

THERE IS MORE THAN ONE KIND OF LEARNING¹

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I wish to suggest that our familiar theoretical disputes about learning may *perhaps* (I emphasize 'perhaps') be resolved, if we can agree that there are really a number of different kinds of learning. For then it may turn out that the theory and laws appropriate to one kind may well be different from those appropriate to other kinds. Each of the theories of learning now current may, in short, still have validity for some one or more varieties of learning, if not for all. But to assume that this will settle our squabbles is, I know, being overly optimistic. Other theorists will certainly not support what I am going to say. Not only will each of them feel that his theory is basic for all kinds of learning, but also each of these others will be sure to object to the general conceptual framework within which my distinctions alone make sense. Thus, whereas I would like to hope that this paper will prove an end to all future papers on learning, I realize that such a hope is mere fantasy or wish-fulfillment on my part or something that my clinical colleagues would undoubtedly dub by some far more unpleasant name.

But, to get down to business; I am going to hold that the connections or relations that get learned can be separated into at least six types. These I shall name as:

1. Cathexes
2. Equivalence Beliefs
3. Field Expectancies
4. Field-Cognition Modes
5. Drive Discriminations
6. Motor Patterns

¹ Address of the Chairman of the Division of General Psychology of the American Psychological Association, Boston, Sept. 7, 1948.

First, let me indicate, briefly, what I mean by each of these six terms and then let me proceed to a more detailed discussion of the conditions and laws for the acquisition, de-acquisition and forgetting of the relations named by each of these six.

1. *Cathexes*. By this term I mean connections or attachments of specific types of final positive goal-object, or of final negative 'disturbance-object' to basic drives. (Note that I have coined the term final 'disturbance-object' to cover what have sometimes been called negative goals.) I shall not argue the question as to how many, or what the basic drives may be. I shall assume, however, that you will agree that there are some. For example, none of you will dispute, I hope, the reality of hunger, thirst, sex or fright. By the learning of a cathexis I shall mean, then, the acquisition of a connection between a given variety of goal-object or disturbance-object—i.e., a given type of food, a given type of drink, a given type of sex-object or a given type of fear object—and the corresponding drive of hunger, thirst, sex or fright. That is, the learning of cathexes is the acquisition by the organism of positive dispositions for certain types of food, drink, sex-object, etc. or of negative dispositions against certain types of disturbance-object.

2. *Equivalence Beliefs*. This term sounds shocking. However, I am going to use it. By an equivalence belief I mean a connection between a positively cathected type of goal and a type of sub-goal or between a negatively cathected type of disturbance-object and a type of what may be called a sub-disturbance object or foyer (i.e., a sort of antecham-

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ganism will tend to approach such a type
of sub-goal or to avoid such a type of
foyer with almost the same readiness
with which it will approach a final goal
or avoid a final disturbance-object.

3. *Field Expectancies.* These I for-
merly called 'sign-gestalt-expectations,'
which latter term (to quote Gordon
Allport (1)), Hilgard and Marquis
(10), 'mercifully' shortened to 'expec-
tancies.' This last term is, however, I
feel, too disgustingly short; so I am
'mercilessly' rechristening these entities
'*field expectancies*.' It is my contention
that when an organism is repeatedly
presented on successive occasions with
an environmental set-up, through which
he moves and relative to which he is
sensitive, he usually tends to acquire an
apprehension not only of each group of
immediate stimuli as it impinges upon
him but he also tends to acquire a 'set'
such that, upon the apprehension of the
first group of stimuli in the field, he be-
comes prepared for the further 'to come'
groups of stimuli and also for some of
the interconnections or field relation-
ships between such groups of stimuli. It
is such sets (or field expectancies)
which make it possible for the organ-
ism, human or animal, to exhibit appro-
priate short-cuts and roundabout routes.
It is also the acquisition of such sets
which make possible the phenomenon of
latent learning when (and if) it occurs.

4. *Field-Cognition Modes.* A careful
analysis of the processes involved in the
appearance of field expectancies indi-
cates, I believe, that the final form and
range of any such expectancy is a func-
tion not only of repetition, *i.e.*, of mem-
ory' in the strict sense, but also of 'per-
ception' and of 'inference.' That is, any
given field expectancy which appears in

a given experimental set-up is a function
of the interacting processes of percep-
tion, memory and inference. The modes
or manners of functioning of perception,
memory and inference are what I am
designating as Field-Cognition Modes.
And I would now assert, further, that in
the course of the usual learning experi-
ment there may be acquired not only a
specific new field expectancy but also
new modes or ways of perceiving, re-
membering and inferring—new Field-
Cognition Modes which will, or may, be
then utilized by the given organism in
still other later environmental set-ups.

5. *Drive Discriminations.* It appears,
from some of the latent learning experi-
ments that rats may have to learn to
discriminate their thirst from their hun-
ger. In the first Spence and Lippitt (18)
experiment the rats were run, when
thirsty, with water down one arm of a Y-
maze and food down the other. Then,
after this preliminary training under
thirst, they were shifted to hunger and
tested to see if in their free choices they
now would immediately choose the food
side. They did not. They continued to
go to the water side. At first blush this
result would be interpreted as a verifica-
tion of the reinforcement theory. The
response of going to the right hand—or
water side—had, it will be said, been re-
inforced by the thirst-reduction which
followed the taking of that side during
the training trials. It is to be noted,
however, that the change from thirst in
the preliminary training trials to hunger
in the test trials should also have
changed (to talk in Hull's language)
(12) the S_D or Drive-Stimulus. Further,
since (according to Hull) the overt re-
sponse of turning right or left gets con-
ditioned not only to the maze stimuli
but also to this S_D , this change in S_D
should have caused some breakdown in
the learned response. Such a breakdown
did not appear. It would seem, there-
fore, that—speaking in this same lan-

guage—the new hunger S_D must, under the conditions of this experiment, have for some reason remained undifferentiated from the original thirst S_D .

However, there are other experiments in which such drive discriminations—to use my language—have proved to be possible and to control the results. Thus, we may recall the Hull (11) and the Leeper (17) experiments in which the animals were run hungry and thirsty on alternate nights. Food was down one alley and water down the other, and both Hull and Leeper found that the animals could learn to turn to the food side when hungry and to the water side when thirsty. Obviously in those experiments, since all the other features were held constant, the two drives—hunger and thirst—were discriminated. It appears that this alternation of thirst and hunger and the different locations of the two corresponding rewards throughout the training trials may have been the crucial factor which in these experiments favored such drive discriminations. But there are undoubtedly other ways of inducing drive discriminations, the laws of which a complete psychology of learning must investigate.

6. *Motor Patterns*. It will be noted that this category has to be included by me because I do not hold, as do most behaviorists, that all learning is, as such, the attachment of responses to stimuli. Cathexes, equivalence beliefs, field expectancies, field-cognition modes and drive discriminations are not, as I define them, stimulus-response connections. They are central phenomena, each of which may be expressed by a variety of responses. The actual nature of these final responses is, however, also determined by the character of the motor patterns at the organism's command. My psychology of learning must, therefore, also include a consideration of the laws governing the acquisition of motor patterns purely as such.

So much for a preliminary survey of what I mean by these six terms. Let us turn now to a more detailed discussion of the conditions and laws for the acquisition, de-acquisition and forgetting of each of these six subject-matters for learning.

(1) *Cathexes*

In the first place, the distinction between 'positive cathexes' and 'negative cathexes' must be more sharply drawn. By a 'positive cathexis' I mean the attachment of a type of positive goal to a positive drive. That is, when a type of goal has been positively cathected it means that when the given drive is in force the organism will tend to apprehend, to approach, and to perform the consummatory reaction upon any instance of this type of goal which is presented by the immediate environment. By a 'negative cathexis' I mean the attachment of a type of disturbance object to a negative drive. That is to say, when a type of disturbance object has been negatively cathected it means that, if the given negative drive is strong, the organism will tend to apprehend and to avoid or to get away from any instance of this type of disturbance object which is presented in the immediate environment. But let us turn now to the acquisition, the de-acquisition and the forgetting of these two types of cathexes.

(a) *Positive cathexes*

It would seem that animals or human beings acquire positive cathexes for new foods, drinks, sex-objects, etc., by trying out the corresponding consummatory responses upon such objects and finding that they work—that, in short, the consummatory reactions to these new objects do reduce the corresponding drives. Hence, here I believe, with Hull (12), in the efficacy of reinforcement or need-reduction. I shall assert, however, that no good experimental evidence has as

yet been obtained. The social dog's upper food and sweet open a made in (or other) the dog sort of kind of whether cathected being c introduced produce convers cathected from fo re-intro hence d Further the exact frequen forcemen out. C for these determin constant the pre method with suc about cl who are cists or wrong. experime that nun of need- no doubt causal v would u form. Next, tive cath

yet been adduced for this conclusion. The sort of evidence one wants could be obtained perhaps for hunger, with a special dog-preparation. If, for example, a dog's esophagus were severed and the upper end brought to the outside so that food taken into the mouth, after chewing and swallowing, would drop out into the open and if also a direct fistula were made into the stomach, so that this food (or other food) could then be re-introduced by the experimenter directly into the dog's stomach, we would have the sort of set-up we need. For with this kind of preparation we could discover whether the dog's hunger would become cathected only to those foods which after being chewed and swallowed were re-introduced into the stomach (and hence produced drive-reduction) and whether conversely his hunger would not become cathected to or become de-cathected from foods which in contrast were not re-introduced into the stomach and hence did not produce need-reduction. Furthermore, with such a prepared dog the exact quantitative laws relative to frequency of trials, amounts of reinforcement per trial, etc., could be worked out. Curves could be fitted. Equations for these curves could be mathematically determined and the magnitudes of the constants could be found. In fact, all the precise techniques of quantitative method could be elegantly carried out with such a dog-preparation and bring about closure for all those psychologists who are probably at heart mere physicists or perhaps mathematicians gone wrong. But, prior to such an elegant experiment, all I will say is that I believe that numbers of repetitions and amounts of need-reduction per repetition would, no doubt, turn out to be the two major causal variables and that the curves would undoubtedly be exponential in form.

Next, it must be asked, how is a positive cathexis once acquired, subsequently

de-acquired. How do we come no longer to love specific foods, specific drinks, specific sex-objects? Here I suspect, we have at present even less evidence. If, however, we were to carry out the experiment with our dog preparation, I would suppose that if food were no longer consistently re-introduced into the stomach each time after it had been chewed and swallowed, the cathexis for this type of food would weaken. That is, the failure of reinforcement would, I believe, break the cathexis. And, again, the precise shape and equation for such a curve of de-acquisition could be obtained.

Finally, what about forgetting? Are positive cathexes weakened by the mere passage of time? There seems to be no controlled evidence. But everyday experience suggests that the forgetting of positive cathexes, if it occurs at all, is extremely slow; so that it probably requires years for such cathexes to disappear through mere passage of time and lack of exercise.

(b) *Negative cathexes*

The conditions for the acquisition of negative cathexes seem to be well summarized by the ancient adage "a burnt child dreads the fire." In other words, negative reinforcement would seem to be the typical way in which a negative cathexis is acquired. And by negative reinforcement I mean pain or some other type of noxious physiological state.

Conditioning experiments with electric shock as the unconditioned stimulus would seem to provide the model experiments. These experiments, as we now have them, suggest that the curves are steeper for the learning of negative cathexes than for the learning of positive ones. Indeed ordinary experience suggests that a single negative reinforcement may have an overpoweringly strong and persistent effect.

Next, what are the laws for the de-

acquisition of negative cathexes. How do we unlearn our fears? My guess would be that the only way a negative cathexis is broken is by forcing the individual to stay in the presence of the fear-catheted type of object under conditions in which this type of object does not lead to any noxious physiological result.

Finally, as to forgetting; again no controlled evidence. But it seems to me probable that for negative cathexes, as for positive ones, there is practically no forgetting. The same old fears so often seem to endure for a lifetime.

(2) *Equivalence Beliefs*

By a positive equivalence belief I mean, as I have already indicated, the attachment of a type of sub-goal to a type of final goal such that this sub-goal comes (for the period during which the given equivalence belief holds) to be sought for as if it were the final goal. And by a negative equivalence belief I mean an attachment between a final type of disturbance object and a type of sub-disturbance object or foyer. Now turn to the acquisition, de-acquisition and forgetting of these two types of equivalence beliefs.

(a) *Positive equivalence beliefs*

Before discussing these further, a further distinction must be drawn between these beliefs, as such, and the mere apprehensions of objects as appropriate *means* leading on to positive goals. Consider a concrete example—the obtaining of high grades in courses. In so far as high grades are sought merely because they are apprehended as specific means (or paths) leading to the goal, say, of love and respect from teacher or parent, the pursuit of high grades does not in my terms involve an 'equivalence belief.' Operationally speaking, the individual does not stop when he has got the high grade, but he

goes on to use it to obtain the finally wanted love and respect. If, on the other hand, the obtaining of A's in specific courses seems to bring at once, and by itself, some reduction of the underlying drive or drives, then there is involved some strength of an equivalence belief in my sense of that term. That is to say, the individual then accepts what was originally a mere *means* as *equivalent* to a goal. He experiences, when reaching this means, some degree of drive-reduction. The precise observations necessary for determining such phenomena are, however, difficult.

Let us imagine first a case with rats. Suppose, it were found that when rats reached a well-practiced type of goal-box (even though now there is no food in it), their stomach contractions or some more basic physiological measure of hunger subsided at least for a time, then I would say that we had evidence for some degree of equivalence belief in these rats to the effect that the given type of goal-box was equivalent to food.

Equivalence beliefs will, however, I believe, get most frequently established, not in connection with such a viscerogenic drive as hunger, but in connection with social drives. I cannot here attempt to argue for the validity and reality of such drives. I hope merely that for the purposes of this discussion you will grant me them. If you will, let us consider again the example of the student working for high grades. In so far as it can be demonstrated that with the reception of the high grades there is some temporary reduction in this student's need for love and approbation, even without his going on to tell others about his grade, then we would have evidence for an equivalence belief. The A's would then be accepted by him as equivalent to the love or approbation to which they were originally a mere means. The difficulty is, of course, that we have no good techniques for measuring the

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varying moment-by-moment strengths of any such drives as the need for love or approbation. Some day, however (perhaps by an improvement of projective techniques), we may acquire such a method. And then, as with hunger, we can see if the drive does actually subside in the mere presence of the high grade.

But what would be the laws for the acquisition, de-acquisition and forgetting of such equivalence beliefs? Tentatively, I would propose primacy, frequency and intensity of need-reductions plus early traumatic experiences as the important causal factors. That is, I would hold that the earliness (or primacy) as well as the frequency of the occasions in the life of the individual in which getting high grades, led to love would be important. I would also suggest that early traumatic accompaniments of this sequence between grades and social approbation would also help to 'fixate' the getting of high grades as an end in themselves. But as to the laws or shapes of the curves of acquisition I am completely in the dark. This is a virgin field in which the clinically minded experimentalist and the experimentally minded clinician might well cultivate conjointly.

And, as to the laws for the de-acquisition of equivalence beliefs, I am equally in the dark. And this is sad, because I would assert that a large part of clinical practice consists in the attempt to break erroneous equivalence beliefs. As long as an equivalence belief is not misleading in the sense that the sub-goal is usually actually followed by the true goal, the belief probably serves some physiological economy. But when an equivalence belief persists, even though now the sub-goal practically never leads to the true goal, such an equivalence belief would seem bad. The subject experiences some drive-reduction (Query: so-called 'secondary gain') but most of his actual drive remains unsatisfied. He

accepts the shadow for the substance. He continues to be a greasy grind, although now as a young adult few praise him for it and many even condemn him. And so we find him at last rushing up to the University Psychiatric Counseling Center, or whatever locally it may be called, to find out why he is so anxious and/or so depressed.

Our question becomes how actually do the clinicians, the psychiatrists, the counselors (or whatever they may be called) break such erroneous beliefs. What are the conditions and laws governing their de-acquisition? Nobody seems to know. The therapists do have considerable success. But any clear, agreed-upon statement of their procedure seems to be lacking. Some talk about the 'transference relation.' Others say that the patient is really a 'feeling sensation' type and that he has been trying to operate as if he were a 'thinking intuition' type. Others 'reflect back' to the patient what he himself says. And some merely give him a good sound lecture and tell him to go about his business. But what is common in all these procedures and why they all in some degree succeed in breaking erroneous equivalence beliefs seems still quite beyond us (or at any rate beyond me).

Finally, as to the forgetting or non-forgetting of positive equivalence beliefs through a mere passage of time, we seem to have little evidence. But general experience suggests that such beliefs are not merely forgotten but have to be unlearned.

(b) *Negative equivalence beliefs*

It must be pointed out that negative equivalence beliefs cannot really be differentiated from negative cathexes. For example, the rat is conditioned against the box which led to shock. He also comes to be conditioned against the type of paths which lead to such boxes. Or again a human individual is shown to

have developed a tendency to avoid self-assertive behaviors. And it turns out that such self-assertive behaviors were followed in childhood by parental disapproval, and parental disapproval was followed by a final physiological disturbance. The individual in question has established an equivalence belief that such self-assertive behavior was equivalent to loss of love and resultant physiological disturbance. And, though when this man grows up, this belief may no longer be correct (his parents may be dead or may in fact like in the man what they punished in the child), he nevertheless continues to avoid self-assertive behaviors in situations in which, instead of such behaviors being punished, they would actually be rewarded.

So once again the problem of the laws and the conditions for the acquisition, the de-acquisition and the forgetting of equivalence beliefs becomes clinically important. But again we have no clear evidence. Are negative equivalence beliefs more rapidly established than positive ones? What are the therapeutic procedures most favorable to their de-acquisition? Are they ever forgotten as a result of mere non-exercise? Let me suggest again that the experimentally minded therapist and the clinically oriented experimentalist get together and find out.

(3) *Field Expectancies*

Here my notions as to the conditions and laws of learning depart perhaps most radically from those ordinarily held. By field expectancies I mean (to recall what I said above) those sets which get built up in an organism relative to a specific environmental field. These are the sets which, after learning, make it possible for the organism not only to choose correctly the particular paths in the field on which he has been practiced but also, in some degree, to perform correctly on short-cuts and

roundabouts not previously practiced. It is, of course, the facts of latent learning plus the facts of taking short-cuts and roundabouts, when forced or permitted, which have driven me and others to the notion that when a rat, or a human being, is practiced in a particular set of activities in a particular environment, an essential part of what he acquires is an expectancy, a sign-Gestalt, a cognitive structure, a cognitive map (to use some of the terms which have been suggested) relative to that environment.

Further (and here I confess I have up to now been somewhat unclear) I used to be so impressed by the latent learning experiments of the type invented by Blodgett (3), in which no reward was introduced during the learning period, that I was apt to formulate the conditions involved in such field-expectancy learning primarily in terms of frequency alone and as if motivation played no role. However, if I did this, I was in error. It is obvious that completely unmotivated animals will not learn. They will go to sleep or otherwise divorce themselves from the task. So it must be emphasized that in the Blodgett experiments, even though the animals were not rewarded, they *were* motivated. Also, from the first Spence and Lippitt experiment (18) and from some of the follow-up experiments by Kendler (13) and Walker (19), it appears that mere exercise, mere exploration under one drive, may not be enough to cause the animals to perceive and to learn the position of reward-objects appropriate to some other drive. Thirsty animals apparently do not notice food, even though the experiment be rigged as it was by Kendler and Mencher (14) to seem to force them to notice that the cups which did not contain water did contain food. Summing it up then, it appears that motivation conditions *are* very important for the building up of field-expectancies. I

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would like in this connection to report briefly an experiment recently done by Gleitman at California. He used a T-maze and trained hungry rats to get equal amounts of food at each end; the two end boxes being quite dissimilar in character. Then these two end-boxes were placed in another room and the rats were introduced into each of them. In one they received a shock and in the other no shock. They were then immediately replaced in the original maze, and 22 out of 25 animals immediately avoided at the choice point the pathway which led to the end-box in which they had just been shocked. This showed that during the previous training they *had* learned which path at the choice point led to which end-box in spite of the fact that they had been equally reinforced in both end-boxes. In other words, their hunger, as well as their exercise had probably led them under these conditions to build up spatial sign-Gestalten which could now be appropriately used for a different response, namely for that of now avoiding the end-box in which punishment had just been received. That is, rats can learn under hunger which path leads to which end-box and they can learn that a given end-box now means punishment and *not* food—even though they apparently cannot perceive water and learn its location when under strong hunger, nor perceive food and learn its location when under strong thirst.

To sum up, I would conclude that motivation conditions must be assumed to play a role in the building up of field expectancies. But this does not mean that I hold that such learning consists in the stamping in of S-R habits by reinforcement. The presence of reinforcement in a particular locus makes that locus a goal which determines what performance will take place but it does not stamp in S-R connections though it

probably does give a special vividness to that locus in the total field expectancy.

The main question is, however, what are the laws determining the acquisition, de-acquisition and forgetting of such field expectancies.

As to acquisition.—First we have to know, for the given species, the facts of their perceptual sensitivity. Obviously the field expectancies, which get built up, can include only such aspects of the environment as the given organism is capable of perceiving.

Secondly, we have to know the facts concerning the ability of the given organism (under the given conditions of motivation) to connect and associate the different parts of the field so that when he is in one part of the field he will *remember* what was present in other parts.

Thirdly, we have to know the facts concerning what, for want of a better name, we may call the animal's '*inference abilities*.' These would state the capacity of the given individual, or species, to extend its expectancies *re* given environmental fields beyond the parts upon which this individual has been specifically exercised. It is these capacities which will underlie the animal's ability to short-cut and to take *Umwege*. Such inference facts, when we have them, will obviously be found to include something about an ability on the animal's part to set up a system, or systems, of orienting coordinates as a result of the presence, or absence, of such and such strategically located cues. All this, of course, sounds complicated. But personally, it seems to me that we are a very long way from any precise laws for such field-expectancy learning, and where we seem to have such laws it is because either overtly (or covertly) we have held constant most of the important circumambient variables. We can work out equations and constants for the development of specific behaviors in specific apparatuses under specific

motivations. But how the form of the specific apparatus plus the nature and magnitudes of the specific motivation enter into and determine these equations, I believe we do not know.

Granted then, that we are still very near the beginning of our knowledge of laws for the acquisition of field expectancies, what can we say about the laws of their de-acquisition and forgetting? Here, I have nothing but simple hunches to offer. These hunches would be, first, that the de-acquisition of field expectancies only takes place when the actual environment is so changed that the previous expectancy is no longer suitable. The de-acquisition of one field expectancy results from the learning of another conflicting expectancy. But to what extent the laws and equations for such *new learnings* will be different from those for the original learnings, I hesitate to say. I should expect the equations to have the same form but that new constants would be required.

Finally, as to forgetting, here in the case of field expectancies, as contrasted with that of cathexes and of equivalence beliefs, I believe that true forgetting (*i.e.*, weakening as a result of the mere passage of time) *does* take place. We don't forget our cathexes and we don't forget our equivalence beliefs, but we do forget particular environmental lay-outs which we have not experienced for long periods of time—though this forgetting obviously obeys the sorts of laws which the Gestalt psychologists have uncovered and not the old simple associationistic ones. The remembered environmental lay-out becomes changed, *i.e.*, simplified or sharpened, as well as weakened by the mere passage of time. Some features become enhanced, others minimized or even dropped out and some wholly new features may be added. The work of the Gestalt psychologists (see for example, Koffka, 15 and of Bartlett 2) all bear eloquent testimony to such non-

associationistic features in the forgetting of field-expectancies.

Turn now to the next category.

(4) *Field-Cognition Modes*

This category is the one about which I am least confident. Perhaps a better name would be field lore—that is, perceptual, memorial and inferential lores. Much of these lores—particularly perceptual lore—seems to be given innately. It is the lore that such and such stimulus configurations are to be taken as cues for such and such perceptions. And although a solid basis of such lore is nativistically given, it can, as we know, be added to and modified by experience. Consider, for example, the experiment of Fieandt (one of Brunswik's students, 6) in which the subjects came, as a result of training, to use a very slight mark on one of each pair of cards, as a cue which led them to perceive this card as having an objectively light shade but as in shadow. The slight mark, often not perceived consciously as such, came nonetheless to be used as a perceptual cue that the card was in shadow and therefore really light. And in so far as this tendency would transfer to new situations, it would be an example of an acquired perceptual lore.

Memorial lore is relatively simple. The one innately given principle seems to be that, if a certain sequence of events has occurred on one occasion, this same sequence of events is likely to occur on subsequent occasions. This principle seems to be innately strong. The complementary principle of mere probabilities of occurrence is one which has to be learned. Brunswik (4) more than any other psychologist has investigated this question of the learning to expect mere probabilities. But even he, I think, has not carried the implications far enough. Thus, for example, it would seem to me that the ability to 'tolerate ambiguity,' a concept developed by Else

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the next category.

Perception Modes

is the one about which
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field lore—that is, per-
ceptual and inferential lores.
lores—particularly per-
ceptual—seems to be given innately.
Such and such stimulus
are to be taken as cues
of perceptions. And al-
though such lore is nativ-
e, it can, as we know, be
modified by experience.
For example, the experiment
of Brunswik's students,
in which subjects came, as a re-
sult, to use a very slight
difference in each pair of cards, as a
cue to perceive this card
differentially light shade but
not the slight mark, often
unconsciously as such, came
to be used as a perceptual
cue. The card was in shadow and
light. And in so far as
it could transfer to new sit-
uations, it is an example of an ac-
quired lore.

is relatively simple.
The given principle seems
to obtain in a sequence of events.
On one occasion, this same
principle is likely to occur on
other occasions. This principle
is very strong. The com-
plexity of mere probabil-
ity is one which has to
be learned (4) more than
Brunswik has investigated
the learning to expect
things. But even he, I
understand the implications
of, for example, it would
be the ability to 'tolerate'
the concept developed by Else

Frenkel-Brunswik (7), probably closely
ties in with this learning to expect mere
probabilities. But the conditions of
early childhood training, or whatever
they may be, which develop this new
memorial principle that allows the sub-
ject to be able to remember not 100 per
cent sequences but merely probable se-
quences, that is, to tolerate ambiguities,
have yet to be subjected to more study.

Inferential lore, in its simplest form,
would like perceptual lore and memorial
lore to be to a considerable extent innate.
It would consist of the simple rules of
space, time, force, and quantity, the
bases for which are certainly innate.
Such lore receives, however, tremendous
additions through specific verbal train-
ing—especially in us human beings. We
men learn verbally all sorts of rules
about time, space, force and quantity.
And these rules we then carry around
with us from one specific situation to an-
other—so that they then underlie and
govern our specific apprehensions, *i.e.*,
our field expectancies, for each new en-
vironmental field.

In a word, I am trying to summarize
under this fourth category all those
principles as to the structure of envi-
ronmental fields which are relevant to
all environmental fields, and which
(whether innate or learned) are carried
around by the individual and applied to
each new field with which he is pre-
sented.

As to the conditions and laws for the
acquisition, de-acquisition and forgetting
of such perceptual, memorial and infer-
ential modes, as distinct from the ac-
quisition of the concrete apprehension
of the particular fields themselves, I be-
lieve we have as yet practically no in-
formation.

Turn now to the fifth category.

(5) Drive Discriminations

Here, also, I have but little more to
say. In my preliminary remarks con-

cerning this category I referred to the
latent learning experiments and to the
Hull and the Leeper experiments which
suggested that rats sometimes may have
to learn to distinguish between their dif-
ferent drives. And I believe that there
are similar learnings required of human
beings. We, too, I believe, often have
to learn to discriminate our true needs.
In fact, I would suggest that sometimes
the task of psychotherapy is not merely,
as I argued above, that of breaking in-
correct, yet traumatically held to, equiv-
alence beliefs. It may also be, on occa-
sion, the helping of the patient to learn
to discriminate his real drives or needs.

But, again, we have practically no ex-
perimental data either for rats or for
men as to how we learn, unlearn, or for-
get (if we do) these drive discrimina-
tions.

(6) Motor Patterns

Guthrie (8) has emphasized the learn-
ing of 'movements.' Where 'movements'
are contrasted with 'acts.' Acts he ad-
mits to be goal directed (although in the
last analysis it would appear that for
him they also must dissolve into com-
plexes of non-goal directed movements).
But, in any event, in calling attention
to movements Guthrie is calling atten-
tion, I think, to what I would mean by
motor patterns. And in default of other
experimental theories about the learning
of motor patterns I am willing to take a
chance and to agree with Guthrie that
the conditions under which a motor pat-
tern gets acquired may well be those in
which the given movement gets the ani-
mal away from the stimuli which were
present when the movement was initi-
ated. Any response (*i.e.*, any move-
ment) which goes off will, according to
Guthrie, get conditioned on a single trial
to whatever stimuli were then present.
Therefore a movement which removes
the individual from out the range of
those stimuli tends to be the one which

remains conditioned to them because no other movements have a chance to occur and to displace it. A motor pattern thus gets learned without reinforcement. I would like to point out, however, that such a learning of motor patterns is of necessity always imbedded in a larger goal-directed activity—a point which is not emphasized by Guthrie. His and Horton's cats (9) did learn stereotyped motor patterns for getting out of their hit-the-barber-pole type of problem-box; but they learned them only because they, the cats, were involved in the larger goal-directed activity of getting to the food in front. And, similarly, I believe that rats learn stereotyped motor patterns for running specific mazes only when these specific patterns actually get them to food. When such specific movements do not succeed, trial and error supervene and new movements get a chance to become conditioned, but again only if these new ones prove in the larger setting to get the animal to his goal.

Finally, however, once a movement sequence gets learned in one situation, it is ready, I believe, to be tried out in other situations. We do build up, I believe, many motor patterns (the old name was sensory-motor skills) which we carry around with us as equipment for behaving in new situations. And, whereas, I do not think we as yet know much about the laws for the learning, unlearning and forgetting of such motor patterns, I am willing to accept, for the present, Guthrie's notions concerning their learning and unlearning. Finally, as to their forgetting I can merely point to the everyday fact that one's skills do seem to get rusty with lack of exercise and the passage of time.

Now to conclude: Let me briefly summarize. There are, I believe, at least six kinds of learning—or rather the learning of at least six kinds of relationship. I have called these six relationships: cathexes, equivalence beliefs, field

expectancies, field-cognition modes, drive discriminations and motor patterns. And, although, as usual, I have been merely programmatic and have not attempted to set up, at this date, any precise systems of postulates and deduced theorems, I have made some specific suggestions as to some of the conditions and laws for the acquisition, de-acquisition and forgetting of these relationships. I feel that once we have thought of really good defining experiments for each of these types of learning we can then hypothesize equations, fit empirical curves and dream up constants to our hearts' content. At least I think I could.

Summarizing more specifically for each of the six kinds of learning the following further suggestions were also made. (1) I suggested that the 'reinforcement' doctrine is probably valid for the acquisition of cathexes. (2) I suggested that this 'reinforcement' principle plus traumatic experience is probably also valid for the acquiring of equivalence beliefs. And I asserted that erroneous equivalence beliefs are a large part of what the therapist has to contend with. (3) I held that reinforcement *per se* is not valid for the acquisition of field expectancies and I also emphasized that Gestalt principles of learning and forgetting, rather than associationistic principles, are of prime importance in the acquiring and the forgetting of such field expectancies. (4) For the acquisition, de-acquisition and forgetting of the field-cognition modes of perception, memory, and inference I had no laws to suggest. The development of such laws would depend upon the carrying out of many more carefully designed transfer experiments than we now have. (5) For the learning of drive discriminations I also had no laws but pointed to types of experiment which seem to have favored the development of drive discriminations. (6) Finally, as to the laws for the acquisition of motor patterns, *per se*, I suggested that Guth-

rie's principle may perhaps be

One last word to complicate the one simple set. I do not know be due to some belief on being sweeping though vague, from others this. No doubt would be able sort of nasty t early childhood unravel this some or in other show him its in chimpanzee more of the theologists, or clinicians, together, and develop that (5, 16) that of. And, if our science is and very unphrenic "in inside of or

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rie's principle of simple conditioning may perhaps be correct.

One last word. Why do I want thus to complicate things; why do I not want one simple set of 'laws for all learning? I do not know. But I suppose it must be due to some funny erroneous equivalence belief on my part to the effect that being sweeping and comprehensive, though vague, is equivalent to more love from others than being narrow and precise. No doubt, any good clinician would be able to trace this back to some sort of nasty traumatic experience in my early childhood. Let, then, the clinician unravel this sort of causal relationship in me or in others and I will attempt to show him its analogue in rats, or at least in chimpanzees or perhaps dogs. For, if more of the theoretical and learning psychologists, on the one hand, and of the clinicians, on the other, don't get together, and soon, there is really going to develop that nasty fission in psychology (5, 16) that we have all been warned of. And, if that fission happens, then our science is really going to suffer a long and very unfortunate period of schizophrenic "institutionalization"—whether inside of or outside of our universities.

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