) considering that the practice with the ten tests led to an average gain of 52 points (not shown in Tables) the transfer effects shown in the Tables (5, 9, and 10; average 8, in Table XXVII; and 13, 8.5 and −2; average 6.5, in Table XXVIII) are remarkably small.

In general it may be said that the transfer effects of memorization range generally between 5 and 15 per cent. Sometimes it is less than 5 per cent. and occasionally has turned out to be negative.

(*c*) *Sensori-motor Learning*. The chief experiments on sensori-motor learning have been carried out by Bergstrom (1894, card-sorting); Blair (1902, succession of typewriter keyboards in which six of the keys had been

TABLE XXVIII

Superiority of the practised groups (2, 3, and 4) over the unpractised group (1). (Subjects, women students, after Sleight.)

Tests	Group 2 (practised in poetry) compared with Group 1 (unpractised)	Group 2 (practised in tables) compared with Group 1	Group 3 (practised in prose substance) compared with Group 1
Tables	32	59	−6
Nonsense	33	9	−62
Poetry	33	−27	−7
Prose verbatim	9	−36	−17
Prose substance	−7	49	52
Letters	−24	−3	27
Average	13	8.5	−2

changed from letters to symbols); Angell and Coover (transfer effects from card sorting to typewriting); Scholckow and Judd (1908, hitting target placed in water); Coover (1916, card sorting); and Webb (1917, transfer effects in learning mazes—rats and humans).

The experiment of Scholckow and Judd represents a variation in transfer experimentation. They taught one group of boys the principles of refraction and left another group without such knowledge. They investigated the influence of this knowledge on learning to hit a target under water. The practice began with the target 12 inches below the surface of the water, which, later, was changed to 4 inches. The groups were equally slow in acquiring facility when the target was at a depth of 12 inches, but when the depth was reduced to 4 inches the group which had been given a theoretical explanation of refraction soon outdistanced the untutored group. As we shall see later in the chapter this experiment has influenced Judd's explanation of the mechanism of transfer.

(*d*) *Discriminative Judgments.* Among the investigations of the transfer of improvement in situations involving discriminative judgment the best are those of Thorndike and Woodworth (1901, previously cited); Judd (1902, Müller-Lyer illusion); Bennett (1907, colours); Angell and Coover (1908, brightnesses); Kline (1909, effect of cancelling *e*'s and *t*'s on cancellation of nouns, verbs, prepositions, pronouns, and adverbs); Whipple (1910, visual perception); and Foster (1911, drawing objects and pictures presented visually for 10 to 60 seconds). Kline [1909] found that practice in crossing out *e*'s and *t*'s on pages of prose hindered the cancellation of nouns, verbs, prepositions, pronouns, and adverbs. In other words, the general gain in efficiency from practice in eye-fixations, marking pages, and attention was more than offset by the confusion effects caused by the change of task. The average gains of the practised and unpractised groups were 1.8 and 3.3 respectively.

(*e*) *Neatness.* This aspect of school work has been studied by Squire (1905, neatness in arithmetic papers to neatness in language and spelling papers); and Ruediger (1908, does ideal of neatness, brought out in connection with, and applied in, one school subject function in other school subjects?). Squire found that neatness developed in arithmetic did not transfer to language and spelling papers. Ruediger reported that an ideal of neatness seemed to transfer to some extent.

(*f*) *Grammar.* This favourite of the formal disciplinarians has been studied by Hoyt (1906, grammar to composition); Briggs (1913, formal grammar *versus* work in composition on ability to check plurals, judge definitions, etc.); and Starch (1915, formal grammar and English usage). The general results may be summed up in Hoyt's words, "The teaching of grammar is of little avail in strengthening one's ability to use language." Starch, however, notices that "study of foreign languages materially increases a pupil's knowledge of English grammar but only slightly increases his ability in the correct usage of the English language."

(*g*) *Arithmetic.* Transfer from training in arithmetic has been studied by Winch [1910, "rule examples" (chiefly computation) to reasoning in arithmetic]; and Starch (1911, mental multiplication upon other arithmetical processes). Starch used eight subjects in his training group and seven in his control group. The practice consisted of multiplying mentally a three-figure number by a one-figure number, doing 50 problems a day for 14 days. His results are given in Table XXIX. The transfer effect was found to be about 26 per cent. of the gain in the practice series itself.

TABLE XXIX

Transfer of training from mental multiplication to other arithmetical operations (Starch).

Abilities tested	Trained persons	Untrained persons	Differences
Adding fractions	40	12	28
Adding three-place numbers	49	10	39
Memory span for numbers	−3	−2	−1
Subtracting numbers	58	35	23
Multiplying four-place numbers	53	29	24
Memory span for words	3	−5	8
Multiplying two-place numbers	47	10	37
Dividing three-place numbers	45	25	20
Average, exclusive of memory span	49	20	29

(*h*) *Latin*. Latin has been the subject of a large number of studies on transfer effects. Among them may be cited: Swift (1906, Latin *versus* German on progress in learning Spanish); Perkins (1914, the effect of emphasizing the derivation of English words from Latin upon the efficiency of Latin instruction); Harris (1915, effect of Latin on ability to spell English words); Partridge (1915, Latin on marks in English obtained in the Regents Examination); Starch (1915, Latin *versus* German on progress of students through the university); Starch (1917, Latin *versus* native ability on English composition); Dallam (1917, Latin on English); College Entrance Board Examinations (1917, Latin on rating in examination); Wilcox (1917, Latin *versus* school records before Latin was begun); Foster (1917, Latin on English spelling); Thorndike (1923, Latin *versus* non-Latin on knowledge of English); Thorndike and Ruger (1923, Latin *versus* non-Latin on growth in English); Briggs and Miller (1923, standards of English obtained in translations of Cicero); Coxe (1923, Latin on spelling of English words of Latin derivation); and Hamblen (1924, Latin *versus* non-Latin on knowledge of English; also on effectiveness of emphasis on English words of Latin derivation).

The general trend of the findings, considering the common content of much of the English and Latin, is that the transfer is much smaller than the Latinists have claimed, the amounts almost invariably ranging below 20 per cent. Perkins [1914] made a study of the effect of emphasizing the derivation of English words from Latin words in the instruction in Latin given to pupils of a commercial course. His two equivalent groups were pupils in second year of Latin, and pupils in the second year of a modern language. The six tests used were spelling, use of words in sentences, definitions and parts of speech, meanings of words and spelling, excellence of vocabulary, and meanings of words and spellings. The results were as follows:

	Averages	
	Latin per cent	Non-Latin per cent
Jan. and Feb., 1914.		
1. Spelling	82.5	72.6
2. Use of words in sentences	57.5	40.6
3. Definitions and parts of speech	69.5	33.3
4. Meaning of words and spelling	57.0	27.5
5. Excellence in vocabulary	36.0	6.8
June, 1913.		
6. Meaning of words and spelling	63.3	12.3
Averages	61.3	32.18
Difference	29.12 per cent	

The difference is impressive. If, however, we turn to the examples of words used in the tests we find in (1) such words as valedictory, competition, occurrence, benevolence, and legible; and in (2) impediment, advocate, reference, anticipate, and subside. These words undoubtedly favour the Latin pupil although the words for (3) taken from *The Tale of Two Cities*, and those for (6) taken from Franklin's *Autobiography* and *Silas Marner* may not have done so. However, the lists used are not given. The real weakness of the investigation is failure to calculate improvement made on a second test when compared with the initial one. Moreover, equating pupils on scores in Latin and in a foreign language may not have secured truly equal groups, since the standards of the two examinations may have been, and probably were, different.

(*i*) *Geometry.* The chief study in this field is Rugg's doctorate dissertation [1916]. The subjects were 326 students in the College of Engineering of the University of Illinois. A group of 78 students in other colleges served as a control group. Five tests were used: 1, arithmetic; 2, arithmetic; 3, imaging letters; 4, painted cube test; and 5, geometrical objects. Thus tests 4 and 5 were strictly geometrical, 3 was quasi-geometrical, and 1 and 2 were non-geometrical. The residual differences in the five tests between the training and control groups were: —1.10, 15.78, 19.4, 14.0, and 48.5 respectively. From which Rugg concludes:

> The study of descriptive geometry (under ordinary classroom conditions throughout a semester of 15 weeks) in which such natural and not undue consideration is given to practice in geometrical visualization as is necessary for the solution of descriptive geometry problems operates:
>
> (1) Substantially to increase the students' ability in solving problems requiring the mental manipulation of a geometrical nature, the content of which is distinctly different from the visual content of descriptive geometry itself.
>
> (2) Substantially to increase the students' ability in solving problems requiring the mental manipulation of spatial elements of a slightly geometrical character (point, line, and plane).
>
> (3) Substantially to increase the students' ability in solv-

ing problems requiring the mental manipulation of spatial elements of a completely non-geometrical nature (no lines or planes).

(4) The training effect of such study in descriptive geometry operates more efficiently in those problems whose visual content more closely resembles that of the training course itself.

But it should also be noted that one-third of the students showed no transfer effects, and that the gain in the geometrical tests was four times greater than in the strictly non-geometrical ones.

(*j*) *Science.* A very good study of transfer in power of observation in science to observation in general was made by Miss Hewins [1916]. The subjects were 34 boys and 50 girls taking first year botany in high school. These were divided into two equivalent groups. The practice was confined to botanical observation and recording. The initial and final tests were partly biological, partly non-biological. The gains in scores are shown in the following table:

	Biological tests	Non-biological tests
Practised Groups		
Boys	8.06	8.97
Girls	6.41	6.20
Average	7.23	7.58
Unpractised Groups		
Boys	3.03	5.37
Girls	−1.24	5.60
Average	0.89	5.48
Residual difference between practised and unpractised groups	6.34	2.10
Percentage gains	33.9	5.4

Median score in biological tests in Series I before practice	18.7
Median score in non-biclogical tests in Series I before practice	39.0

Making allowances for gains due to practice, the transfer effects are 5.4 per cent. for non-biological and 33.9 per cent. for the biological tests.

Bookkeeping and science are much superior. But the transfer effect of any subject is quite small.

> "If," as Thorndike states, "our inquiry had been carried out by a psychologist from Mars, who knew nothing of theories of mental discipline, and simply tried to answer the question, 'What are the amounts of influence of sex, race, age, amount of ability, and studies taken, upon the gain made during the year in power to think, or intellect, or whatever our stock intelligence tests measure?' he might even dismiss 'studies taken' with the comment, 'The differences are so small and the unreliabilities are relatively so large, that this factor seems unimportant.' The one causal factor which he would be sure was at work would be the intellect already existent. Those who have the most to begin with gain the most during the year. Whatever studies they take will seem to produce large gains in intellect."

In the second study an additional 5000 pupils were tested. Averaging the results of both studies in the order of subjects, we find:

Subject		1922-23	1925-26	Average
Algebra, geom., trig.	(V)	+2.33	+3.64	+2.99
Civics, econ., psy., sociol.	(II)	+0.27	+5.50	+2.89
Chem., physics, gen. sci.	(IX)	+2.64	+2.77	+2.71
Arith. and bookkeeping	(IV)	+2.92	+2.28	+2.60
Physical training	(T)	+0.66	+1.00	+0.83
Latin, French	(VIII)	+1.64	−0.07	+0.79
Cooking, sewing, stenog.	(VIII)	−0.47	+0.19	−0.14
Biol., zool., bot., phys., agric.	(III)	−0.90	+0.60	−0.15
Dramatic art	(D)	−0.29	−0.67	−0.48

The claims of mathematics are seemingly sustained, but Latin sinks in the scale. Combining the two we get 1.89, which is only 1.81 greater than the average of physical training (T), cooking and sewing (VIII), and dramatic art (D).

The transfer gains, however, play such a small rôle, when compared with the native intelligence of the pupil, that Thorndike is forced to the following conclusion:

"By any reasonable interpretation of the results, the intellectual values of studies should be determined largely by the special *information, habits, interests, attitudes,* and *ideals* which they demonstrably produce. The expectation of any large difference in general improvement of the mind from one study rather than another seems doomed to disappointment. The chief reason why good thinkers seem superficially to have been made such by having taken certain school studies, is that good thinkers have taken such studies, becoming better by the inherent tendency of the good to gain more than the poor from any study. When the good thinkers studied Greek and Latin, these studies seemed to make good thinking. Now that the good thinkers study Physics and Trigonometry, these seem to make good thinkers. If the abler pupils should all study Physical Education and Dramatic Art, these subjects would seem to make good thinkers. These were, indeed, a large function of the program of studies for the best thinkers the world has produced, the Athenian Greeks. After positive correlation of gain with initial ability is allowed for, the balance in favour of any study is certainly not large. Disciplinary values may be real and deserve weight in the curriculum, but the weights should be reasonable."

THEORIES OF TRANSFER

As a result of their experiments in 1901, Thorndike and Woodworth concluded "that spread of practice occurs only where *identical elements* are concerned in the influencing and influenced functions." Later, these identical elements were called identities of substance (matter) and identities of procedure (method). Geography helps history because maps are common to both. Addition improves multiplication because multiplication is largely addition. French helps German because the methods of study used in the two languages have the common element of hunting up words in a dictionary. Writing and spelling, learned in connection with any subject, transfers to all others in which writing and spelling are used. All study helps all other study in so far as it involves learning the habit of sticking to a task for an hour or two at a time. In this sense Bagley's third identity (ideal or aim) can be conceded. The ideal of work, for example, may transfer from the intellectual tasks of the winter time to the chores of wood chopping and painting boats which confront us in summer time at the country cottage.

This theory of identical elements is a perfectly reasonable one. Out of the millions of specific reactions, each with its specific connection in the nervous system, some of them are bound to be common to several situations. The greater the number of these common elements, the greater will be the transfer effect.

A second theory of transfer, stoutly maintained by Judd [1927], is that of *generalization of experience*. A few quotations will be given to show the trend of his argument.

> "Trained intelligence is particular in its content but general in its method. It is characteristic of human thinking that wherever one encounters any phenomenon one tends to interpret it in terms of general categories."
>
> "The human power of generalization is so intimately related to the evolution of language that the two cannot be thought of as existing separately. Words are records of generalization, and their use implies the power to apply to new experiences the established classifications expressed in language."
>
> "The unfortunate effect of the use of such words as 'discipline' and 'transfer' is that prejudices have been created which in reality have no bearing on the main issue. The student of psychology should keep his mind fixed on the real problem and should use all these terms only in so far as they help him to formulate and answer the question: How does the education which the pupil receives in school affect his subsequent thinking and conduct?
>
> "If there is any one who asserts that mathematics or Latin or science will train the general powers of discrimination or observation or reasoning, that person is wrong. If, on the other hand, any one asserts that all training is particular, that the mind is made up of many independent special modes of thinking, that person is just as wrong as his opponent.
>
> "There is no guaranty in its content that any subject will give general training to the mind. The type of training which pupils receive is determined by the method of presentation and by the degree to which self-activity is induced rather than by content. It is not far from the truth to assert that any subject taught with a view to training pupils in methods of generalization is highly useful as a source of mental training, and that any subject which emphasizes particular items

of knowledge and does not stimulate generalization is educationally barren."

"A study of scientific methods powerfully reinforces the general concept of organization which is expounded by the *Gestalt* psychologists. 'Scientific method' is a name for a mode of thinking which is so general that it becomes impossible to think of it as a single item of experience."

"When the ends thus described are attained, transfer of training, or formal discipline, has taken place because it is the very nature of generalization and abstraction that they extend beyond the particular experiences in which they originate."

At first glance the theories of Thorndike and Judd seem to be hopelessly at variance. Further consideration, however, shows that the identical elements in Judd's generalization may be the specific habits of language engendered by the process. Language is, *par excellence*, the medium by means of which knowledge can be transferred from one situation to another; it is the grand "short-circuiter" of learning. And the language need not be vocal; the sub-vocal variety used in thinking is just as effective.

For example, a teacher of mathematics in all kinds of mathematical situations—algebraic and geometrical problems—trains his pupils to ask themselves, "Is this a new problem? If it is, what must be done to turn it into a form with which I am familiar?" Every new problem must be twisted into the form of an old problem whose solution is already known; otherwise, it can never be solved. Now this generalization in the form of a language habit is applicable to every mathematical situation. It is the way that mathematicians are made. But a further step can be taken. The teacher may show that the principle—the new must always be solved or interpreted in terms of the old—is of universal applicability. In so far as this extension is made mathematics may be said to have a general disciplinary value far beyond the confines of the subject itself. Its disciplinary value, however, resides in the formation of specific language habits which have applicability to other, non-mathematical situations. Judd's generalization of experience, therefore, on analysis resolves itself into nothing more nor less than the formation of specific language habits having applicability to situations other than those in which they were learned. Since the effective use of language is limited to those who are intellectually endowed, the clever people, as Thorndike found, will benefit more from any kind of an education than those to whom this gift is denied.

EDUCATIONAL IMPLICATION OF THE EXPERIMENTAL FINDINGS

If the transfer effect or disciplinary value of a subject is as small as it has been proved to be, then curricula for schools will have to receive their sanction on other than disciplinary grounds. No longer shall we be able to introduce Latin, or mathematics, or science into the course of studies and defend our actions on disciplinary grounds. As a matter of fact, the great interest in curriculum study found on the North American continent was stimulated by the experiments on transfer. If transfer proves to be a broken reed, then studies whose content is socially useful must be emphasized. Hence the purging of courses of study of elements which were palpably unpractical and unsuited to present-day civilization. Hence the job analyses, whose object was to determine the frequency of use in life situations of certain skills and forms of knowledge. If cube root is not used by one in a million once in a lifetime, it must be cast out of the course in arithmetic and something more useful substituted for it. This movement, which is leading to valuable improvements in curricula, may, however, go too far. Greek cannot be defended as a universal study on disciplinary grounds, but Greek culture has played such an important part in the history of mankind, that a few choice spirits should certainly be encouraged to study the language. A distinction should, therefore, be drawn between the common minimum which all should acquire, and the variations which can be superimposed on this minimum. Life is varied, and our schools, which are a part of this varied life, should reflect its richnesses. Not only do we require different types of schools, but also a variety of studies within these schools.

Over-narrowness in types of school and subjects of study must be sedulously avoided. The preservation of a nice balance between minimum essentials and the variety which adds spice to life is the task of the administrator.

Secondly, experiments in transfer show that the transfer is greatest when common elements are involved. Man, through language habits, has developed a tool of transfer which is denied to all sub-human species. Judd's generalization of experience, as we have seen, is merely the acquirement of language habits. Hence the method of teaching children becomes of greater importance than the subjects used to teach them. Teachers should be trained to present their material in such a way that wide transfers of language habits are assured. The difference between good and bad teaching is a matter of method rather than content.

Thirdly, studies and methods of study may affect the emotional as well as the intellectual life of the pupil. Little is known about emotional spread—the effect of studies in interests, prejudices, attitudes, and ideals—but in the aggregate it is probably very great. As Thomson [1925] pertinently remarks: "As a child grows up he develops ways of looking at the world, and ways of reacting to situations; he grows up into a conservative or a radical, a solicitor or a plumber, an atheist or a Wesleyan Methodist, a patriot or a cosmopolitan, and the difference between these are not merely differences in skill or in knowledge or in power of reasoning, not merely differences in the integration of knowledge into science, but also in the integration of the feelings and emotions into sentiments."

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3

An Experimental Test of the Law of Assimilation

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This investigation is concerned with the following problem: To what extent will a motor response that has been contiguously associated with a given perceptual stimulus be aroused by a similar stimulus with which it has not been associated, and to what extent is the likelihood of arousal a function of the degree and kind of similarity involved?

This problem is never encountered in the early literature of association. Some of the modern writers have assumed the truth of the doctrine, and a few have classified the phenomenon under a separate law of association as the Law of Assimilation[1] by Carr (10), Response by Analogy by Woodworth (66), and the Law of Redintegration by Hollingworth (27). On the other hand a few like Spencer (1, 54, 59), Bain (1, 59), Ward (58), and Peters (47) have attempted to explain the phenomenon

[1] The term 'Assimilation' was used by Ward (58), Spencer (54), and Drobisch (18), but they meant by the term a particular process of 'blending or fusion' of a presentational continuum —a similarity that amounts to identity only.

in terms of the conventional Law of Similarity. This law states that the thought of an object tends to arouse the thought of a similar object. In other words, an object makes you think of similar objects that you have known. Here, the similarity involved is between the stimulus and response items. Obviously the law is applicable only to ideational sequences and not to perceptual-motor connections. To account for the fact that a new perceptual object will call out a motor response associated with a similar perceptual stimulus on the basis of this law, they assumed that the new perceptual object first aroused an image or idea of the similar object, and that this image then evoked the motor response in accordance with the Law of Contiguity. In the first connection between the new stimulating object and the image or idea of the similar object, the stimulus and response items are similar, and hence this part of the associative sequence is to be classified under the conventional Law of Similarity. Among these writers, the doctrine has been based upon general observation and its truth has never been experimentally tested in a systematic fashion.

Much the same condition is found in the allied field of conditioning responses. We have at present a very considerable amount of experimental work[2] to determine the conditions under which a response will be called out by a substitute stimulus. These experiments in the main, however, have employed the method of contiguous or simultaneous presentation of the two or more stimulating objects. Watson (60) has made a few observations on an infant relevant to the principle of similarity. He first conditioned a fear response to a white rat on the basis of simultaneous presentation, and then made a few tests to discover whether or not this fear response would also be transferred to other objects. He found that the fear response was elicited by a rabbit, to some extent by a dog and a fur coat, slightly by cotton wool, hair of the human head, and a Santa Claus mask, and not at all by the room, the table, and the infant's blocks. He, however, has not discussed in his account the differentiating princi-

[2] See 11, 12, 13, 42, 60, and others.

ple involved, which is presumably that of similarity of the stimulating objects.

The Law of Assimilation is also encountered in the discussions of the development of language, the formation of class concepts, and the process of generalization. Writers frequently comment upon the fact that the naming responses in young children are often elicited by and attached to novel objects on the basis of their apparent similarity to objects with which they are familiar, as when they persist in calling all men 'papa,' or refer to a squirrel as 'Kitty' when it is first seen. Such illustrations are likely to convey the impression that this type of reaction is a relatively infrequent phenomenon—occurring only in these mistaken identifications. Other writers, however, have noted that this transference of the naming responses from familiar to unfamiliar objects on the basis of their similarity occurs automatically all the time, and that this process is of fundamental importance in the development of language and of class concepts. Still other writers have insisted that this process of transference is not limited to the naming responses, but that it is a universal phenomenon applicable to all types of responses. They have also urged that this transference of responses from familiar to novel objects and situations on the basis of similarity which occurs on an automatic and unreflective level with children constitutes the germinal prototype of the process of rational generalization. For example, this general point of view has been explicitly developed by Carr (10). The transference of responses on the basis of similarity has also been recognized as a principle involved in scientific generalization—a fact that is attested by the well-known canon of inductive logic that states that what is true of one situation is also true for all similar situations. While there has been considerable experimental work done in the field of generalization and the formation of class concepts (Moore (40), Fisher (22), Hull (28), Peterson (48), Campion (9), Gengerelli (24), Shaffer (53), Eljasch (19), Hüper (30), and Lohbauer (39)), yet the principle of similarity has been taken for granted and has never been experimentally verified in a systematic way.

This transference of a response from a given stimulating object or situation to a similar stimulus obviously belongs to the general field of transfer. If the doctrine were true, similarity of the stimulating situations would constitute at least one of the conditions under which transfer occurs. Is such similarity one of the conditions of transfer? In spite of the wealth of experimental work on transfer,[8] we know practically nothing as to the exact conditions under which transfer does or does not take place, the conditions under which it is positive or negative, and the factors that determine the amount of transfer. We have a number of theories that purport to state the nature of these conditions, but only one of these employs the concept of similarity—that of Thorndike (55, 57) and Poffenberger (49). But in this theory, the similarity involved is that of bonds of connection, or similarity of the two activities in respect to their stimulus-response constituents, rather than a similarity of the two stimulating situations. Is similarity of the two stimulating situations a favorable or an essential condition of transfer? No one has proposed such a conception, and I know of but two experiments that have attacked the problem from this standpoint—that of Wylie (67) with animal subjects, and that of Bruce (8), an unpublished thesis in the field of memory.

The factual character of the doctrine that responses and attitudes that have been attached to given stimuli are likely to be elicited by similar stimuli with which they have not been so associated in times past, is also involved in the problem of trade mark infringements. Obviously business firms attempt infringement of successful trade marks on the assumption that the public attitudes and responses to these distinctive trade marks will likely be elicited by highly similar ones (Barber (2), Paynter (43-45), and Rogers (51, 52)). Judicial practice, however, seems to hold that an infringement is involved only when the two trade marks are so highly similar as not to be distinguished in ordinary experience. Consequently the

[8] Some of the recent articles and a few of the earlier ones of importance are included in the Bibliographical Section (3-8, 14-17, 20, 23, 25, 26, 29, 31-35, 37, 38, 41, 46, 49, 50, 55-57, 61-65).

various experimen... ... ... een primarily concerned with testing the likelihood of memory confusion of a pair of such stimuli (Katzaroff (36), Feingold (21), Paynter (44, 45)), and have not attempted a systematic investigation of the more general principle involved.

The principle at issue is thus involved in such phenomena as association, conditioning, the growth of language ability, the formation of class concepts, generalization, transfer of training, and trade mark infringement. The principle is frequently not even recognized, and when recognized its truth is generally taken for granted. In only a few cases has the principle been subjected to an experimental investigation.

This experiment was initiated on the belief that a principle of such widespread and fundamental importance from the standpoint of the integration and organization of behavior should be experimentally established and systematically investigated. In our procedure the subjects are required to memorize a series of paired associates to the point of mastery. In the recall test, twenty-four hours later, novel stimuli exhibiting certain specified kinds and degrees of similarity, were substituted for the original stimuli. The amount of recall for the substitute stimuli was compared with that for the standard condition in which the original stimuli were employed.

The influence of three kinds of similarity was investigated: 1. Similarity of nonsense syllables in respect to spelling. 2. Similarity of word stimuli in respect to meaning. 3. Similarity of visual patterns.

EXPERIMENT I SIMILARITY OF NONSENSE SYLLABLES IN RESPECT TO SPELLING

The memory materials consisted of seven lists, each containing fourteen paired associates. The stimulus items consisted of two hyphenated nonsense syllables, and the response item was a word of four letters. Each pair was presented for four seconds. All the subjects memorized each of the seven lists and in the same order. Each list was learned in a single sitting, and recall was tested the following day preceding the memorization of the subsequent list. Each list was mastered to the point of a successful immediate recall with a chance order of presentation. All subjects were given one practice list before beginning the experiment proper.

The nature of the stimulus alterations employed in the subsequent recall test are specified and illustrated in Table I. Both the number and the position of the spelling alterations were varied. Each of the seven conditions of recall occurred twice in each list of fourteen paired associates, and the ordinal position of each of these seven conditions was systematically varied from list to list so that each condition occupied every ordinal position.

TABLE I

Conditions	Original Stimuli	Response Words	Recall Stimuli
Same Syllables	REB-QIM	Wolf	REB-QIM
1st Letter of 1st Syllable Changed	HUD-LEP	Fist	XUD-LEP
2d Letter of 1st Syllable Changed	TOQ-BEX	Jury	TIQ-BEX
1st and 2d Letters of First Syllable Changed	KAJ-ZOY	View	NEJ-ZOY
1st Letter of 2d Syllable Changed	VAH-MIZ	Nose	VAH-PIZ
2d Letter of 2d Syllable Changed	WUL-GIC	Vase	WUL-GOC
1st and 2d Letters of Second Syllable Changed	JEC-POR	Mask	JEC-NAR

Records were obtained from thirty-seven subjects. Since each condition occurred twice in each of the seven lists, we obtained from each subject fourteen recall scores, and hence the average group value for each of the seven conditions is based upon 518 scores. The data are given in Table II. The first column lists the seven conditions. The second column gives the average number of response words recalled per individual for the seven lists—the maximum possible value being fourteen. The probable error

TABLE II

Conditions	Average No. Recalled	Prob. Err. of Mean	Percentage Recalled
Same Syllables (Control)	9.4865	.2992	67.761
1st Letter of 1st Syllable Changed	5.6216	.2325	40.154
2d Letter of 1st Syllable Changed	8.3784	.2085	59.846
1st and 2d Letters of First Syllable Changed	5.5135	.3003	39.382
1st Letter of 2d Syllable Changed	5.7297	.2760	40.926
2d Letter of 2d Syllable Changed	7.5405	.2241	53.861
1st and 2d Letters of Second Syllable Changed	5.2703	.2414	37.645

of these means is also given, and finally the amount recalled is stated in percentage terms.

1. All alterations of the stimulus reduced the amount recalled as compared with the standard condition of no change. All of the differences are statistically significant.

2. An alteration of the first letter of either of the two syllables of the stimulus item reduces the recall more than does a change of the middle or vowel letters of these two syllables. Both differences are statistically significant.

3. A change of two letters in either of the two syllables reduces the recall score more than does a change in either of the two letters singly. The differences are consistent but they are too small to permit a confident conclusion.

Stated in terms of similarity we may conclude:

1. A substitute stimulus exhibiting a similarity to the original stimulus in respect to four of the six letters elicited a large amount of recall, but an amount that is significantly less than for the standard condition.

2. There is no conclusive evidence that the amount of recall varies significantly with the degree of similarity.

3. The amount of recall varied significantly with the locus of similarity in spelling. Similarity in respect to the initial letter of either syllable is more conducive to recall than a similarity in respect to the middle or vowel components of the two syllables.

EXPERIMENT II SIMILARITY OF WORD STIMULI IN RESPECT TO MEANING

The memory material consisted of three lists—each containing twelve paired associates. Both stimulus and response items consisted of meaningful words, the response item being a word of six letters. Each pair was presented for three seconds. All the subjects memorized each of the three lists, and in the same order. Each list was learned in a single sitting, and recall was tested on the following day preceding the memorization of the subsequent list. Each list was mastered to the point of a successful immediate recall with a chance order of presentation. All subjects were given one practice list before beginning the experiment proper.

In the subsequent recall, twenty-four hours later, three conditions were employed. In one condition the original stimulus word was retained as a norm for comparison. In the other two conditions, new words were substituted for the stimulus words, and these new words exhibited one of two degrees of meaning similarity to the original stimulus words.

The procedure of selecting these similar words was as follows: For each stimulus word of the lists, the experimenter assembled a series of words exhibiting a wide range of similarity in respect to meaning. These words were then ranked by twenty judges in respect to their degree of similarity to the original stimulus words. From each series the experimenter selected for the recall test those words which exhibited two degrees of similarity according to the unanimous opinion of the judges.

Samples of the original stimulus words, the two similar words, and the response words are given in Table III. The words that were employed as stimuli in the subsequent recall test are italicized. Each of the three conditions of recall occurred four times in each list of twelve paired associates, and the ordinal position of each of these three conditions was systematically varied from list to list so that each condition occupied every ordinal position.

Records were obtained from fifty-four subjects. Since

TABLE III

The stimulus words used in recall test are italicized

Original Stimulus Words	Degree of Similarity		Response Words
	1st Degree	2d Degree	
SNAKE	*SERPENT*	TURTLE	WEALTH
FIGHT	BATTLE	ARGUMENT	LEDGER
HOUSE	COTTAGE	*BARN*	BREEZE

each condition occurred four times in each of the three lists, we obtained from each subject twelve recall scores, and hence the average group value for each of the three conditions is based on 648 scores. The data are given in Table IV. The first column lists the three conditions. The

TABLE IV

Conditions	Arithmetic Mean	Prob. Err. of Mean	Percentage Recalled
Same Word (Control)	6.0185	.2167	50.154
1st Degree of Similarity	3.9074	.2165	32.562
2d Degree of Similarity	1.3519	.1640	11.266

second column gives the average number of response words recalled per individual for the three lists—the maximum possible value being twelve. The probable errors for these means are also given, and finally the amount recalled is stated in percentage terms. The differences between the means are thoroughly reliable, and the mean values for the individual subjects exhibit the same trend with but three inversions of order in the one hundred and eight comparisons.

The results prove quite conclusively that a response associated with a stimulus word tends to be aroused by a novel word having a similar meaning, and that the likelihood of arousal varies directly with the degree of similarity.

EXPERIMENT III SIMILARITY OF VISUAL PATTERNS

The memory material consisted of three lists—each containing fifteen pairs of items. Each pair consisted of a visual pattern as the stimulus and a word of three letters as the response item, and it was presented for three seconds. All the subjects memorized each of the three lists, and in the same order. Each list was learned in a single sitting, and recall was tested on the following day preceding the memorization of the subsequent list. Each list was mastered to the point of a successful immediate recall with a chance order of presentation. All subjects were given one practice list before beginning the experiment proper.

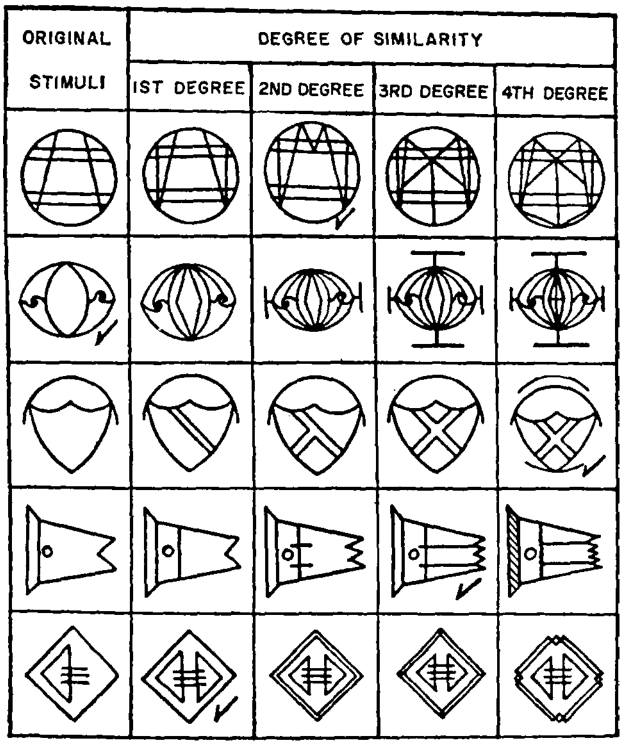
Five conditions of recall were employed in the subsequent test, twenty-four hours later. In the first condition the original visual pattern was retained as a norm for comparison. In the other four conditions, new visual patterns were substituted for the original stimuli, and they exhibited one of four degrees of similarity to those stimuli. The visual patterns employed as stimuli were drawn by the experimenter, and for each of the original stimuli were drawn a series of patterns exhibiting a wide range of similarity of appearance. Each series of patterns was arranged in order of similarity by each of fifteen judges, and from each series were selected for the recall test four patterns exhibiting four gradations of similarity according to the unanimous opinion of the judges.

Samples of the patterns exhibiting all four gradations of similarity are given in Table V. Those patterns used in the recall test are checked. Each of the five conditions of recall occurred three times in each list of fifteen paired associates, and the ordinal position of each of these five conditions was systematically varied from list to list.

Records were obtained from twenty-six subjects. Since each condition occurred three times in each of the three lists, we obtained from each subject nine recall scores,

TABLE V

The stimuli used in recall test are checked



and hence the average group value for each of the five conditions is based upon 234 scores. The data are given in Table VI. The first column lists the five conditions. The second column gives the average number of response words recalled per individual for the three lists—the maximum possible value being nine. The probable errors for these means are also given, and finally the amount recalled is stated in percentage terms.

The average amount recalled varies directly with the degree of similarity without a single exception. The dif-

TABLE VI

Conditions	Average No. Recalled	Prob. Err. of Mean	Percentage Recalled
Same Pattern (Control)	7.6154	.1684	84.616
First Degree of Similarity	5.8077	.2349	64.530
Second Degree of Similarity	4.4231	.2144	49.146
Third Degree of Similarity	4.0769	.2229	45.299
Fourth Degree of Similarity	3.2692	.2218	36.324

ference between the normal condition and the first degree of similarity is statistically reliable, and the averages for all of the subjects conform to the group results. The difference between the first and the second degree of similarity is also reliable and only three of the individual means fail to conform to the average results for the group. The difference between the second and the third degree of similarity is not statistically reliable, and the same order of difference is exhibited by ten of the twenty-six subjects, and the reverse order by four subjects of the group. The difference between the third and the fourth degree of similarity does not exhibit any high degree of reliability, but the same order of difference is exhibited by sixteen subjects and the reverse order by but two subjects of the group of twenty-six individuals.

We may thus conclude that responses associated with a visual pattern tend to be aroused by a similar visual pattern, and that the likelihood of recall tends to vary directly with the degree of similarity.

CONCLUSIONS

1. Our experiment has established the fact that a naming response that is contiguously associated with a visual stimulus is likely to be aroused by a visual stimulus with which it has not been so associated, whenever the two stimuli exhibit certain kinds and degrees of similarity.

2. The effectiveness of three kinds of similarity has been demonstrated—similarity of nonsense syllables in respect to spelling, similarity of word stimuli in respect to

meaning, and similarity of visual patterns. There are no data on the relative effectiveness of these three kinds of similarity.

3. With similarities of meaning and visual patterns, the likelihood of recall varied directly with the degree of similarity. There was no evidence that the likelihood of recall varies with the amount of similarity of nonsense syllables as measured by the number of letters in common.

4. With nonsense syllables employed as stimuli, the likelihood of recall varied with the locus of the common letters.

5. Presumably this phenomenon can be obtained with other modes of stimulation, auditory, *e.g.*, with other than naming responses, with other kinds of similarity, and with wider degrees of dissimilarity than those employed. It is presumed that we are here dealing with a general principle, whose limitations are as yet undefined.

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4

Transfer of Training in Reasoning

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By transfer is meant the learning influence of one training activity on a different activity. A study of learning methods gives promise of yielding significant results in support of the theory that transfer is a major factor of learning among the higher mental abilities. In the present study, data are put forth to substantiate the above mentioned theory. Most previous studies on the subject are open to the objection that transfer effects probably resulted from the combined effect of similarity of test material and similarity of method in responding to the subject-matter. By method is meant the more or less generalized responses exclusive of identical responses carried over from specialized training to the end test. It is well known that the factors of method and content must be controlled separately before valid conclusions may be made as to the amount of transfer which takes place.

Headway has been made showing transfer among the complex mental functions. In the first statistical study on this topic [Woodrow, 1927] the subjects who received special training in method gained on the average 31.6 per cent more than the control subjects. For control and experimental groups, the two end tests included memorizing poetry, prose, Turkish-English vocabulary, dates, and the span of comprehension for consonants. The interpolated practice lessons included memorizing poetry verbatim and learning columns of paired nonsense syllables. While the transfer gains are reliable, it cannot be certain that they are not the result of a carry over of similar content.

In arithmetic, transference of method [Overman, 1931] has been obtained when the end tests and the training lessons were similar. Group *A*, having practice only in different types of two-place addition, gained forty-six per cent in similar two- and three-place additions and subtractions. Group *B*, having practice and instruction in generalizing, improved 67.6 per cent. Group *C*, having practice and analysis of principles involved, gained 53.8 per cent. Group *D*, from practice, analysis, and generalizing, made a gross gain of 63.8 per cent. In the foregoing results, it is impossible to tell how much transfer was due to similarity of content and how much to method.

Transfer effects have been obtained [Winch, 1923] in reasoning from one type of subject-matter to another. One group of experimental subjects made a residual transfer gain of one hundred thirty per cent in solving the following type of logical problems: "There is a large lake in a forest, and the forest is in a country called Finland. Which is the larger, the lake or the whole country of Finland?" The experimental group were trained over a period of ten weeks.

Special training in analysis and formulation of definitions gave a measurable amount of transfer [Meredith, 1927]. The end tests were merely definitions of ordinary terms. The training *S*'s were given three lessons of five short experiments dealing with magnetism, and requiring from five to ten minutes each. They were followed by illustrations and discussions as to the requirements of a good definition. Both groups wrote definitions about the experiments. The result was a gain of 9.2 per cent by the experimental group and a loss of seven per cent by the control group.

The problem of the present study is to ascertain the transfer effect on one kind of material by training in reasoning in material of an utterly different kind. One hundred and thirty-five subjects took part. There were seventy-six twelve- and thirteen-year-old pupils from the seventh and eighth grades. I employed two groups of elementary S's of thirty-eight each. The two groups were paired in regard to their IQ's. In CA their range was fifteen months. The mean IQ of the elementary experimental S's was 107.23; the sigma of their IQ's was 6.70, and the average mental age, 169.71 months. The average IQ of the elementary control S's was 106.76; the sigma of their IQ's was 6.59 and the average mental age, 163.97 months. A total of fifty-nine adult cases are reported. They were paired in two groups according to O. S. U. Psychological Centile scores. The average centile score of the thirty-one experimental S's was 61.35, and the average centile for the twenty-eight control S's was 61.20. In the experimental group twenty-three of the thirty-one subjects were in the centiles fifty to one hundred, while eight were below the fiftieth centile. Of the twenty-eight control subjects twenty-one were above the fiftieth centile and seven were below the fiftieth centile. The rho was .96; the *r* was calculated between the corresponding numbers within the various deciles.

The four groups took the initial test and the end test, both consisting of fifteen of Aesop's Fables. The task in both tests was to write the lesson conveyed by each fable. The experimental group were given twelve carefully planned lessons treating several phases of simple analysis, abstraction, and generalization. Each lesson lasted twenty minutes. One year later the S's of the seventh and eighth grades were re-tested.

The items of the initial and final tests were paralleled in regard to difficulty. The PE difficulty units corresponding to the different items were calculated from answers secured from ninety-one elementary-school pupils. The per cent of pupils answering each question correctly was converted to PE units from a table such as Garrett's table of the normal curve expressing percentages in PE units. In Table I are the names of the fables and their PE difficulty units.

An answer was scored as correct if it gave a direct state-

TABLE I

Fables comprising end tests in transfer of reasoning from one kind of training material to another, showing names of fables and difficulty in terms of PE units

	Difference		Difference
The Widow and the Hen	4.98	The Ant and the Grasshopper	5.10
The Crab and Her Mother	5.27	The Thirsty Pigeon	5.52
The Angler and the Little Fish	5.88	The Lioness	5.93
The Bundle of Sticks	6.20	The Miser	6.29
The One-Eyed Doe	6.26	The Fox and the Mask	6.38
The Creeking Wheels	6.38	The Jackass in Office	6.44
The Ass and the Grasshopper	6.44	The Goat and the Goatherd	6.58
The Trumpeter Taken Prisoner	6.58	The Countryman and the Snake	6.66
The Lion and the Bulls	6.74	The Great and the Little Fishes	6.81
The Swallow and the Raven	6.81	The Sick Kite	6.81
The Two Pots	6.90	The Mountain in Labor	6.90
The Old Woman and Her Maids	7.01	The Stag and the Pool	7.01
The Wolf and the Shepherds	7.23	The Fir Tree and the Bramble	7.37
The Gnat and the Bull	7.37	The Fox and the Lion	7.37
The Lion and His Three Counselors	7.73	The Wolf and the Horse	7.99
Mean	6.48		6.58

ment of the lesson involved, or an unmistakable suggestion of the right answer. For example, consider the fable of "The Widow and the Hen." It goes as follows:

"A widow kept a hen that laid an egg every morning. Thought the woman to herself, 'If I double my hen's allowance of barley, she will lay twice a day.' So she tried her plan, and the hen became so fat and sleek, that she left off laying at all."

The best answer is: "Figures are not always facts." The following other answers were also counted as right:

"Don't be greedy, let good enough alone"; "Greed does not always pay"; "Be content with what you have"; and "Don't count your chickens before they are hatched."

The following and other similar answers are wrong: "To be selfish is to be useful"; "Don't be anxious"; "Be patient"; "Keep what you get and try for no more"; and, "One hen can't do the work of two." All papers were scored independently by two persons with results that were in almost exact agreement.

The interpolated training of the experimental S's included twelve lessons of twenty minutes each, given to instruction and practice in reasoning. Four lessons were on analogies of the following form: Prince is to Princess as King is to ———. The pupils completed exercises in supplying one, two, and three missing terms. Analogies given in every lesson were written on the board or mimeographed. The material was also read to the pupils. The S's read their answers and gave the steps by which they arrived at the results. They wrote out their steps of reasoning. In like manner, four lessons were given on the analysis and practice with abstractions and generalizations being made from the concrete to the general and from the general to the concrete. Four reading lessons were given emphasizing comprehension and analysis of behavior situations. Attention was called to the best answers. Pupils wrote the mental steps by which they arrived at the answers. Tabular results appear in Table II. The experimental group of elementary S's gained 64.03 per cent as a result of being trained in reasoning during the interval between the end tests. The corresponding control group lost 0.09 per cent. The experimental adult group gained 23.70 per cent. The control adults gained 7.31 per cent. The transfer was 16.27 per cent. Combining the two experimental groups, the average gain was 37.96 per cent. Combined, the control groups gained 5.59 per cent. The residual gain was 32.52 per cent.

The number of sigma difference units in the difference of the mean scores of the initial and the final tests is 2.78 for the experimental groups combined. The control groups made no significant gains as a result of special practice.

TABLE II

Average number of correct answers to fables in two end tests by experimental and control elementary-school and college subjects showing transference of reasoning in one kind of material to reasoning in another

	Experimental elementary S's	Control elementary S's	Experimental adults S's	Control adults S's	Experimental total S's	Control total S's
Test I	3.28	3.16	7.93	8.07	5.35	5.17
Test II	5.35	3.13	9.81	8.66	7.38	5.45
Gross gain	2.07	−.03	1.88	.59	2.03	.29
Gain per cent	64.03	−.09	23.70	7.31	37.96	5.59
N	38.00	38.00	31.00	28.00	69.00	66.00
Gain	2.07		1.29		1.74	
Per cent transfer	64.03		16.27		32.52	

The sigmas and correlation coefficients are put forth for experimental and control S's in the initial and final tests. The sigma of the elementary experimental group scores in the initial test is 2.19; in the final test it is 2.68; the r of the same scores is 0.65. The sigma of the adult experimental scores in the initial test is 2.75; in the final test it is 2.96; the r of the gross scores is 0.54. The sigma of the elementary control group scores in the initial test is 1.62; in the final test it is 1.67; the r is 0.57. The sigma of the adult scores in the initial test is 2.76; in the final test it is 2.82; the r is 0.65.

The more intelligent seventh- and eighth-grade pupils made greater transfer gain than the less intelligent, while the more intelligent adults made less gain than the less intelligent adults. The seventh- and eighth-grade S's of the upper fifty per cent, according to intelligence test scores, made a gross gain thirty per cent greater than that of the lower half. Similarly, the upper fifty per cent, according to the initial reasoning test scores, gained eleven per cent more by transference than the lower half. On the other hand, the upper half of the adults, selected according to intelligence test scores, gained thirty-seven per cent less than the lower half. Similarly, the upper fifty per cent, selected according to the initial reasoning test scores, gained sixty-two per cent less than the lower half.

During the year after the training, the control S's tended to show a larger non-practice gain than the experimental S's. The retention test was given the elementary subjects a year after the training, care being taken to give each pupil the same test which he took at the close of the training series. The mean score of twenty-five cases of the experimental group was 5.72, which represents a non-practice gain of .37 of a point. The final test scores at the close of the training lessons, by the pupils who did not take the re-test, averaged 4.7, which is slightly less than the group average of 5.35. The mean of the twenty-seven who made up the control for the re-test group was 5.81, which represents a non-practice gain of 2.68 in a year's time. The average of the final test by the control S's who did no take the re-test was 4.00 which is slightly greater than the group average of 3.13.

DISCUSSION

In all the above mentioned experiments where training was devoted to instructions on abstracting, analyzing, and generalizing, a relatively large amount of transfer resulted. When the final test material differed entirely from the training matter, the transfer amounts were as great as when the end tests resembled the interpolated content. Transfer would seem to depend on the analysis of the mental steps involved in learning; on descriptions of various aspects of the problems under consideration; on comparisons in regard to similar problems; methods and practice in definitions; on proceeding from the concrete to the general and *vice versa* with respect to reasoning in the training material, and on intentional efforts to apply learning methods.

Several factors lend support to the theory that general transfer takes place in the form of the learning curve. In the present study the upper half of the seventh- and eighth-grade S's, using mental test scores as a criterion, made larger transfer gains than the lower half. The lower half of the adults made greater gain than the upper half. The experimental pupils of the elementary grades during the year after the study maintained their gains, while the control S's improved only about as much as the experimental S's gained as a result of two weeks of special training. The lack of measurable growth among the experimental S's during the year following the training may be due to the combined influence of forgetting and maturation, the one tending to decrease and the other tending to increase reasoning efficiency. Although transfer effects as found in studies of record have not appeared to take place in the form of the learning function, if our theory is correct, they may be shown to take place in such a way by the use of small measuring units. Experiments are now under way by the writer to test this assumption.

If it turns out that transfer conforms to the curve of learning, it would not appear to be contrary to existing theories. Transfer as a result of similarities between test material and the training material is to be accounted in terms of increasing complexity of the learning activities. Transfer from the viewpoint of generalizations may be thought of as integrated activities in which the way of doing the interpolated performances is applied to the performance of the end tests.

The organization theory, meaning that transfer takes place in the degree to which activities learned during practice become organized components of the end test responses, implies learning to be a unitary experience which grows more complex with practice. It would seem reasonable to suppose that progress is much the same in generalized as in specialized learning, that is, according to the negatively accelerated learning curve.

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5

Transfer of Training in Learning to Hit a Submerged Target

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The present study was undertaken to afford a general check upon the early study of transfer of training by Scholckow and Judd [1908]. In that experiment, fifth- and sixth-grade boys threw darts at a target placed at different depths under water. The results have been widely quoted as important to the theory of transfer of training by generalization.

The intent has been to attack the same problem, and in a fashion essentially similar, but no claim is made that the experimental conditions duplicate those of the earlier study. Available accounts of this study, including the original report published by Judd in 1908, are not adequate to permit a perfect duplication, or to afford a detailed comparison of results. Furthermore, certain variations in the procedure have been purposely introduced to secure a wider variety of evidence on the problem.

INVESTIGATION BY SCHOLCKOW AND JUDD

The study described by Judd was intended to show the relation between knowledge of a principle and practical behavior in a situation to which the principle applies. The subjects were two groups of boys, of fifth- and six-grade school status, equated on the basis of teachers' judgments as to their brightness. The boys were required to throw a small dart at a target placed under water. Judd's account of procedure and results follows:

> In this experiment one group of boys was given a full theoretical explanation of refraction. The other group of boys was left to work out experience without theoretical training. These two groups began practise with the target under twelve inches of water. It is a very striking fact that in the first series of trials the boys who knew the theory of refraction and those who did not gave about the same results. That is, theory seemed to be of no value in the first tests. All the boys had to learn how to use the dart, and theory proved to be no substitute for practise. At this point the conditions were changed. The twelve inches of water were reduced to four. The difference between the two groups of boys now came out very strikingly. The boys without theory were very much confused. The practise gained with twelve inches of water did not help them with four inches. Their errors were large and persistent. On the other hand, the boys who had the theory, fitted themselves to four inches very rapidly. Their theory evidently helped them to see the reason why they must not apply the twelve-inch habit to four inches of water. [Judd, 1908, p. 37.]

The number of subjects, details as to the apparatus, the verbal instructions given the tutored group, and the quantitative results were not reported.

PRELIMINARY INVESTIGATION OF TECHNIQUES

In an effort to duplicate as nearly as possible the work of Judd and Scholckow, attempts were made to throw a feathered dart at a target under water. The dart used was of hardwood with a steel point, the feathers being waterproofed with collodion. A bathtub served as tank. Certain difficulties were at once apparent. (1) A strong tendency was found for the dart to ricochet from the surface of the water. (2) Successful penetration of the water and subsequent removal of the dart disturbed the surface of the water so greatly as to require considerable time between throws. (3) The dart itself proved difficult to control as to direction. There was no certainty that the dart would travel in the direction desired.

In view of these difficulties, comparative evidence was secured for dart-throwing and a function believed to be similar; namely, shooting an air gun. Thirty-four boys, selected from those serving as subjects in the later experiment, made a total of thirty-four hundred dart-throws towards a circular target of seven inches diameter at a range of eight feet. Fifteen hundred and twenty throws missed the target. On the other hand, only four air gun shots out of four hundred twenty, made by forty-two of the subjects, missed a circular target of 3.4 inches diameter at a range of fifteen feet. It was further found that the shot penetrated the surface of the water easily and disturbed the surface but slightly. The use of feathered darts was accordingly abandoned as impracticable. It was concluded that the air gun would serve adequately the chief purposes of this study.

DESCRIPTION OF EXPERIMENT

Ninety boys in the eighth grade of a suburban junior high school served as subjects in the experiment. Each was assigned to one of three groups of equal size, designated as: Control group, Experimental group A, and Experimental group B. Table I shows the mean age of each group to be about fourteen years, and the mean percentile rank for intelligence as measured by the Otis Group Intelligence Scale, to be about seventy. Marksmanship was not tested for all subjects in advance of the experiment, but the preliminary experimentation had shown only negligible differences in this regard between two groups of twenty-one boys each, closely matched in age and intelligence (mean distance from center of target, for one group, at a range of fifteen feet, was .65 inch; for the second group, .59 inch).

TABLE I

Mean, chronological ages and percentile ranks of subjects

Group	*N*	CA	PR
Control	30	14-0	72.0
Experimental group *A*	30	14-0	70.7
Experimental group *B*	30	14-2	71.8

The air gun (unrifled) was a fifty-shot repeater, firing copper-plated steel BB shot with sufficient force to penetrate the depths of water used and still retain enough velocity to perforate a cardboard target. Standard rifle targets were used, held in a horizontal position in a galvanized iron mounting. The tank was a twenty-gallon tub of galvanized iron, with a bottom diameter of twenty inches, a top diameter of twenty-four inches, and a depth of eleven inches. The subjects fired from a platform eighteen inches high, at a distance of eight feet from the center of the target. The elevated position of the marksman assured that the tank would not obscure part of the target. The subjects used the door frame of the laboratory as a rest while shooting. The experimenter recorded each hit as it was made.

All of the work was done outside of school hours. The subjects participated with enthusiasm in what they regarded as a uniquely desirable activity. Each boy was warned not to give any other boy any clue as to how to hit the bull's-eye. The strong flavor of competition is believed to have been adequate to prevent direct instruction of one subject by another. Three or four boys served as subjects during one period, but each one worked in private.

On the basis of the preliminary work, shots falling in the bull's-eye or the ring next to it in the rifle target were regarded as successful. This allowance was found sufficient to guarantee that at the close range used success would be attained as soon as the subject knew where to aim. The mean distance from the center of the target for four hundred twenty shots (ten shots each by forty-two boys) fired at a range of fifteen feet was .62 inch, with a mean deviation of .31 inch. At eight feet the mean error might be expected to become .33 inch, and the mean deviation .17 inch. The "zone of success," on the other hand, was bounded by a circle with a radius of .69 inch.

At the same time, the amount of refraction at the depths employed (six inches and two inches) was great enough so that a boy who aimed at the bull's-eye where it appeared to be was practically certain to miss it by a sizeable margin. For a boy of average height (sixty inches), the position from which he viewed the target was such that the angle of incidence for his line of vision and the surface of the water was fifty-one degrees. At this angle the apparent displacement of the target was 3.11 inches when the water was six inches deep, and 1.04 inches when the water was two inches deep.

PROCEDURE FOR CONTROL GROUP

As each of the thirty boys constituting the Control group entered the laboratory, he was shown where to stand and told that a record was to be kept of the number of trials necessary before he made three consecutive hits in either the bull's-eye or the ring next to it. The first of these three successful hits was recorded as the one at which mastery of the problem was reached. Two problems were set for each subject, the first with the water at the depth of six inches, the second with the depth of the water changed to two inches. The subjects were required to satisfy the criterion of three consecutive successful hits for the first problem before undertaking the second.

PROCEDURE FOR EXPERIMENTAL GROUPS

For each of the experimental groups, conditions and requirements for the control group were duplicated, with one addition. This was the provision of an elementary explanation of the theory of refraction, which each subject was permitted to study until he declared that he understood it. He was not, however, allowed to ask questions. For group *B* the explanation was identical with that for group *A*, except that one sentence was added. This sentence specifically called attention to the fact that changing the depth of water changed the amount of the refraction. Following is the complete explanation, which was accompanied by Figure 1:

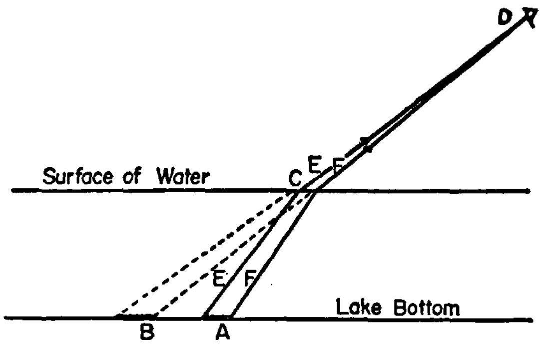

Fig. 1. Diagram illustrating explanation of refraction.

EXPLANATION OF REFRACTION, AS PROVIDED FOR GROUP A

Everything we see is visible because light comes from it to our eye. Objects under water, when seen from above, do not appear in their true positions. Thus, in the diagram, the rock at *A* seems to be at *B*.

This deception is caused by the refraction, or bending, of the light beam *ACD* at the surface of the water. The light rays are bent because light moves faster in air than it does in water. The side of the beam marked *E* escapes from the water before the side marked *F* reaches the air. Therefore, side *E* gets ahead of side *F* and the ray of light is actually bent.

We are not used to light rays being bent. Consequently, we suppose that the stone lies in a straight line from our eye, and we make the mistake of thinking that it is at point *B*.

Added Explanation for Group B. It is easy to see from the diagram that the deeper the lake is, the farther the real rock A will be from the image rock *B*.

RESULTS FOR FIRST PROBLEM

Tables II and III present the major results for the first problem, that of learning to hit the target at a depth of six inches. Both experimental groups learned more rapidly than the control group, and group *B* learned more rapidly than group A. It will be recalled that Judd reported that in the first trials the boys who knew the theory and those who did not "gave about the same results." In the present study, not only did theory seem to be of value in the first trials, but the addition of a few words to the explanation measurably increased the apparent value of the theoretical information.

TABLE II

Trials required for mastery of first problem (depth six inches)

Group	Mean	SD
Control	9.10	5.23
Experimental group *A*	8.50	4.08
Experimental group *B*	7.73	3.51

TABLE III

Group differences for first problem (depth six inches)

Groups compared	Difference between groups	SD difference	Critical ratio	Chances in 1000
Control − *A*	.60	1.21	.49	688
A − *B*	.77	.98	.79	785
Control − *B*	1.37	1.15	1.19	883

The differences in the rate of learning between the three groups on these first trials are not of high statistical reliability (Table III), perhaps because the groups were small. The differences for the two experimental groups are, however, in the same direction.

RESULTS FOR SECOND PROBLEM

Precisely parallel differences are found in the data of Tables IV and V, for the second problem, that of learning to hit the target at a depth of two inches. Here the differences are in keeping with the differences reported by Judd, although the degree to which the instructed subjects (group A and B) surpassed the uninstructed (the Control group) appears to have depended upon the explicitness of the theoretical explanation, as may be seen from the fact that the scores for group B were lower than those for group A. Table V indicates that the differences are somewhat more reliable than those for the first problem.

TABLE IV

Trials required for mastery of second problem (depth two inches)

Group	Mean	SD
Control	6.03	2.62
Experimental group *A*	5.37	2.28
Experimental group *B*	4.63	2.08

TABLE V

Group differences for second problem (depth two inches)

Groups compared	Difference between groups	SD diff.	Critical ratio	Chances in 1000
Control − *A*	.66	.63	1.05	853
A − *B*	.74	.56	1.32	907
Control − *B*	1.40	.60	2.34	990

TRANSFER FROM FIRST TO SECOND PROBLEM

It is to be regretted that no numerical data from Judd's experimentation are available. The account quoted appears to imply that transfer, as measured by improvement from the first to the second problem, took place only for the instructed group. In the present study, transfer, in the sense of improvement, occurred both with and without tuition, and in amounts not greatly differing for the three groups. Table VI shows a mean gain of 3.1 trials for each group, the relative gain being greatest for group *B*, the boys given the most complete explanation.

TABLE VI

Summary table showing improvement from first problem to second problem

Groups	Mean of trials required		Gain in trials	Percentage of improvement
	At 6 inches	At 2 inches		
Control	9.10	6.03	3.07	34.1
Experimental group *A*	8.50	5.37	3.13	36.5
Experimental group *B*	7.73	4.63	3.10	40.3

The task of learning to hit a submerged target appears to be in the field of motor learning, and the original study generally has been interpreted as belonging to this category. However, Judd's report indicates little concern with motor skill but instead, an interest chiefly or solely in the learner's adaptation to the principle of refraction. In the present study the motor skill element is negligible—the real problem is to discover where to aim. Once a boy made this discovery, he had no difficulty in meeting the criterion for successful performance. This statement is supported by the fact that many of the boys in each group made such significant remarks as, "I see the catch," "I got it now," and the like.

INDIVIDUAL DIFFERENCES

Within each group there were large individual differences in the speed with which the boys "saw the trick," as indicated by the standard deviations reported in Tables II and IV. Two boys in the control group, one in group A, and one in group *B* solved the first problem at the first shot. It seems probable that these boys knew they could not hit the bull's-eye by aiming at it, made a lucky guess, found themselves successful, and repeated. On the other hand, twenty or more shots were required for the solution of the first problem by two boys in the control group, one in group A, and one in group *B*. These boys took a long time to discover where to aim. In fact, they probably took a long time to discover that aiming at the apparent position of the bull's-eye would not bring success. Once this discovery had been made, it was not hard to find that the place to aim was below the bull's-eye.

POINTS OF DIFFERENCE FROM STUDY BY SCHOLCKOW AND JUDD

Certain differences in findings from the early study reported by Judd have been noted. In view of these differences, it should be emphasized that the present study does not claim to duplicate the details of the earlier one, but merely to make a fresh attack upon the same problem in a comparable manner. It would manifestly be impracticable to repeat the earlier study, in view of the limited accounts of it that are available.

The following factors may account in part or in whole for differing findings: (1) The skill studied was changed from throwing a dart to shooting an air gun. (2) The subjects were probably from two to three years older than in the earlier study. (3) The explanation of refraction may have taken a different turn. Evidence has been presented to show that a slight change in this explanation may affect results. (4) Other phases of experimental procedure may have differed. (5) The extent to which motor learning was a factor may have varied. If in the earlier study the subjects had to learn to throw darts as well as to adapt to refraction, the task was a more complex one than that set for the subjects of the present investigation. (6) In part differences may be due to chance. The number of cases in the earlier study is not known, and measures of reliability were not commonly reported at that time. The reliability of the present results is greater than "mere chance," but less than "practical certainty."

SUMMARY AND INTERPRETATION

(1) Under experimental conditions similar to those in the original Scholckow and Judd study, knowledge of theory was found to facilitate transfer. In this respect the original findings were confirmed.

(2) However, theoretical information was of aid not only in transfer from one situation to another, but also in making the original adjustment to the first situation. This result is in contrast to findings reported in the earlier work.

(3) The definiteness or completeness of the theoretical information had a direct effect upon both initial learning and transfer. Even in as simple a problem as that presented to the subjects of this study, it is clearly important for a teacher to consider carefully the adaptation of any theoretical statement to the specific needs of the learner. In the original investigation the influence of the precise form of the theoretical explanation seems to have been disregarded.

(4) In the present study the importance of individual discovery of the solution—of the emergence of a sudden insight, in Gestalt terms—is apparent. In each group and for each problem the typical boy worked unsuccessfully for a time, then quite abruptly reached a solution. The appearance of this moment of discovery undoubtedly was hastened in many cases by the theoretical explanations provided for the experimental groups.

(5) On the other hand, the large individual differences within each group and the consequent overlapping of the various groups in speed of learning suggest that success in the type of problem presented is probably condi-

tioned by other factors in addition to knowledge of a theoretical principle as formulated by a teacher. Such additional factors may include fluidity and variability of behavior when faced by a problem, a habit of verifying one's judgments, and the ability to formulate a general principle for oneself.

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6

The Similarity Paradox in Human Learning: A Resolution

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Behavior is a continuous, fluid process, and activities learned in the laboratory are as much a part of it as a trip to the county fair. The segments which an experimenter arbitrarily selects for analysis are inextricably imbedded in this expanding matrix and are interpretable only in terms of its interactions. Transfer and retroaction experiments are explicit attempts to gauge these interactions, and the similarity variable—that is, the homogeneities existing among the materials successively practiced—turns out to be the most important factor as well as the most puzzling.

The classic statement of the relation between similarity and interference in human learning, as found in most textbooks in psychology, is that "the greater the similarity, the greater the interference." Although this law is traceable mainly to the work of McGeoch and his associates (10, 13, 14), there are many other experiments which superficially appear to substantiate it. When carried to its logical conclusions, however, this law leads to an impossible state of affairs. The highest degree of similarity of both stimulus and response in the materials successively practiced is that where any simple habit or S-R association is learned. The stimulus situation can never be precisely identical from trial to trial, nor can the response, but they are maximally similar—and here the greatest facilitation (ordinary learning) is obtained. *Ordinary learning, then, is at once the theoretical condition for maximal interference but obviously the practical condition for maximal facilitation.* Here is the fundamental paradox, and this paper suggests a resolution.

EMPIRICAL LAWS OF TRANSFER AND RETROACTION AS FUNCTIONS OF SIMILARITY

Transfer and retroaction in human learning are among the most extensively cultivated fields in experimental psychology, yet there are no clear-cut generalizations which satisfactorily bind the data together. The difficulty may be traced in part to the bewildering variety of procedures, materials and experimental designs employed by different investigators, a phenomenon perhaps characteristic of a young science. But some of the confusion can also be laid to the fact that in a large proportion of experiments the theoretically relevant relations are patently unspecifiable: the subjects merely learn List A and then List B, or Maze I and then Maze II, and either positive or negative effects may result, depending upon quite unanalyzable conditions. The purpose of this paper is to clarify the similarity function in human learning, and to accomplish this end only those experiments can be utilized wherein the *locus* of similarities is specifiable, as being between stimulus members, response members, or both. This analytic approach, although it may be considered inappropriate by some theorists and makes use of only part of the data, does give rise to a coherent and consistent picture.

When *transfer* is studied, one is interested in the effect of a specifiable prior activity upon the learning of a given test activity. When *retroaction* is studied, one is interested in the effect of a specifiable interpolated activity upon the retention of a previously learned activity. In both cases the experimenter arbitrarily "lifts" segments of a continuing process for analysis, and it would be expected that common laws would apply to both samplings. In the present context it can be shown that identical functions of similarity apply to both transfer and retroaction data, which simplifies the theoretical task considerably. Figure 1 gives symbolic representation to three basic learning paradigms, *A* that in which stimulus members are varied in the materials successively practiced while responses are functionally identical, *B* that in which responses are varied and stimuli are functionally identical, and C that in which both stimulus and response members are simultaneously varied. It will be seen that in so far as similarity relations are concerned, the test for transfer is simultaneously the interpolated activity when the entire retroactive sequence is followed. The term "functional identity" is used here to make explicit the fact that *true* identity among either stimulus or response processes is a will-o-the-wisp, approached but never attained. Functional identity of stimuli in successive trials or tasks exists when the situation is objectively constant (*i.e.*, when the same stimulus nonsense syllable appears on the screen

Transfer and Retroaction Paradigms

TRANSFER

Paradigm A	$S_1 \rightarrow R_1$	$\underline{S_2} \rightarrow R_1$	$S_1 \rightarrow R_1$
Paradigm B	$S_1 \rightarrow R_1$	$S_1 \rightarrow \underline{R_2}$	$S_1 \rightarrow R_1$
Paradigm C	$S_1 \rightarrow R_1$	$\underline{S_2} \rightarrow \underline{R_2}$	$S_1 \rightarrow R_1$

RETROACTION

Fig. 1. Paradigms indicating the locus of variation among the successively practiced materials. A, *stimulus variation;* B, *response variation;* C, *simultaneous stimulus and response variation.*

or the same choice point is approached on repeated trials in the maze); functionally identical responses are those which the experimenter, at any given level of analysis, scores as being the same (*i.e.*, no matter how the subject says CYF or how the rat maneuvers about a turn, it is scored 'correct'). Functional identity thus becomes the limiting case of maximum similarity.

1. Let us first consider *paradigm* A, the condition in

which stimulus similarity is variable and responses are functionally identical. The transfer portion of this paradigm will be recognized as nothing other than a symbolic statement of *stimulus generalization*. In Hovland's classic study (9), for example, a galvanic skin response is first conditioned to a tone of a certain frequency (S_1—R_1), then the test tone is presented and the extent to which the same response is made to it measured (S_2—R_1). Hovland found that the greater the similarity between practice and test stimuli, the greater the amount of generalization (or positive transfer). The same results are regularly found wherever this paradigm can be identified, whether the materials be motor or verbal, meaningful or nonsense, or of any other nature. McKinney (15) required subjects to respond with a correct letter upon seeing each of four geometrical designs and then measured transfer of the same responses to alterations of these designs; when Yum (25) varied the similarity of visually presented nonsense-syllable stimuli, positive transfer was the result, the magnitude increasing with stimulus similarity.

While retroaction data derived from this paradigm are not so extensive, the available evidence is consistent in revealing *facilitation*. Hamilton's (7) subjects learned lists of paired-associates in which the stimuli were geometrical forms and the responses were nonsense syllables. Although responses were 'identical' on original and interpolated lists, the stimulus forms varied from 'identity' through two degrees of similarity, as independently indexed in terms of generalization, to complete neutrality. The magnitude of retroactive facilitation decreased regularly as similarity among the stimulus members decreased, effects of approximately zero magnitude being obtained with neutral stimuli. The empirical law for this paradigm is: *where stimuli are varied and responses are functionally identical, positive transfer and retroactive facilitation are obtained, the magnitude of both increasing as the similarity among the stimulus members increases.*

2. The situation in which stimuli are constant and responses are varied, *paradigm B*, is the standard associative and reproductive inhibition paradigm and, as might be expected, a large number of experiments (*cf.* 1, 6, 21)

testify to the fact that *interference* is produced under these conditions. However, there is also a large body of evidence showing positive transfer under the same conditions. The latter evidence may be discounted on two grounds: (a) In many cases the so-called transfer response has been *learned previous* to the experimental situation. In many of Tolman's sign-learning studies, for example, animals trained to traverse the route to a goal by one path or means, such as running, will shift readily to another means, such as swimming, if the original behavior is blocked. Similarly, Wickens (23) has shown that a human subject who has learned to avoid the shock which follows a tone by an extensor movement of his finger, when his palm is down, 'transfers' immediately to a flexion movement when his hand is then placed palm up. In such cases, the new learning in the experimental situation is the sign-value or meaning of the distinctive cue. A variety of overt behaviors has previously been associated with this mediation process—the human subject brings to the experiment a rich repertoire of pain-avoiding movements, and he would lift his head without new training if his nose were inserted between the electrodes! (b) In other cases what is measured as positive transfer under conditions fitting this paradigm can be shown to be attributable to '*practice effects,*' *i.e.*, the subject is learning how to learn nonsense syllables or learning how to learn mazes, and these general skills or habits counteract the interference inherent in the design. Siipola (20), for example, obtained small amounts of positive transfer for a code-substitution task, yet concluded from the large numbers of intrusions that actual negative transfer was being masked by a general 'practice effect.'

Bugelski (2) required his subjects to learn an original list of 10 paired nonsense syllables (such as *toc-nem*) and then interpolated three additional lists, the experimental subjects having identical stimuli and varied responses (such as *toc-rul*) and the control subjects having both members varied (such as *cos-rul*). Although insignificant amounts of positive transfer to successive lists were obtained in both conditions, the inherent interfering character of the stimuli-identical paradigm was revealed in the fact that the experimental subjects showed a marked decrement upon relearning the first list while the controls showed continued facilitation. Clearest evidence for negative transfer and retroactive interference under the conditions of this paradigm is offered in a recent monograph by Underwood (21). In measuring transfer, subjects learned 0, 2, 4, or 6 lists of meaningful paired-associates *prior* to learning a test list; in measuring retroaction, 0, 2, 4, or 6 interpolated lists were learned *after* the original learning of the same test list; in both cases, recall of the test list was measured after a delay of 25 minutes. Both negative transfer and retroactive interference were found, increasing in magnitude with the number of prior or interpolated lists having the same stimulus members but different responses.

But what about the *degree* of similarity among the varied responses in this paradigm? Perhaps because of the difficulty in defining response similarity, there are relatively few data here. In a recent experiment by Osgood (17), original learning of a set of paired letter-pairs and meaningful adjectives (such as *c.m.—elated*) was followed by three types of interpolated items, each subject serving as his own control by learning an equal number of items in each similarity relation (such as *c.m.—high, c.m.—left*, or *c.m.—low*); all subjects finally relearned the original list. Although interference was obtained under all conditions, it was significantly *less* for similar meaningful relations. One of the conditions of Bruce's (1) extensive investigation with nonsense-syllable paired-associates substantiates this finding. We may now state the empirical law for this paradigm: *where stimuli are functionally identical and responses are varied, negative transfer and retroactive interference are obtained, the magnitude of both decreasing as similarity between the responses increases.*

3. *Paradigm C*, where both stimuli and responses are simultaneously varied, is directly generated when the standard memory drum is used and lists of material are learned in constant serial order. Similarities are between items having the same serial position on successive lists, and each item serves simultaneously as a response to the

preceding item and a stimulus for the succeeding item. Whatever interpolated lists are given, stimulus and response similarities must be simultaneously varied through the same degrees. McGeoch and McDonald (13) and Johnson (10) have employed this procedure with meaningful materials, finding retroactive interference to increase with the degree of similarity. Melton and Von Lackum (16) report the same result for nonsense syllables. McGeoch and McGeoch (14) and Johnson (10) find the same result to hold for transfer when this paradigm is used.

An important experiment by Gibson (6) also fits this paradigm. Her materials and procedures were identical with those reported above for Hamilton (7). The Gibson experiment was actually the first of the series. Visual stimulus forms were varied through independently measured degrees of generalization, as was the case in Hamilton's study, but here responses were different and neutral. Negative transfer and retroactive interference were obtained, their magnitudes decreasing as stimulus similarity decreased and approximating zero with neutral stimuli. It should be noted that in both studies approximately zero transfer or retroaction was found when stimuli were neutral, regardless of response identity or difference. The empirical law for this paradigm: *when both stimulus and response members are simultaneously varied, negative transfer and retroactive interference are obtained, the magnitude of both increasing as the stimulus similarity increases.*

There are a considerable number of substantiating studies which have not been cited here, but if this writer's survey of the literature has been adequate, *there are no exceptions to the above empirical laws.* There are few studies where more than one relation is systematically explored, with the same materials, procedures and subjects, and for this reason it is difficult to quantify these relations. An exception is a study by Bruce (1). One set of nonsense pairs (such as *req-kiv*) was learned by all subjects and transfer to several variations was measured: where stimuli were varied and responses were constant (*zaf-kiv* or *reb-kiv*) positive transfer was found as compared with a control condition, the amount being greater when stimuli were more similar: where responses were varied and stimuli were constant (*req-vor*), negative transfer was found. The condition in which stimuli were constant and responses were highly similar (*req-kib*) was slightly superior to the control condition (both members neutral). Although this result appears to contradict the empirical law for this paradigm, it will be found to fit the hypothesis presented in the latter part of this paper: if ordinary learning is to be theoretically feasible, high degrees of response similarity must yield facilitation.

ATTEMPTED INTEGRATIONS OF THE DATA

A series of attempts to integrate the facts of transfer and retroaction can be traced in the history of this problem. As early as 1919 Wylie (24) had made a distinction between stimulus and response activities, stating that the transfer effect is positive when an 'old' response is associated with a new stimulus but negative when an 'old' stimulus must be associated with a new response. 'Old' in this context merely means that the member in question has previously been associated with another stimulus or response. This principle is valid, of course, within the limits of its gross differentiation. But (a) it takes account of neither stimulus nor response similarities and (b) it leaves the fundamental paradox untouched. Since successive responses are never precisely identical, even in ordinary learning, we are always associating stimuli with 'new' responses and hence should inevitably get negative transfer.

Robinson was one of the first to perceive clearly this paradox and in 1927 he offered what is now known as the Skaggs-Robinson Hypothesis as a resolution. As shown in Fig. 2, this hypothesis states that facilitation is greatest when successively practiced materials are identical (point A); facilitation is least, and hence interference maximal, with some moderate degree of similarity (point *B*); and facilitation increases again as we move toward neutrality (point *C*) but never attains the original level. Note that while point A defines maximum similarity (identity) and point C defines minimum similarity (neutrality), point *B* actually specifies no degree of similarity at all, but merely says that somewhere there is a low point in the facilitation curve. Several experiments (3, 4, 8, 11, 19) combine to give rough validation to this poorly defined hypothesis, especially the *A-B* sector of it.

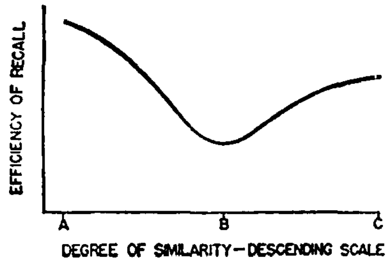

Fig. 2. The Skaggs-Robinson Hypothesis: point A *specifies maximum similarity (identity) and point* C *minimum similarity (neutrality) among the successively practiced materials; point* B *merely indicates the low point in the curve for efficiency of recall.*

The series of studies by McGeoch and his associates (10, 13, 14) ran into direct conflict with this hypothesis and the experimental evidence supporting it. Using meaningful words, they consistently found that as the judged similarity of the original and interpolated materials increased, interference also increased. The highest degree of similarity they could obtain, where close synonyms appeared on the two serial lists, yielded the most interference. There was no evidence here of facilitation as one approached identity. In *The Psychology of Human Learning* (12) McGeoch offered two alternative rapprochements between his data and the Skaggs-Robinson Hypothesis: (1) He distinguished 'similarity of meaning' and 'degrees of identity' as two different dimensions of similarity, each having a different interference function. This distinction was suggested by the fact that some of the experiments supporting the hypothesis (8, 11, 19) had employed numeral and letter combinations with similarity indexed by the number of identical elements. Unfortunately, in other substantiating studies, materials were used in which identical elements were no more readily specifiable than with meaningful words. Dreis (4), for example, used code-substitution, and Watson (22) used card-sorting. Furthermore, this type of resolution implies an analysis of meaningful similarity that would segregate it from identity of elements, and this has not been done. (2) At a later point, McGeoch tried to resolve the difficulty by stating that his results applied only to the portion of the Robinson Curve between *B* and *C*, *i.e.*, that the maximum similarity of his materials only reached point *B*. However, given the multidirectional shape of this theoretical function and the fact that point *B* defines no degree of similarity, not only could any obtained data be fitted to some portion of it, but it could always be argued that the similarity of one's materials fell *anywhere* between A and C. In other words, this second suggestion is incapable of either proof or disproof.

Perhaps the clearest experimental evidence against either of McGeoch's resolutions appears in the results of a recent experiment by the writer (17). *Also* using meaningful materials in the traditional retroaction paradigm, interference was found to *decrease* as the meaningful similarity among the response members increased. Not only would these results seem to fit 'degrees of identity' rather than 'similarity of meaning' as the functioning dimension, despite the nature of the materials used, but they fall within the *A* to *B* sector of the theoretical curve.

Quite apart from the apparent negative evidence in the McGeoch studies, the Skaggs-Robinson Hypothesis is inadequate on several grounds. It does, to be sure, allow ordinary learning to occur. But (a) it contains a dual function of facilitation in relation to similarity without specifying at what degree of similarity the shift occurs; (b) no specification is made of the locus of similarities within the materials practiced (whether among stimulus members, response members or both), and we have seen that both the direction and the degree of either transfer or retroaction are empirically predictable from such specification. One of the most recent attempts to integrate these data has been made by Gibson (5). She followed Wylie's lead in differentiating between stimulus variation and response variation, and she added to this picture the refinement of stimulus generalization, derived from Pavlovian conditioning principles. Gibson's two theoretical laws were: (1) if responses are *identical* facilitation is obtained, its amount increasing with the degree of stimulus generalization (similarity); (2) if responses are *different* interference is obtained, its amount increasing with the degree of stimulus generalization (similarity). These hypotheses fit much of the data in the field and further serve to integrate the phenomena of human learning with those of the animal laboratory. But they are insufficient. (a) No account is given of the *degree* of response similarity, and this appears as one of the relevant variables. (b) We have one function (increasing interference) when responses are different and another (decreasing interference) when responses are 'identical'; and one would anticipate, therefore, a strange, abrupt shift in function somewhere along the line as the degree of response difference is reduced. (c) The fundamental paradox remains: responses can never be truly identical but must always be different to some degree, yet ordinary learning can occur.

THE TRANSFER AND RETROACTION SURFACE

The formulation proposed here makes full use of Gibson's analysis, but, utilizing data which have recently become available, goes beyond it. It is quite literally constructed from the empirical laws presented above, and this can be demonstrated by use of Fig. 3, which provides a rational framework within which the data can be integrated. The vertical dimension represents the direction and degree of *either* transfer or retroaction; degrees of response similarity are distributed along the horizontal dimension. The parenthetical numbers refer to the sequence of steps to be followed in allocating the data.

Let us first consider the ordinary learning of an association, the case in which the same materials are used for original and interpolated activities. Here functionally identical stimuli and responses are successively repeated and maximal facilitation is obtained, allowing us to locate the first point as shown (number *1*). The phenomena of

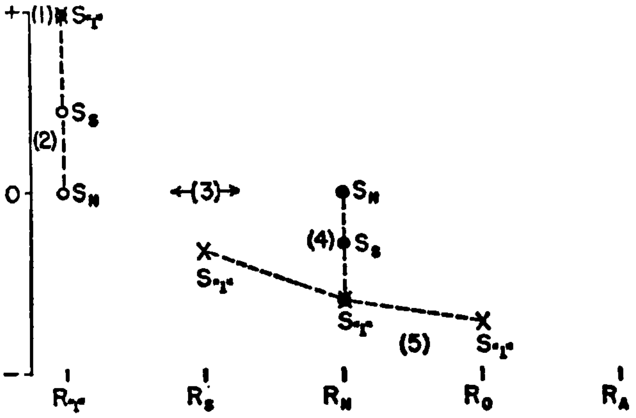

Fig. 3. Allocation of experimental data: vertical, direction and degree of either transfer or retroaction; horizontal, degrees of response similarity. Numbers in parentheses refer to steps in analysis followed in text.

positive transfer (stimulus generalization) and retroactive facilitation when responses are identical and stimuli varied are represented by the series of open circles (number 2): as the degree of stimulus similarity decreases from 'identity' less and less facilitation is obtained, effects of zero magnitude being found when stimuli are neutral. Data reported by Hovland (9) and Hamilton (7) are typical. As pointed out earlier, the fact that Hamilton and Gibson (6) used the same materials and procedures, with the single exception that responses were the same in the former case and different in the latter, provides an extremely useful comparison (see number 3); where stimulus members are neutral, effects of approximately zero magnitude are obtained in both experiments, allowing us to link the Gibson and Hamilton data together on the zero-effect base line. In other words, variations in the relation between response members are of no consequence when stimulus members are completely unrelated. The Gibson experiment itself, along with other substantiating studies, provides data for the condition in which responses are different and neutral while stimulus similarity is varied. Here negative transfer and retroactive interference are regularly obtained, increasing in magnitude as the similarity of the stimulus members increases, and these data are represented by the series of solid circles (see number 4). There remains to be included the condition in which stimuli are constant and response similarity is varied. The fact that 'identity' of stimulus and variation of response yields negative transfer and retroactive interference is amply testified to by a number of studies (1, 6, 21). Experiments in which the *degree* of response similarity is systematically varied, as those by Bruce (1) and Osgood (17), show that interference is *less* for similar responses than for neutral ones. Since the latter study included a condition in which responses were neutral and stimuli functionally identical, thus matching the final point of the Gibson data, it is possible to link the two sets of facts together. These data are represented by the connected series of X's (number 5).

The pattern of empirical points established here sharply limits the possible theoretical functions that can be generated. By visually tracing the series of X's, for example, including the point for ordinary learning, a fairly well-defined curve becomes apparent, this curve representing the function for stimulus 'identity.' A family of such stimulus-relation curves has been constructed to fit both these empirical points and the requirements of common sense, and they appear in Fig. 4. The function for *stimulus neutrality* is a straight line of zero effect, reflecting the reasonable fact that response variations are of no consequence when successive stimulus situations are completely unrelated. Given this as a zero-effect base line, increasing the similarity among stimuli yields a progressive maximization of *both* facilitation and interference, the actual direction of the effect being dependent upon response relations. The greatest facilitation and the greatest interference are possible only with functional *stimulus identity*. Intermediate transfer and retroaction effects fall between these limits depending upon degrees of stimulus similarity. The points for antagonistic responses, showing

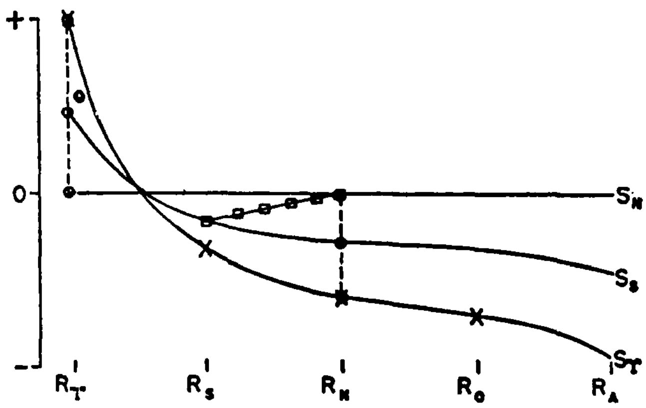

Fig. 4. Family of stimulus-relation curves constructed from data in Fig. 3; series of open squares represents data obtained by McGeoch and his associates (see text).

a final, sharp increase in interference, are admittedly hypothetical. However, the writer has recently reported (18) evidence for a special form of *reciprocal inhibition* associated with the successive learning of meaningfully opposed responses. The assumption is made here that this inhibitory effect is maximal when responses are directly antagonistic.

But how do the classic findings of McGeoch and his associates fit this hypothesis? In a real sense, they serve as a crucial test of it, being both well substantiated and in apparent conflict with other results. It will be remembered that these investigators employed a method wherein the similarity of *both* stimuli and responses varied simultaneously and through the same degrees, actually from neutrality of both to high similarity (but not identity) of both. As may be seen from the row of open squares in Fig. 4, the present hypothesis *must* predict gradually increasing amounts of interference under these conditions, and this is precisely the result obtained in these studies.

Although Fig. 4 provides a useful method of demonstrating the congruence of empirical data and theoretical functions, it does not offer a clear picture of the hypothesis as a whole. To do so requires a three dimensional form, representing stimulus similarity, response similarity and degree of effect as simultaneously interrelated variables. Figure 5 presents what may be termed *the transfer*

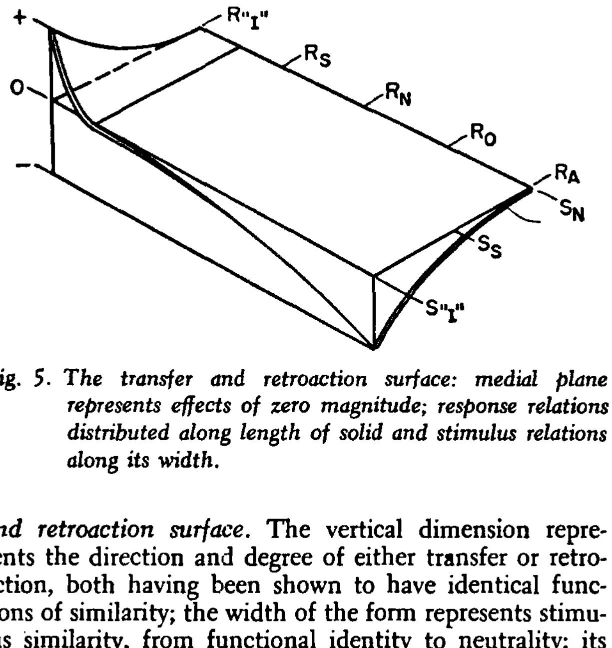

Fig. 5. The transfer and retroaction surface: medial plane represents effects of zero magnitude; response relations distributed along length of solid and stimulus relations along its width.

and retroaction surface. The vertical dimension represents the direction and degree of either transfer or retroaction, both having been shown to have identical functions of similarity; the width of the form represents stimulus similarity, from functional identity to neutrality; its length represents response similarity, varying from functional identity, through neutrality, to direct antagonism. The median horizontal plane indicates effects of zero magnitude, and it may be seen that the condition of stimulus neutrality is co-extensive with this plane regardless of response variations while the remainder of the surface intersects this plane at a point between response 'identity' and response similarity. Finally, it is apparent that we have here a smooth, unbroken sequence of transfer and retroaction functions, facilitative relations rising above the median plane and interfering relations falling below it. There are no reversals in these functions nor any abrupt shifts between identity and similarity. Identity becomes merely the limiting case of maximal similarity.

CERTAIN ADVANTAGES OF THIS HYPOTHESIS

By way of summary, certain advantages which this hypothesis offers in comparison with those which have preceded it may be indicated. (1) *All existing empirical data in the field are consistent with it and find representation upon the transfer and retroaction surface.* This statement is by necessity limited to those data wherein the locus of the similarities is specifiable and also by the adequacy of the writer's survey of the literature. The first limitation is not a serious one. If results can be shown to be lawful, and hence predictable, when such specification of the similarity relations is possible, the conflicting and confused results obtained under unspecifiable conditions are presumably attributable to unanalyzable variations in the paradigms employed. Witness the conclusive inconclusiveness on the question of formal discipline! This state of affairs illustrates why it is so difficult to make recommendations for efficient human learning in practical situations. What, for example, are the loci of similarities when the student simultaneously studies French and Spanish?

(2) *The phenomena of both transfer and retroaction are integrated within a single framework, in so far as the similarity variable is concerned.* It is common textbook procedure to study transfer under learning and retroaction under forgetting, as if these processes were somehow different in kind. The present analysis, it is felt, is a step in the direction of integrating the problems of human learning. Another step in the same direction is also suggested here: distinctions are often made in terms of mean-

ingful vs. nonsense materials, meaningful similarity vs. degrees of identity, and so on. It should be pointed out that data substantiating each of the three empirical laws derived above have been obtained with meaningful and nonsense materials, with materials varying in terms of meaningful similarity as well as degrees of identity. There is here, of course, the underlying problem of defining similarity. It may be defined operationally in terms of generalization (*cf.* Gibson, 5), although this definition is inherently circular since the phenomenon of generalization is nothing other than a case of positive transfer with functionally identical responses. Any precise behavioral definition of similarity will require much more knowledge of the nervous system than we have at present. In practice, degrees of similarity have been specified informally by experimenters or formally by a sample of judges, which probably suffices for our present rather gross purposes.

(3) *Although constructed directly from existing empirical evidence, this hypothesis does go considerably beyond it, predicting phenomena that have not as yet been observed.* For one thing that portion of the transfer and retroaction surface where increasing similarity of response (high degrees) is accompanied by increasing facilitation remains to be explored by standard procedures, the Robinson group of studies having used a memory span technique.[1] It will also be noticed that the theoretical surface requires that, regardless of the degree of stimulus similarity, all functions must become facilitative at precisely the same degree of response similarity, somewhere between identity and high similarity. In other words, just as the degree of response variation is inconsequential when stimulus members are neutral, so there must exist (according to this hypothesis) some definite degree of response similarity for which all variations among stimuli will yield zero effect. This is a novel but necessary predic-

[1] An as yet uncompleted investigation by Mark W. Harriman at Johns Hopkins University appears to be filling in this gap in our empirical knowledge. With functionally identical stimulus members, responses on original and interpolated lists are varied by extremely small degrees, such as having the singular and plural of the same word on two lists, and the predicted results seem to be forthcoming.

tion from theory that sets an intriguing experimental problem. It is not inconceivable that this common shift-over from facilitation to interference at a certain degree of variation among responses may reflect a basic characteristic of the nervous system,—but this is all assuming that the present hypothesis will be found valid in terms of constantly accruing facts.

(4) Finally, *this hypothesis resolves the fundamental paradox with which this paper began—the fact of ordinary learning becomes theoretically feasible.* The transfer and retroaction surface describes a system of curves within which the condition of ordinary learning, with functionally identical stimuli and responses in the materials successively practiced, is continuous with other relations. Identity is here merely the limiting case of maximal similarity, and no abrupt shifts of function are required to account for the fact that learning occurs.

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7

Stimulus Predifferentiation: Some Generalizations and Hypotheses[1,2]

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At one time it was an accepted dictum in the field of verbal learning that attaching a new response to an old stimulus, according to the A-B . . . A-K paradigm, would lead to negative transfer. About 15 years ago a number of experiments began to be reported in which the same paradigm was used to produce positive transfer. The main characteristic of these new studies was the fact that the two sets of responses were sufficiently different that there was essentially no generalization between them: neither incompatibility nor facilitation. It was hypothesized that the pretraining "predifferentiated" the stimuli so that they were more "distinctive," or less "confusing." In recent years a substantial number of experi-

[1] This report is based on work done under ARDC Project No. 7706, Task No. 27001, in support of the research and development program of the Air Force Personnel and Training Research Center, Lackland Air Force Base, Texas. Permission is granted for reproduction, translation, publication, use, and disposal in whole or in part by or for the United States Government.

[2] The author expresses his thanks to Dr. J. M. Vanderplas and Dr. Harold W. Hake for reviewing the manuscript.

ments have been devoted to this problem of stimulus predifferentiation; many potentially relevant variables have been investigated, some methodological improvements have been suggested and incorporated into later studies, and a number of hypotheses have been offered to account for the positive transfer obtained under these conditions.

Recently the writer surveyed a number of articles and dissertations on the topic of stimulus predifferentiation in an attempt to discover what generalizations could be made from frequently conflicting results, and what evidence could be found for or against the various explanatory hypotheses. The results of this effort are incorporated in the first two sections of this paper. In the third section some suggestions are made concerning additional variables, the consideration of which might contribute to clearing up some of the ambiguities which presently exist. It should be emphasized that an attempt was made to limit the survey to those studies which conformed strictly to the stimulus predifferentiation paradigm, and it is believed that the survey is fairly complete within that area. Likewise, in the interests of brevity, the writer has sternly resisted the temptation to discuss the various issues in the larger context of transfer of training, although in many cases such an extension would be relevant. For these reasons it is hoped that the reader will use the conclusions and recommendations of this paper as a guide to the literature rather than as a substitute for it.

GENERALIZATIONS

The survey indicated that there is enough agreement in results among the various experiments to provide generalizable conclusions in two broad areas. One of these has to do with the kind of verbal pretraining given, and the other concerns the amount of such training.

The categories of pretraining and the results achieved with each kind are summarized below, with examples of each category given in Table 1. The terminology is, in part, that suggested by McAllister (28).

TABLE 1

Examples of possible S-R pairs used during different kinds of predifferentiation training when the transfer task involves moving a control upward in response to a red light and downward in response to a green light

	Pretraining		Transfer Task	
Kind of Pretraining	Stimulus	Verbal Response	Stimulus	Motor Response
Relevant S-R	Red light	"Up"	Red light	Up
	Green light	"Down"	Green light	Down
Relevant S	Red light	"Cow"	Same as above	
	Green light	"Horse"		
Irrelevant S	Bright light	"Cow"	Same as above	
	Dim light	"Horse"		
Attention	Red light	None	Same as above	
	Green light	None		
No pretraining	None		Same as above	

Categories of Pretraining

Relevant S-R. In this type of pretraining the stimuli used for the pretraining task are identical to those used in the transfer task, and the responses used in the pretraining task are somehow symbolic of, or bear a sign-significate relation to, the responses used in the transfer task. Strictly speaking, this type of pretraining is used in the classical "transfer of training" studies and should not be considered an example of stimulus predifferentiation. Stimulus predifferentiation is usually characterized by the fact that the responses used in pretraining and in the transfer task are completely independent of one another, whereas in Relevant S-R training the kind of transfer obtained depends greatly upon the relationship which exists between the two sets of responses. This type of pretraining has been used, however, in studies which derived their rationale from the predifferentiation hypothesis (7, 10, 12, 28). In those cases in which it has been used, Relevant S-R training has proved to be equal or superior to any other kind of verbal pretraining.

Relevant S. In this type of pretraining the stimuli used are identical to the ones used in the transfer task, but the responses are completely different from those used in the transfer task (1, 3, 8, 10, 11, 12, 13, 14, 15, 16, 17, 21, 25, 28, 34, 36). This is the kind of pretraining specified, for example, in the predifferentiation hypotheses of Gibson (18) and of Miller and Dollard (29). In most of the studies in which Relevant-S pretraining was compared with some other type of pretraining, the experimental group performed better on the transfer task than a group given no pretraining or a group given any other kind of pretraining, except directed attention (see below). Battig, however, has shown that the effectiveness of Relevant-S training decreases as task complexity increases (10).

Irrelevant S. This type of pretraining is most often given in order to obtain a control group having the same "performance set" as the experimental group. The stimuli used in the pretraining task are different from those used in the transfer task but are equated with them in difficulty. In none of the experiments surveyed was the performance of the Irrelevant-S group on the transfer task superior to the performances of groups given training in attention or given no pretraining at all (1, 8, 13, 14, 15, 17, 28).

Attention. In this type of training, S is not required to make any sort of overt differential responses to the stimuli during the pretraining period, in the sense of learning "labels" for them. He is required, however, by instructions or some other means to attend to the distinctive characteristics of the stimuli. This type of training was consistently superior to Irrelevant-S training. It was as effective as Relevant-S training in 50 per cent of cases in which they could be directly compared (3, 11, 12, 13, 21, 25, 34, 36).

No pretraining. This group starts on the transfer task without any previous experience in the experimental situation. In general, this type of control group is unsatisfactory in that there is no control for the factors of performance set or attention.

A summary of the results obtained in experiments in which various kinds of predifferentiation training were given indicates that the following generalizations may be made: (*a*) *Relevant S-R* training (if it can be accepted as falling into the category of stimulus predifferentiation) is the most effective form of verbal pretraining; (*b*) *Relevant-S* pretraining is, in most cases, more effective than any pretraining method except *Relevant S-R* training; (*c*) *Irrelevant-S*, or performance set, pretraining is usually poorer than other pretraining methods; and (*d*) *Directed Attention* pretraining is often as effective as *Relevant-S* pretraining.

Amount of Predifferentiation Training

Of the studies included in the survey, the following were concerned with varying the amount of predifferentiation training: Arnoult (3), Baker and Wylie (7), Baldwin (8), J. H. Cantor (14), Gagné and Baker (16), Goss (21), and Rossman and Goss (35). The results of these experiments are summarized in Table 2. In order to facilitate comparisons among the experiments, the number of pretraining trials reported has been expressed as the number of experiences S had with each stimulus.

These results may be summarized, perhaps, by the following generalization: *Positive transfer from stimulus predifferentiation training may be expected after a minimum of 4 to 8 pretraining trials and reaches a maximum after 8 to 12 pretraining trials.* Support for this generalization may be found in the recent experiment by Arnoult (3). Using a transfer task (shape recognition) which was quite different from those used in the other experiments cited above, he measured two levels of achievement on the transfer task as a function of the number of pretraining trials. The curves obtained were monotonic, negatively accelerated functions which rose rapidly and tended to level off in the vicinity of 8 to 10 pretraining trials.

TABLE 2

Summary of experiments in which amount of pretraining was varied

Experiment	Comparison	Greater Number of Trials Produced More Positive Transfer
Baker and Wylie (7)	2 trials vs. 0 trials	No
	8 trials vs. 0 trials	Yes
Gagné and Baker (16)	2 trials vs. 0 trials	No
	4 trials vs. 0 trials	Sometimes
	8 trials vs. 0 trials	Yes
Rossman and Goss (35)	4 trials vs. 1 trial	No
Arnoult (3)	(5-15) trials vs. (1-4 trials)	Yes
Baldwin (8)	24 trials vs. 6 trials	No
Goss (21)	20 (avg.) trials vs. 12 (avg.) trials	No
Cantor (14)	24 trials vs. 12 trials	No
	72 trials vs. 12 trials	No

HYPOTHESES

The next step in surveying the data on stimulus predifferentiation was to examine the implications of the results summarized above with respect to the various hypotheses which have been offered to account for the positive transfer obtained from predifferentiation training.

These hypotheses can be grouped more or less adequately into five categories which have been labeled as follows: (*a*) acquired distinctiveness of cues; (*b*) reduction in intralist generalization; (*c*) increased meaningfulness; (*d*) attention to cues; and (*e*) performance set. Each of these hypotheses was examined in turn in an attempt to weigh the evidence for and against it. It should be remembered that any experiment in which positive transfer was obtained can be considered as supporting whatever hypothesis the experimenter used as a starting point. Consequently, only those studies were considered in which two or more hypotheses were directly compared, or in which a test was made of a deduction from one of the hypotheses.

Acquired distinctiveness of cues. This hypothesis was formulated by Miller and Dollard (29) and states that

> . . . learning to respond with highly distinctive names to similar stimulus situations should tend to lessen the generalization of other responses from one of these situations to another since the stimuli produced by responding with the distinctive name will tend to increase the difference in the stimulus patterns of the two situations (30, p. 174).

The increase in differentiation which results from this process is called the acquired distinctiveness of cues. Miller and Dollard further hypothesize that removal of the verbal responses by repression ". . . will remove the basis for acquired distinctiveness and increase the amount of primary generalization" (30, p. 174).

The first part of the Miller-Dollard hypothesis implies that the amount of positive transfer resulting from verbal predifferentiation training will be a function of the degree to which the stimulus items are clearly differentiated during that training. One way in which the degree of differentiation may be varied is by varying the specificity of the verbal labels attached to the stimuli. Hake and Eriksen (22) required their Ss to learn 2, 4, or 8 labels for 16 different stimuli. Following this training they were required to relearn to discriminate among the stimuli using 2, 4, or 8 new labels. All possible combinations of these conditions were investigated. The results showed that the specificity of the labels affected the speed of learning in both the pretraining and the transfer task, but that the speed of learning in the transfer task was independent of the number of labels which had been used during pretraining. Likewise, Robinson (34) found that the specificity of the labels used during paired-associate pretraining did not affect performance on a transfer task which required S to make same-different discriminations among the stimuli; and, in a second experiment, Hake and Eriksen found that label specificity did not affect subsequent recognition of forms (23). Thus it would appear that, while the Miller-Dollard hypothesis of acquired distinctiveness of cues seems eminently reasonable, the only available tests of a deduction from the hypothesis fail completely to support it.

Only one of the experiments covered by this survey included an attempt to test that part of the Miller-Dollard hypothesis dealing with the effects of repression. Rossman and Goss (35) had Ss learn to associate nonsense shapes with nonsense syllables to a criterion of mastery. One group was then given a single postcriterion trial on which electric shock was administered with every response. It was assumed that this noxious accompaniment to the response would lead to repression of the newly acquired associations. When these Ss were compared on the transfer task with Ss who had received a normal postcriterion trial, no difference in performance was detected. The extent to which these results can be considered as harmful to the Miller-Dollard repression hypothesis depends, of course, on the extent to which one is willing to assume that the noxious stimulus was adequate for the purpose.

Reduction in intralist generalization. In 1940, E. J. Gibson formulated the following hypothesis (18, p. 222): "If differentiation has been set up within a list, less generalization will occur in learning a new list which includes the same stimulus items paired with different responses; and the trials required to learn the new list will tend to be reduced by a reduction of the internal generalization." The amount of the predicted transfer would be maximally positive in the case where there was a minimum of interlist response generalization. This hypothesis is very similar to the Miller-Dollard hypothesis and would lead to the same predictions with regard to the transfer of predifferentiation training. Gibson further hypothesized, however, that "Generalization will increase to a maximum or peak during the early stages of practice with a list, after which it will decrease as practice is continued" (18, p. 206). A subsequent experiment produced results confirming this prediction (19). It can be argued, then, that if paired-associate pretraining produces an initial increase in the tendency to confuse the stimulus items, followed by a decrease, then the transfer of such training should be negative after just a few trials, then positive as the number of pretraining trials increases. Gagné and Baker (16) found no difference in the amount of transfer after two or four pretraining trials, and Rossman and Goss (35) found one and four trials to be equivalent with respect to the amount of transfer obtained (see Table 2). It should be pointed out that in the Gagné and Baker experiment neither of these experimental groups was consistently superior to a group receiving no pretraining at all, whereas in the Rossman and Goss experiment a zero-practice control group was not included. As was the case with the Miller-Dollard hypothesis discussed earlier, it would appear that, while the primary predifferentiation hypothesis has been supported by many experiments, attempts to test a specific deduction from the main hypothesis have yielded negative results.

Increased meaningfulness. Some writers have considered the possibility that the positive transfer from predifferentiation training is primarily due to an increase in meaningfulness of the stimuli as a result of having new responses associated with them. Arnoult has suggested, for example, the following:

> If meaningfulness is measured in terms of the number of independent associations linked with a stimulus item, it would seem reasonable that adding a new association to a particular stimulus (by predifferentiation training) should make subsequent learning of that stimulus item easier (1, p. 402).

This idea is consistent with Noble's suggestion that:

> . . . the procedure of endowing stimuli with the properties of meaningfulness (m) or familiarity (f) may constitute one unambiguous definition of Thorndikean "identifiability," which in turn may be related to such current notions as "predifferentiated structure," "distinctiveness," "cue-value," and "recognizability" (32, pp. 96-97).

Essentially the same idea has also been expressed by Dysinger (15). Some support for this hypothesis can be found in the experiments surveyed. Arnoult (3) found that in at least a limited way the amount of positive transfer to a recognition task was a function of the meaningfulness of the response terms used during paired-associate pretraining. McAllister (28), likewise, found more positive transfer resulting from the use of some sorts of response terms than others, and the difference may have been due to differences in the meaningfulness of the response terms. On the other hand, Campbell and Freeman (12) found no relation between the meaningfulness of the responses used during pretraining and performance on a subsequent recognition test. A closer examination of the Arnoult and McAllister experiments suggests, furthermore, that the increase in meaningfulness occurring in both these experiments was not a function solely of the "number of associations" possessed by the response terms themselves, but rather was due to the introduction of a factor which might be called "belongingness" (37), i.e., the introduction of a pre-experimentally learned relationship between a *pair* of terms.[3] In the first case (Arnoult) this type of relationship existed between the stimuli and the responses used during pretraining, and in the second case (McAllister) it existed between the two sets of responses. While it is not unreasonable that increasing the meaningfulness of the response terms used during pretraining might enhance the effect of verbal pretraining, it appears doubtful that this factor alone will account for all of the positive transfer obtained from predifferentiation training.

Attention to cues. The results obtained by Hake and Eriksen in the discrimination learning study in which response-specificity was varied led them to conclude:

[3] For example, consider the four responses *kitchen*, *furry*, *Persian*, and *feline*. In terms of conventional measures of "meaningfulness" (31), these terms (considered by themselves) are probably arranged in descending order, i.e., they would elicit successively fewer associations. They are in ascending order, however, in terms of their "belongingness" to the stimulus word *cat*.

> The results appear to us to emphasize the importance of the general *labeling* process rather than factors related to the particular labels used. We may judge from our results and from others that the perceptual gain resulting from labeling practice appears to occur as long as Ss have a decision to make about the stimuli on each trial. The labeling task given to our Ss seems merely to have provided a context which defined an objective for them (22, pp. 166-167).

Similarly, Robinson suggested that the critical features of the pretraining were (*a*) attention to the stimuli and (*b*) active search for identifying features. He concluded that his results demonstrated that ". . . the learning of the arbitrary names for the . . . [stimuli] . . . did not produce any further change in stimulus discriminability" (34, p. 114). Those experiments which have included specific training methods based on directed attention to critical cues have, however, yielded ambiguous results. Campbell and Freeman (12), Robinson (34), and Smith and Goss (36) found this type of training to be as effective as standard verbal paired-associate training. G. N. Cantor (13) and Goss (21), on the other hand, found it to be less effective. Arnoult (3) and Kurtz and Hovland (25) obtained ambiguous results. In none of these experiments, however, was an attempt made to discover the extent to which Ss may have provided labels of their own invention during the pretraining procedure. It will probably not be possible to evaluate this type of training method adequately until measures of this factor are included as part of the data in experiments using directed-attention training.

Performance set. Recently experimenters in this area have been concerned that the positive transfer obtained in predifferentiation experiments might, at least in part, be due to the transfer of such general factors as "warm-up" or "learning-how-to-learn." The term "performance set" is used here to designate all such factors. In general, concern over factors of this sort has been evidenced by the inclusion of special control groups which provide the possibility of measuring their effect. Most often the training of these groups is of the Irrelevant-S sort, although occasionally simple familiarization training has been used. The results appear to be unequivocal. In every case in which this type of training has been compared with verbal paired-associates training of the Relevant-S type it has yielded significantly less transfer. J. H. Cantor (14) and Smith and Goss (36) found it to be equivalent to no pretraining at all. It seems clear that transfer of predifferentiation cannot be accounted for in terms of general factors of this sort.

Summarizing the evidence for and against the various hypotheses which have been offered to account for the transfer of predifferentiation training leads to the following conclusions: (*a*) The *acquired distinctiveness of cues* and *reduction of intralist generalization* hypotheses imply, in general, the same kinds of operations and lead to the same kinds of predictions. Specific tests of deductions from these hypotheses have failed, however, to receive any experimental support. (*b*) In its simplest form the *increased meaningfulness* hypothesis is also operationally equivalent to the first two in that the increased meaningfulness of the stimulus is presumed to result from the acquisition of a label (association). It is a corollary of this hypothesis that an increase in positive transfer should result from increasing the meaningfulness of the label itself. This corollary has not received unequivocal experimental support. (*c*) All three of the foregoing hypotheses may logically be compared with the *attention to cues* hypothesis, which states that the learning of a verbal label is not an essential part of the predifferentiation process. This sort of comparison may properly be made only if some control is effected over the tendency for Ss to provide labels of their own choosing during the pretraining session. (*d*) On the basis of the experimental evidence available the transfer of predifferentiation training cannot be accounted for on the basis of transfer of general factors such as "warm-up" or "learning-how-to-learn."

OTHER CONSIDERATIONS

The primary conclusion to be derived from this survey of experiments on the transfer of predifferentiation training is that the hypotheses which have so far been offered to account for the transfer phenomenon are all unsatisfactory. They all appear to be stated in testable terms; yet, with one exception, there seems to be no experimental basis for choosing among them. When a reproducible effect, such as the production of positive transfer through predifferentiation training, can be accounted for on the basis of a variety of equally plausible hypotheses, it is likely that all of the hypotheses are dealing with superficial aspects of the situation. It becomes neçessary, then, to re-examine the whole problem to determine wherein we have failed to discern the crucial factors, to manipulate the most relevant variables, and to organize our thinking along the most appropriate conceptual dimensions. We must determine whether the superficiality of our thinking is due to a failure of observation or of definition.

With these objectives in mind, let us examine once again the various hypthoses now current. Three of the four remaining hypotheses may be discussed together: *acquired distinctiveness of cues, reduction in intralist generalization*, and *attention to cues*. The first question we may ask of these hypotheses is: What is a cue? It is strongly implied that the word *cue* refers to a stimulus characteristic which may be independently varied. Examination of the experiments generated by these hypotheses, however, reveals that the existence of cues is characteristically inferred from the fact that learning produces more reliable discrimination responses. There is no objection to making such an inference, but it can be argued that there are more efficient ways of investigating the importance of cues in learning and transfer. For example, Kurtz (26) recently showed that either positive or negative transfer could be obtained, depending upon the presence or absence in the transfer task stimuli of the particular cues which had provided the basis for discrimination during the pretraining. Kurtz used two-dimensional forms as his stimuli, and it is not too difficult to manipulate cues objectively in stimuli of this sort. What, though, is a cue when the stimulus is verbal? Is the cue a letter, a pattern of letters, a sound, or perhaps the connotations of the verbal symbol? Any or all of these may be cues, and some experimenters have used these definitions explicitly while others have failed to specify which definition was being used. Needless to say, we cannot determine whether any "cue" has acquired distinctiveness unless we know precisely what is meant by a cue.

An examination of the concept of "distinctiveness" (a term which Gibson [19] has also used in connection with reduction in intralist generalization) leads to many of the same questions. Acquired distinctiveness can be inferred from positive transfer or from a reduction in intralist errors, but it is potentially a more powerful explanatory concept when measured independently of the phenomenon it is designed to explain. The writer has previously pointed out (1, 3) that distinctiveness is a stimulus attribute which can be measured by psychophysical methods, and that an increase in distinctiveness should be accompanied by a change in the threshold for discrimination. It has been shown that an increase in distinctiveness (in this sense) follows upon verbal paired-associates training when the perceptual test is one of delayed recognition (3) but not when the test is one of same–different discrimination (1). These results imply that the acquired distinctiveness affects not the perception of the stimulus but the memory of it, which is consistent with the results obtained by Lawrence and Coles (27).

As before, it is easier to discuss these concepts in connection with form stimuli than in connection with verbal stimuli. It is hard to imagine the ways in which a verbal stimulus becomes more distinctive until it is decided what the cues for recognition are. Likewise, the usefulness of an explanation based upon *attention* to cues will be slight until it is possible to specify more adequately what attention is and to make some guesses about how it operates to facilitate recognition or memory.

The hypothesis that transfer of predifferentiation training results from increasing the *meaningfulness* of the stimuli derives essentially from the core-context theory of meaning and has not had the sort of formal theoretical development that exists for the more behavioristic hypotheses of Miller and Dollard (29) and Gibson (18). No mechanism for accomplishing the transfer has been postulated beyond the simple assumption that more meaningful stimuli are more easily learned and more easily remembered. Even within this simple conceptualization, however, there remain many unanswered questions. What, precisely, is meant by the *meaningfulness* of a stimulus? Attempts have been made to quantify the meaningfulness of verbal materials (Glaze, [20], Noble [31]), and attempts are currently in progress to develop meaningfulness scales for nonsense forms, but it remains questionable whether these measures so far developed will be adequate to account for predifferentiation transfer. What are the differential effects on learning of stimulus meaning and response meaning, when meaning is defined as the number, intensity, or latency of associations? And, parenthetically, are these three definitions of meaning equivalent? What would be the effect on transfer of requiring S to learn several responses to each stimulus, each to a partial criterion; would this be as effective as learning one response thoroughly? What is the relation between *meaningfulness*, defined by the associations elicited by a single term, such as a stimulus or a response, and *belongingness*, defined as an existing connotative or denotative relationship between a pair of terms either stimuli, responses, or both? Are these equivalent with respect to their effect on transfer? Most of these questions are susceptible to experimental test on the basis of concepts presently available, but it is the writer's belief that no real understanding of them will be achieved until the whole problem of meaning has been more satisfactorily resolved.

The foregoing discussion does not nearly exhaust the questions which could be asked concerning the hypotheses which have so far been proposed, but they should indicate that all these hypotheses have described the stimulus-response situation in terms which are superficially plausible but which are not easily quantified or, in some cases, even operationally specifiable. To state the case in the most obvious terms, stimuli are not usually simple events which can be described as "lights," "forms," or "words," nor can responses be described in terms equally simple. Stimuli and responses are not independent entities which can be adequately described solely in terms of themselves, but rather they are always members of a class whose size and class-characteristics are a function of the total experience of the individual subject. Naturally, psychology cannot hope to deal with the total apperceptive mass of each subject in relation to each stimulus and each response, and usually it is not necessary to do so. It is possible, however, to deal with smaller classes which are highly relevant. For example, in an experiment on paired-associates learning the stimuli and responses to be learned constitute important classes, and the individual items derive important attributes from the fact that they are members of these classes.

A recent experiment by Attneave (6) provides an excellent demonstration of this principle. He was interested in investigating the "schema" hypothesis, which has been proposed by Bartlett (9), Oldfield (33), Woodworth (38), Hebb (24), and others. Attneave required a group of Ss to draw repeatedly from memory a prototype nonsense form; he then required them to learn differential responses to a set of nonsense forms which were random variations on the prototype. The group which had practiced drawing the prototype, which was a "mean" of the variations, was significantly better at the paired-associates learning task than was a group which had practiced on an irrelevant form. The results were interpreted as showing that the memory of the prototype had served as a "schema" about which the variations might be organized and learned.

The results obtained in this experiment are similar to those obtained in usual predifferentiation experiments, but the kind of pretraining used was wholly different. These results cannot be accounted for by any of the hypotheses so far discussed because no formal *pre*differentiation of the stimuli was involved. They pose a problem not only for predifferentiation hypotheses but also for all current conceptualizations about transfer of training.

Attneave suggests that schema learning is always involved in predifferentiation training. In the course of the pretraining the subject learns at least three things about the class of stimuli within which differentiations are being made: (*a*) the central tendency of the class; (*b*) how its members may differ from one another; and (*c*) the dispersion of the class—i.e., how much its members may differ from one another on the several dimensions of variability. While many other things about the stimuli are undoubtedly also learned, these three class parameters together form the "schema" to which the individual stimuli are related.

Looking at the problem of predifferentiation training (and the whole problem of transfer of training) from this point of view leads to an experimental program somewhat different from that which has existed up to now. The primary requirement for such a view is that a thorough knowledge of the stimulus be available. The discriminable attributes of the stimulus must be quantitatively related both to its physical structure and to various experiential factors. Research of this sort is already in progress on both verbal (31) and nonverbal (2, 4) materials. When it is possible to describe stimuli in these terms it will be possible to manipulate the conditions of an experiment in such a way that the specific factors responsible for transfer can be identified.

It is not necessarily true that the kinds of hypotheses generated by experiments of this sort will be very different from the kinds currently available. The difference will be that the sorts of hypothetical constructs and intervening variables which will be formulated will be based on detailed knowledge of the functional relationships between the discriminable stimulus attributes, on the one hand, and structural and experiential factors on the other. The fact that it is somewhat easier to obtain such functional relationships in the case of nonverbal than of verbal stimuli (4) suggests that nonsense forms may come to be preferred over nonsense syllables as the ideal stimuli for transfer studies. In any case, it is the thesis of this discussion that more satisfactory hypotheses to account for transfer effects will be developed only when it becomes possible to give a more adequate quantitative description of the stimulus.

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Transfer of Learning

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CHARACTERISTICS OF TRANSFER

A number of phenomena which suggest some basic processes in transfer have been identified. *Stimulus generalization*, first studied in the development of the classical conditioned salivary responses, was observed when, in learning to respond to one tone by salivating, the dog would come to respond the same way to a new but similar tone. This phenomenon is frequently observed in the school when a child who learns to think of "7" upon seeing "4 plus 3" later thinks of "7" when he encounters "3 plus 4" (a useful form of transfer), or when he encounters "4 minus 3," a case of negative transfer.

Response generalization is the converse of stimulus generalization. Suppose that a student learns to respond to a signal by pushing a lever and acquires considerable skill. Later he is required to pull the lever instead of pushing it. Very often it appears that the early experience, instead of hindering, as one might expect, actually helps. Apparently, the student does not learn to make a certain movement. On the contrary, he learns to achieve a certain result. If he learns to achieve a certain result by one movement he may also have increased his ability to produce that same result by a very different movement.

Marked positive transfer is usually found when there is a mere shift from one sense organ to another. Using the left eye to judge the areas of geometric shapes will result in an improvement in that task when the right eye is used. Practice in reading Braille with one finger will help in reading it with another finger. Such tactile discrimination has interesting complications, however. Practice in the two-point threshold on one part of the skin will transfer to adjacent areas of skin or to symmetrical areas, but will not transfer to the same extent to other areas (14).

When we shift, not merely from one sense organ to another, but from one task to another, the transfer of discrimination or perceptual judgment may be great or small. In the classical experiment of Thorndike and Woodworth, various amounts of transfer were obtained, transfer being extensive for similar areas and similar shapes, somewhat less for different areas, and still less for different shapes. In judgments of distance we may find almost zero transfer when practice on one method of judging distances is applied to another method (14); however, we may find considerable transfer when practice in judging one distance is applied to other distances (15). In the somewhat more complex discrimination problem of selecting and crossing out all the *e*'s on a page of print, we find both positive and negative transfer. The experience may facilitate the crossing out of some letters, but may actually interfere with later success in crossing out other letters (28, p. 310).

In the realm of motor learning, *bilateral transfer* or *cross education* has frequently been observed. In learning to juggle balls, or in tracing a complete pattern with one hand, the skill that is acquired is often transferred to the other hand. This is especially likely to happen if the pattern followed by one hand is clockwise and the other hand is tested on the same pattern but in counterclockwise direction (35, p. 738-43).

Marked positive transfer occurs whenever the learner works on a series of complex but similar problems. Monkeys faced with the problem of discriminating between two stimuli rapidly acquire a learning set. After solving a few discrimination problems, they approach the next problem in a more systematic and businesslike manner and with a definite improvement in performance. There may be some danger of expecting too much from this very primitive form of transfer, but there seems no question that a certain amount of gain will occur (18, 30). The same phenomenon appears when human subjects learn nonsense syllables. At the outset, a difficult list of nonsense syllables may require 35 or 40 trials. After learning many such lists, however, a list of equal difficulty may be mastered in 15 to 20 trials (28, p. 306).

In mastering a complete task, there is often something to be gained from preliminary practice in distinguishing between the stimuli involved in the complete task. This is called *predifferentiation.* A student, for instance, faces the task of pushing a switch forward when a green light flashes or pulling it toward him when a red light flashes. Clearly he will gain some advantage in watching the lights and merely saying "ahead" when he sees green and "back" when he sees red. This is not surprising. He will also gain some advantage, moreover, if, before his actual training, he has had experience in making some irrelevant response to the green light (saying "cover") and making a different response to a red light. There will be some, perhaps just as much, advantage if he is required in some way to attend to the two stimuli even if he is not directed to respond to them in any definite way. The advantage to be had from such predifferentiation may appear after 4 to 8 experiences with the stimuli and may reach its maximum in 8 to 12 such experiences (1).

The results of transfer are likely to appear at any time after the experience in the training task. The amount of transfer seems to be affected little, if at all, by the interval between practice in the training task and the test on the criterion task (28, p. 339). In general, moreover, such transfer effects are retained remarkably well. Broad transfer effects such as confidence or general approach may be retained long after the details of the practiced task are forgotten (35, p. 750).

CONDITIONS AFFECTING THE AMOUNT OF TRANSFER

As has been suggested, transfer is clearly affected by the similarity in the tasks being performed. We can expect more transfer when the training task and the criterion task resemble each other in their over-all characteristics (9). Along with this loose, general similarity between the two tasks, there has been considerable interest in the more molecular aspects of similarity. Especially in the related field of retroactive facilitation and inhibition, much work has been done on the similarity between the stimuli of the two tasks, as one problem, and the similarity of the responses in the two tasks, as a distinct problem.

Suppose a student has come to respond to the stimulus (S_1) "Largest city in France" by (R_1) thinking "Paris," and is now asked to acquire a new S-R connection, (S_2) "Capital of France"—(R_1) "Paris." The experience with the first connection will facilitate the acquisition of S_2-R_1. Here we note that the two responses are identical and that S_1 and S_2 are somewhat similar. The two S's could, in fact, be easily confused. Suppose, on the other hand, that a student has just learned "Largest city in India"—"Bombay" and now has to learn "Capital of India"—"New Delhi." As in the previous example, the two stimuli are somewhat similar but here the two responses are quite different. In this case the first experience may readily interfere with the learning of the new connection. These general facts were established by early investigations of Poffenberger and of Bruce and have been supported by the classical work of Gibson in the role of sensory generalization. More recently both Schlosberg and Osgood (35, p. 754) have expressed the trends in systematic schemata, the *extremes* of which can be indicated by the following rules: When the two responses are identical (Paris, Paris) then the greater the similarity in the stimuli, the greater the amount of positive transfer. When the two responses are quite different (Bombay, New Delhi), then the greater the similarity in the stimuli, the greater likelihood of negative transfer. When the stimuli in the two tasks are quite different (area of New Guinea, population of Omaha), zero transfer should be expected and this should hold whether the two responses are similar or quite different. In actual fact, however, Bruce (28, p. 319) did find some positive transfer in such a case if, by some chance, the response to these two stimuli should be identical (say, 317,000). Finally, according to the schema, when the two stimuli are similar (Capital, Largest City), we should expect positive transfer when the two responses are identical, or highly similar, with transfer becoming zero, and finally negative as the two responses become more and more different.

The schema predicts maximum interference when the two stimuli are identical and when the two responses are completely different. There is evidence, however, that the greatest interference does, in fact, not occur when the responses are markedly and conspicuously different. Sorting soiled cards into bin X and clean cards into bin Y, for instance, will produce only slight interference when the task is completely reversed and the soiled cards are later sorted into bin Y (21). When dealing with a given stimulus, the most devastating thing may not be a shift to a very different response, but a shift to a response that is only slightly different (6). It is when the two responses have so much similarity that they can be confused that we get our greatest negative transfer. After this, as the two responses become increasingly different, and consequently more likely to be differentiated, the amount of negative transfer steadily decreases.

If negative transfer is produced in part by failure to differentiate between similar responses, it is not surprising that with increased practice on the training task we will find less and less negative transfer in these unfavorable circumstances (27). Lacking this differentiation such results seem paradoxical, since we would expect that if a little practice produces some negative transfer, a great deal of practice would produce more. The seeming paradox can be explained, of course, by the fact that, with additional practice, the various responses in the training task are becoming more and more clearly differentiated or isolated from each other (36).

Apart from the exception just discussed, we find in general that the amount of transfer is increased with the amount of practice on the training task (9). Animals are more likely to learn how to learn when their first tasks are definitely learned to completion. The more completely we memorize one list of nonsense syllables, the more likely we are to use that mastery in learning a similar list of nonsense syllables (28, p. 335-337).

It is clear that sometimes there is more transfer from Task A to Task B than from Task B to Task A. There is more transfer, for instance, from the learning of poetry to the learning of nonsense syllables than in the reverse direction (35, p. 745). The directionality in this illustration could come from the fact that the poetry is easier, or that it is more natural, or that it suggests a useful technique such as the use of rhythm, or that it is a more inclusive task, calling for rote memory and for other things as well. It has proved very difficult to disentangle one of these characteristics from the other. Even when we concentrate on the relative difficulty of the task, we find that in some experiments difficulty is a function of the stimulus, in others a function of the response, and in still others a function of the linkage between the two (7). It is not surprising, then, that the results have not been consistent. We get reports that there is more transfer from hard tasks to easy (23), from easy to hard (2), and that it makes no difference whether we go from easy to hard or from hard to easy (16). In a desperate attempt to make sense of the confusion, one might suggest that the answer may depend on the likelihood of a reasonable degree of success (ratio of reinforcement) on the training task. If the training task is so difficult as to prevent any successes whatever, then we could expect little skill to be made available for transfer. If the training task, however, although difficult, still permits some measure of success (an effective ratio of reinforcement), we might expect that more total skill might be developed here than with practice on the easier task. In the task of learning difficult pitch discrimination, for instance, or in tracking targets that can appear with great suddenness, Lawrence and Goodwin (25) claim an absolute advantage for practice on an easier training task. Those who had practiced on the easier task and then transferred to the difficult task were able, even on the first trial, to surpass those who had been practicing on the more difficult task itself.

When the criterion task is the performance of a fairly complex task, such as driving a car, will there be any gain from practice in the various components, such as braking, steering, or accelerating? In general, such practice will help but will be less effective than practice in the total complex task (3, 22). As we point out later, however, practice on a complex task is not always feasible, and we may be forced to rely on practice of the components. If there is only a limited amount of time for practice on the components of a task, it may be better to provide considerable practice on one important component than to spread the practice evenly over all. Naturally enough, one should seek a key component (10).

To what extent can performance on a criterion task be facilitated by preliminary work in conceptualization, or by concentration on the general principle involved? Here again the results vary. Judd's classic study showed that a student can increase his skill in hitting an underwater target by a study of the principles of refraction. This has been confirmed by other experiments (14). In some experiments pictured representations have helped in the performance of a motor task (28, p. 310). Valuable results have been obtained by the deliberate use of such conceptual schemata as subjective scales. When people learn to use a mental scale in classifying the intensity of tones, for instance, they can later use this scale in classifying gray shades according to brightness. The development of charts in identifying certain pitches will bring about increased skill in identifying new tones provided the latter fall within the range of the chart used (14). But there is negative information also. Hilgard and his associates (20) found that elaborate conceptual aids had little advantage over sheer rote practice in problem solving, and other investigations (23) have reported failures along with successes. Gibson (14) has suggested that verbalization and emphasis on general principles are especially likely to be helpful when there is one fairly simple constant error to be dealt with, as in the case of shooting at an underwater target.

There is some suggestion that transfer is more likely to occur in extroverted than in introverted students, and in younger than in older students. Not enough work has been done on these topics to make us very confident about the answer.

Intelligence has long been assumed to be an important factor in transfer. It would be amazing if the intelligent student were not more successful in perceiving and formulating the general principle; and if, as we have suggested, this formulation of the general principle is an aid in transfer the bright student should have an advantage. Conversely, we might expect that negative transfer when it occurred would be a more serious problem for the brighter student. To support this latter expectation, there is some evidence. Whenever the retention of previous habits or the utilization of a previously learned rule tends to act as a handicap, then bright students are more affected by that handicap (32, p. 443).

BASIC MECHANISMS IN TRANSFER

Naturally enough, the principles invoked to explain the facts of transfer tend to reflect the basic theoretical orientation of the explainer. Given the connectionistic position of Thorndike and of the later Hullians, it is natural to explain transfer from A to B by suggesting that some of the connections, or bonds, that are involved in A are also involved in B. These bonds, strengthened by practice with A, are available for the performance of B. To one less suspicious of abstract entities such as principles or general ideas, however, it is natural to suggest that the thing transferred is a general principle or concept, or, more recently, a cognition. This classical generalization theory represents the position of Judd, and, approximately, the position of the current field psychologists.

The classical controversy between the theory of identical elements and the generalization theory is still with us. More and more the proponents of the basic theories underlying these rival views are looking to the facts of transfer for tests of the validity of the basic theories. The phenomenon of response generalization, for instance, was seized upon as supporting the generalization theory and as refuting both the theory of identical elements and the underlying connectionistic approach. In this phenomenon, it will be remembered, sorting soiled cards into one bin may actually help when the task is reversed and the soiled card must now be placed in the opposite bin. The field psychologist quickly points out that the thing learned in the first experience is not a single action but the general task to be performed. This general task is the same in both circumstances. To the current connectionist groups, however, the phenomenon seems to provide a bit of a challenge, since the specific S-R connection is actually reversed from task to task. To meet this challenge, the modern connectionist analyzes the task into two elements, a naming sequence and an executive sequence. The naming sequence involves the connection S (Seeing soiled card) -R (Saying, "Soiled card"). The executive sequence involves S (Hearing self say, "Soiled card") -R (Placing card in bin X). The first connection is identical for the two tasks. Only the executive sequence is reversed. Whenever the naming sequence constitutes a considerable part of the entire task we would expect a net gain from the first experience (1; 21; 26; 28, p. 326). As a comment on the intricacy of the problem, it should be mentioned that even here there is an argument as to whether "naming" helps by developing a specific S-R connection or whether the "naming" makes its contribution by establishing a general idea that "naming is important" (17).

TRANSFER IN RELATION TO EDUCATION

When should we rely on transfer instead of direct practice to develop competence in some trait? There are times, of course, when we have no choice. Very often direct practice is unavailable. We cannot provide direct practice in the use of an electronic computer as yet unbuilt to solve problems that have not yet been stated. And yet we may wish to have some competence developed by the time the machine is available or as soon as the problems are set. There are other times when the deliberate provision of direct practice is almost unthinkable. We are unlikely to bring ourselves to provide practice for children in dealing with intense grief or the full fury of an atomic attack. Even when direct practice is available and morally justifiable, it may sometimes be enormously expensive. In a very thorough analysis of this problem, Lawrence (24) suggests that the cost of direct practice should include the money that may be lost while the machines or materials are diverted to training purpose, the time of the instructors, the materials consumed, and the risk of damage that a partially trained person may inflict on himself, on others, or on the machine. Can this total cost be reduced by relying on transfer from less expensive or less hazardous preliminary training? The answer would depend, of course, on the cost of this preliminary training and on the extent of its contribution to the criterion task.

To obtain a more precise view of the part that transfer might play in the total criterion task, Lawrence (24) suggests that the cost of attaining a given level of proficiency in that task be described as a product of: (*a*) proficiency to be attained, (*b*) the ratio of desired proficiency to initial proficiency, (*c*) the number of trials required to produce a unit of proficiency, and (*d*) the cost per trial. He suggests that experience with a preliminary task can have little effect on the desired efficiency or on the cost per trial. The transfer from such a preliminary task will have greatest yield when it enables the student to master the criterion task at a more rapid rate. Other things besides rate of learning might, of course, be transferred (24; 35, p. 737). It would be quite reasonable, for instance, to expect that the transfer from the original task would enable the student to begin his practice on the criterion task at a higher level, and this would affect the ratio of desired proficiency to initial proficiency. When the desired proficiency is very high, however, the ratio will be little affected by changes in initial proficiency, and, for this reason, transfer operating in this way will make only a moderate contribution. All in all, then, transfer from a preliminary task will prove to have most value when it can increase the rate at which the criterion task can be learned.

Such a complex formula is, of course, only one approach to the problem and may seldom be feasible to use. But in deciding whether or not to rely on transfer, we must be influenced by some estimate of the relative costs of achieving a given proficiency by transfer as opposed to direct practice.

We must not assume that practice on a training task is always second best, to be justified only by the greater expense or risk of direct practice. There are times when practice on the training task will produce more gain on the criterion task than would a comparable amount of practice on the criterion task itself. This has happened, it will be remembered, when the criterion task is so difficult that the ratio of success may be extremely low.

Even when the total cost of transfer is clearly greater than that of direct practice, and even when direct practice is feasible, we may prefer to rely on transfer for the sake of some concomitants not considered likely in the direct practice. Latin may be more costly than direct practice in inducing a mastery of English. In the minds of some people, however, this greater cost may be offset by the intrinsic familiarity with Latin to be had from the indirect practice or by the feeling of kinship with other educated people. In the final assessment of the claims of transfer *versus* direct practice, we must also keep in mind the possibility that the attitudes or general approaches induced by the more expensive transfer may persist longer than those generated by direct practice (35).

School Subjects

Since there may be much to be gained from relying on transfer, we are led to ask whether there are some school subjects that especially merit such reliance. If a given subject could be depended upon to transfer to a great many areas, we should expect it to have some influence on the kind of ability that shows up on a test of general intelligence. The most important research on this problem is found in studies by Thorndike and his associates (4) and by Wesman (34). Thorndike devised a plan for comparing the tested intelligence of students exposed to different school subjects. By working with thousands of cases, he was able to establish subgroups taking exactly the same subjects, except that one group took mathematics whereas the other took stenography or cooking. Similar groups permitted comparing the transfer values of each important pair of subjects. In general, there were only negligible gains in intelligence as a result of following particular subjects, and the differences were far too small to be given weight in planning curriculums.

Arithmetic. Practice in some phases of arithmetic frequently produces increased skill in other phases. Children who practice only 110 addition couplets are just as able to add the remaining 90 couplets as children who practice all 200 couplets. Practice with "6 plus 8" will help not only with "8 plus 6" but also with "6 plus 7" and "6 plus 9" as well. Transfer of this kind can be greatly increased by deliberate efforts to that end. Transfer is enhanced when children are encouraged to work out rules to describe what they are doing. Even in Grade II, children may conclude that "Whenever you add 1, it is always the next number." These homemade generalizations, when sharpened and refined by the teacher, can be used with unpracticed material (32, p. 437).

Spelling. In view of the unsystematic nature of English spelling, it is not surprising that transfer in this field can be either positive or negative. Learning to spell *search* may help us to spell *learn*, but may make it more difficult for us to spell *journey*. Typically, experience with the base form *play* will help with the derived form, *playing*. But *create* may get in the way of *creating*. And knowledge of the derived form, *excelling*, may hinder the correct spelling of *excel*. At one time it was suggested that spelling rules are useless, and that each form of each word should be taught as a separate problem. Fortunately, however, this pessimistic view is not supported by research.

Under certain circumstances, some rules can be useful. To be on the safe side, we should concentrate on a very few rules which: (*a*) cover a great many words, (*b*) do not have too many exceptions, and (*c*) are not too difficult to learn. As with arithmetic, there is more transfer when the student studies a group of similar words, works out his own rules, and submits these to the scrutiny of the teacher (32, p. 437-438).

Latin. At one time, of course, Latin was supposed to have tremendous transfer powers and was thought to induce almost all the intellectual virtues. Although these great hopes have not been borne out, it does appear that Latin can be used to increase a student's ability to spell and to read English words and to help in the mastery of other languages. This transfer is more likely to occur if, during the study of Latin, students are deliberately encouraged to apply their new knowledge to aspects of other languages (32, p. 438). Students who have taken several years of high-school Latin get along better in college than students who have had no Latin or very little. The more recent investigators, of course, have been careful to secure groups of comparable intelligence, and with such controls we find that the students who take Latin are significantly ahead of their intellectual peers who do not take Latin. The superiority is most marked in languages, but is also found in nonlinguistic subjects (32, p. 439). Although this superiority could not be due to "tested" intelligence, it could be due to some of the qualities that induced the students to take Latin in high school. Such things as academic-mindedness or parental pressure may have led students to take Latin and may also have led to superior college grades. These possibilities should be considered before finally accepting the very plausible hypothesis that Latin contributed valuable knowledge, skill, or habits of study.

Science. Even when taught as straight subject matter, a study of science will bring about some increase in ability to use scientific concepts and facts in a functional way (31; 32, p. 438). Such transfer is increased when the possibility of practical application is stressed throughout the teaching. As yet there is considerable doubt about the contribution of high-school science to college performance in scientific subjects. Of some 16 investigations, half showed an advantage for high-school science and half did not. Of the five studies controlling intelligence, two showed an advantage and three did not (32, p. 439).

Mathematics. There is some evidence that geometry, when specially taught, can bring about a definite improvement in ability to think logically about materials in other fields (19). Students who take a fair amount of mathematics in high school seem to perform better in college mathematics. This is true even when the influence of intelligence is controlled. So far, however, there has been no control of interest in mathematics, academic-mindedness, academic drive, or the like. We cannot tell whether high-school mathematics actually helps, or whether the high-school course merely selects people who would do well anyway (32, p. 440).

Attaining Maximum Transfer

The amount of transfer induced can be increased by the method of teaching used. Although no set of principles can be expected to apply to all conditions, there are a few very general rules that cover a fairly wide range of situations.

Bring Out the Feature to be Transferred. The "thing" to be transferred from one experience to another could be a fact, a method, a general principle, an attitude, or a way of life. The teacher of geometry may wish his students to achieve a more logical approach to other subjects. The teacher of Latin may hope that his students will carry to other experiences a greater understanding of the mechanics of language, a more intimate view of English words, or a more adequate grasp of ancient history. In any case, this feature to be carried away should be highlighted and brought into focus. Geometry should be taught so that the logic stands out. Latin constructions should be shown in relation to the linguistic problems that they meet.

Develop Meaningful Generalizations. Transfer is more likely to take place when the thing to be transferred is a generalization, a conscious insight, a constant error to be dealt with, or a rule that can be understood. Skill in memory is likely to transfer if accompanied by deliberate lessons on how to memorize. An understanding of the principle involved can transfer to increased skill in shooting at underwater targets or in assembling apparatus (28). Conceptual schemes developed in one kind of discrimination can help when new tasks in discrimination are encountered (14).

When the amount of transfer hinges on the mastery of a formula or general rule, it is important to be sure that the students become clearly aware of the formula or rule. There is some suggestion that it is better for the students themselves to work out this principle or formulate the rule or describe the essential method or approach. According to one study (32), too precipitous a verbalization of the rule or principle may actually reduce the amount of transfer. In any case, of course, it is important for the teacher to examine the generalizations suggested by the students and to suggest further exercises if those generalizations are not acceptable.

Provide a Variety of Experiences. Whenever it is a principle or generalization that is to be transferred, it is most important to use a variety of experiences to develop the generalization. The basic generalizations of algebra may be grasped more dependably if many different symbols are substituted for the all-pervasive *x*. If the notion of rhythm is the concept to be transferred, the concept should be developed in many different contexts, in the dance, in prose, in the graphic pattern, as well as in poetry. The concept of justice should not be chained to a single play or fable if it is to be transferred freely to other experiences.

Practice in Application to Other Fields. Not only should the element to be transferred be brought into clear focus and, if possible, formulated and clearly structured, but it should also be applied to fields outside the subject matter of the lesson. If the teacher of geometry hopes to induce a logical approach to mystery stories or military strategy or ethics, he should give his students practice in applying the rules to these or other fields. If the teacher of Latin hopes to establish notions that may be useful in other languages, he should encourage the attempt to find examples of a particular construction in other languages. The teacher of mathematics will frequently show how a given concept works in boat design, physics, and the prediction of changes in population.

Although it is clear that we should give much practice in making applications to other fields, we must not conclude that the subject matter itself should be a conglomerate of many fields. There is nothing to suggest, for instance, that the geometry course should consist of equal parts of Euclid, of exercises in syllogisms, and of logical analysis of ethical problems. One laboratory study at least suggests that intensive practice in a single task (such as tracing a design in a mirror) brought about more transfer to new tasks than did an equal amount of practice spread over four different but related activities. Such a conglomerate of diverse activities, in fact, may retard the appearance of the crucial insight on which transfer often depends.

Practice in Transfer. Finally, there is some hope that, just as students can learn to learn, so they may have some success in learning to transfer (28, p. 338). One experience of successful transfer may enable a student to extract more transfer from his next experience, assuming, of course, that there is some transfer to be extracted. Apparently the mechanisms responsible for transfer, whatever they may be, seem adequate to insure that even transfer itself may be made to transfer.

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9

Transfer to a Motor Task as Influenced by Conditions and Degree of Prior Discrimination Training[1]

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Various conditions of verbal pretraining and stimulus predifferentiation apparently facilitate subsequent discriminative behavior (1, 2, 3, 4, 7, 10, 12, 14, 16, 17).[2] Several explanatory mechanisms for such facilitation have been proposed. In an attempt to ascertain the relative adequacy of certain of the proposed explanatory mechanisms, this study investigated acquisition of discriminative

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[2] These and many subsequent references are illustrative. Additional relevant studies are cited in 2, 7, 12, 17.

motor responses as a function of degree of mastery of seven different conditions of prior discrimination training.

EXPLANATORY MECHANISMS

Acquired distinctiveness of cues. One explanation of instances of positive transfer is that prior discrimination training assures the occurrence of distinctive verbal response-produced stimuli, whose presence increases the dissimilarity of the stimulus compounds to be discriminated (5, 8). In several studies in which verbal pretraining and stimulus predifferentiation experiences have improved subsequent discrimination behavior, determination of the contribution of this acquired distinctiveness of cues has been hampered by lack of controls for receptor-orienting responses, learning to pay attention to stimuli, or "learning set" (1, 3, 9). The results of other studies in which those factors were controlled have been inconclusive as to whether experimental strengthening of discriminative verbal responses occasioned positive transfer over and above that brought about by activation of pre-experimentally acquired discriminative verbal responses (7, 10, 16). But greater achievement due to the latter source would also be consistent with the mechanism of acquired distinctiveness.

Some investigators, however, have failed to obtain positive transfer with one or more different combinations of conditions of verbal discrimination training and of subsequent discriminative verbal or motor responses (1, 2, 9, 12, 14, 17). Several reasons for both inconclusive and negative results now seem plausible. First, the discriminative verbal responses may not have always been strengthened sufficiently (1, 2, 14). Second, dissimilarity of the prior training and the test situations may have reduced the strengths of the discriminative verbal responses in the latter task (14). Further, when the sets of discriminative responses of both verbal pretraining and transfer tasks have the same general topography (e.g., if both require selection and then saying letters of the alphabet [9]), care must be taken to avoid negative transfer due to conflict between the discriminative responses of the first task and those to be acquired in the subsequent test for transfer.

During pretraining experiences to control for exposure to stimuli of the transfer task, already-established repertoires of discriminative responses to those stimuli may have been activated and possibly strengthened. Or such responses may have been aroused during the first few trials of the transfer task. In either case activation of already-acquired discriminative verbal responses to stimuli of the transfer task may have been sufficient to offset any advantages stemming from experimental strengthening of discriminative verbal responses. Other factors which may account for failure to observe facilitation are (*a*) the set to respond rapidly (14), (*b*) marked complexity of the motor task (2), (*c*) use of tasks which emphasize "motor" rather than perceptual processes (12), (*d*) a criterion task requiring discriminative judgments (1), and (*e*) if the discrimination task is too easy, enhanced distinctiveness resulting from dissimilar response-produced stimuli might be a superfluous aid (8).

Other explanatory mechanisms. In addition to the acquired distinctiveness of cues it is possible that strengthening of receptor-orienting responses to the most discriminable aspects of stimuli during discrimination training may have been an alternative or supplementary reason for positive transfer to the test tasks (11). The *S*s may then give different names or identifications to the more discriminable parts (8, 15, 16). Furthermore, warm-up—in the form of learning to attend to stimuli or "learning to learn"—may take place during verbal pretraining, thus bringing about positive transfer to later tasks (1, 2, 3, 4, 7, 10, 14, 16, 17). And, as noted previously, many *S*s may have learned discriminative names for task stimuli prior to their experimental experiences which are then activated during discrimination training to serve as the actual source of any subsequent facilitation (7, 10). One problem which this proposed source of facilitation introduces is that the nature and course of changes in such responses during discrimination training have usually been inferred rather than directly observed. In addition, facilitation attributable to experimental strengthening of discriminative responses should then be measured against a baseline of positive transfer due to activation of already available responses.

Also accorded explanatory significance have been (*a*) knowledge of relevant stimulus attributes (1); (*b*) "learning . . . to use these stimuli as cues for action" (18, p. 759); (*c*) avoidance of "confusing stimuli perceptually" (9, p. 161); (*d*) a "general labeling *process*" (9, p. 166); and (*e*) "a context which provided an objective" (9, p. 167). Before the influence of these factors can be assessed, however, further specification in terms of stimulus-response principles appears to be required.

RATIONALE FOR EXPERIMENTAL DESIGN

To meet various of the shortcomings of previous investigations, in the present study external stimuli appeared in one place; they did not involve form; they differed along only one dimension (intensity of white light); and they were sufficiently similar that differential reinforcement was necessary to bring verbal discrimination to a high level. Also, it was presumed that the criterion task used here emphasized "perceptual" processes, that it was not inherently difficult, and that the to-be-learned manipulative responses were compatible with the discriminative verbal responses. Controls were introduced for the possible effects of activation of discriminative names brought by Ss to the experiment, of "learning set," and of attending to the stimuli.

Seven conditions of prior discrimination training were used in this study. Three of the seven conditions involved the strengthening of discriminative verbal responses by means of experimentally controlled differential reinforcement. Nonsense syllables were the responses in one condition; the other two conditions involved familiar names supplied by E and familiar names selected from each S's repertoire. These latter two conditions were introduced to see whether sets of responses which differed somewhat from each other and from nonsense syllables with respect to source, form, and initial strengths would influence the amount of positive transfer to the criterion task. Any attempt to separate the confounded variables of source, form, and initial strengths, however, seemed premature. In the condition in which each *S* supplied familiar names, reinforcement by *E* saying "right" was substituted for the paired-associates procedure employed with the other two conditions.

Three additional conditions of the study were aimed at providing decreasing levels of activation of discriminative names already in *Ss*' repertoires. Instructions for the first of these were for *Ss* to look at or "see" the stimuli, discriminate among them, and learn to give them different names overtly. Overt occurrence of pre-experimentally acquired responses permitted assessment of possible upward trends in the accuracy of such responses. Also, any effects of saying pre-experimentally acquired labels aloud could be determined by comparison with the second condition, wherein *Ss* were given the same instructions except that verbalization was to be covert. In addition to activation of previously acquired verbal discriminations, both conditions presumably involved a "learning set" without, however, accompanying *E*-manipulated differential reinforcement. The third of these conditions, which involved instructions to see and discriminate among the stimuli, was viewed as establishing a condition of even less activation of already available naming responses.

A final condition allowed for experience in attending to the stimuli with, presumably, minimum arousal of discriminative labels. Serving as the baseline for these seven conditions of prior discrimination training was a control group which learned the manipulative responses without prior experimental exposure to the task stimuli.

Seven degrees of mastery were employed in order to obtain a description of the relationship between extent of facilitation and amount of prior training under each of the conditions except the one involving nonsense syllables for which there were only six degrees. Thus there were 48 different experimental combinations of conditions and degrees and a control group (no prior training).

METHOD

Subjects. There were 10 Ss in each of the 48 experimental groups and 20 in the control group. All were students in the introductory psychology course at the University of Massachusetts. Their assignment to the groups was random within the limitation that each group consist of approximately half men and half women.

Apparatus and stimulus materials. The verbal-motor discrimination apparatus consisted of two circular apertures in a vertical panel for presentation of light and word stimuli, and a 4-in.-high lever for manipulative responses placed vertically in the center of a horizontal panel. The light stimuli were presented in the lower of the two apertures and the stimuli for verbal responses in the upper one. The lever, which replaced the four toggle switches used earlier (7), was pivoted for movement downward in slots extending forward, backward, left, and right. Completion of the downward movement resulted in pressure against a button at the end of each slot. Depression of each button activated a microswitch which lit bulbs on the back of the apparatus to indicate S's choice to E. If the choice was correct, the stimulus light was automatically turned off.

In order to increase the difficulty of the discrimination, as well as to reduce the glare of the most intense light, intensity of the light at the position of S's eyes was reduced from the 3.40 ft.-candles of previous studies (7, 10) to 1.80 ft.-candles. Average readings for the other intensities of white light were approximately 1.50, 1.30, and 1.10 ft.-candles.

TABLE 1

Summary of conditions and degrees of prior training

Prior Training Conditions	Code	Degrees (Criteria of mastery or numbers of trials)						
Learn different nonsense syllable responses to similar intensities	NS	3/4	6/8	9/12	11/12	100%	200%	—
Learn different familiar word responses supplied by *E*	FWES	3/4	6/8	9/12	11/12	100%	200%	M*
Learn different familiar word responses supplied by each *S*	FWSS	3/4	6/8	9/12	11/12	100%	200%	M
Instructed to look at, discriminate among, and give different names to stimuli overtly	SDNO	32	44	56	68	108	132	M
Instructed to look at, discriminate among, and give different names to stimuli covertly	SDNC	32	44	56	68	108	132	M
Instructed to look at and discriminate among the stimuli	SD	32	44	56	68	108	132	M
Instructed to see or look at the stimuli	S	32	44	56	68	108	132	M
Control	C	0						

* *172 trials to match those given to the NS-200% group.*

Procedures for discrimination training. Table 1 summarizes the conditions of prior training and the amount of training for each of the 49 groups. The three verbal-learning conditions involved, respectively, differential strengthening of nonsense syllable labels (NS), or of familiar words supplied by E (FWES), or by each S (FWSS). The nonsense syllables *jer, vol, cuf,* and *nig,* were paired with decreasing intensities in that order. *Very bright, bright, dull,* and *very dull* were the familiar word labels for decreasing intensities which were supplied by E.

For the three verbal-learning conditions, and also for the other four conditions, the control group defined the lowest or zero level of mastery. The six additional criteria of increasing mastery for each verbal-learning condition were 3 correct responses in a 4-trial block (3/4), 6 correct responses in two successive 4-trial blocks (6/8), 9 correct responses in three successive 4-trial blocks (9/12), 11 correct responses in three successive 4-trial blocks (11/12), 100% overlearning based on two times the number of trials to the 11/12 criterion (100%), and 200% overlearning based on three times the number of trials to the 11/12 criterion (200%). However, in order to avoid differential elimination of slow learners from the 11/12, 100%, and 200% criterion groups, alternative criteria of 96, 144, and 192 trials, respectively, were used for some Ss in those groups. Because markedly more trials were required to learn nonsense syllables than familiar words to the 200% criterion, a matching condition (M) was introduced to equate all conditions with the NS-200% group in terms of number of trials. To do so 172 trials were administered to an additional group within each of the other six training conditions.

Learning in the NS and FWES conditions was by the paired-associates technique with lights presented for 2 sec. followed by light-syllable or light-familiar word pairs for 2 sec. Interpair intervals were 4 sec.

The FWSS condition was obtained by instructing Ss to look at the stimuli and try to give each different light a different name which, if it were correct, E would designate as "right." After E had indicated which was the "right" name for a given light, they were to continue to give that name to that stimulus. Because Ss had to see the stimuli and try out some names in order to produce sufficient different responses for E to select four for differential reinforcement, no responses were reinforced during the first 12 trials. Labels given to each intensity during those trials were recorded, and, on Trial 13, E began to select from among the labels and differentially reinforce four of them by saying "right" for only one name for each intensity. Whenever possible, E selected the particular names for each intensity so that they would conform to familiar ways of describing decreasing intensities (e.g., dim, medium, medium bright, bright), or of ordering events (e.g., A, B, C, D). As before (10), however, some Ss gave sets of labels, such as personal names, which could not be ordered in this fashion. The duration of each intensity on each trial for this condition, and also for the conditions described below, was 4 sec., with 4-sec. interstimulus intervals.

In Cond. SDNO (Seeing, Discriminating, Naming Overtly), Ss were told to look at the lights, discriminate among them, and to provide different names for them which were to be said aloud. These verbalizations were recorded. Instructions for Cond. SDNC (Seeing, Discriminating, Naming Covertly) were the same except for the stipulation that the names need not be said aloud. The third activation condition (Cond. SD; Seeing, Discriminating) required Ss to look at and discriminate among the lights but discriminative naming was not mentioned. Upon completion of SDNO, SDNC, or SD experiences Ss were asked how the lights they had seen differed and how many different ones had occurred. Under Cond. S (Seeing) discrimination instructions were omitted altogether in order that Ss would have experience in attending to the stimuli with minimum arousals of a "learning set" or a set to develop or elicit names.

The number of trials administered to Ss in these conditions ere intended to equal the pooled means of trials required by three verbal-learning conditions to attain each of the successively more stringent criteria of mastery. In practice they somewhat exceeded the values they were to match. Specific trial values were 32 to match 3/4, 44 for 6/8, 56 for 9/12, 68 for 11/12, 108 for 100%, and 132 for 200%. The 172 trials of the M groups matched those for the NS-200% group.

Procedure for manipulative task. Approximately 1 min. was required to introduce Ss to the criterion task which was to push the lever in a different direction for each intensity. Correct responses—which, Ss were informed, turned off the light—were movement of the lever to the right for the very dull light, backward for the bright light, left for the very bright light, and forward for the dull light. The same instructions were given to the controls who had not previously seen the lights. Intertrial intervals were not held constant; they averaged about 4 sec.

Two changes from procedures of the most closely related previous studies (7, 10) were made. A noncorrection method was used, instead of a correction method, and the number of trials was increased from 48 to 72.

RESULTS

Discrimination training. Means and *SD*'s of the total number of trials for each level of mastery of responses acquired under Cond. NS, FWES, and FWSS are summarized in Table 2. With some exceptions, attainment of the successively more stringent criteria required increasing numbers of trials, and familiar words, particularly those supplied by *E*, were learned more rapidly than nonsense syllables.

Condition SDNO was included in order to determine the extent to which Ss would be able to use pre-experimentally acquired verbal responses to discriminate among the four intensities. Also of interest was whether such discriminations would improve with increasing amounts

TABLE 2

Means and SD's of trials, including criterial trials
(*N* = 10 for each group)

Criteria of Mastery	Conditions					
	NS		FWES		FWSS*	
	Mean	*SD*	Mean	*SD*	Mean	*SD*
3/4	28.4	9.9	13.2	3.6	36.0	24.7
6/8	51.2	17.2	20.0	8.2	41.2	23.7
9/12	55.2	23.7	26.4	8.0	50.0	23.3
11/12	62.0	21.3	54.0	23.9	82.0	22.6
100%	108.8	49.9	86.0	36.3	134.4	49.7
200%	156.0	42.2	81.6	37.1	95.2	51.1
M	—	—	172.0	—	172.0	—

* Means include the first 12 trials during which there was no differential reinforcement of responses.

of experience with the intensities. The overt naming responses of the seven SDNO groups were therefore recorded and analyzed to see how many Ss had reached a criterion of responding to each intensity with a distinctive name on two or three of its three occurrences during the last 12 trials for each amount of experience. Such discriminative responses were made by 6 of the 70 Ss in

Cond. SDNO; and 3, 2, 1, and 0 lights were so labeled with distinctive names by 24, 32, 6, and 2 Ss, respectively. Thus Ss instructed to respond differentially to the intensities with names already in their repertoires were able to do so, but at an average level which was lower than the 9/12 criterion. Although the N's of 10 for the separate groups were too small for reliable conclusions, increased experience did not seem to produce an upward trend in the levels of accuracy of these activated repertoires of discriminative responses.

TABLE 3

Means and SD's of correct responses for the first 24 and all 72 motor learning trials

Cond.	Degrees (Criteria or trials)													
	3/4 or 32		6/8 or 44		9/12 or 56		11/12 or 68		100% or 108		200% or 132		*M*	
	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
	First 24 Trials													
NS	9.5	4.8	8.0	2.1	9.6	1.8	11.3*	3.4	9.0	4.1	11.4*	5.3	—	—
FWES	7.6	3.5	9.1	4.7	8.3	4.6	9.2	2.7	10.3*	4.8	11.1*	4.7	8.2	2.4
FWSS	10.0*	2.5	7.6	2.8	8.2	4.3	9.9	3.7	10.5*	4.4	9.9	4.2	9.7	3.8
SDNO	8.4	3.6	8.7	2.1	9.9	2.9	6.2	3.0	7.9	3.2	9.2	4.0	7.0	2.8
SDNC	10.3*	5.8	7.4	3.9	6.9	3.6	8.4	2.6	7.3	4.0	7.1	3.7	9.2	2.7
SD	8.0	2.2	7.7	3.6	9.2	3.4	10.6*	3.8	7.4	3.0	10.9*	3.7	10.2*	4.1
S	8.0	3.7	6.5	2.2	6.5	3.0	7.8	2.2	7.6	3.2	8.8	2.7	8.7	3.1
	All 72 Trials													
NS	31.3	9.0	34.7*	5.0	37.0*	6.4	46.0*	10.3	39.4*	13.1	41.6*	12.0	—	—
FWES	34.5*	7.7	32.6*	11.7	31.3	12.5	37.8*	8.0	42.5*	9.6	44.2*	11.5	37.1*	6.4
FWSS	43.0*	5.7	31.2	13.2	35.2*	13.2	38.6*	11.8	42.9*	13.0	37.5*	13.9	42.6*	11.0
SDNO	30.1	9.1	34.1*	5.9	34.3*	10.3	28.8	8.8	30.6	9.6	36.8*	9.9	27.9	7.6
SDNC	37.4*	13.5	29.4	10.9	29.9	13.0	33.9*	10.7	31.4	12.4	30.6	10.8	36.2*	10.9
SD	32.0	8.8	34.3*	13.2	33.3*	9.6	36.2*	12.6	31.4	9.0	34.9*	12.0	37.9*	8.0
S	28.6	10.1	26.4	12.9	22.1	11.4	28.6	7.2	26.0	14.3	30.5	12.3	30.0	12.1

* Different from Cond. C means at .01 level for first 24 and for all 72 trials for 457 *df*.

Acquisition of manipulative responses. Table 3 presents means and *SD*'s of correct responses for the first 24 trials and for all 72 trials of learning differential manipulative responses. Means for Cond. C for the first 24 and for all 72 trials were 7.3 ($SD = 1.74$) and 26.4 ($SD = 5.65$), respectively. All of the means of the verbal learning conditions (NS, FWES, FWSS), both for the first 24 and for all 72 trials, were above the corresponding means of

Cond. C. For the first 24 trials means of two of the SNDO groups (68, M), three of the SDNC groups (56, 108, 132), and none of the SD groups were less than or equal to the mean of Cond. C. All of the 21 72-trial means of the SDNO, SDNC, and SD conditions were larger than the C mean of 26.4. Two of the means of the S groups for the first 24 trials and three of those for all 72 trials were smaller than the corresponding means of the controls. The over-all pattern of these differences thus suggested (*a*) an initial and continuing superiority of the verbal-learning conditions to Cond. C, (*b*) less marked differences initially, but for all 72 trials more pronounced superiority of the seeing and discriminating conditions (SDNO, SDNC, SD) to the controls, and (*c*) approximate equality of Cond. S and C both for the first 24 and for all 72 trials.

The *F* of 2.80 for Conditions of the Conditions × Degrees analysis of variance (Table 4) for just the first 24 trials was significant at the .05 level. Further comparisons of pairs of means for all degrees of each of the conditions by *t*'s, and a nonsignificant *F* for Conditions when the S groups were removed, indicated that the effect of this variable was due entirely to the inferior performance of the S groups. Therefore it was concluded that during the first 24 trials, the average levels of performance of all training conditions but S were essentially the same.

The error term of the analysis of variance ($N = 10$; $df = 438$) and the SD of Cond. C were used to obtain the difference between the mean of 7.3 for Cond. C and Trial 1-24 means of each of 48 training groups necessary for significance at the .01 level (13). This value was 2.68. Two each of the NS (11/12, 200%), FWES (100%, 200%), and FWSS (3/4, 100%) groups, one of the SDNC (32) groups, three of the SD (68, 132, M) groups, and none of the SDNO and S groups exceeded the Cond. C mean by this amount. The significantly superior values are the Trial 1-24 means in Table 3 with asterisks.

The analysis of variance for means based on all 72 trials is summarized in Table 5. Because *t*'s had indicated that means of the 200% and M criteria within any of the

TABLE 4

Analysis of variance for conditions and degrees for the first 24 trials

Source	*df*	*MS*	*F*
Conditions (C)	6	39.96	2.80*
Degrees (D)	5	24.90	1.75
C × D	30	11.06	—
Error	438	14.26	

* $P < .05$.

TABLE 5

Analysis of variance for conditions, degrees, and 24-trial blocks

Source	*df*	*MS*	*F*
Conditions (C)	6	377.93	9.02*
Degrees (D)	5	83.04	1.98
C × D	30	42.44	1.01
Error (*b*)	438	41.91	
Blocks (B)	2	2719.08	358.72*
C × B	12	26.85	3.54*
D × B	10	9.33	1.23
C × D × B	60	6.86	—
Error (*w*)	876	7.58	

* $P < .01$.

conditions did not differ significantly, scores for Ss in these two criteria groups for each training condition were pooled so that comparisons were made for six rather than seven degrees of experience under the seven conditions. Blocks of trials and its interactions with the other variables were the "within" sources of variance (13).

Conditions, Blocks, and Conditions × Blocks yielded *F*'s significant at beyond the .01 level. The more specific differences contributing to the significant *F* for Conditions were investigated by *t*'s whose error variances were based on the "error (*b*)" mean square. These analyses indicated that the three verbal-learning conditions, which did not differ from each other, were superior to the SDNO, SDNC, and SD conditions, which also did not differ from each other. In turn, the seeing and discriminating conditions resulted in more correct responses than Cond. S.

The difference between the Cond. C mean and the Trial 1-72 means of the training groups required for significance at the .01 level was based on a *t* whose error variance was computed from the "error (*b*)" mean square ($N = 10$; $df = 438$) from Table 5 and the Cond. C *SD* of 5.65 for all 72 trials (13). This value was 6.14. Those means for all 72 trials in Table 3 which exceeded the Cond. C mean of 26.4 by 6.14 are designated by asterisks. Differences of less than this amount were obtained for one each of the NS (3/4), FWES (9/12), and FWSS (6/8) groups, for four each of the SDNO (32, 68, 108, M) and SDNC (44, 56, 108, 132) groups, for two of the SD (32, 108) groups, and for all seven of the S groups. Means of the separate groups of each of the discrimination training conditions were summed and averaged for all degrees of mastery or experience and then compared with the mean of Cond. C. The differences required for significance at the .01 level were again computed from the "error (*b*)" mean square (N's $= 70$ or 60) and the *SD* for Cond. C. All of the differences except that between Cond. S and Cond. C exceeded those of 4.44 for Cond. C with FWES, FWSS, SDNO, SDNC, SD, and S, and of 4.54 for Cond. C and NS. Thus there is little doubt that all of the discrimination training conditions but seeing the intensities led to significant facilitation of motor learning.

The significant *F* for Blocks merely indicated that learning occurred during the 72 motor discrimination trials. As noted above, all of the training conditions but S were at the same level during Trials 1-24. During the next two 24-trial blocks the NS, FWES, and FWSS conditions acquired the manipulative responses more rapidly than the SDNO, SDNC, and SD conditions which were learning faster than the S groups. The significant *F* for Conditions × Blocks reflects these differences in rates of strengthening of the manipulative responses.

An additional analysis of variance (Table 6) indicated that neither the main effects of Cond. NS, FWES, and

TABLE 6

Analysis of variance for correct responses for all 72 trials for cond. FWES, FWSS, and NS and for cond. SDNO, SDNC, and SD

	FWES, FWSS, and NS			SDNO, SDNC, and SD		
Source	*df*	*MS*	*F*	*df*	*MS*	*F*
Cond. (C)	2	46.64	—	2	116.62	1.15
Deg. (D)	5	443.99	3.54*	5	5.03	—
C × D	10	136.82	1.08	10	96.67	—
Error	182	125.52		192	101.70	

* $P < .01$.

FWSS nor their interaction with degrees was significant. Means for each degree of mastery of these conditions were therefore pooled and plotted as the upper curve of Fig. 1. Similarly the *F*'s for the main effects of Cond. SDNO, SDNC, and SD and the Conditions × Degrees interaction were not significant (Table 6). The means of these conditions for each amount of experience were averaged to obtain the middle curve. Because means for each amount of Cond. S fell below those for the other conditions they have been plotted separately.

Inspection of Table 3 and Fig. 1 suggests that degree of experience affected acquisition of manipulative responses in only a small part—primarily between the 9/12 and 11/12 points for both separate and pooled NS, FWES, FWSS conditions—of the 6 × 7 × 3 matrix of the analysis of variance of Table 5. Therefore, that neither the main effect of this variable nor its interaction with verbal training conditions was significant is not surprising. Because results for the verbal learning conditions were consistent with theoretical expectations (8), however, the effects of degree of verbal learning only (Cond. NS, FWES, FWSS) were tested (Table 6). The significant *F* suggests that amount of facilitation was directly related to degree of mastery of the experimentally strengthened discriminative verbal responses. Further comparisons with t's indicated that the only significant differences between pairs of means for each degree of the

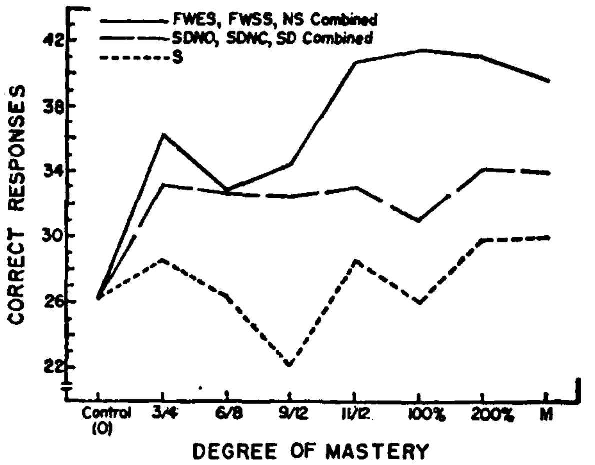

Fig. 1. Mean correct responses for increasing degrees of mastery or amounts of pooled FWES, FWSS, NS *conditions, pooled* SDNO, SDNC, SD *conditions, and Cond. S.*

three conditions combined were the comparisons of the 3/4, 6/8, and 9/12 criteria with each of the more stringent criteria. The F's for different amounts of SDNO, SDNC, and SD experiences (Table 6) and, although not shown here, of Cond. S, were not significant.

DISCUSSION

Discrimination Training

The discriminative verbal responses to the light intensities which were learned to increasing degrees of mastery were nonsense syllables, familiar words supplied by E, or labels selected from Ss' repertoires. In general, as reported previously (10), nonsense syllable responses were learned to each criterion at slower rates than the familiar word discriminations.

Discrimination among the intensities resulting from varying amounts of experience in using pre-experimentally acquired verbal responses was investigated by means of a condition in which *Ss* were instructed to look at the lights, discriminate among them, and give each a different name overtly. The findings for this condition confirmed earlier observations (10) that *Ss* so instructed were able to respond to the lights differentially with names acquired prior to their experimental exposure to the intensities. The levels of discriminative accuracy attained, however, were lower than a 9/12 learning criterion, and there was no evidence that increasing amounts of experience in trying to respond to the different lights with different names brought about greater accuracy. Without further differential reinforcements, therefore, activation of pre-experimentally acquired discriminative verbal responses was apparently complete by 32 trials and, for relatively similar light stimuli, fell short of higher levels of discriminative proficiency.

Acquisition of Motor Responses

Conditions. At least for 11/12 and greater degrees of mastery, acquisition of nonsense syllable names and of familiar word labels (whether supplied by E or by S) yielded significantly greater transfer to the motor discrimination than seeing, discriminating, and naming overtly or covertly, or of seeing and discriminating without explicit instructions to name discriminatively. The three conditions involving seeing and discriminating also produced positive transfer in contrast to seeing the stimuli, which had no facilitative influence.

The obtained superiority of the three verbal learning conditions was more striking than that reported by Goss (7) in an earlier study, and it contrasted with the nonsignificant differences among five discrimination training conditions reported by Holton and Goss (10). The more difficult discrimination required plus, perhaps, the noncorrection procedure seem the most likely sources of differences between the present and earlier observations with similar apparatus. In general, differences between the present findings and results of other studies, particularly those in which prior discrimination training produced no facilitation, probably reflect one or more of the factors noted in the introductory section, most of which were eliminated or minimized by the apparatus and procedures of this experiment.

There were no differences in motor learning associated with the three conditions of verbal learning. Possibly so long as verbal response-produced stimuli are dissimilar, amount of facilitation will be equal. Alternatively, the observed homogeneity may be due to use of particular but not yet completely specifiable combinations of sources, forms, initial strengths, and modes of reinforcement of the three sets of responses. The seeing, discriminating, naming-overtly condition and, as will be noted below, probably the seeing, discriminating, naming-overtly and seeing, discriminating conditions involved activation of already acquired discriminative responses. Such activation was necessarily confounded with warm-up in the form of "learning set" and experience in attending to the stimuli. The seeing condition controlled for attending to the stimuli. Therefore, it seems warranted to conclude that experimental strengthening of discriminative verbal responses had facilitative effects over and above positive transfer attributable to activation and warm-up. The apparatus and stimuli eliminated the factors of relevant-irrelevant attributes of stimuli and enhanced distinctiveness due to receptor-orienting responses.

In the seeing, discriminating, naming-overtly condition the intensities were named with some differential consistency. On the basis of comparable results for learning the manipulative task the occurrence of similar consistent verbal responses, although they were not observed, can be postulated for the conditions that involved seeing, discriminating, and covert naming and seeing and discriminating with no instructions to name. Therefore, the more rapid acquisition of the motor task associated with these conditions than with seeing the stimuli lends some support to the hypothesis of acquired distinctiveness based on dissimilar response-produced stimuli. But "learning set" may have been a contributing or crucial factor.

Conditions of seeing, discriminating among, and naming the intensities overtly or covertly were no more effective than the condition in which the instructions were to see and discriminate. The three types of experience evidently did not involve different levels of activation of pre-experimentally acquired discriminative labels. Little is known about the consequences of overt vs. covert vocalization of well-learned responses, but because of extensive experience with both levels of response amplitude it would not be surprising that the effect is negligible. Also, instructions to name stimuli discriminatively and to discriminate among them may be redundant, since any discriminating by adult humans almost certainly involves some sort of discriminative verbal responses. Therefore the possible difference between the two conditions in which naming was stimulated and Cond. SD would lie in the degree and accuracy of Ss' use of pre-experimentally acquired names. Again, with well-learned responses, there might be no difference.

Degree. Primarily because of differences between the three lower and the 11/12 and more stringent criteria, degree of mastery of familiar word or nonsense syllable responses influenced rate of motor learning. Due to the more difficult discrimination required and/or the noncorrection procedure presumably, the increase from the 9/12 to 11/12 points was more marked than that obtained by Goss (7).

While differences among Holton and Goss' (10) groups and those of this study who had learned to, or exceeded, the 11/12 criterion were not significant, Cantor (4) found some evidence of a decline in positive transfer between four and 12 blocks of 32 discrimination training trials each.

The means for the motor learning of the verbal-learning conditions to the 3/4 criterion were higher than the performance of the controls. Therefore, even lower levels of mastery are necessary to determine the form of the curve declining to the level of the control or no experience level. Regardless of the "distance" between the 0 and 3/4 points along the abscissa, such curves would be approximately sigmoidal in form (8).

As noted above, analysis of the overt responses of the seeing, discriminating, naming-overtly groups indicated that without differential reinforcement maximum discriminative accuracy is reached in 32 trials. It is not surprising, therefore, that amount of seeing and discriminating experiences was not related to extent of positive transfer.

SUMMARY

The experiment was concerned with transfer to a discriminative motor task as a function of degree of experience with different conditions of prior discrimination training with stimuli of the manipulative task. There were 48 experimental groups of 10 undergraduates each and a control group of 20. Four white lights of different intensities served as stimuli to which different familiar words supplied by *E*, *S*-supplied familiar words, or nonsense syllables were learned to six or seven criteria of increasing mastery. Other *Ss* were given equated amounts of experience in seeing, discriminating, and naming the stimuli either overtly or covertly, seeing and discriminating, or seeing. All *Ss* then had 72 trials to learn to move a lever in a different direction for each intensity.

With exception of the seeing experience, all types of

activation or acquisition of discriminative verbal responses resulted in some positive transfer. There were no differences among the three types of verbal-learning experience which, for higher degrees of mastery, were superior to the three types of experiences which involved seeing and discriminating. Amount of positive transfer increased with mastery of verbal responses but did not vary with amount of any of the seeing and discriminating experiences. The over-all pattern of the results was considered consistent with the hypothesis that dissimilar verbal response-produced stimuli increased the distinctiveness of intensities.

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10

The Transfer Value of Given and Individually Derived Principles[1]

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While there is general recognition of the value of organized learning for memory and transfer, differences on how that organization should be taught and attained lead to quite different theories of transfer. On the one hand are those who decry outside direction of learning when one is interested in transfer. Katona (7) using card and geometric puzzles found that his memorization group was significantly poorer on invention and transfer to new problems than his "Help" group that had only examples. He concluded ". . . that formulating the general principle in words is not indispensable for achieving application," (7, p. 89) but he was unwilling to say that learning of principles in words is always less efficient than by example. He put teaching the result as the worst method, teaching by stating the principle as intermediate, and teaching by example as best. However, Hendrix (5) found that with a mathematical principle those groups that discovered the principle independently and left it unverbalized exceeded those who discovered and then

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verbalized, and both exceeded in transfer those who had the principle stated for them and then illustrated.

Opposed to Katona and Hendrix are those like Craig who concluded: "The more guidance a learner receives, the more efficient his discovery will be; the more efficient discovery is, the more learning and transfer will occur" (1, p. 72). In a further study with college groups and with the same method of having the S pick out that alternative among five which does not fit a principle (2) he verified that significantly more such problems were solved when the principle was stated above it than when the S was given only the instruction that one of the five items did not belong. One should note, however, that he found no difference between his groups on transfer to new principles nor was there any difference in retention after 3 or 17 days, although at 31 days the difference favored the directed group.

While Craig's experimental results actually gave little or no support to his claim that guidance is desirable for transfer, more serious opposition to Hendrix and Katona came from Kittell (8). He found that "intermediate direction" (stating a principle) was significantly superior to both the "minimal direction" (told only that one of five alternatives would not fit) and "maximal direction" (*E* told the principle and worked out the answer for S), with minimal direction definitely the inferior method. His subjects were sixth graders while Craig's were college students. That difference in age and educational level may explain the contrast in results. Also Kittell's low number of successful solutions (means only 4.59 for intermediate direction and 1.93 for minimal direction out of 15 principles) suggests that the problems based on linguistic arrangements and meanings may have been too difficult for the *Ss*. If that were the case, then following directions in the stated principle was about the only way to solve the problem when unprovided with sufficient apperceptive mass and experience. Haslerud (4) found that while naive rats transferred anticipatively from forced turns near the goal into prior free units of a maze just as well as when those goal turnings had been established by trial and error, only active trial and error cul-desac elimination in the goal region could readjust an established pattern in the prior free units. If a similar limitation on effectiveness of guidance is present in human Ss, then one might expect any advantage primarily in young Ss and that mainly on their initial learning but none for memory and transfer where Ss have sufficient background to derive a solution themselves.

While the Katona and Hendrix concept of how to get maximal transfer seems to have face validity, at least for adults, their controls and statistical supports are unsatisfactory. When one draws his conclusions on the basis of one principle, e.g., the sum of the first *n* numbers, a question remains of how much of the conclusion is a function of the particular problem used or the selection of individuals for the various groups. More convincing differentiation of principle given from principle derived would seem to require homogeneously varied problems posed in quantity to the same individuals. A likely material has been found in an extension of the familiar cryptogram "Come to London" in the Stanford-Binet. An unpublished pilot study by the junior author under the senior author's supervision indicated an advantage for memory of the independent solving of such coding principles. The present study extends a similar method to transfer. The hypothesis tested is that principles derived by the learner solely from concrete instances will be more readily used in a new situation than those given to him in the form of a statement of principle and an instance.

PROCEDURE

Subjects for the experimental group were 76 members, ranging from freshmen to seniors, of two general psychology classes at the University of New Hampshire. The control group of 24 students in another psychology class ranged from sophomores to seniors.

The experimental groups were each given two coding tests, the second being administered one week after the first. The control group was given only the second test. All tests were administered by the senior author.

The first test composed of 20 coding problems was designed to give the students two types of experience: (*a*) problem solving with specific directions for deciphering the code printed above each problem, and (*b*) problem solving with no directions given. The first part of each problem was the four-word sentence "They need more time," followed by the same sentence in code. A different code was used in each problem. The second part of each problem was the four-word sentence "Give them five more," which the *Ss* were asked to translate into the code for that problem. The given and derived problems were alternated so that the *S* would solve approximately equal numbers of each kind. As a control for differences between the codes, there were two test forms, A and B. The same codes were used in both, but those for which directions were given in form A had to be deciphered by the *S* in form B, and vice-versa. The problems were arranged in approximately the apparent order of difficulty. Examples of moderately easy coding rules are: "For each letter of the sentence write the letter that follows it in the alphabet." "Write the first two letters of each word and then the last two letters of each word." To introduce the test the senior author told the *Ss* that the test was an experiment in cryptography. He wrote an illustrative code on the blackboard and purposely worked it out partly incorrectly to encourage remonstrances from the group that a system or principle was possible. *Ss* were asked to solve the problems in the order they appeared on the test and to do as many as they could in the time allotted. Since the 45 minutes allotted was ample time for all but one or two students in each group, the test was essentially a power rather than a speed test. The *Ss* were not told that they would be retested on the same material.

The second test printed only in one form was given to both the experimental and control groups. Again, the 20 codes used in the first test were used, but instead of the common sentence of Test 1, there were 20 different English sentences 14 to 18 letters in length followed by four translations into code. Only one translation was correct, and the *Ss* were asked to check it. They were told that the other three were simply letters arranged in random order. They were not told that numbers had been assigned to letters of the alphabet and that letters for two of the four codes had been selected according to the order in which those numbers appeared in a list of random numbers. The third false code was composed of letters of the English sentence arranged according to random numbers. The order in which the four codes followed the sentence and the order in which the problems were arranged on the test were also random. No mention was made of the previous test, nor was the purpose of the test told until both tests had been given and the results compiled.

RESULTS

The data for each individual in the experimental group consisted of four scores: (*a*) Number of correct codings on Test 1 problems where the rule was given, hereafter called G_1 scores. (*b*) Number of correct codings on Test 1 problems where the coding principle had to be derived by the S, hereafter called D_1 scores. (*c*) Correct alternatives for those codes in Test 2 that had been G type in Test 1. (*d*) Correct alternatives for those codes in Test 2 that had been D type in Test 1. In the control group the score was the total number of correct alternatives on Test 2. Any coding was considered correct if no more than 1 of the 16 letters was wrong, since carelessness rather than lack of understanding of the principle was probably responsible for the lone error.

Since there was no difference between their results, the two experimental classes were combined. The analysis of results, however, was carried through separately for Forms A and B of Test 1 because a difference significant at the .05 level indicated that the 10 odd and the 10 even problems had not been exactly equated for difficulty. Nevertheless, the direction of results for both A and B groups showed equally high differentiation of G and D situations.

Test 2 performance of the experimental group was significantly different from that of the control group. The means, 15.74 and 10.75 respectively, differ beyond the

.001 level. Apparently something is transferred from the Test 1 experience.

The crucial comparisons are between the G and D kinds of problems. For both Forms A and B on Test 1, significantly more G problems were correctly coded: 8.86 and 8.36 against 5.86 and 4.88 for G and D respectively. The results for Test 2 a week later are given in Table 1. If the differences are added algebraically to the Test 1 scores given in the previous sentence, one obtains the

TABLE 1

Difference in number of problems successfully coded between the transfer test (test 2) and the initial learning (test 1) with each individual as his own control

	N	$\bar{x}$ diff	σ diff	$\sigma_{\bar{x}}$ diff	t	signif.
			$D_2 - D_1$ Problems			
Test A	36	2.83	3.34	.56	5.06	$p < .001$
Test B	40	2.50	3.30	.52	4.81	$p < .001$
			$G_2 - G_1$ Problems			
Test A	36	−0.83	2.37	.38	2.17	$p < .05$
Test B	40	−1.03	2.21	.35	2.94	$p < .01$

nearly equal transfer scores of Craig's experiment (2). But since each individual was his own control for both G and D problems on Tests 1 and 2, it is legitimate to use the subtraction method to find the standard error of the difference for paired observations. The correct identification of those codes which had been D type on Test 1 increased 46% while those which had been G decreased 10%. Both changes are significant, at the .001 and .05 to .01 levels respectively. There is reason to think that both curtailing time to make Test 2 a speed test and increasing time to greater than a week between the learning of the codes on Test 1 and the transfer on Test 2 would accentuate the differences.

DISCUSSION

This experiment has added strong support to the contention of Katona and Hendrix that independently derived principles are more transferable than those where the principle is given to the student. Even though *Ss* produced more correct codings on the original learning when the principle was stated for them, on the "payoff," "applying" to use Katona's term, the advantage definitely passed to those principles derived by the student himself. Fast and accurate learning or performance under immediate guidance is no guarantee of transfer to new problems without such support. From Craig's and our experiments the conclusions just stated are supported by results on college students, but testing of grammar level students by principles of a more suitable level of difficulty than used by Kittell (8) might show a wider application. Our coding method could be easily adapted for that purpose.

The obtained results of this experiment do not follow from inadequate controls. The alternate Forms A and B allowed each principle to be given (G) and derived (D). Individual differences with respect to problem-solving in the *Ss* were ruled out since each person responded to 10 G and 10 D problems on Test 1 and the follow-up of each of these on the transfer Test 2. The control group's much poorer performance on Test 2 indicated that a genuine transfer function was present. Making time on each test practically unlimited pushed the G and D types of presentation to their limit as power tests.

Two possible weaknesses in the transfer Test 2 need to be examined. With four alternatives for each problem, a chance score would average 5. The control group had 10.75 problems correct; this showed good adaptation to the test but significantly less than the 15.74 of the experimental groups. The question whether the better performance of the experimental group was just the result of a second session of practice on coding problems can probably be answered by reference to the study by Warren (9). He found that adults on letter-symbol substitution rapidly attain a plateau on transfer problems because of "learning sets" from early childhood. Coding is in that class of simple activities for adults where experience and practice as such make little difference after the first 10 minutes. Even if one took the maximum change of 37% during Warren's 16 five-minute periods, it would be less than the nearly 50% advantage of our experimental group over the control group. The second possible weakness arises from the randomized construction of the false alternatives of the transfer test. It is conceded that a person might try to solve the problems by excluding the three alternatives because of their random characteristics rather than by trying to recognize and verify some consistent principle in the one true alternative. However, the principles must have played a significant role in the solutions because without them the results of the control and experimental groups would have been equal since they had the same instructions and equal opportunity to use this abortive device.

The theories of transfer found in current educational psychologies are inadequate to explain the present experiment. The senior author plans to develop in another place a theory that transfer is fundamentally an anticipative rather than a perseverative function and that to get transfer one must always counteract the finality of a goal (3). A stated principle to some extent, and even more Kittell's "maximum guidance" of E doing the problems for S after giving him the principle, practically stops transfer, like other goals. Hendrix (6) states from Thorndike that only 5% of high school students have language ability sufficient to receive a ready-made sentence and find readily illustrations in their own background to provide the prerequisite to meaning. If the results of the present experiment can be verified for a wider range of ages and apperceptive masses, then the implications for a direct attempt to teach for transferable principles can not be neglected.

SUMMARY

The educationally important question of how much guidance is desirable if one is interested in transfer was

tested experimentally by a new use of coding. Each of 76 college students as his own control translated into 20 different codes a common four-word sentence, with the rule given for half of the problems and required to be derived solely from example for the other half. As in previous studies on initial learning, the Ss did significantly better on those problems with the rule given. However, a week later on a multiple-choice transfer test consisting of 20 different sentences, one for each of the 20 coding principles of the first test, the selection of the adequate code from three specious ones made by randomizing letters gave very different results. The scores were significantly increased for those problems which had formerly been derived as contrasted with a significant decrease for those problems where the rule had formerly been given. A control group of 24 college students given only the second test proved by significantly poorer performance than the experimental group the value of transfer from the first test. The results give strong support to the postulate of Hendrix that independently derived principles are more transferable than those given. The apparent contradiction with Kittell's study of children was explained by the smaller apperceptive mass in the child, and the prediction was hazarded that as naivety is lost, the probability of transfer from learning which is minimally directed is increased.

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11

A Comparison of Three Varieties of Training in Human Problem Solving[1]

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Recently Adams (1954) compared the efficiency of single-problem training and multiple-problem training in producing transfer to a new problem. He showed that single-problem training resulted in more transfer than did the multiple training procedure. Approaching the same issue, but using different experimental techniques, Callantine and Warren (1955) have shown that the greater the number of training problems the greater will be the amount of transfer on a test problem. These results seem contradictory.

Apart from differences in the problems used, apparatus, and Ss, an essential difference in the two experiments appears to be in the degree of mastery of the training task before the test for transfer. As Callantine and Warren

[1] This study was carried out while the first author was a N. S. F. Predoctoral Fellow in psychology at Yale University studying under the direction of Carl I. Hovland.

point out, all of Adams' single-problem training Ss mastered the training task while many of his multiple-problem training Ss did not. Their Ss, on the other hand, were trained to a criterion so that on any trial they continued until they made the correct response.

This difference in procedure is analogous to the differences in procedure between the experiments of Harlow and Warren (1952) and Hayes, Thompson, and Hayes (1953). Analysis of these superficially discrepant results shows that two factors seem to be necessary to produce a high degree of transfer in these "learning set" types of situation: (*a*) a moderate to high degree of learning must be achieved on a single problem (Adams, 1954; Hayes, Thompson, & Hayes, 1953), and (*b*) there must be presentation of several problems in order to insure learned generalization (Callantine & Warren, 1955; Harlow & Warren, 1952; Hayes, Thompson, & Hayes, 1953). In addition there is a third factor to consider. Research by Wolfle (1935; 1936) and Grether and Wolfle (1936) has also shown that increasing the amount of variation in the stimulus from trial to trial decreases the rate of learning. These factors suggest that the reason Adams' multiple-problem training group was inferior to his single-problem training group was the low degree of learning achieved by the multiple group within any *one* training problem; an experimental procedure involving only two trials per problem would be expected to produce little learning on any one problem.

From this analysis, taking all three factors into account, it is first hypothesized that Ss who have a chance to achieve a high degree of learning within a single problem and also receive training on more than one problem will be superior to both a single-problem training group and a multiple-problem training group of the type used by Adams. Extrapolating from Adams' results, a second minor hypothesis is also formed: other things being equal, single-problem training will result in more transfer of training than will multiple-problem training when learning within any one problem in the multiple training procedure is minimal.

METHOD

In order to test the two hypotheses three groups of Ss were compared in an experiment which in procedure essentially duplicated that of Adams (1954). All Ss received the same number of training trials, 192. The three groups of Ss differed in the distribution of training trials. Group I received all of its training, 192 trials, on a single problem. Group II was trained for 8 trials on each of 24 different problems. Group III was trained for 64 trials on each of 3 problems. The Ss received 24 test trials following training. The test problem was the same for all Ss.

As in Adams' experiment, training and testing consisted in presenting each S with a series of easily discriminable pairs of geometrical figures,[2] e.g., a circle and a triangle. The S was required to choose a response, the correctness of which depended on the spatial arrangement of the stimulus pairs. The stimuli were presented in four horizontal-vertical arrangements, and S had the opportunity to make any one of four responses to each pair. For example, the circle could be either to the left, right, above, or below the triangle. For each of these arrangements a different response was correct. The Ss responded by pressing one of four numbered buttons set in a key box[3] before them. If the correct response was made, a small bulb by the key box lighted. If the response was incorrect, nothing happened.

A single pair of geometrical figures constituted a problem. Since there were four horizontal-vertical arrangements of a single pair, the complete presentation of a problem required four trials. Group I had, then, 48 complete presentations of its single problem in training, while Group II had two complete presentations of each of 24 problems in training, and Group III had 16 complete presentations of each of its three training problems.

The stimulus pairs were presented on slides at a rate of one slide each 10 sec. using a Revere automatic projector. Within blocks of eight the slides were randomized with the restriction that no adjacent slides be identical. When S pressed the correct button for a stimulus pair his response was recorded on an

[2] The authors wish to thank Jack Adams for making it possible for them to use the same geometrical figures which he had used.

[3] The authors wish to express appreciation to the Southern New England Telephone Company for the use of their key boxes in this experiment.

Esterline-Angus recorder. The use of slides, the key boxes and lights, and a 20-channel Esterline-Angus recorder made it possible to run up to 20 Ss at one time.

The Ss were obtained from the twelfth-grade class of the East Haven, Connecticut, High School.[4] They were assigned randomly to groups for the experiment. While originally equal numbers of Ss had been assigned to each group, apparatus failures and school absences resulted in the present unequal distribution of Ss. Group I contained 19 Ss, Group II contained 12 Ss, and Group III contained 32 Ss.

When Ss arrived for the experiment they were each seated in front of a key box. The procedure of showing pairs of geometrical figures and the operation of the key box and lamp were briefly explained. The Ss were cautioned to press only one button for each slide and were told that all responses were being recorded.[5] After the instructions were read and questions answered, the experiment proceeded. The slides were shown without interruption except for brief pauses of 10 sec. following Slides 36, 72, 108, 144, and 192 for changing the magazine in the projector.

RESULTS

Figure 1 shows the mean errors for each of the three experimental groups during training and in the test series; the errors are shown for blocks of eight trials. Since the distributions of errors are skewed, the statistical evaluation of the data has been made using appropriate nonparametric methods.

Examination of Fig. 1 shows that the error scores of all three groups diminished during training, with, as expected, Groups I and III improving more rapidly than Group II. The temporary decrements in the performance of Group III on Blocks 9 and 17 coincide with the introduction of the second and third training problems for that group. Kruskal and Wallis' (1952) *H* statistic was used to test for possible initial differences among the

[4] Carl Garvin, Principal of East Haven High School, generously cooperated with the authors in making this study possible.

[5] Actually, Es had no way to prevent the pressing of more than one button per slide or to record this kind of behavior. However, even though this practice probably occurred it would appear not to be a differential factor among the various groups.

groups at the start of training. These differences were not found to be significant ($P > .30$).

The two original hypotheses suggest two critical comparisons as tests. The major hypothesis was that training

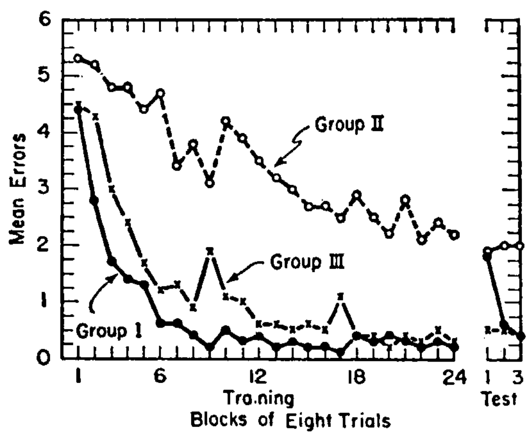

Fig. 1. Mean errors, by blocks of eight, on training and test trials.

which gives a chance for both learning within a problem and generalization between problems will be superior in producing transfer to training which allows only one of these mechanisms to operate; Group III should make fewer errors in the test series than either Groups I or II. The second hypothesis was that, other things being equal, more transfer will result from training which produces a high degree of learning within a single problem than from training on many problems with only a minimal amount of learning on each; Group I should make fewer errors in the test series than Group II. Whitney's (1951; Mosteller & Bush, 1954) extension of the U statistic allows a simultaneous test of these two hypotheses within the framework of the over-all design. This test rejects the null hypothesis, III = I = II, in favor of the predicted alternative, III < I < II ($P < .01$). Both original hypotheses are therefore supported.

With reference to Fig. 1, it will be seen that the superiority of Group III over Group I is almost entirely due to errors on the first block of the test series. The single-problem training group, I, is not initially superior to the multiple-training group, II, but the performance of Group I improves rapidly in the series of test trials and by Block 3 is equivalent to that of Group III. This is similar to the result obtained by Adams. In that experiment he found his single-problem training group not to be initially superior to his multiple group in the transfer test, but in the later stages of the transfer test the single-problem group improved more rapidly than the multiple-problem group and was significantly superior.

DISCUSSION

The present results are seen as reconciling the apparent discrepancy between the experiments of Adams (1954) and Callantine and Warren (1955). When the noncorrection technique used by Adams is employed there is likely to be a low degree of learning within any block of trials, and the learning of a large number of problems at a low level of mastery is less efficient in producing transfer than concentration on thorough learning of a single problem. Hence, continued practice is essential to produce adequate original learning. On the other hand, the correction technique used by Callantine and Warren insures a high degree of learning within each of the multiple problems, and here the greater the variation in stimulus conditions the greater the transfer.

Results involving transfer after training on a number of successive tasks are typically explained in terms of "learning to learn" (Harlow, 1949: McGeoch & Irion, 1952). This seems to imply that new habits are set up within each problem. We are more inclined to conceptualize the process as the learning of a single class of habits to a complex set of cues. Successful transfer would involve learning responses to the complex pattern of stimuli produced both by the immediate environmental stimuli, e.g., the pattern of the slide, and traces from prior stimuli. Specifically, in the present type of problem the following must be accomplished: (*a*) one must learn that up-down variation is associated with Responses 1 and 2, while left-right variation is associated with Responses 3 and 4; (*b*) one must learn that one figure of the stimulus pair, e.g., the circle, is associated with Responses 1 and 3, while the alternate figure, e.g., the triangle, is associated with Responses 2 and 4; and finally (*c*) proficiency must be acquired in making the appropriate response to the combined stimulus. Thus, traces of prior stimuli as well as immediate environmental stimuli determine the response.

With this formulation it is clear why both a high degree of learning on each single problem and stimulus variation are necessary. A high degree of learning within a single problem provides for a strong stimulus-response association. If prior stimulation is to guide subsequent responses, however, the relevant stimulus traces must be discriminated from irrelevant stimuli. This means training on a second problem where the stimulus objects differ, and differential reinforcement can produce discrimination. After extensive variation in the stimulus materials the stimulus trace from a correct choice, or an incorrect choice, will be a relatively invariant, discriminated part of the stimulus complex.

SUMMARY

Sixty-three Ss, divided into three groups, were given different distributions of 192 discrimination problem training trials and then tested on the same transfer problem. Groups I, II, and III were trained on 1, 24, and 3 problems, respectively. On the transfer test Group III proved to be superior to both Groups I and II, while Group I was superior to Group II.

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12

Problem Solving Behavior and Transfer

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This paper was originally presented as part of a symposium entitled "A New Look at Transfer of Training," at the February, 1959, annual meeting of the American Educational Research Association in Atlantic City, New Jersey. Professor Schulz was then teaching at Northwestern University. Preparation of the paper was supported in part by the Northwestern University Department of Psychology—School of Education Carnegie Project. It also appeared in the Harvard Educational Review, *1960,* 30, *pp. 61-77.*

The student attends an educational institution on the presumption that the training he receives there will enable him to solve problems in later life more adequately and efficiently than would be the case had he not received this training. Such effective use of previous learning is an illustration of positive transfer and is the desired outcome of education. However, it is also possible that the training which the student receives may often, alas perhaps more often than we are aware of, make the student less able to solve certain new problems. In short, a student's training may transfer both positively and negatively to subsequent performance.

When a student learns a given technique or subject matter, this knowledge and the conditions under which it was acquired will necessarily affect the student's subsequent performance in situations requiring this technique or subject matter, whether we want it or not. Whatever we teach, explicitly or implicitly, has the potentiality for causing transfer to vary in direction and amount.

As an example of the complexity of the problem consider a class that has been given a large amount of practice in solving certain mathematical problems with a long cumbersome definitional formula. This has been done in the hope that, given mastery of the definitional formula, the student will gain a better understanding of the concept which is defined by the formula. Next a simpler, more economical computational formula is derived for the class; however, for one reason or another the students are not given very much practice with the less time consuming method. It is not unlikely, on the basis of our knowledge of the inverse relationship between the strength of a habit and the rate at which it is forgotten, that some students will remember only the cumbersome method at some later point in time and thereby have their efficiency impaired in the solution of problems amenable to the simpler treatment. Thus, in terms of computational efficiency, their classroom work has transferred negatively, although it still remains an open question as to whether the mastery of the cumbersome method has, in fact, facilitated comprehension of the concept.

The timeworn exhortation to *teach for transfer* is not enough. We need to know more explicitly and precisely when, how, and what or what not to teach in order to produce positive transfer. This need is exceptionally acute with respect to the area of problem solving because of the extent to which problem solving behavior is involved in virtually all activities of an intellectual nature.

Until we know what the variables are that cause problem solving behavior to vary in predictable ways, our teaching is less likely to be as effective as it might be. Therefore, it is prerequisite to our success as educators that we discover the laws which describe the functional relations between various kinds of antecedent variables and later problem solving performance. Given the discovery of such laws it will be possible to adopt training procedures and curricula which maximize the likelihood of positive transfer and minimize the chances of negative transfer in subsequent problem solving behavior. It is the purpose of this paper to suggest an approach which, hopefully, will "transfer positively" to the research task of determining the needed laws.

In a recent review of the research literature in the problem solving area, Duncan [1959] cites over 100 titles which appeared in the 11-year period from 1946 to 1957.[1] In spite of this large amount of research we have made very little progress toward the specification of the aforementioned laws. In fact, the most outstanding feature of the research in the field of problem solving is its failure to provide an articulate body of empirical relations. Thus Duncan makes the following plea repeatedly, ". . . the basic need in problem solving is experimental determination of the functional relationships between dimensionalized independent variables and problem solving performance."[2] Similar pleas have been made in the past by other writers.[3] Yet for one reason or another we have been largely unsuccessful in determining these functional relations. It is not the purpose of this paper to attempt a detailed analysis of possible shortcomings of the previous research. There are, however, two factors which seem to be especially pertinent and deserve to be mentioned briefly.

First, much of the research on problem solving continues to take the form of what Woodworth and Schlosberg [1954] have called the process-tracing experiment. In such experiments the experimenter concerns himself almost exclusively with the response aspects of the problem situation. That is, given the presentation of the prob-

[1] I am indebted to Dr. Carl P. Duncan for so generously making available to me a rough draft of his manuscript entitled, "Research on Human Problem Solving." Dr. Duncan's suggestions and criticisms have also been most helpful in the preparation of the present paper.

[2] Duncan, *op. cit.*, p. 425.

[3] For example, B. J. Underwood, "An Orientation for Research on Thinking," *Psychological Review*, LIX (1952), 209-220; R. S. Woodworth and H. Schlosberg, *Experimental Psychology* (2nd ed., New York, 1954).

lem, what does the subject do in achieving a successful solution? Thus the subjects are asked to introspect or "think aloud." The sequences of responses leading to various correct and incorrect solutions are meticulously analyzed. The responses of "good" problem solvers are compared with those of "poor" problem solvers, and so forth. Now, without seeming to disparage this type of research in any way, it seems worth pointing out that this approach has a very important limitation: it assumes that events observable during problem solving are themselves the primary determinants of the behavior exhibited in the problem situation. The tenability of this assumption is open to serious doubt in so far as the laws of learning and transfer of training apply to the problem solving process. It is one of the most outstanding characteristics of any organism's behavior that its present performance in any situation is in large part, whether directly or indirectly, affected by the past performances of that organism. Therefore, because of its neglect of these antecedent performances, the process-tracing experiment may often provide very little information concerning the actual determinants of the responses which led to the successful solution of a problem. This limitation of the process-tracing experiment is easily corrected by actively manipulating antecedent conditions,[4] or stimulus variables as they are sometimes called, in conjunction with the detailed analysis of response processes.

Secondly, the ease with which it is possible to suggest new potentially relevant independent variables bears a direct relationship to the rate at which the development of a research area may proceed. Previous research and theory in problem solving has not been especially productive in suggesting independent variables which are likely to cause systematic variation in problem solving behavior. Therefore, in addition to the need for giving more consideration to antecedent conditions, there is a need for a conceptual framework which will suggest those ante-

[4] "Antecedent performances" comprise everything the organism has done prior to the experiment proper. "Antecedent conditions" comprise all the stimuli to which the organism has been exposed.

cedent variables which are most likely to be relevant. It is hoped that the conceptual framework which will be presented in this paper will be a step in the direction of meeting both these needs.

The present conception involves a more complete and systematic exploration of the implications of the obvious fact that an organism's present performance in a given situation is to a large extent a function of that organism's past performances in somewhat similar situations. That is, it is proposed that problem solving behavior be considered from a transfer of training point of view. The concern will not be concentrated on the theoretical implications of a transfer conception of problem solving, rather an effort will be made to show that this conception is a tool, as it were, for generating research hypotheses of the type which are likely to result in the discovery of functional relationships between dimensionalized stimulus variables and problem solving performance.

SOME PRELIMINARY CONSIDERATIONS

Rigorous definition of what is meant by problem solving is not possible. However, for the purposes of the present discussion two delineating criteria have been applied in selecting the situations and the behavior to be discussed. (a) Problem solving is high on the dimension of discovery of—as opposed to being told or shown—the correct response. This distinguishes problem solving from conditioning and rote learning where there is a minimum of response discovery.[5] (b) Problem solving is that which occurs in situations presumed by the experimenter to elicit such behavior. The broadness of this definition is consistent with the exploratory spirit in which the present discussion is undertaken.

Transfer is defined by the operations illustrated in Figure 1. Transfer is said to have occurred when there is a reliable difference between the experimental and control

[5] See, for example, Underwood, *op. cit.*, D. M. Johnson, "A Modern Account of Problem-Solving," *Psychological Bulletin*, XLI (1944), 201-29.

group performance on Task B. This difference will be attributed to the effect of Task A on the experimental group's Task B performance; assuming, of course, the two groups have not been differentially treated in any relevant respect other than exposure versus non-exposure to Task A. Positive transfer is demonstrated when the experimental group's performance is superior to that of the control group. Negative transfer has occurred when the control group's performance is better than that of the experimental group.

	Task A	Task B	
Experimental Group	X	X	Reliable Difference
Control Group		X	

Fig. 1. Operations for defining transfer.

Task A may be conceived of in several ways. First, it may be taken to be the composite of an organism's past performances. Second, Task A may be considered some particular constellation of past performances. Finally, Task A may be a specific single past performance. Task B is always the problem situation, or the problem-to-be-solved. Furthermore, in so far as the situation under consideration permits, an effort will be made to specify the relevant stimuli and responses associated with Task A and Task B at a molar behavioral level.

The application of the present conception is not viewed as a panacea, rather as merely a step in the right direction. Hence, no attempt will be made to provide exhaustive or representative coverage of the literature on problem solving. The situations to be discussed have been selected deliberately because they seemed most suitable for illustrative purposes.

DETOUR PROBLEMS

The detour situation is one of the simplest situations generally regarded as involving problem solving behavior. As Köhler has pointed out, all problem situations require some form of detour. If the path to the goal were direct, there would be no problem. The simplicity of the typical detour situation makes it ideal for the initial application of the present analysis.

There are many variants of the detour problem; however, the characteristic features of this problem can be diagrammed as shown in Figure 2. The organism (O) is

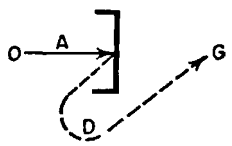

Fig. 2. Typical detour problem.

prevented from direct approach to the goal (G) by a barrier. This barrier can be physical or psychological. The stimulus of interest in this situation is G. The most likely initial response in this situation is response A (direct approach). This response fails to solve the problem. The solution to the problem is accomplished by making response D (detour approach). For the present analysis, let Task A performance consist of making response A to stimulus G (O's past experience in approaching goal objects directly). Task B involves learning to make response D to stimulus G. It has usually been found that this sequence of performances leads to negative transfer. That is, the subject must first unlearn or extinguish the tendency to make response A to the stimulus G before he can learn to make the new response D to that stimulus.

As shown in Figure 3, an explicit translation of the detour problem into our transfer paradigm can be made by considering the performance sequence discussed above as being the one which takes place in the experimental group. The appropriate control group is a group which

	Task A		Task B	
	Stimulus	*Response*	*Stimulus*	*Response*
Experimental Group	G —	A	G —	D
Control Group			G —	D

Fig. 3. The transfer paradigm applied to the detour problem.

has not had any previous experience in making response A to stimulus G. If the control group performance on Task B is significantly better than that of the experimental group, it would seem, in view of the operational congruence, that the laws of behavior concerning negative transfer in the classical learning literature may also apply to the detour situation.

Unfortunately, the defining experiment described in Figure 3 has not been performed. One way of doing such an experiment with animal Ss using food as a goal object, would be to deprive the control group animals of all opportunities for G-A learning prior to their introduction to the detour situation. This could be done by feeding the animal intravenously from birth, since most G-A learning probably occurs, especially with laboratory animals, in approaching the food tray and water bottle in the living cage. The experimental group is given a specified amount of G-A learning.

A second, somewhat less drastic, way of doing this experiment would be to prevent the control group from making direct approach responses to some one kind of goal object. The experimental group is, of course, again given a specified amount of direct approach training with this goal object. Now with this object as the goal both groups are placed in the detour situation. This second procedure has the advantage that the experiment could be done with children. However, with either of the foregoing procedures it does not seem unreasonable to expect that the control group might learn the detour approach more rapidly than the experimental group which has performed the direct approach response prior to being confronted with the detour problem. Exactly how reasonable this expectation is likely to be can only be decided after this experiment has actually been carried out. Nevertheless, by conceiving of the detour situation in the present terms, and by utilizing what is already known about transfer from the psychology of learning, it is possible to identify a large number of potentially relevant independent variables which should be manipulated in the detour situation. Let us consider just a few of these variables for illustrative purposes.

Suppose goal objects which vary systematically along some dimension of similarity are selected. Then the similarity relationship between Task A and Task B goal objects is manipulated. It would be expected that the experimental group's solution of the detour problem would become increasingly proficient as the goal object in Task B is made progressively less similar to the goal object involved in Task A. Finally, when the Task A and Task B goal objects are completely dissimilar, the experimental group would be learning a new response to a new stimulus in Task B and no negative transfer would be expected to appear [Underwood, 1949, p. 303].

A second variable of obvious potential significance is the degree of Task A learning. In the paired-associate learning situation it has been shown that the likelihood of finding negative transfer during Task B decreases as the degree of Task A learning increases up to 100 trials of over-learning when the lists are of the type requiring that a new response be learned to an old stimulus [Underwood, 1949; Atwater, 1953; Mandler, 1954; Mandler, and Heineman, 1956]. Intuitive appraisal of the detour situation would, perhaps, suggest exactly the opposite result. Needless to say, our intuitions are often misleading. On the other hand, it would not be surprising that the laws describing the effect of degree of learning in paired-associate learning are different for the detour situation.

Suppose next that the retention of the detour approach was tested 48 hours after acquisition. Would the experimental group be less able to recall the detour approach as a consequence of having previously performed the direct approach? Similarly, though additional groups would be required, one might systematically vary the degree of Task B learning and test for the retention of Task A. Would there be a decrement in the retention of the direct approach response as a consequence of learning the detour approach to higher and higher degrees? An affirmative answer to the first question would be expected on the basis of traditional studies of proactive inhibition, [Underwood, 1949] and a similar affirmative answer to the second question can be predicted from studies of retroactive inhibition [Thune and Underwood, 1943].[6]

Clearly, at this point no decision can be reached on any of these issues because these variables have as yet not been manipulated in the detour situation. Furthermore, it should be borne in mind that the important point in the present connection is not whether the laws of learning and problem solving are the same or different; rather it is the fact that by utilizing the more highly developed learning literature, testable hypotheses are easily generated. These hypotheses, in turn, provide a basis for directing research in an area where directed systematic research is urgently needed. So much for the detour situation; let us now consider a somewhat more complex problem solving task.

FUNCTIONAL FIXEDNESS

This class of problems and the term functional fixedness was originated by Duncker [1945]. The term refers to a situation in which the subject has become so "fixed" in his perception of an object that he is unable to perceive new or unusual uses for it. For the present analysis one of these problems as it was used by Adamson [1952] will be considered. Given a few thumbtacks, three small pasteboard boxes, three candles and some matches, as well as various and sundry other but inappropriate objects, the problem is to mount the candles in burning position on a vertical surface such as a wall. The problem is most easily solved by tacking the boxes to the wall. Then by melting a little wax on each box the candles can be

[6] The terms "proactive" and "retroactive inhibition" may be unfamiliar to some readers. If two tasks, A and B, are to be learned in that order, and if the learning of A makes it more difficult to retain B, then "proactive inhibition" has occurred. If, on the other hand, the learning of B makes it more difficult to re-learn or remember A, we have a case of "retroactive inhibition." Complementary terms are "proactive" and "retroactive facilitation," which occur, respectively, when learning A makes it easier to recall B, or learning B makes it easier to remember A.

mounted. In Adamson's experiment, for one group of subjects the tacks were presented in one box, the matches in a second box, and the candles in the third box. For a second group of subjects the boxes were left empty. The difference in the performance of these two groups in solving the problem was rather dramatic. In the first group only 12 of 29 subjects solved the problem, while in the second group 24 of 28 subjects achieved solution of the problem. As can be seen from Figure 4, the outcome of

	Task A		Task B	
	Stimulus	*Response*	*Stimulus*	*Response*
Experimental Group	Boxes	Containers	Boxes	Candleholders
Control Group			Boxes	Candleholders

Fig. 4. The transfer paradigm applied to a functional fixedness problem.

this experiment is readily understood when viewed from a transfer point of view. Thus, by giving the experimental group filled boxes the prior learning that the function of a box is that of container was directly associated with the problem situation. In the control group this prior learning was not associated with the problem situation by leaving the boxes empty. Therefore, in Task B, given an old stimulus, the learning of new function is required and the situation conforms once again to the negative transfer paradigm. The conformity would be even closer if the control group had no prior experience with boxes as containers. Nevertheless, it is quite clear that "reminding" the subject directly concerning his past performances with boxes has a marked effect on his immediately subsequent problem solving behavior.

A study by Birch and Rabinowitz, [1951] though again not employing an absolute control group, further illustrates how the present conception applies to functional fixedness. In their experiment the subjects were confronted with two objects initially unfamiliar to them, a switch and a relay. One group learned to use the switch in completing an electrical circuit during Task A. A second group used the relay to complete the circuit. Task

B consisted of a version of the Maier two-string problem. This problem consisted of presenting the subject with two strings suspended from the ceiling to the floor at a distance great enough so that he could not take hold of one string and walk over to reach the other one. The subject is told he must discover a way to tie the two strings together using only the objects at hand. The problem is solved by attaching a weight to one string so that it can be set swinging like a pendulum. The subjects of both groups could use either the switch or the relay as a pendulum bob. It was found that 17 of 19 subjects used the object for a pendulum bob which had *not* been used in Task A. Now suppose the group using the relay in Task A was given only the relay in Task B and compared with a control group that had never used a relay. It seems quite reasonable in light of Birch and Rabinowitz's findings that such a control group would perform better on Task B than the relay group. In other words, once the stimulus "relay" is associated with the response "electrical appliance" it is more difficult to attach a new response "weight" or "pendulum bob" to this stimulus.

The two-string problem as used above, but without Task A, provides still another illustration of the fruitfulness of a transfer conception of problem solving. Typically, the subject is given a familiar object such as a scissors, plier, or screwdriver and is told he can use this object to solve the problem. Here there is, as in the Adamson experiment, no explicit Task A. However, all that is needed to incorporate this situation into the present framework is the simple assumption that the subject's past performances with a plier, scissors, or whatever common implement he is given, constitute Task A. Thus, if these subjects were compared with subjects to whom a plier was a completely unfamiliar object, we would expect the latter group to be superior in performing Task B where the response plier as a weight is required.

Having shown that a transfer conception fits this type of problem situation, we are now in a position to formulate hypotheses suggested by the traditional transfer principles. Again degree of Task A learning is undoubtedly an important variable. This variable could be manipulated experimentally with unfamiliar materials or in terms of records of the subject's past performances with familiar objects.

Even more interesting in the present situation is the possibility of manipulating the functional similarity of the components of Task A and Task B. The first step would be to scale various implements along dimensions of functional similarity. In a pilot study one of my students, Mr. Jerome Eisner, was able to obtain ratings on a seven-point scale as to the functional similarity of various objects with respect to the function of a pendulum bob. Among the objects he scaled were such things as a fishline sinker, light chain ornament, scissors, knife, fountain pen, and mousetrap. Next, by using these ratings, he manipulated functional similarity in the two-string problem. One group of subjects was given an object rated as highly similar in function to a pendulum bob. A second group was given an object of intermediate similarity. A third group received an object of low similarity. Rate of solving the two-string problem was directly related to functional similarity. In a study of the two-string problem by Battersby, Tauber, and Bender, [1953] functional similarity as judged by the experimenter was manipulated and similar results were obtained.

Another promising avenue of investigation is suggested by Harlow's work on learning sets [1949]. Harlow and others have shown that not only do a variety of organisms, such as cats, raccoons, monkeys, and children, learn to solve a particular problem, but they also learn a transferable mode of attack so that when they encounter subsequent problems of a similar nature they are able to solve them with increasing efficiency. This may be viewed as non-specific transfer of "learning-to-solve." Suppose a group of subjects was given many successive Task A-Task B sequences of the functional fixedness type; might it not be expected that they would, as it were, become progressively less functionally fixed?

Similarly, if Task A is arranged so that it suggests the function of the object which is required for solution of the problem in Task B, we may expect positive rather than negative transfer. Suppose an experimental group is given Task A, where Task A consists of repairing a pendulum clock by attaching the pendulum. How will this group's performance on the two-string problem compare with a control group not given Task A?

As in the detour situation, we find that a transfer analysis of the functional fixedness situation also leads to a wide variety of researchable hypotheses.

LUCHINS JAR PROBLEMS AND ANAGRAMS

The water measuring problem developed by Luchins [1942] is readily interpreted in the present context. The subject is presented, actually or symbolically, with three jars (A, B, and C) of known capacity. He is then required to solve a number of problems in which he must deliver a specified quantity of water by appropriately filling and emptying the three jars at his disposal. After a practice problem, the subject is given five problems which have only a single solution, namely, using jars A, B, and C in accordance with the formula B – A – 2C. Next, problems number 7 and 8 are given. These problems can be solved either by means of the B – A – 2C method, or by more direct methods, A – C and A + C. Finally, a ninth problem is given which can only be solved by the direct method.[7]

The analysis of this situation in terms of transfer of training is straightforward, and is shown in Figure 5. The superior performance of the control group on Task B is a well established fact. [Woodworth and Schlosberg, 1954]. The familiar negative transfer paradigm is evident from Figure 5. In Task A with problems 2-6 the experimental group performs the B – A – 2C response, given the stimulus jars A, B, and C. With Task B and problems 7-9 the new response A – C, A + C must now be made to the old stimulus, the three jars.

[7] This is the order of presentation used by Luchins. Others have used the problems in different orders, sometimes omitting the ninth or the practice problem. For a discussion of some of the different forms and interpretations of the test, see E. E. Levitt, "The Water Jar Einstellung Test as a Measure of Rigidity," *Psychological Bulletin*, LIII (1956), 347-70.

It has been found that increasing the number of problems requiring the B − A − 2C response increases the amount of negative transfer [Youtz, 1948]. This finding suggests that the law describing the effect of degree of

	Task A Problems (2-6)		Task B Problems (7-9)	
	Stimulus	*Response*	*Stimulus*	*Response*
Experimental Group	3 jars	B − A − 2C	3 jars	A + C A − C
Control Group			3 jars	A + C A − C

Fig. 5. The transfer paradigm applied to a Luchins problem.

Task A learning on problem solving performance may in fact differ from the law describing this variable's influence in the rote learning situation.

Clearly, depending on the relationship of the performances required in Task A and Task B, this situation can be used to study either positive or negative transfer. Most of the previous research has concentrated on the negative transfer relation. Investigation of this situation when subjects are given problems during Task A which lead to positive transfer during Task B is needed. It would also be of considerable interest to vary both stimulus and response similarity in this situation. For example, to manipulate response similarity suppose Task A required a B + C or B − C response. To manipulate stimulus similarity, we might have Task A consist of cutting a board to a certain length by appropriately manipulating boards of known length. Then for Task B, the subject is given the water jars problem. How will the control and experimental groups' performance compare under these conditions?

Studies employing anagrams as a problem solving task so closely parallel the work on water jars that the foregoing analysis may be applied directly to this situation [Maltzman and Morrisett, 1952; Rees and Israel, 1935]. An anagram is simply a word with the letters scrambled. In terms of the present analysis, during Task A the subjects in the experimental group solve anagrams in which the letters are so arranged that a rule can be applied for solution.[8] During Task B these subjects are given anagrams which require a new rule for solution. The control group, by virtue of not having had Task A, solves the Task B anagrams more rapidly because they do not have an old rule to interfere with the learning of the appropriate rule for solution.

Though it could be shown that other problem solving situations also fit the transfer paradigm, I believe the preceding examples are sufficient to provide the outline of the present approach. Therefore, the remainder of this paper will be devoted to a discussion of the implications of the present approach in more general terms without reference to a particular problem situation.

CONCLUDING CONSIDERATIONS

The present analysis has revealed a curious paradox. We have seen that in each of the situations that were analyzed, the situation's status as a *problem* was largely contingent upon the fact that the sequence of performances conformed to the paradigm for negative transfer. It was quite apparent that by changing the relationship between Task A and Task B performances the situation could easily be used to study positive transfer, in which case the *problem* might no longer be a problem. Yet the latter approach as a means of discovering the ways in which to enhance the efficiency of problem solving behavior has not received much attention. That is, we have rarely attempted to study the conditions which are likely to produce positive transfer in these problem solving situations. The need for such study is apparent.

Returning to the negative transfer aspect of problem solving, schedule of reinforcement is likely to be an extremely important variable. It is a rather well established principle that a response which is reinforced less than 100% (intermittent schedule) of the time will be more resistant to extinction than one which is always reinforced

[8] For example, the first letter of the word always in the third position of the scrambled version, second letter in last position, etc.

(continuous schedule) [Jenkins and Stanley, 1950]. Suppose it is assumed that before a new response can be learned to an old stimulus, the old response to that stimulus must first be extinguished. For example, in the detour situation the direct approach response to the goal stimulus in Task A must be extinguished before the detour approach response can be made to that stimulus in Task B. Hence the resistance to extinction of the Task A response is a critical consideration in this situation, as well as in any other problem situation of this type. The studies thus far conducted in simple learning situations such as a T-maze or finger maze, reveal that intermittent reinforcement of Task A responses does lead to an increase in the amount of negative transfer in performance in Task B.[9] Therefore, since most "real life" learning and problem solving behavior is most likely to be accompanied by intermittent rather than continuous reinforcement, this variable must not be overlooked.

The phenomena of retroactive and proactive inhibition have only been given passing mention in the previous discussion. However, the importance of studying the retention of Task A and Task B should not be underestimated. The fact that these phenomena are called to our attention by the present transfer conception of problem solving

[9] For example, J. H. Grosslight, J. F. Hall and W. Scott, "Reinforcement Schedules in Habit Reversal—a Confirmation," *Journal of Experimental Psychology*, XLVIII (1954), 173-4; K. Johdai, "Extinction as Due to the Changed Direction of a Psychological Force," *Journal of Experimental Psychology*, XLIX (1955), 193-9; E. L. Wike, "Extinction of a Partially and Continuously Reinforced Response with and without a Rewarded Alternative," *Journal of Experimental Psychology*, XLVI (1953), 255-60. In a very recent study, reported after the present paper was completed, Adamson used anagrams and 50% intermittent versus 100% continuous reinforcement of a given solution to these anagrams during Task A. He found the resistance to extinction of the Task A response during Task B was significantly greater following 50% reinforcement than following 100% reinforcement. Hence, the acquisition of a new rule for solving the anagrams in Task B was retarded in the 50% group. These results provide direct support for the validity of the present conception. See R. Adamson, "Inhibitory Set in Problem Solving as Related to Reinforcement Learning," *Journal of Experimental Psychology*, LVIII (1959), 280-2.

may be one of the most important contributions this conception can make. For from the standpoint of educational practice the study of retention is often of greater significance than the study of immediate transfer effects. Our interest lies in what the student does with what he learns after he has left school and applies this learning to problems in later life. Proactive inhibition has recently been identified as one of the major causes of forgetting [Underwood, 1957]. Yet the study of retention of Task B in the foregoing problem situations remains essentially a virgin topic.

In the learning of paired-associate lists it is possible to produce tremendous amounts of negative transfer when the Task A-Task B relationship is such that the S-R pairs learned in Task A are presented again in Task B with the S-R pairings rearranged. Thus if the subject had learned the pairs *lovely-resting* and *robust-heathen* during Task A, he would be required to learn the pairs *lovely-heathen* and *robust-resting* during Task B. This type of Task A-Task B relationship produces even more negative transfer than the learning of a new response to an old stimulus type of relation discussed previously [Besch and Reynolds, 1958]. To the best of my knowledge, this type of relationship has not been studied in connection with problem solving. Suppose, for example, that in the Luchins situation the subject solved problems involving the measuring of gasoline with the formula $B - C + A$. Next he measured water with the formula $C - A + 3B$. Then he is asked to measure oil with the solution being $A - 2B + C$ and so forth during Task A. In Task B, the subject is given problems where the same liquids must be measured, however, the formulae for solution are rearranged. Would this group perform even more poorly than a group which is required to solve the same problems with an entirely new formula?

It is also possible that if some of the complex problems of everyday life could be analyzed into their S-R components, it would be found that the relationship just described would be characteristic of the most difficult problems. Thus, suppose we have trained our students to make a particular set of responses to a particular set of stimuli. Later the student finds himself confronted with a problem which contains this set of stimuli. However, for a variety of reasons (*e.g.*, errors in original training, errors in original learning, new discoveries, etc.) the stimulus-response pairings required for solution of this problem are different from those required previously. How can we minimize the amount of negative transfer this student is likely to encounter?

Whether or not problem solving behavior is "really" mere transfer of training or something more grandiose is unimportant. More important is the fact that the continuity that this conception establishes between the psychology of problem solving and the more highly developed psychology of learning makes relatively easy the generation of testable hypotheses in the former area. Furthermore, these hypotheses are concerned with systematic manipulation of antecedent variables, a type of research much needed in the area of problem solving. The present approach, if successful, has the desirable quality of keeping the number of concepts in the science of behavior at a minimum.

Finally, the present conception has the important virtue of keeping research on problem solving, at worst, one step removed from possible application in matters of educational practice. That is, insofar as it is correct to assume that education is predominantly a problem in transfer of training, the task of translating laboratory findings into the technology of education should be facilitated by the present conception of problem solving. It is, of course, a rare occasion when laws of behavior isolated in the laboratory can be applied directly in the operational situation. More usually, the laboratory findings, no matter how conclusive, require a certain amount of checking under "field conditions" before practical and specific applications can be made. Therefore, until this research is done and the data are in, it would be little more than idle speculation to attempt to state the ultimate implications which the present conception of problem solving is likely to have for current educational practices. Such matters cannot be judged on *a priori* grounds, at least not at the present stage of empirical and theoretical development in this area.

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On Transfer and the Abilities of Man

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In 1954 I published a paper in the Journal of this Association on learning and human ability (5). My intent was to draw together these two fields of enquiry, and present a conceptual framework relating each to the other to mutual advantage. In summary the broad features of that theory are these:

(1) The abilities of man, including the reasoning, number, perceptual, and spatial abilities, and whatever is subsumed under intelligence, are attributes of behaviour, which through learning have attained a crude stability or invariance in the adult, and, as they develop in the child, exhibit considerable stability over limited periods of time at particular age levels.

(2) Biological factors in the formation of ability are not excluded. These fix limiting conditions. The implication is that within these boundaries the range of variation in ability attributable to learning is substantial. Thus emphasis is diverted from biological to environmental determination in the formation of ability.

(3) Cultural factors prescribe what shall be learned

and at what age; consequently different cultural environments lead to the development of different patterns of ability. Those abilities which are culturally valid, and correlate with numerous performances demanded by the culture, are those that show a marked increment with age.

(4) Abilities emerge through a process of differential transfer and exert their effects differentially in learning situations. Those that transfer and produce their effects at one stage of learning may differ from those at another.

(5) The concept of a general intellective factor, and the high correlations between many psychological tests, are explained by the process of positive transfer, the distinctive abilities which emerge in the adult in any culture being those that tend to facilitate rather than inhibit each other. Learning itself is viewed as a process whereby the abilities of man become differentiated, this process at any stage being facilitated by the abilities already possessed by the individual.

These are a few of the main points of the theory. Criticism of the 1954 paper suggests a need for some refinement and elaboration. Specific criticism has been directed to the concepts of ability and transfer (1, 4). I propose to elaborate my understanding of these concepts, to review recent experimental evidence on some aspects of the theory, and to comment on a few of its implications and further extensions.

THE MEANING OF ABILITY

The term ability is used in a variety of ways. It may refer to measures of performance in any situation, these measures being subject to error in relation to an underlying latent variable, which is presumed to be a continuous monotonic increasing function of the observed measures of performance. The observed measures locate the individual on the latent variable within some range of error. The concept of a latent variable is analogous to that of a parameter in conventional statistical usage. It is a parametric continuum. The latent variable is a necessary construct in that it permits a concept of error, and error in any situation being a departure from a fixed, true, standard, latent, or parametric value. This meaning of ability is operational, and the introduction of a latent variable, although in itself postulational, and not operational, is not incompatible with this meaning but is a necessary logic adjunct to it. All this is in effect an elaboration of Thurstone's statement that "an ability is a trait defined by what an individual can do" (13). The trait here refers to the latent variable, and the remainder of the statement to measured performance. Scores on psychological tests, measures of performance on learning tasks, and behavioural observations of many kinds are subsumed under this broad meaning of ability. The statement that a learning curve is a description of the change in ability with repetition implies this usage.

Consider now the meaning of ability as a factor in the methodology of factor analysis. This is in effect a derivative of the meaning I have elaborated above. An individual's factor score is a derived measure. It is the weighted additive sum of measures of performance on separate tasks, the weights being obtained by a process of mathematical analysis. Such derived measures are commonly used in physics. For example, the volume of a sphere is given by the formula $V = 4/3\ \pi r^3$, the volume being a function of the radius which is the independent variable. Such functions as this may be called *determined functions*, and the dependent variable is a derived or determined measure. A factor score also implies a latent factor variable, since both the weights and the separate performance measures which determine it are in practice subject to error. The term factor, as distinct from factor score, usually connotes the latent variable. I construe the concept of ability as a factor score to be essentially operational, since it is a determined function of operationally determined variant values.

The term ability is frequently used in a third sense to refer to some attribute of the state of the organism. This state may be vaguely identified with neurophysiological structure and process which is modified by environmental and genetic factors. The state of the organism is presumed to be functionally related, somehow, to observable performance in particular tasks. The persisting notion that differences in intelligence are related to differences in some state of affairs inside people's heads belongs to this class. Likewise the statement that an ability is a learned acquisition, although not entirely learned, implies this usage. The term ability when used in this general way is strictly postulational. No very precise theoretical propositions have as yet been formulated about the nature of ability when construed in this sense, but it may one day be possible to do so. By linking learning and human ability some of the speculations on the neurophysiological bases of learning may have useful implications for ability theory.

No incompatibility exists between the various meanings of the term ability described above. Ability as a latent variable is a necessary logical implication of operationally determined measures of performance. Ability as a factor score is a derived measure. It also implies a latent factor variable. The use of the term ability to refer to some postulated state of the organism is compelled upon us by all those circumstances that require the use of theoretical constructs in science. In the scientific study of ability all these meanings must of necessity co-exist and supplement each other. Of course, different words may be used to distinguish these different meanings, but this is perhaps unnecessary.

In my 1954 paper I used the term ability in all the various ways described above. Likewise I drew a distinction between, on the one hand, performance measures which had attained a crude stability or invariance in relation to learning, and, on the other hand, performance measures which were subject to marked increment or decrement through learning, or its cessation, over relatively short intervals of time. I pointed out that ability was frequently taken to refer in common usage to these more stable measures, and that what was ordinarily meant by intelligence, the perceptual and spatial abilities, reasoning abilities, many of the basic psychomotor abilities, and the like, seemed to belong to this class. All the above distinctions which were implicit, if not explicit, in my previous work apparently led to certain difficulties in the minds of some of my readers.

THE CONCEPT OF TRANSFER

The concept of transfer occupies a crucial position in any theory attempting to relate learning to human ability. For many years I have been aware of logical difficulties in the commonly accepted concept of transfer. Others, including T. W. Cook (3, 4) have also been aware of this problem. It has long been apparent to Cook, myself, and others that transfer is the more general phenomenon and learning a particular case. In previous theoretical speculation, although pointing to these logical difficulties, I attempted to employ the common interpretation of these terms, but deviated, in the views of some, to an unorthodox position.

I propose now to present a general formulation of the concept of transfer. This formulation generates a number of particular cases. Learning, as conventionally regarded, is one.

Nothing will be said here about the psychological nature of transfer and why or how it occurs. The intent is merely to clarify and restructure the concept, open the way for new forms of experimentation, and provide a conceptual framework within which existing experimental data can be related and reinterpreted.

Transfer, in my view, can best be regarded in terms of the mathematical concept of a function. When two variables are so related that the values of one are dependent on the values of the other, they may be said to be functions of each other. It is customary to distinguish between dependent and independent variables, the value of the dependent variable being dependent on variation in the independent variable. The idea of function is descriptive of change in something with change in something else.

The essence of the idea of transfer, also, is concomitant change, and in the simplest case implies change in performance on one task with change resulting from practice on another.

Consider a simple transfer model comprising measures of performance and amount of practice on two tasks. Denote these by x, y, and t_x, t_y, respectively. Thus we have four variables which may be assumed to be functionally related. The model may, of course, be generalized to include any number of variables, but for the present exposition attention will be directed to the four-variable model only. We may write an expression of the kind $y = \phi(x, t_x, t_y)$, which simply means that performance on one task is some unspecified function of performance on another task, and measures of amount of practice on the two tasks. This expression, and all similar expressions, may be spoken of as transfer functions. Let us now consider particular cases of this general transfer function.

Case I. When the two tasks are considered the same, and y is the same as x for all values of y and x, and t_x is the same as t_y for all values of t_x and t_y, the expression $y = \phi(x, t_x, t_y)$ becomes a two variable function and reduces to $y = \phi(t_y)$ or $x = \phi(t_x)$. These are functions relating measures of performance on a task to measures of amount of practice on that task, and are general expressions for conventional learning curves. In this sense learning is a particular case of transfer and depends on the identity of x with y, and t_x with t_y. Geometrically this means that the model has been reduced from four to two dimensions. All points fall in a plane or the function is a line, instead of a four dimensional surface.

Case II. When t_x and t_y are very large indeed, and additional practice, or its cessation, produces little effect on performance, a state of crude invariance of performance with practice is attained. Here we may measure performance on x and y for a group of individuals and study the relationship between the two sets of measures. I contend that the correlations between many tests, commonly called tests of ability, are in effect correlations between performances which through learning have attained a crude invariance. If this is so it follows in many cases that the correlations between tests of ability can be viewed as particular cases of the more general transfer function. Thus we have a useful linkage between learning and ability.

Case III. When t_y is a constant, that is, where no practice is allowed on the y task, we have a situation involving three variables, y, x, and t_x. We may consider the function $y = \phi(x, t_x)$, or $y = \phi(t_x)$, or $y = \phi(x)$. This latter expression is descriptive of change in y, practice being held constant, with change in x resulting from practice. In effect it implicitly defines the amount of practice in terms of change in performance and not in terms of an independent measure, as, for example, the number of trials. This particular function is of some interest in that it suggests modifications in current thinking on transfer and opens up new lines of experimentation.

The simple transfer function $y = \phi(x)$, when stated in this form, generates, of course, an indefinite number of particular functions. Some of these are deserving of comment. First, consider the case where no change occurs in y with change in x. Here no transfer in fact occurs. Second, consider the case where a constant increment occurs in y with increase in x; the curve describing the relationship is linear and has a positive slope within fixed ranges of the variables. This is a case of linear positive transfer. Similarly the case of linear negative transfer can be recognized. Third, many cases of non-linear transfer functions can be identified. For example, y may show an increase with x over some range of x and no increase or a decrement over some other range. This means that, in the learning of a task, positive transfer may occur at one stage of learning, and no transfer or negative transfer at another. All non-linear cases of this kind may be spoken of as examples of differential transfer, the rate of transfer differing at different stages of learning. Fourth, consider a case where y continues to change after no improvement in x is observed. If examples of this type could be demonstrated experimentally, this would argue for a modification of behaviour through overlearning, despite the absence of any observable change in performance on the learning task itself. This in turn would suggest some continuing modification in the neurophysiological state of the organism during overlearning.

All the above can be greatly elaborated and stated in more precise mathematical form. In general the introduction of the idea of a transfer function argues very simply for the use of the concept of continuous covariation in the study of transfer, and the discarding of discrete concepts. Many of the existing concepts in the field of transfer are, in effect, discrete, such as the conventional distinction between positive and negative transfer. The concern of the experimentalist has frequently been with the demonstration of the presence or absence of certain effects, rather than with the mapping of descriptive continuous functional dependencies. The point has been reached in our experimental exploration where concepts of a more general, and consequently more powerful, nature are required. The ideas on transfer I have presented here are a small step in this direction.

The concept of a transfer function has important implications for experimental design. It is possible in practice to design experiments which embody the ideas on transfer presented here. Such designs are in certain cases general extensions of the existing designs used in experiments on transfer. A variety of new problems will of course present themselves. Unfortunately time does not allow a treatment of experimental design here.

The above discussion points to the following very general formulation. At any given point in time the organism may be said to be in a particular state. The concept of the state of a system is of importance in physics. It has a role in psychology also. This state undergoes continuous change because of a large number of circumstances both inside and outside the organism. One set of factors leading to a change in state is the behaviour of the organism in response to specific environmental circumstances, e.g., the performance of a task. Any change of state leads, theoretically, to changes in an indefinitely large number of other possible forms of performance. Any covariation which can be identified between any two or more forms of performance is conceptualized as a transfer function. Certain aspects of the state of the organism attain a crude stability or invariance and are less susceptible than others to modification through continuing behaviour and other factors over limited ranges of time. These, postulationally, are invariants of the state of the organism, which in turn are functionally related to certain invariants in particular observable behaviours. What we conventionally regard as the abilities of man are among these invariants. The whole process of growth and development is directed towards the reduction of uncertainty in the behaviour of the organism, and to the establishment of these invariants. Thus behaviour becomes organized, or structured, and to some extent predictable. The most noteworthy examples of the prediction of human behaviour in psychology are made through a knowledge of these invariants. Although concern here is with the study of ability, what we conventionally regard as personality, as distinct from ability, can be similarly conceptualized. Characteristics of personality, attitudes, and the like can be viewed as attributes of behaviour which have attained some stability through a lengthy learning process. The discovery of these invariants in the behaviour of man is one of the primary objects of psychological endeavour.

RECENT EXPERIMENTAL EVIDENCE

Supporting evidence for the theory relating learning to human ability is substantial, and is scattered broadly through both the psychological and the cultural anthropological literature, and elsewhere. I do not propose to attempt to collate or review this evidence here, but shall confine my remarks to two recent series of studies: those by Edwin A. Fleishman (7, 8, 9) of the Air Force Personnel and Training Research Centre at Lackland Air Force Base, and those by Dr. Alastair Burnett (2) of the Hospital for Mental and Nervous Diseases, St. John's, Newfoundland.

The design of the Fleishman studies involved the administration of learning tasks to groups of subjects and the recording of measures of performance at a number of stages of practice. In one study the learning task was a Complex Coordination Test and in another a Discrimination Reaction Time Test. In both tasks subjects were required to make motor adjustments or manual responses to visual stimulus patterns. In addition to the learning tasks, a variety of tests were administered to the subjects involving spatial, verbal, perceptual, psychomotor, and other abilities. Intercorrelations were obtained between all measures including measures of performance on the learning task at different stages of practice. The correlations were analyzed using standard factorial methods. One study involved 197 and the other 264 subjects.

These studies show conclusively that substantial and systematic changes occur in the factor structure of the learning task as practice continues. The abilities involved at one stage of learning differ from the abilities involved at another stage. Thus conclusive experimental evidence exists to support the hypothesis of differential transfer. Further these studies suggest that certain non-motor abilities, e.g., spatial and verbal, have a greater involvement in the earlier stages of learning of a psychomotor task than in the later stages, whereas the opposite applies to certain motor abilities, e.g. reaction time and rate of movement. This is in line with the notion that cognitive abilities play a more important role in the earlier stages of the learning of a motor task than in the later stages, when performance becomes organized in the form of a habituated psychomotor response pattern. Generalizations on this point are of course precarious because the distinction between cognitive and motor abilities is arbitrary, and because much will depend on the nature of the particular learning task.

Some of Fleishman's results raise important questions on the nature of adult learning. His studies show that factors specific to the tasks themselves increase in importance as practice continues, suggesting the formation of specific or "within-task" abilities, not involving previously established abilities. Acceptance of this finding might require some modification of the theoretical position that adult learning is in large measure a process involving the reorganization or integration of that which has been previously learned. It would mean also in practice that our capacity to predict human behaviour would be limited, and would ever remain in some large measure indeterminate. These findings are not acceptable as they stand, and fortunately there are avenues of escape from the experimental data. The most plausible of these is that the tendency for the specific "within-test" variance to increase with practice is a result of the selection of reference variables of ability, and that with the inclusion of new and different reference variables a higher proportion of this variance at the more advanced stages of learning can be brought under control. If adult learning is a reorganizing or integrating process, involving different ability patterns at different stages, it seems reasonable to suggest that individuals may exhibit differing facilities in organizing or integrating their abilities to cope with a new task. Thus we are led to hypothesize certain basic integration abilities, which may have an important role to play in adult learning. Considerable information exists in the literature of factor analysis (6, 10) on the existence of such integration abilities, but this information has not, hitherto, been brought to bear on the problem of adult learning. Fleishman is currently engaged in a comprehensive investigation of the involvement of certain integration abilities in performance at different stages of practice on psychomotor tasks. It is hoped that these studies will show that a higher proportion of the specific within-task variance can be brought under control. If experimentation demonstrates that this is not so, then we may be required to revise existing theories of adult learning and make some radical changes in our current concepts of predicting human behaviour, or evolve another line of attack.

In passing I should mention that the Fleishman experiments provide the empirical buttressing for a number of important hypotheses in my theory which in turn provides a theoretical basis for his experiments. Although I had previously discussed the design of experiments of this type, their conduct, which is very exacting and laborious, lies well beyond the range of the facilities of most university investigators.

Dr. Burnett's work carried on at McGill and at the Hospital for Mental and Nervous Diseases in Newfoundland was directed towards the assessment of the abilities of individuals living in relatively isolated outport communities. Burnett's work shows conclusively that the pattern of ability of children reared in relatively isolated outport communities differs markedly from that of children reared in urban centres. In the isolated Newfoundland environment certain perceptual and motor abilities are developed to a high level, whereas verbal and reasoning abilities are less well developed. A retardation in abstract thinking and concept formation seems to occur. These findings go well beyond the simple observation that tests which may be appropriate in one culture may be inappropriate in another, with its implication for the development of culture-free tests. Everything we know suggests that different environmental demands lead to the development of different ability patterns. The concept of a culture-free test is a misconception because the abilities of man are themselves not culture-free. The extensive body of literature on the abilities of individuals reared in cultures markedly different from our own in general supports this conclusion, and adds substantial evidence for the role of learning in the formation of abilities.

SOME IMPLICATIONS OF THE THEORY

The implications of the theory for contemporary theoretical and experimental work in psychology are numerous. I propose to comment on a few of these implications here.

One implication is that some of the data obtained from the study of human ability using an individual difference approach, may with reinterpretation have relevance for learning theory, and that the methodology of ability studies may be brought to bear on learning problems. The substantial body of data, summarized in my 1954 paper, on the relationship between psychological test performance and age in different cultural groups has, for example, direct relevance for learning. Fleishman's work is an excellent example of the application of a methodology extensively used in the ability field to problems which have, among others, important implications for any theory of learning in the adult. Quite recently I have devoted some thought to the problem of intra-individual variability in ability patterns, or profile analysis, and have formulated hypotheses with implications for learning theory which can be investigated by the study of such profiles. This work bears on the organization or integration of abilities through learning. It is still at a rudimentary stage and further comment on it would be unwarranted.

Learning theorists have frequently preoccupied themselves with somewhat involved theories to explain the nature of the learning process itself. They have, of necessity, tended to restrict themselves to a range of learning tasks of low-order generality. No satisfactory methodology has emerged for describing particular learning tasks, or indicating how one task differs from another, other than by a process of simple inspection. The ability theorists have, however, developed descriptive classificatory systems, which, regardless of their many faults, do have some degree of generality in relation to many forms of human behaviour. Since abilities are clearly involved in the learning process, it follows that particular learning tasks can, at different stages of learning, be described in terms of particular ability patterns. We have, then, a method for describing particular learning tasks and differentiating them one from another. Thus it may be possible to make progress in removing learning theory from the context of particular tasks. Students of human ability have long been aware that, just because two tasks looked on inspection very much the same or different, they were not of necessity the same or different in terms of the behavioural responses which they elicited. In consequence it has been the practice, in the ability field, to describe the task situation or the stimulus in terms of the response. This approach underlies the whole of factor analysis. The description of the stimulus in terms of the response is an approach which has apparently not found favour in the learning field where it undoubtedly has application.

A further implication of the theory resides in its emphasis on environmental factors in the formation of ability. In my early training in psychology I was led to believe that man's abilities were irrevocably fixed and rendered unchangeable by biological endowment. This position is no longer tenable. Although it is conceded that biological factors fix certain boundaries, all the evidence seems to suggest that the range of variation that results from learning is, indeed, very great. If this is so, it immediately raises questions of value and social responsibility. It means that a society, through control of the environment and the educative process, can in some considerable degree determine the patterns of ability which emerge in its members. This has long been known to apply in the formation of attitudes. Its extension to the field of man's abilities broadens the area of social responsibility which the psychologist, through his knowledge of human behaviour, may one day be required to f...

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