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THE HISTORY AND PRESENT STATUS OF THE LAW OF EFFECT[1]

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Throughout history those concerned with the control of human behavior—parents and educators, businessmen and lawmakers—have acted on the belief that rewards and punishments are powerful tools for the selection and fixation of desirable acts and the elimination of undesirable ones. This commonsense view of the nature of learning has not received undivided support from the professional students of behavior. Almost forty years after the first enunciation of the law of effect as a formal doctrine of learning, the problem of reinforcement is still the subject of heated controversy among the proponents of rival theories of learning. The question has been asked time and again whether this conflict is irreconcilable (47, 142). Perhaps there is some truth in all, or at least some, of the conflicting views, and the main source of difficulty may be the proneness of many theorists to attempt an explanation of *all* learning in terms of their favorite hypotheses. After years of debate, the possibility of a one-principle theory of learning—whether this principle be contiguity, substitution, reward, or expectancy—is still doubtful. This doubt is greatly strengthened by a survey of the history of the law of effect and an evaluation of its present status.

HISTORICAL SOURCES[2]

Even though the law of effect is invariably associated with the name of Thorndike, who first used this phrase (264), the principles embodied in the law have a long history antedating by many years the modern laboratory studies of learning. The formulation of the law reflected

[1] The writer wishes to express his great indebtedness to Professor Gordon W. Allport who first suggested this review. His unceasing interest and generous advice have been invaluable.

[2] The law of effect was previously reviewed in this journal by Waters (312). The writer has greatly benefited from this discussion. For the sake of a unitary presentation some of the problems raised by Dr. Waters are discussed again in this paper.

the impact on psychology of three major trends in the history of thought: associationism, hedonism, and evolutionary theory.

Associationism. Ever since Aristotle first wrote of the laws of association of thoughts (similarity, contrast, and contiguity), many philosophers and psychologists have treated the problems of learning, memory and coherent thinking in terms of associations formed among mental elements. Hobbes, Locke, Hartley, Hume, Mill, Wundt, and many more subscribed to the doctrine of association of ideas. Some pinned their faith on the principle of contiguity alone; others introduced similarity as a basic law; some writers, such as Hartley, suggested physiological foundations for the streams of associations. But in spite of divergencies about this or that aspect of the doctrine, they all agreed that mental organization resulted from the joining and weaving together of elements—for a long time the term *idea* predominated—into the content of consciousness. Associationism is, almost by definition, elementaristic and connectionistic. It is elementaristic because it needs conceptual elements which get associated. It is connectionistic because it deals with the principles of connection among these elements. These two basic characteristics of associationistic thought are made explicit in the law of effect.

Hedonism. Associationism is concerned with the laws of connection of mental elements: it has little to say about the role of motives in the acquisition of learned responses. Yet the study of learning cannot proceed long before coming face-to-face with the problem of motivation. In its approach to motivation, modern learning theory has deep roots in the philosophy of hedonism, and the development of the psychology of learning has been characterized by a stubborn defense of hedonistic principles on the one hand and a struggle for the emancipation from hedonism on the other. To the hedonist, pleasure and pain are the governing principles of behavior. The search for pleasure and the avoidance of pain are the mainsprings of conduct, individual and social, and the basis of social interaction and organization. The idea is very old indeed. Plato saw in pleasure and pain important motives of human action; Aristotle called them the basis of the will. Hobbes' interpretation of social life revolved around man's seeking of pleasure and avoidance of pain. The pleasure-pain principle finally found its most systematic expression in the doctrine of utilitarianism (Bentham) which regarded self-interest as a sufficient principle to account for most of individual and social action. The pleasure-pain principle was still very much alive when the law of effect was formulated.

Evolution. The doctrine of evolution served to bring associationism and hedonism more closely together. The problem of *adaptive* behavior became central. Some responses to environmental stimuli are superior to others and lead to the survival of the fittest. The question at once arises how these superior adaptive responses are *selected* from the multiplicity of responses of which an organism is capable, and then *fixated* and perpetuated. To those who tried to answer this question, hedonism and the pleasure-pain principle provided the principle of selection, and the laws of association the mechanism of fixation.

Early Formulations of a Law of Effect

The convergence of associationsim, hedonism, and evolutionary doctrine is exemplified in the formulation of what is in substance a law of effect by Spencer (**250**) and by Bain (**8**). Their theories have been reviewed and analyzed in detail by Cason (**33**) but since they embody salient features of the effect doctrine they will be briefly summarized here. Spencer's (**250**) basic assumption is that in the course of natural selection there has been established in the various species a correlation between the pleasant and the beneficial, and a similar correlation between the unpleasant and the injurious.[3] That which is pleasant is maintained and repeated and proves beneficial to the biological organism. That which is painful is abandoned and the organism is protected from injury. Spencer then described a physiological mechanism for the selection of useful pleasant acts and the elimination of harmful unpleasant acts. Organisms respond to environmental stimuli in a highly variable, essentially *random* fashion with diffuse discharges of neural energy. Among these random responses there will be sooner or later one (accidental) response which is successful (for example, produces food for the animal). After success will immediately come pleasurable sensations with a large discharge of nervous energy toward the organs engaged in the successful act, e.g., in eating. This heightened discharge of nervous energy will render the successful channels of muscular action more permeable. On recurrence of the circumstances the neural discharge will no longer be diffuse but will be channeled into the successful movements. With every repetition the successful channels will be made more and more permeable, until stable nervous connections have been organized.

[3] This assumption is echoed in James' *Principles* where we read: "If pleasure and pains have no efficacy one does not see why the most noxious acts, such as burning might not give thrills of delight, and the most necessary ones, such as breathing cause agony" (**132**, p. 143). Another exponent of a pleasure-pain theory of learning was J. Mark Baldwin (**9**). See also Pyle (**215**).

Bain (8) formulated a similar theory: spontaneously begun movements which are accidentally successful cause pleasure, and with the pleasure there is an increase in vital energy. A few repetitions of this coincidence of pleasure and movement lead to a neural connection so that pleasure or the idea of the pleasure will evoke the successful movement at once. In the case of pain, the sequence is the reverse, leading to a decrease of vital activity and a blocking of movement.

The theories of Spencer and Bain have been summarized in some detail here because certain salient features of the pleasure-pain theory have proved extremely persistent in later developments:

1. The organism is said to respond to the problem situation by *random* movements and spontaneous discharges of the muscles. Later analyses of trial-and-error responses have been attacked because they seemed to imply that an animal's initial responses in a problem situation are random (148). This would be an extremely hazardous assumption to make, for in a problem situation an animal will rarely respond completely at random; even his trial-and-error responses always have a certain directionality though they may at first appear random from the observer's viewpoint. Thoughtful analysts of trial-and-error learning have recognized this fact and pointed out that the assumption of randomness is not necessary for a trial-and-error theory of learning (240).

2. The causal efficacy of pleasure and pain in fixating successful responses and eliminating unsuccessful ones is explicitly asserted. Pleasure and pain are purely psychic concepts and mechanistically inclined thinkers have been reluctant to endow them with such power over muscular responses (29, 33, 112). Hence the need to invoke physiological correlates of pleasure and pain to reduce their action to mechanistic principles. This need was felt not only by Spencer and Bain but also by later writers continuing the tradition of a pleasure-pain theory of learning (252, 264, 297). Most of these physiological explanations have been highly speculative, as are certainly those advanced by Spencer and Bain. There is, for example, no evidence to support the contention that pleasure is accompanied by heightened neural activity and pain by lessened activity. This may sometimes be the case but the opposite may be equally true, especially in the case of intense pain. The trouble was that a pleasure-pain theory was really a theory of psychic causes leading to physical effects. Physiological explanations have been largely ad hoc and invoked for the sake of consistency with basic axioms about the mechanistic nature of the learning process.[4]

3. The third feature which the pleasure-pain view bequeathed to modern learning theory is its insistence that a repetition of a successful response will

[4] The vexatious issue of hedonism was sidestepped by Hobhouse (110) who described effect in terms of confirmation and inhibition. Similarly Holmes (114) speculated that the congruity or incongruity of an act with the activity in progress constituted its effect. Such formulations, however, only postpone the consideration of the role played by satisfaction and annoyance. As Stephens (257, 258) has pointed out, if an organism unexpectedly stumbles on a valuable outcome, the failure of the response to confirm the expectancy will not prevent the response from recurring. Similarly, a congruous expected response may be eliminated if it leads to unpleasant consequences.

strengthen it. There are many cases where this is true but probably equally many where the opposite is equally true, where continued repetition of a response will weaken that response (59).

4. Let us finally note the central emphasis which a pleasure-pain theory puts on the motor (movement) aspect of the connection between situation and response. It is the movement which is strengthened by pleasure and increased neural activity. Later formulations of the law of effect, especially in conjunction with conditioning principles (127) have placed equal emphasis on the strengthening of afferent-*efferent* connections by reinforcement. Critics have insisted that movements as such are rarely learned and used by animals only as means to an end. It is probably safe to say that changes in the *stimulus as the animal perceives it and interprets it* (3, 29, 30) are just as essential as the fixation of successful movements.

The New Associationism: Thorndike's Laws of Learning

The combination of a modified pleasure-pain (success-failure) philosophy and connectionism found its most systematic and challenging expression in the work of Thorndike. Even though he later disclaimed hedonism (273), Thorndike's thinking has reflected that of the earlier exponents of the pleasure-pain principle as well as the associationist doctrine. His central importance derives from the fact that he embarked on a monumental series of experiments, extending over half a century, to obtain empirical verification of his laws of learning. Partly on the basis of his study of the problem-solving behavior of animals, Thorndike formulated two basic laws of learning: *the law of exercise* and *the law of effect.*

Other things being equal, the *law of exercise* makes learning a function of the number of repetitions of the stimulus-response connections.

The law of effect was stated as follows: "Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connection with the situation weakened so that, when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort the greater the strengthening or weakening of the bond" (264, p. 244). More succinctly expressed, the law as originally stated asserts that success stamps in and failure stamps out.

In the light of careful experimental analysis Thorndike later abandoned the law of exercise, or at least he relegated this law to a very minor position, and for him the law of effect became altogether central to the learning process (267, 269). At various times Thorndike's statements of the law of effect have been accompanied by an exposition of its probable physiological correlates. The physiological locus of reinforcement by effect is put squarely in the neurons (e.g. **264**, **267**, **269**, **270**, **273**). Satisfying and annoying processes are "related to the maintenance and hindrance of the life processes of the neurons" in whose changing synaptic conduction learning consists (**264**, p. 24). In other words, pleasure or satisfaction facilitates synaptic conduction, renders synapses more permeable, whereas discomfort or pain blocks synaptic conduction.

The subsequent history of the law of effect has shown two major foci of interest.

1. The first major focus of interest has been Thorndike's law itself. Ever since its first formulation it has been subjected to a long series of attacks, attacks which were vigorously countered by Thorndike and his associates both on the level of theoretical discussion and in the experimental laboratory. In the course of these discussions Thorndike himself has shifted his theoretical position to some extent and reformulated the law. Concurrently many of the important parameters of reinforcement (such as amount of reward, frequency of reward, delayed reward, spread of effect, etc.) were investigated.
2. A second focus of interest has developed around the integration of the law of effect with the facts and theories of conditioning. In Hull's systematic theory of learning (**127**), the law of effect occupies a central position. On the other hand, the law of effect has been singled out for attack by those who are opposed to a conditioning-reinforcement type of learning theory (e.g., Tolman and his associates: **289**, **294**, **315**).

The law of effect has thus become an issue around which systematic differences in behavior theory have been crystallized. Defense and condemnation of the law of effect have almost become symbolic of widely divergent approaches to the problems of behavior analysis and learning theory. The discussion which follows will be centered around these two foci of interest and will also attempt to cover a representative selection of studies dealing with the parameters of reinforcement.

The Classical Objections to Retroaction

Let us turn first to a consideration of the classical objections to the law of effect. In the language of the law of effect, the consequence of a response strengthens the response, i.e., the effect "works back" on the connection which it follows. Such a formulation involves the assumption of retroaction. Critics asked, how can an effect work upon a response which is already passed (**29**, **33**, **46**, **212**, **219**)? The logical difficulty is obvious, and Thorndike has not been inclined to minimize it. He has suggested that the physiological equivalent of the connection does not vanish instantaneously but is still present when the consequence (reward or punishment, knowledge of results) occurs (**269**, p. 481). The original statement concerning backward action should then be modified to read that one type of after-effect (satisfier or annoyer) acts on other after-effects of a connection (the persisting physiological equivalent of the original connection).

The same logic for bridging the temporal gap between connection and consequence and avoiding the cul-de-sac of retroaction underlies Hull's use of the concept of *stimulus-trace.* Upon cessation of a stimulus (exteroceptive or proprioceptive) the afferent impulse continues its activity for a finite short period, and a short-range temporal integration is achieved. Hull writes, "This perseverative stimulus-trace is biologically important because it brings the effector organ en rapport not only with environmental events which are occurring at the time but with events which occurred in the recent past, a matter frequently critical for survival" (**127**, p. 385). Not only may the physiological correlate of a stimulus or response persevere in time but symbolic processes may help the human learner and, to a lesser extent, the animal learner to bridge the temporal gap between the occurrence of the connection and the action of the satisfier (**192**).

There have been other attempts to slip from under the horns of the dilemma of retroaction. Accepting the apparent fact of backward action at its face value, retroflex circuits in the brain (**297**), neural irradiation (**96**), changes in the electrical resistance of neural connections (**251**) and their readiness to conduct (**252**, **267**), the rearousal of just-active pathways (**44**)—all these were suggested as possible mechanims mediating the action of satisfiers and annoyers. These physiological explanations can be called neither right nor wrong. They are clearly speculative and many steps removed from the level at which experimental verification is at present possible.

The most radical solution of the problem has been to deny retroaction altogether and to affirm that in spite of the appearances to the contrary effect always operates in a forward direction. What is modified is not the connection which is passed and done with but the *stimulus* on the occasion of its next appearance. "The burned child shuns the fire not because pain did anything to his movements, but because, since that pain, the stimulus has changed; it is now flame plus fear, no longer flame plus curiosity" (**112**, p. 218f.). Similarly, Carr ascribed the fixation and elimination of responses primarily to the *sensory consequences* of an act rather than to the strengthening of S-R connections. "These consequences do not influence the portion of the act that preceded them . . . they do affect the subsequent functioning of the act" (**29**, p. 96). Whether or not it is possible to cut the Gordian knot of retroaction by insisting that effects exert their influence in a forward direction, criticisms such as those of Hollingworth and Carr were important because they shifted the emphasis from the *response* to the *stimulus*.

In trying to evaluate the arguments for and against retroaction, one is struck by the fact that the battle has been fought largely on the plane of logic (**312**). There can be no doubt that rewards and punishments following responses modify these responses on the occasion of their recurrence. Not enough is known about the basic mechanisms of learning to decide whether consequences of a response can or cannot act back on a connection or whether perforce they must exert their effect in a forward direction. Operationally speaking, the consequences of a response always act in a forward direction since they can only be tested on the occasion of a future occurrence of the stimulus situation. Physiologically speaking, when all is said and done we had best admit our ignorance of the action of cortical neurones. Perhaps part of the confusion has stemmed from a premature reification and physiologizing of the terms involved. The words *connection* and *effect* are logical constructs, the words *stimulus* and *response* are generic terms denoting classes of events with certain properties in common (**237**). It is only too tempting to forget the limitations of these concepts and to manipulate abstractions as if they were clearly delineated entities with equally unequivocal physiological counterparts. For analytical purposes we need such concepts as *stimulus*, *response*, and *connection*, but are we not over-optimistic when we assume that the nervous system makes its division along the same lines? If misplaced physiological concreteness is avoided one may escape logical difficulties such as the dilemma of retroaction.

What Is the Nature of Satisfiers?

Whereas some critics were most concerned with the mechanisms mediating effect, others focussed their attention on the nature of the satisfiers and annoyers to which reference is made in Thorndike's law. Although Spencer and Bain, in whose tradition Thorndike continued, frankly invoked pleasure and pain as agents responsible for the fixation and elimination of responses, Thorndike's law has been a law of *effect*, not *affect* (**113**). He carefully defined satisfiers and annoyers in terms independent of subjective experience and report. "By a satisfying state of affairs is meant one which the animal does nothing to avoid, often doing such things as to attain and preserve it. By a discomforting state of affairs is meant one which the animal avoids and abandons" (**264**, p. 245). Although admittedly free of hedonism, such a definition of satisfiers and annoyers has faced another serious difficulty: the danger of circularity. The critic may easily reword the definition to read: "The animal does what it does because it does it, and it does not do what it does not do because it does not do it." This *reductio ad absurdum* is probably not entirely fair but it points up the danger of the definition in the absence of an *independent* determination of the nature of satisfiers and annoyers. The satisfying or annoying nature of a state of affairs can usually be determined fully only in the course of a learning experiment and cannot then be invoked as a causal condition of learning without circularity (**108**). In their experimental work Thorndike and his associates have made no significant attempts to establish the satisfying or annoying nature of their rewards and punishments independently of the learning experiment. The tacit assumption has been made throughout that the words "Right" and "Wrong" and/or small money gains and losses are, indeed, satisfying and annoying states of affairs. As Tolman and his co-workers have argued on the basis of strong experimental evidence, such events (including shocks) can equally well be conceived as signals or "emphasizers," providing the subjects with information as to the correct response (**294**). We shall return shortly to the problem of cognitive (informative) vs. satisfying and annoying after-effects of a connection.

Thorndike's definition of satisfiers and annoyers has been defended against the stigma of circularity by emphasizing its empirical adequacy (**171**). "The primary fact is that there is a state of affairs, whatever its psychological classification, which happens as a result of, or at least after, an act. The state of affairs may be symbolic . . . or it may be primarily sensory . . . or it may be complexly perceptual" (**171**, pp. 576–577). This statement is fully in line with Thorndike's own reformulation of the nature of a satisfying state of affairs. In his more recent writings Thorndike has conceptualized the action of satisfiers in terms of the *O.K. reaction* (**273**, **277**), a fundamental confirming reaction brought to bear by the organism on connections between situations and acts. He describes the O.K. reaction as the "unknown reaction of neurones which is aroused by the satisfier and which strengthens connections on which it impinges" (**273**). This reaction is independent of sensory pleasure. It is "far from logical." It strengthens "connections which are wrong, irrelevant, and useless." It is said to bear little relation to the intensity of the satisfier. Neither specific motivation nor effects relevant to a specific motivation are any longer assumed.

Stripped of virtually all defining properties and qualifications, the law does indeed have a very wide range of applicability but only at the expense of vagueness. The sum and substance of the argument now is that something happens in the organism (nervous system) after an act is performed. The fact that something happens influences further action. This something is, however, so little defined that it has almost no predictive efficiency. The O.K. reaction has no measurable properties, the conditions for its occurrence are so general as to embrace almost every conceivable situation. Hence the operation of the O.K. reaction can be inferred only *ex post facto*, after learning has taken place. But here we are impaled again on the horns of the dilemma of circularity.

Perhaps because the reformulated empirical law of effect had a degree of generality which made it an unsatisfactory tool for the prediction of specific facts of learning, several auxiliary principles were gradually introduced in the connectionist framework: the principle of *belongingness*, according to which S-R correlations which "hang together" are learned more easily than connections lacking this quality; the principle of *impressiveness* which states that vivid stimuli are favored in learning; the principle of *polarity* according to which stimulus-response sequences function most readily in the order in which they were practiced; the principle of *identifiability* stating that the most easily identifiable connection is most easily learned; and finally the principle of *availability* which has it that the more available a response the more easy it is to connect it to a stimulus (**227**, **269**). It is easy to see that most of these principles are closely akin to the concepts of Gestalt psychology. According to Brown and Feder (**18**), "these conditioning factors which for Thorndike are so many *ad hoc* hypotheses, are logically prior to his other laws." These writers claim that Thorndike's theory of learning could be successfully rewritten in terms of Gestalt psychology. Whether or not this assertion is true,[5] the use of these auxiliary principles serves to point up the inadequacy of the O.K. reaction as a universal principle of learning.

It is very probable that the modifications in Thorndike's description of the process of reinforcement were largely influenced by the results of his own experimental labors over the years. Most of his experiments were concerned with rote verbal learning and the acquisition of simple skills (**269**), and it is the results of these experiments which were interpreted in terms of the confirming reaction and such auxiliary concepts as belonging and identifiability. Other advocates of the law of effect, especially those whose experimental interests have been more in the fields of conditioning and discrimination learning, have tended to insist

[5] Certainly Thorndike would not agree. He professes his "inability to understand" Gestalt psychology. He feels suspicious of the concepts of Gestalt psychology "because for a time they were offered as a substitute for psycho-vitalism" (**267**, p. 125). He concludes that a connectionist theory is "far simpler and more in accord with what the neurons are and can do" (p. 131).

on a version of the law in which the term *effect* retained much more of its original meaning and had direct reference to the primary and secondary needs of the organism. Let us consider, for example, a recent formal statement by Hull of his *law of primary reinforcement* which, in his own words, is distinctly related to the law of effect. The law reads as follows:

> Whenever an effector activity occurs in temporal contiguity with the afferent impulse, or the perseverative trace of such an impulse, resulting from the impact of a stimulus energy upon a receptor, and this conjunction is closely associated in time with the diminution in the receptor discharge characteristic of a need, there will result an increment to the tendency for that stimulus on subsequent occasions to evoke that reaction (127, p. 80).

Reduction of a need is a critical factor in the reinforcement process, and by *need* Hull means a condition in which any of the commodities necessary for the survival of the organism are lacking or deviate seriously from the optimum (127, p. 17). Disturbances in the "optimal conditions of air, water, food, temperature, intactness of bodily tissue, and so forth" give rise to needs and activities of the organism for the search of survival. It is the highly important adaptive role of the learning process to reinforce those movements which will lead to need reduction and thus help the animal to survive. Hull recognizes, of course, that much learning takes place in the absence of immediate reduction of biological needs. To account for such learning, the principle of secondary reinforcement is invoked. "The power of reinforcement may be transmitted to any stimulus situation by the consistent and repeated association of such stimulus situation with the primary reinforcement which is characteristic of need reduction" (127, p. 97). Thus *all* reinforcement depends directly or indirectly on the reduction of "viscerogenic" needs. Since reinforcement is a basic condition of learning, it follows that learning is primarily an adaptive process in the interest of species survival. Biological needs provide the foundation on which the complex structure of a lifetime of learning is built.

Hull's formulation of the process of reinforcement has been echoed by many other investigators in the field of learning. In a long series of experiments (some of which will be discussed in detail below) O. H. Mowrer, for example, has attempted to produce experimental evidence and theoretical argument for a version of the law of effect which is closely parallel to that of Hull in its emphasis on tension reduction. A typical statement by Mowrer claims that

> . . . learning is dependent not upon the mere association, or temporal contiguity, of stimuli (or responses) but rather upon the occurrence of a state of affairs which has been variously designated as goal-attainment, problem-solution,

pleasure, success, gratification, adjustment, reestablishment of equilibrium, motivation-reduction, consummation, reward (190).

Elsewhere Mowrer makes it unequivocally clear what he means by satisfaction. He writes:

> Most writers agree that satisfaction ought to be equivalent to pleasure and dissatisfaction equivalent to pain. But, then, what do we mean by "pleasure"? One view is that we can equate pleasure to drive or tension-reduction and pain to drive or drive-increase. . . . Satisfaction, pleasure, and drive-reduction are strictly equivalent (185).[6]

Mowrer's interpretation of effect or a satisfying state of affairs is frankly and emphatically hedonistic.[7] Ultimately all behavior modification is mediated by pleasure (tension-reduction). The statement has a very familiar ring. The history of learning theory has completed a cycle, and the views of Spencer, Bain, and Baldwin are with us again though transplanted into the conditioning laboratory and accompanied by great refinement and sophistication in experimental techniques and procedures. The historian of learning will be especially interested to note that while Thorndike, the original advocate of the law of effect, has frequently dissociated himself from a hedonistic interpretation, there has emerged a group of *neo-hedonists* whose interpretation of the law is much closer to its historic roots. They were willing to assume the responsibility of hedonism in order to make the law more useful in the design and interpretation of experiments.

To many psychologists hedonism today is no more acceptable than it was at the turn of the century. A vigorous attack against the preservation of a hedonistic doctrine in the law of effect has been leveled by Allport, who has been especially concerned with the implications of a law of effect for the growth and development of personality. A strict adherence to a hedonistic law of effect would imply that "personality grows through the production of tentative trial-and-error acts, some of which are selected by the Great God Pleasure for establishment, and some by the Great God Pain for banishment" (2, p. 154). Allport believes that the main fallacy of hedonism is the confusion of the *by-product* of a complex process with the process itself. He would agree that pleasure (tension-reduction) at times is an indicator of the successful accomplishment of an act (4) but emphatically denies that pleasure

[6] Similar emphasis on tension-reduction as the basic process of reinforcement is found in the writings of Meunzinger (199) in connection with his work on the effect of electric shock on learning.

[7] There is an important way in which such a view differs from that of the classical hedonists. For the hedonists pleasure was a motive, whereas for the modern learning theorists it is tension, or discomfort, which is the motive while pleasure is the result of tension-reduction which leads to learning (192).

is the main causative factor in the fixation of responses. He also takes issue with the law of effect because it perforce implies a segmental approach to the learner, ascribing learning to the satisfaction of this drive or that drive with the main emphasis on the reinforcement of *responses* (**3**). For Allport it is the person who learns, is rewarded, and utilizes his success in his future adjustemnt. Allport thus pitted an ego-oriented psychology of learning against the neo-hedonism of the law of effect. To this controversy we shall return presently.

In attempting to evaluate the controversy which has raged around the definition of satisfiers one is struck by the key importance of the hedonistic issue. Certainly hedonism is an immediate ancestor of the law, and now that the principle of effect has reached an uneasy maturity it is clear that it cannot deny its origin without sacrificing much of its vigor. When the law is stripped of hedonistic implications, when effect is not identified with tension-reduction or pleasure (as by Thorndike), the law of effect can do no more than claim that the state of affairs resulting from a response in some way influences future responses. Such a statement is a truism and hardly lends itself to the rigorous deduction of hypotheses and experimental tests. If a neo-hedonistic position is frankly assumed (as, e.g., by Mowrer) the law becomes an important tool for research, provided "satisfaction" is independently defined and not merely inferred from the fact that learning has occurred. The hedonistic position can be defended for the range of events for which it is experimentally demonstrable. It is probably fair to say that up to the present this range is fairly narrow and largely in the domain of animal learning.

The main source of difficulty has been the universality claimed for tension-reduction as a principle of reinforcement. Hull and Mowrer, on the one hand, and Allport, on the other, probably would agree that reduction of the food need is a critical factor in a rat's learning the true path through a maze. With the aid of the principle of secondary (and higher order) reinforcement, tension-reduction can then be made the ultimate condition of all learning. Such a generalization Allport rejects. Crucial experimental tests to arbitrate between these two interpretations of more complex forms of learning still seem to be lacking. For the time being, therefore, acceptance or rejection of the hedonistic postulate must remain a matter of philosophical preference.

The Nature of Punishment

Thorndike's View of the Effect of Punishment

The preceding discussion has dealt primarily with the nature and action of satisfaction (reward). As originally stated, the law of effect was a two-pronged statement, putting the stamping-out powers of punishment on a par with the stamping-in effects of reward. In the light of experimental results Thorndike no longer considers punishment as an effective agent for the elimination of wrong responses. Indeed, again as a result of experimental evidence, a punished connection is now held to be strengthened more by sheer occurrence than it is weakened by punishment. Punishment is not the dynamic opposite of reward; it is not even capable of overcoming the effect of sheer occurrence (269, 279). This view is, of course, an implicit resurrection of the law of exercise since sheer occurrence (frequency) is now credited with a certain degree of effectiveness in fixating responses, an effectiveness, moreover, which surpasses that of punishment.

An impressive number of experimental investigations have been conducted by Thorndike and his associates to demonstrate the ineffectiveness of punishment (162, 163, 165, 166, 223, 268, 269, 276, 299). In most of these experiments the punishment consisted of the announcement *Wrong* made by the experimenter, accompanied sometimes by a small fine or an electric shock. The effect of the punishment was assessed in terms of the number of repetitions of the punished response as compared to the number of repetitions that would be expected on the basis of chance, i.e., if the subject were guessing at random. The results of the experiments led Thorndike to conclude that punishment, instead of weakening or "stamping-out" the wrong response, may have a variety of effects depending on the specific nature of the annoyer and the propensities of the organism. The important point is that what an animal is led directly to do by an annoyer may or may not make the repetition of the punished response less likely (269). If punishment does lead to the elimination of a response, its action is *indirect*: it leads to variability of behavior, thus increasing the opportunities for the occurrence of the correct response which is then reinforced by the direct action of the satisfier (OK reaction).

Thorndike's revised view of punishment startled the psychological public. Not only did it contradict the belief in the practical value of punishment which had become almost an axiom in our social life, it was also clearly at variance with a considerable body of other experimental data. We shall now consider (1) the criticisms leveled against Thorndike's work on the effect of punishment, and (2) other theoretical and experimental developments bearing on the role of punishment in learning.

Criticisms of Thorndike's View of The Effect of Punishment

One serious criticism of Thorndike's data was statistical in nature.

The effect of punishment was measured in terms of the deviation of the obtained number of repetitions from the number of repetitions to be expected by chance. Thus, if there were five possible responses to an item, Thorndike would consider the chance expectation of each response to be .20, and the obtained percentages were expressed as deviations from .20. Thorndike was thus applying the principle of indifference which considers alternative events equally likely in the absence of information to the contrary. The trouble is that there seems to be some information to the contrary, viz., that the "natural choice frequency is usually not a chance one" (**123**). Whether or not Thorndike's correction for chance is adequate is an empirical question. It is necessary to compare the extent of actual repetition without after-effect with the extent of repetition following announcement of *Wrong*. Experimenters who used an empirical rather than an a priori baseline for the evaluation of the effects of *Wrong* find that punishment does have a weakening influence which is commensurate with the strengthening influence of reward (**254**, **255**, **283**). Thorndike and his associatesfail to find such an effect even when favoritism of response is taken into account (**162**, **166**).

Failing to find a significant weakening of connections following the announcement of *Wrong*, Thorndike denied the efficacy of punishment in general.[8] Yet even within the restricted framework of a typical Thorndike experiment, and with as highly specific a "punishment" as the announcement of *Wrong*, the results vary considerably with the parameters of the experimental situation. The medium by which the punishment is conveyed is important: it may make a difference whether *Wrong* is announced by spoken word or by, say, a signal light. The effect of the medium, the sheer "something happening" after a response may be powerful enough to obscure the weakening influence of punishment. Such, at least, was the conclusion of Stephens who argued that whenever punishment strengthens a bond, such strengthening may be due to the physical medium, whereas reward and punishment have directly opposite effects when measured from the baseline of informationless after-effects (a flash of light, a nonsense syllable) (**255**).[9] Jones, (**139**) on

[8] While the majority of experiments were done with human subjects, Thorndike illustrated the superiority of reward also with animal subjects (**268**).

[9] Thorndike and his associates also found that informationless after-effects such as a click led to more frequent repetitions than the announcement of *Wrong*. They were unwilling to conclude, however, that punishment had weakened a response and argued that a "neutral" after-effect was ambiguous enough to lead to self-administered reward since the subjects were free to interpret the neutral signal as a reward (**165**, **166**). Such an argument comes dangerously close to question-begging. It indicates an unwillingness even to consider the possibility that punishment may have a weakening effect on a connection.

the other hand, found that the medium by which punishment is conveyed makes little difference and that the elimination of errors depends on the *position* of the punished response in the pattern to be learned.

Whatever the effect of the medium by which the punishment is conveyed, the initial strength of the association will be an important determinant of the effect. A strong association can be considerably weakened, a weak association which is close to zero cannot be interfered with much further. Stephens was able to show experimentally that *strong* connections (responses which appeared early and were persistent) are markedly and dependably weakened by the announcement of *Wrong* whereas initially weak ones are not. Correspondingly, *Right* has little, if any, influence on initially strong associations but considerably aids in making weak bonds strong (254, 256). Tilton (284, 285) also emphasizes the importance of taking the initial strength of a response into account.

There is another feature of the experimental situation which Thorndike fails to take into account but which may be, at least in part, responsible for his failure to find a direct effect of punishment. In most of the experiments, there was only *one* right response and several wrong alternatives. Thus it was the subject's task to learn positively only one item, but to eliminate several items. By virtue of its uniqueness, the right response is "figural" (has, as it were, high visibility for the subject), whereas a wrong response is one in a long homogeneous series of wrong responses (213). These considerations receive experimental support from the work of Dand (45) who equated the number of right and wrong alternatives and presently found an announcement of *Wrong* to have a definite weakening effect. Lorge (162) reports that the potency of a punishment is less the higher the initial probability of obtaining a right response (though not reliably so). It is safe to say that the pattern and sequence of *Rights* and *Wrongs* are at least partial determinants of the fixation and elimination of responses.

The complex ways in which the effects of *Wrong* vary with the parameters of the experimental situation highlight the need for caution in generalizing about the effects of punishment. Even when the punishment consists of a simple announcement of *Wrong*, the experimental evidence is still inconclusive. An announcement of *Wrong* is only a very special type of punishment situation. Indeed, one may question the appropriateness of the term *punishment* or *annoyer* for the description of an effect which is primarily informative in nature. In most of Thorndike's situations, the connections to be learned are purely arbitrary and it is easily obvious to the subject that being right or wrong cannot possibly reflect on his intelligence. He may be neither gratified by being right nor annoyed by being wrong. *Wrong* may merely serve as a signal to substitute one arbitrary guess for another. We must reiterate the need to define satisfaction and annoyance independently before invoking them as determinants of the learning process.

Other Studies of the Role of Punishment

When we turn to punishments other than an announcement of *Wrong* or slight monetary losses, the uncertainty of Thorndike's generalization becomes even more apparent. An impressive array of experimental evidence can be marshalled in support of the proposition that punishment is an effective condition of learning. We shall first review the empirical findings and shall then consider the main theoretical issues concerning the nature of punishment and the mechanisms of its action.

Society has devised many and varied punishments for those who offend against law and convention but in the psychological laboratory punishment has come to be almost synonymous with electric shock. Shock is easily administered and it is always sure to have a physiological impact though its psychological effects are highly variable (**296**). Thus most of the data on punishment come from studies on electric shock.

A review of the literature on punishment raises the following problems:

1. Does punishment have any significant effect on learning?
2. Is the effect of punishment generalized to the entire learning situation or does it lead only to the elimination of specific punished responses?
3. What is the mechanism by which punishment exerts its effects? Does punishment weaken stimulus-response bonds? Does it affect the general level at which the organism functions? Or is its effect indirect, leading to increased variability of behavior and ultimately to differential reinforcement by reward?
4. Does the main effectiveness of punishment reside in the *information* about errors with which it provides the learner or does it have a more immediate influence on associative strength?

Does punishment have any significant effect on learning? We have already seen that Thorndike answers this question in the negative but that the validity of this conclusion from his data is doubtful. The answer to this question is, trite as it may sound, yes and no. It is rather unfortunate that Thorndike and his associates have tended to put the issue on an all-or-none basis. The question should not be, "does punishment have an effect on learning or does it not?" but rather, "under what conditions is it effective and under what conditions does it fail to show results?"

Against the blanket assertion that punishment is not instrumental in the elimination of wrong responses it is possible to cite a long list of papers covering more than half a century of experimental work which report that punishment is an effective condition of learning.

Without attempting to review these experiments in detail, we shall summarize the main findings.

1. A large number of different organisms have been spurred on to better learning by punishment:[10] earthworms (325), cockroaches (261, 300), dancing mice (324), rats (e.g., 20, 54, 56, 57, 111, 116, 155, 193, 196, 199, 200, 201, 202, 204, 206, 301, 307, 322), chicks (36), cats (53), and, last but not least, human beings (10, 12, 13, 19, 24, 25, 26, 27, 41, 55, 79, 80, 87, 137, 138, 172, 173, 217, 218, 302).

2. Punishment, usually by electric shock, has improved learning in a variety of problem situations: discrimination boxes, mazes, reaction-time procedures, serial learning, mirror tracing, multiple choice situations.

3. Punishment has been found to affect different measures of learning. In many studies, in which responses are scored as either right or wrong, punishment has led to a decrease in the number of errors (10, 13, 24, 25, 26, 27, 36, 41, 54, 55, 56, 57, 79, 80, 87, 111, 116, 137, 138, 172, 173, 193, 196, 199, 200, 201, 202, 204, 206, 218, 301, 302, 307, 326). When speed of learning is measured in terms of the number of trials required to reach criterion, we find that punishment often results in a smaller number of trials (19, 24, 25, 26, 27, 36, 41, 54, 55, 111, 137, 155, 171, 193, 196, 199, 201, 202, 204, 206). Some investigators report the fact that punishment reduces the time required for learning (24, 25, 41, 55, 79, 80, 87, 206, 302) but time has proved to be a difficult measure to interpret since punishment typically leads to a cautious hesitant attitude on the part of the learner so that a decrease in the number of trials and errors may be accompanied by an increase in time per trial (10, 24, 25, 26, 172, 173, 206, 218).

4. The effectiveness of punishment may also be demonstrated by retention tests. There are experimental reports suggesting that (1) punished groups remember the learned task better (27, 41, 56, 79, 80, 81, 206, 301), (2) punished groups are less subject to retroactive inhibition (27). Different measures of retention show the effect of punishment to unequal degrees, however (41, 81, 301).

On the basis of the experimental evidence it is thus possible to assert at least that punishment works in some situations some of the time. But note the qualifier *some*. None of the papers cited would justify the conclusion that punishment works in all situations all of the time. On the contrary, the experimental evidence emphasizes the extent to which the *effectiveness or non-effectiveness of punishment depends on the parameters of the experimental situation.*

1. The intensity of the punishment is important. As Yerkes and Dodson (326) pointed out more than forty years ago, and many investigators since have confirmed, there is in a given learning situation an *optimal* intensity of punishment. The relationship between intensity of punishment and learning efficiency is not linear over the total range of intensities. Once the optimal intensity has

[10] Conditioning studies employing a punishing UcS are not included here.

been exceeded, efficiency of learning may decrease (36, 53, 87, 302, 326, 328). There is moreover, no one optimal intensity for any given organism. The optimal intensity will vary significantly with (1) the difficulty of the task and (2) the stage of learning at which punishment is applied.

2. There has been rather general agreement that relatively severe punishment (intensive shock) is most effective in the learning of simple habits such as black-white discrimination and simple maze patterns, and that relatively mild punishment is optimal in the case of difficult tasks such as more complex types of discrimination and exacting maze patterns (12, 26, 36, 53, 326, 328).

3. The effects of shock may become disruptive (12, 79, 80, 87, 294, 303), especially if applied early in the learning process (25, 206, 218). Shock may also lead to increased variability of responses as compared with milder forms of punishment (260).

4. As to the stage of learning at which punishment is most profitably introduced, we have already referred to Stephens' general conclusion that the greatest effect is achieved with strong connections (254, 256). This conclusion is borne out by the findings of Valentine (301) and by a systematic investigation of Bunch (25) who got better results with a few shocks late in the learning than with many more administered earlier in the process. The results of his experiments led Bunch to believe that for a given task there is an *optimal combination of position in learning and number of trials with shock.* This generalization clearly reflects the dependence of the effect of punishment on the experimental parameters.

Any generalization about punishment based on a single experiment, or even a series of experiments, thus becomes extremely hazardous. For a given organism, there are combinations of conditions under which punishment is effective. If the experimenter works outside this range of combinations, punishment fails to yield significant results (Thorndike and co-workers). If he works with several combinations of conditions, he may easily find that punishment is effective in one case but not in the other and certainly that it varies in effectiveness (12, 26, 87). If he works with only one set of parameters, he may be tempted to over-generalize. Such seems to have been the case in Thorndike's experiments on the effect of *Wrong* since most of the connections subjected to punishment were probably rather weak (254), and the punishment hardly very intensive in nature. Such lack of consistency in results need not lead to a counsel of despair regarding the possibility of generalization about punishment, but merely to an insistence that any such generalization should be based on functional relations covering a wide range of parameters and not on isolated, restricted experiments.

A special problem of control needs to be mentioned here. In an attempt to hold as many of the experimental conditions as possible "constant," the experimenter may decide on a certain type and intensity of punishment and then adhere to it throughout the course of the experiment. He would then overlook the fact that the effectiveness of a punishment changes as a function of the number of times that it has been administered. In the case of shock, there are clear-cut experimental data to substantiate this point. Investigating electric shock as a motivating stimulus in conditioning experiments, Kellogg (**143**) found that in order to maintain a flexion reflex of *fixed extent* it is necessary to administer relatively large voltages at the beginning of the series and to reduce the stimulus intensity toward the end of the series. He also found that the intensity required varied with the distribution of practice. In addition, Kellogg reported wide individual differences among his subjects (dogs) in sensitivity and emotional reaction to the same physical shock. He concluded:

> It seems reasonable to infer that in learning experiments where the physical properties of an electrical stimulus are constant, the effects produced in the same subject at different times must differ greatly. The attempt to maintain "constant motivation" by this method probably defeats its own purpose (**143**, p. 95).

Considerable individual differences in reactions to shock have also been reported for human subjects (**296**). Too few experimenters have heeded Kellogg's admonition to consider the effectiveness of a shock at a given point in time rather than its physical intensity.

Is the effect of punishment general or specific? In the framework of connectionism, it is always a *specific* bond which is strengthened or weakened. Thus Thorndike and his co-workers have always concerned themselves with the history of *specific* rewarded and punished responses, finding punishment ineffective. Other investigators have found, however, that punishment may have a much more general effect: there may be a general improvement in performance, with unpunished responses benefiting along with the specifically punished ones (**10**, **12**). Even if the punishment itself is made non-specific ("non-informative shock"), and not administered as a consequence of any particular response, there is still a facilitating effect (**11**, **12**, **19**, **25**, **79**, **80**). The work of Muenzinger is of especial importance here. In a series of carefully controlled experiments he was able to show that (1) shock administered to rats *after* the choice-point in a maze facilitates learning and is more effective than hunger-motivation alone (**193**, **196**); (2) shock *before* the choice-point decreases the efficiency of learning[11] (**204**) as does shock at the moment of choice (**71**); and (3) if shock is administered throughout the maze, its effect is only slightly less than with punishment *after* the choice point (**199**). These findings apply to shock for *right* responses as well as shock for *wrong* responses. When both right and wrong responses are shocked, there is no summation of effects though learning efficiency is greater than if either right or wrong choices alone are followed by shock (**204**).

[11] This retardation is probably due to the persistence of position habits which become fixated (**70**, **101**).

The question asked at the beginning of this section, whether the effect of punishment is general or specific, turns out to be an unwise one. Clearly, the effect may be general, specific, or both, depending on the way the experiment is arranged and the measurements are made. In a conditioning experiment, for example, in which punishment is employed for the purpose of establishing a discrimination, the effect is of necessity highly specific. A response to, say, one tonal frequency is reinforced, and the responses to other frequencies are weakened or eliminated. It would be obviously nonsensical to expect unpunished responses to be affected in the same way as punished ones. At the other extreme of the continuum of specificity-generality is a situation in which punishment is administered without reference to any particular response, say, after a block of trials. In that case, the only possible effect that punishment *can* have is general, inducing an attitude of caution, serving as an incentive, etc. The general conclusion on the generality-specificity issue is that punishment has both general and specific effects, sometimes one, sometimes the other, sometimes both, depending on its role in the total experimental situation.

What is the mechanism by which punishment exerts its influence? There has been considerable speculation about the events in the organism which mediate the effect of punishment. This is, of course, an example of the search for intervening variables. Punishment is the independent variable, the effects of punishment (positive and negative) are the dependent variables. In terms of what hypothetical events can the relationship between these two variables be conceptualized? The answers which investigators have suggested fall into three broad classes: (1) mechanisms of action which are held to be specific to punishment, (2) explanations which reduce whatever effect punishment may have to the ultimate action of rewards, and (3) explanations which ascribe the effectiveness of punishment to the information or perceptual emphasis which it provides.

1. *Mechanisms specific to punishment.* Throughout his writings on punishment Thorndike has referred to the weakening or stamping out of stimulus-response bonds. Such weakening effects as he has found were ascribed to processes detrimental to the conductivity of the neurones. For the last fifteen years, however, he has argued against significant effects of punishment and therefore has not further concerned himself with a mechanism to account for the weakening of specific S-R bonds by punishment, putting the entire burden of explaining learning on the action of rewards.

An important suggestion as to the mechanism by which punishment affects the emission of specific responses comes from students of operant conditioning (68, 237). Their experiments deal with the effects of negative reinforcement (punishment) on the lever pressing behavior of rats. The general conclusion is that punishment results in a *suppression* rather than *weakening* of a response. Administration of punishment temporarily depresses the rate at which responses are emitted, but the suppressed behavior will be released when circumstances are more favorable. In terms of Skinner's concept of reserve, punishment affects the relation between reserve and rate of emission of responses but has no effect upon the reserve itself. The effect of punishment is therefore emotional, leading to a temporary depression of function but not to a permanent weakening or elimination of responses. These findings not only serve to emphasize the differential effects of reward and punishment on the emission of responses but also convey an important methodological warning: the strength of a response at the end of a period of punishment cannot be used as a reliable index of the true long-range strength of the response (68). Punishment is not a mechanical stamping-out process. Its effect must be considered in relation to the experimental situation.

The *emotional* effect of punishment is also emphasized in Guthrie's attempt to account for the elimination of wrong responses (90). For Guthrie, the fundamental condition of all learning is conditioning by contiguity in time. It is *movements*, not acts, which are conditioned to stimulus cues. The more aroused the organism, i.e. the greater the emotional excitement, the more varied and intense the movements which occur. These varied and intense movements produce a correspondingly high degree of proprioceptive stimulation. Proprioceptive stimuli are important cues to which responses are conditioned. Hence punishment facilitates conditioning (of withdrawal responses, for example) not because punishment is annoying but because punishment arouses the organism and provides a multiplicity of cues for the establishment of conditioned responses. Guthrie insists that the action of punishment cannot be in any way clarified by referring to the action of annoyers. Suppose we define annoyance as a state of affairs which the animal avoids or changes. "But this ability to avoid is just what is necessary to explain unless we assume that learning has already taken place. If annoyance means avoidance, we had to learn to be annoyed at annoyers (by emotional reinforcement). It is what the punishment makes an organism do that counts not what it makes him feel!" (90, p. 14).

From what may be called stimulus and/or response explanations of punishment we turn now to explanations which put the main emphasis on the changes wrought by punishment in the subject's approach to the learning situation and his general manner of performance. We are dealing here with qualitative descriptions of performance under punishment. Several investigators have stressed the attitude of *caution* which the subject assumes when he is punished for incorrect responses. The learner is circumspect, he steps warily (10, 24, 25, 26, 172, 173, 218). Muenzinger has found that a punishment or obstacle makes the subject *pause* before he proceeds with his response (196, 200, 202, 203). During this pause he is exposed to relevant stimulus cues. In this regard shock and an enforced delay (200, 203) or an obstacle such as a gap (202) after the choice-point are equivalent. According to Honzik and Tolman (116) a state of heightened vigilance may make this increased exposure to stimulus cues especially effective. Thus punishment is described as making the organism more sensitive and vigilant, and as enhancing the animal's opportunity to learn about the situation and indulge in successful vicarious trial-and-error. Similar qualitative observations describe punishment as raising the general incentive level (57, 79, 80).

2. *Explanation of effective punishment in terms of reward.* When Thorndike jettisoned punishment as a condition of learning, he left open the possibility that it may have an indirect effectiveness by speeding up the occurrence of acts

which are rewarded. Thus *punishment may be a condition for the eventual application of reward* and in this way a determinant of learning, though one step removed from the locus of the critical event. Hull's theory of primary reinforcement which makes all learning contingent on need-reduction clearly implies such an approach to punishment. The reduction of the effect of punishment to the action of reward has been made explicit in the writings of Mowrer (**181**, **183**, **184**, **185**, **186**, **188**). According to the conception which Mowrer presents, *all motives* involve tension or discomfort. Thus there is discomfort or tension due to hunger just as much as there is discomfort or tension due to shock. In this sense, all learning, dependent as it is on motivation, involves punishment, the punishment inherent in the motivating tension. Given the motivation (tension), learning occurs when the tension is reduced. Tension may be reduced either through the occurrence of a consummatory response (e.g., reduction of hunger by feeding) or through *escape from punishment* (e.g., escape from shock). In Mowrer's own words, " . . . it is therefore meaningless to say that one type of learning is 'through reward' and another type is 'through punishment'; each is an essential aspect of a single dynamic process" (**184**, p. 424). Similarly Muenzinger (**199**) argues that it is "poor logic and bad science" to compare learning by reward and learning by punishment. Without a primary state of imbalance and its subsequent reduction there can be no learning. Punishment is merely a way of increasing this imbalance or tension in order to make the subsequent need reduction more effective as a reinforcing state of affairs.

Such a conceptualization assigns to punishment a much more important systematic role in learning than does Thorndike's. For Thorndike, the effects of punishment are variable and unpredictable. Punishment has no vital place in his picture of the learning process. In the view represented by Mowrer and Muenzinger, on the other hand, punishment is at the very fountainhead of learning. For punishment induces tension, and without tension the behavior which leads to learning by reinforcement would not occur. Thus the law of effect again becomes a law of reward and punishment. It has, however, undergone a crucial transformation. Formerly *either* reward *or* punishment was believed to be a determinant of learning, and punishment was, as it were, reward with a negative sign attached to it. The modern version is a law of reward *after* punishment, for punishment provides the stimulus which makes a reward a reward. Both reward and punishment are integral parts of the learning situation, provided they occur in the proper sequence and are functionally connected with each other. This analysis of the role of punishment, which Mowrer has defended on the basis of his experimental work, has all the beauty and all the dangers of simplicity. Here is an integrated, coherent account of learning, *granted the basic assumption that learning cannot occur without initial tension and subsequent tension-reduction.* It is precisely the granting of this assumption which is one of the controversies with which modern learning theory abounds. Thus we have come in our discussion to the same parting of the ways to which our discussion of reward has led us. In which direction progress lies depends on whether or not tension and tension-reduction can stand up as necessary (though not sufficient) conditions of learning.

3. *Punishment or emphasis?* In general, an individual is subjected to punishment only when he has committed a wrong. In learning experiments, the same procedure has usually been followed. The subject is punished when he commits an error. Sometimes it follows unequivocally from the nature of the situation what

constitutes an error (e.g., blind alley in a maze), at other times the experimenter decides arbitrarily which responses are to be considered right and which ones wrong (e.g., in learning word-number combinations in a Thorndikian experiment). Thus errors and punishment have been associated in investigations of the law of effect until Tolman, Hall and Bretnall (294) decided to explore the effect of punishment for *right* as well as for wrong responses. If punishment does, indeed, weaken associative connections, then punishment for correct responses should slow up the learning process, whereas punishment for wrong responses should result in faster learning. These expectations were not confirmed. Tolman, Hall, and Bretnall found that (1) adding a shock to an auditory signal for right responses did not slow up learning significantly and (2) adding a shock to an auditory signal for wrong responses did not speed up learning but on the contrary slowed it down. Reinforcement of right responses was always more effective than reinforcement of wrong responses. These findings were so strikingly at variance with what the traditional law of effect would have predicted that the article reporting these findings was entitled "A disproof of the law of effect."[12]

Tolman, Hall, and Bretnall believed that the reinforcing stimuli—both auditory signal and shock—do not stamp in or stamp out responses but rather serve to *emphasize* (perceptually or cognitively) the reinforced responses. Moreover, an emphasis upon correct responses favors learning whereas emphasis on wrong responses does not. Any relatively violent emotional stimulus may in addition have disruptive effects which will counteract the favorable influence of emphasis. Other experimenters took up the idea of administering punishment for right as well as wrong responses. The work of Muenzinger has already been referred to. In the first of his series of experiments he found that punishment for right responses and punishment for wrong responses were equally effective (193). In a subsequent repetition of his experiment he found shock for wrong responses inferior (196). In one experimental variation, shock was administered for correct choice, incorrect choice, and at the time of feeding. All three conditions accelerate learning significantly (57). As in other studies of punishment the intensity of the shock relative to the task is a crucial parameter (87).

The experiment of Tolman, Hall, and Bretnall was of considerable systematic significance because it shifted attention from punishment as an automatic mechanism to punishment as a perceptual event. The learning task may be regarded as a problem which the learner tries to solve. Anything which helps the learner to distinguish correct from incorrect responses facilitates learning. A punishment may merely serve to designate a given response as either right or wrong at the pleasure of the experimenter.

Information Versus Effect

The efficacy of punishment for right as well as wrong responses is closely linked to a general problem which has been repeatedly raised in

[12] Goodenough (84) criticized Tolman's findings on statistical grounds but later experiments have tended to confirm Tolman's results (119, 214, 234).

discussions of the law of effect. Can the efficacy of rewards and punishments be explained in terms of the *information* which they convey to the subject about the correctness or incorrectness of responses? If the effect of reinforcement were always mediated by information, it would not be possible to speak of a direct effect of satisfaction (need-reduction) or annoyance on stimulus-response connections. Rather, the hypothetical sequence of events in the organism would then become: stimulus-response-reward or punishment-information-strengthening (or weakening) of the association. In such a description, the mechanism by which information leads to the strengthening of associative connections would be left open. One could, like Tolman, think of the formation of sign-gestalt expectations (288, 291) or assert that information leads to tension-reduction and that learning through information is only a special case of the law of effect. The problem of effect versus information has not always been posed in all-or-none terms. Granting that both factors may be operative, investigators have been interested in gauging their *relative* importance.

Information is a concept which does not easily lend itself to experimental ayalysis. There is always the vexing possibility that what the experimenter considers to be mere information may act as reward, and conversely that what the experimenter believes to be a reward (need reduction) may serve as a source of information. There may, moreover, always be self-administered information (229). Nevertheless, experimental situations have been designed which at least made it possible to speculate about the role of information in the learning process. These experiments have been concerned with (1) clearly non-informative after-effects, (2) the question whether a learner need necessarily be aware of what he is learning, (3) the relevance of after-effects to the connections which they strengthen.

Non-informative after-effects. In our discussion of punishment experiments were cited which show that punishment administered without reference to a specific response facilitates learning. Similarly, rewards given after a block of trials and not following a particular correct response have been found effective (60, 281). In general, however, effects which give specific information have been found superior to non-informative ones (60, 224, 298).

The study of "neutral" after-effects has a direct bearing on the problem of effect versus information. An after-effect is "neutral" if it neither rewards, punishes, nor informs. Nonsense words, meaningful words, flashes of light, and clicks have been used as after-effects which merely happen after an S-R connection, presumably without telling the subject anything about the correctness of his response. Several investigators have found that such after-effects strengthen responses to some extent (165, 166, 255). The very ambiguity of such after-effects has made it possible to interpret them in different ways. Thorndike and his co-workers believe that ambiguous after-effects *may* be interpreted as rewarding by the subject and hence function as rewards (165, 166). Stephens (255), on the other hand, suggested that the mere fact that *something* happens after a response, that the connection is "attended to" facilitates learning. Something happening after a response may, however, be interpreted by the subject as conveying information. "Neutral" after-effects have repeatedly failed to influence learning when the factor of information was carefully controlled (37, 38, 298).

It seems that the use of neutral after-effects is a highly unsatisfactory procedure. The fact that an experimenter considers an after-effect neutral does not, of course, guarantee that the subject interprets the event in the same way. It seems puzzling that some experimenters first call an effect neutral, then turn the tables on themselves and assert that it was not neutral after all but rewarding. If it is neutral after-effects that we want to study, we should make sure that the effects are indeed neutral and then consider them neutral throughout. On the other hand, if we are interested in the effect of rewards, the study of ambiguously neutral after-effects provides a very devious avenue of approach to the problem.

Awareness of what is being learned. If learning by reward can take place while the learner is not aware of what he is learning, then certainly effect and information could not be considered coextensive. Thorndike repeatedly claimed that rewards are effective even if there is little or no opportunity for inner rehearsal of the right responses (266, 269, 275), though he has been contradicted on that point (37, 298). Thorndike and Rock (282) then offered what they consider conclusive proof that learning without awareness of what is being learned can, indeed, take place.[13] In their experiment, learning depended on the *discovery of a principle* by the subjects. Yet there was only gradual improvement under reward and no sudden increase in successful responses. It seemed that subjects achieved insight without knowing that they did. The interpretation of this experiment depends entirely on the assumption that gradual improvement indicates lack of awareness. It turned out, however, that even subjects who are *explicitly taught* the principle on which their learning depends still may show gradual improvement (131). The point here is that understanding a principle and using it are two different things. Slow improvement may reflect lack of insight but also may be due to the learner's inability to translate his understanding into smooth, efficient action. This attempt to prove learning without awareness must be considered doubtful. On the other hand, positive evidence

[13] For other experiments demonstrating that subjects can learn without being aware of what they learn, see Thorndike (269, 276).

has been offered for the importance of awareness (104) and knowledge of results (7, 82, 140, 225) for successful learning. Whether or not learning without awareness of what is being learned can take place must remain an open issue.

The role of relevance. A satisfying after-effect, according to Thorndike, does not act logically but in a mechanical way. It is likened to a natural force applied to do work and need not, therefore, be *relevant* to the activity of the organism at the time at which the reward is given. Suppose a subject is engaged in activity directed toward goal A. In the course of his trial-and-error behavior he is given a reward which leads not to goal A but to an irrelevant goal B. Nevertheless the response which led to the reward is reinforced. Such findings have been reported for both human and animal subjects (164, 271, 274, 276), casting doubt on the theory that rewards function primarily as sources of information relevant to the learning task. How conclusive are such experiments on the role of relevance? Relevance and irrelevance are terms which refer to the *experimenter's* interpretation of the situation. The subject's interpretation and the wants under which he operates are an altogether different matter. The only conclusion which can be drawn from such experiments is that responses which occur in temporal proximity to a reinforcing state of affairs are strengthened (Hull). Relevance is a normative term reflecting the experimenter's judgment.

Even if we agree that relevance is a useful dimension for the description of reward, the evidence on the role of relevance remains inconclusive. There are strong indications that, at least in some experimental situations, rewards irrelevant to the subject's motivation fail to facilitate learning. The work of Wallach and Henle (305, 306) is a case in point. In a typical Thorndike situation (learning word-number combinations) the subjects were informed that they were participating in an experiment on extra-sensory perception and that responses called right on a given trial might or might not prove correct on subsequent occasions. These instructions rendered the rewards irrelevant to the subject's task—guessing numbers by ESP. As a result, rewarded responses were not repeated more frequently than punished ones, nor did these experimenters obtain a level of repetition of wrong responses at all comparable to that reported by Thorndike. Wallach and Henle conclude that "with the learning motive eliminated no automatic effect of reward seems demonstrable." Their argument is further strengthened by the fact that a change in instructions, telling the subject that responses would no longer vary in a random fashion caused a highly significant increase in the number of rewarded responses that were repeated.

The results reported by Wallach and Henle strongly suggest that rewards may not act as illogically and blindly as Thorndike's findings had indicated. The subject's *attitude* toward the learning situation needs to be taken into account. In the absence of explicit instructions not to learn, subjects will by and large instruct themselves to learn and utilize whatever cues the situation provides. A reward, even an irrelevant reward, is such a cue because a rewarded response is different from the majority of responses which remain unrewarded. Thus there is a tendency to follow rewards and to consider them relevant if not dissuaded by instructions to the contrary. Wallach and Henle have clearly shown that a change in attitude can resist the "mechanical" impact of a reward. Investigators of human conditioning have long recognized the importance of subtle attitudinal factors (**108**). Attitudinal factors are probably of equal importance in the study of S-R connections by Thorndike's technique.

The issue of information *versus* effect remains unsettled. The difficulty is that it has not been possible to prove conclusively that rewards do not yield information even when experimenters hope and believe that they do not. An experimenter provides what he considers a non-informative reward but the subject may extract information from it, especially if he is a subject capable of symbolization.

The Spread of Effect

There is one pehnomenon which more than any other has bolstered the view that rewards act mechanically and blindly. This phenomenon is the spread of effect. Again the pioneer investigation was carried out by Thorndike. In 1933 he published evidence that a reward strengthens not only the connection which it directly follows and to which it belongs but also the connections which *precede and follow* the rewarded response (**270**). The closer in the series an item is to the rewarded connection the more it benefits from this spread of effect. Thus there is a double (*before* and *after*) gradient of effect, with the items preceding the reward showing somewhat less strengthening than those following the reward. Degree of spread depends primarily on the number of serial steps separating an item from the reward and not so much on sheer temporal proximity.

Thorndike considered this finding an independent proof of the law of effect (**272**). The discovery of the spread phenomenon gave Thorndike new confidence not only on backward action but also in the mechanical action of satisfiers. Again in his own words:

> The satisfier acts (upon a neighboring punished connection) unconsciously and directly, much as sunlight acts upon plants, or an electric current upon a neighboring current, or the earth upon the moon. From a satisfier issues a strengthening force which the connections absorb. Which of them will absorb it depends on the laws of nature not of logic and teleology (**270**, p. 48).

As to the particular mechanism responsible for the spread, Thorndike entertained two hypotheses: (1) *the scatter hypothesis:* the strengthening effect, being not logical but a biological force will sometimes miss its mark, striking preceding and succeeding connections. The gradient thus represents the decreasing probability of chance errors in the action of the satisfier. (2) The *spread hypothesis:* the confirmatory reaction caused by the reward may be diffuse, spreading out its influence over a range of items.

Thorndike's findings have been repeatedly confirmed with both human and animal subjects (**15**, **72**, **134**, **135**, **136**, **197**) and the phenomenon became known as the "Thorndike effect." Agreement ceases, however, when it comes to an interpretation of the phenonenon. Discussion has centered around a number of related questions: (1) Is the gradient of effect really double-winged or is the *before* gradient spurious? (2) Is the gradient of effect a gradient of variability? (3) Is the gradient a result of perceptual emphasis on the rewarded response rather than of the mechanical action of satisfiers? (4) Is the gradient of effect really a gradient of response habits?

1. *Is the gradient of effect bidirectional?* For Thorndike one of the most important characteristics of the spread of effect is its bidirectionality: rewards exert their influence both in the forward and backward direction. The interpretation of the *before* (backward) gradient runs into a methodological difficulty, however. A connection which precedes a reward *also* follows a reward. In Thorndike's experiments rewards often followed each other rather closely. In criticizing Thorndike's data Tilton therefore raised the question whether the backward gradient might not be a spurious function of a large forward one (**286**). He also pointed out that serial position needs to be taken into account with this type of material. Putting these considerations to the test—taking serial position into account and attempting to eliminate the influence of the forward gradient from the backward gradient—Tilton still found a double gradient but from *both success and failure.* In the case of the *failure* gradients, the backward one is more pronounced, while the forward gradient is the more pronounced in the case of success. Other investigators also report the existence of a failure gradient (**197**, **332**). The double-winged gradient is thus confirmed, it is true, but we find that the effect of *Wrong* spreads as well as the effect of *Right.* These results render doubtful Thorndike's theory of the scatter or spread of the confirming reaction. As an alternative explanation Tilton suggests that connections which the experimenter regards as discrete are not functionally separate. Items in a series may be sufficiently unified to be affected by success and failure as a unit (**286**). It is not entirely clear, however, why such "sequential unity" should manifest itself as a double

gradient. Further doubt is cast on this hypothesis by Zirkle's finding that the rewarded item and the adjacent punished ones need not be qualitatively similar in order for the Thorndike effect to appear (**331**). Even more strikingly, Zirkle has demonstrated that it is the *response* adjacent to the rewarded item which is strengthened, not the adjacent S-R connection. When the relative order of wrong series items about a right item is shifted from one presentation to the next, there is no Thorndike effect when repetitions of S-R connections are counted. However, a clear-cut Thorndike effect appears when repetitions of *responses* are counted by *step-position* alone without regard to the shifting positions of the stimulus items. It has also been found that reward strengthens the *early* wrong responses to near-by stimuli not merely the last response (**174**).

2. *Spread of effect as a gradient of variability.* There have been other reformulations of the Thorndike effect. Muenzinger and Dove (**197**) disposed of the scatter hypothesis which ascribes the spread of effect to inaccurate placement of reward and consequent uncertainty of recall. A gradient is present, with a steeper slope than usual, even if the successful response is learned beforehand so that there can be no possible confusion between the correct response and the surrounding wrong ones. For a gradient of uncertainty Muenzinger and Dove substitute a gradient of uniformity or variability. Success produces a gradient of uniformity: this is in essence the original Thorndike effect. Failure produces a gradient of variability: not only does the wrong response itself tend to be varied but right responses near the wrong one are not repeated with the same degree of uniformity as those farther away in the series. Spread of variability under punishment is also reported by Stone (**260**). This analysis still leaves open the question as to what intervening mechanism the changes in variability should be ascribed.

3. *Is the effect gradient due to perceptual organization?* The systematic differences in approach to the problem of effect in general are necessarily reflected in interpretations of the spread of effect. Again we find those who regard rewards and punishments primarily as perceptual events opposing those who think of the mechanical action of satisfiers and annoyers. The conditions under which the Thorndike effect generally appears are especially favorable to an analysis in perceptual terms. The rewarded item is an isolated *Right* in a long homogeneous series of *Wrongs.* The announcement of *Right* may be regarded as a figure against a homogeneous ground of *Wrongs.* Wallach and Henle were the first to suggest that it is the extreme *crowding* of items which interferes with memory for wrong responses and contributes to the subject's tendency to repeat them (**305, 306**). The relation between perceptual isolation and the Thorndike effect was systematically investigated by Zirkle (**332**). He found a clear-cut positive relation between the degree of isolation of a rewarded response and the steepness of the effect gradient

(especially the *after* gradient). He also found a failure gradient around a perceptually isolated wrong response. On the basis of his experimental results Zirkle proposes a *theory of isolation* to account for the Thorndike effect. "A satisfier isolates tendencies which are at hand when it happens . . . responses neighboring upon a key response *tend to become isolated themselves* because of their association with the key response" (**332**, p. 312f.). A theory of isolation is, of course, a perceptual theory of the spread of effect. That which stands out in a homogeneous series becomes a focus of retention (similar to the von Restorff phenomenon). This interpretation is closely akin to Tolman's law of emphasis. Thorndike and Zirkle thus clearly represent the opposition between an interpretation of the effect gradient in terms of "hit-and-miss" mechanical reinforcement on the one hand and a view stressing the laws of perceptual organization on the other.

4. *Is spread of effect a gradient of response habits?* In terms of an orthodox connectionist analysis the Thorndike effect indicates that the influence of reward spreads to neighboring *S-R connections*. It is the S-R connection that is the basic unit of analysis. It is possible, however, to account for at least part of the effect gradient by an analysis of the *response* sequence, disregarding as it were the stimulus side of the S-R connection. We have already referred to Zirkle's finding that a Thorndike effect can be demonstrated when responses are counted by step-position even though the stimulus order is changed from presentation to presentation. The problem of response habits was systematically attacked by Jenkins and Sheffield (**136**). They found that (1) when the rewarded response itself was not repeated few adjacent errors were repeated and the Thorndike effect failed to appear; (2) when the rewarded response itself was repeated it was accompanied by a high level of repetition of errors and a typical effect gradient. Thus repetition of the rewarded response appears to be a necessary condition for the appearance of the Thorndike effect. The spread of effect may therefore be due not to the automatic action of reward but to the subject's tendency to repeat the same sequence of responses from trial to trial, i.e. the subject's "guessing habits." Responses are not independent of each other: choice of a response depends on the preceding responses. A repeated rewarded response ensures that "the errors following reward will be frequently preceded by the same response, i.e., will have a common antecedent stimulus." It is interesting to recall in this connection that preliminary rehearsal of the rewarded response yields an especially steep effect gradient (**197**) and that, on the other hand, a gradient fails to appear when the subject is not motivated to repeat the rewarded response (**305, 306**). Guessing sequences have also been reported with other types of multiple-choice responses (**72**), and there seems little doubt that at least part of the Thorndike effect can be analyzed in these terms. The demonstration of guessing habits does not necessarily dis-

prove the automatic spread of reward as conceived by Thorndike. It may well be that both some "stamping-in" mechanism and response habits interact in the production of the Thorndike effect.

As in the case of other conditions of reinforcement, it is necessary to stress the dependence of the spread of effect on the parameters of the experimental situation. As we have seen, the Thorndike effect fails to appear when subjects lack the motivation to learn (**305**, **306**) though distraction does not seem to affect it (**136**). In animal subjects, increase in drive results in a higher frequency of repeated responses in general, with a consequent flattening of the after-gradient (**134**). Similar results are obtained with an increase in incentive (**135**). Under such conditions, only responses *preceding* the reward yield a statistically reliable gradient. Such delicate dependence of the gradient on particular experimental conditions counsels against hasty analogies between the action of satisfiers and the "action of the earth upon the moon."

Parametric Studies

We have repeatedly stressed the functional dependence of the effects of reinforcement on the parameters of the experimental situation. We now turn to a consideration of studies that are primarily concerned with the influence of such parameters of reinforcement. No exhaustive survey will be attempted but the major types of functional relationships will be illustrated.

Amount of Reinforcement

Thorndike's original statement of the law of effect included the assertion that "the greater the satisfaction or discomfort the greater the strengthening or weakening of the bond." Demonstration of the correlation between amount of reinforcement and degree of learning has proved much easier with animal subjects than with humans. The motivation of animal subjects is more easily controlled and hence quantitative variations in reward are more effective. Increasing the food ration of a hungry rat affects behavior more drastically than adding 0.4 cent to the announcement of *Right* in a Thorndike experiment.

Turning to animal studies first, there are a number of experiments with different species and different learning tasks which support the generalization that increases in reward lead to improvements in learning and performance. Chickens (**86**) as well as rats (**42**) run down a runway at a faster speed when the amount of food reward is increased. Increments in reward also cause chicks to learn a maze with fewer errors (**320**), rats to show greater resistance to the extinction of a conditioned response (77), and chimpanzees to tolerate longer delays between the presentation of the stimulus and the response (208). Although the functional relationship between amount of incentive and performance varies from situation to situation, it is clear that the relationship is not linear. Equal increments in incentive do not lead to equal increments in performance. Thus Crespi, who was especially concerned with the quantitative relation between amount of incentive and performance reports a sigmoid relationship over a wide range of variation (42). Hull, on the other hand, believes that the relationship is best represented by a simple positive growth function (127).

The mechanism by which increments in reward exert their influence is still under discussion. Hull has suggested that an increase in the amount of reinforcement raises the limit to which the curve of habit strength approaches as an asymptote, although the rate of approach may be constant for all amounts of reinforcement. Hence increases in the amount of the reinforcing agent result in greater increments of habit strength per reinforcement. Hull also points out that in a conditioning situation the reward provides an important component of the stimulus situation which is conditioned to the response being reinforced. In such a situation a large reward stimulus evokes stronger, more vigorous, and more persistent responses than a small reward stimulus. Thus an increment in reward not only increases the amount of consummatory activity (need reduction) but also provides a more distinctive cue to the animal being conditioned (127). In this connection it is important to distinguish between sheer physical amount of reward and the amount of activity involved in the consumption of the reward. Wolfe and Kaplon were able to show that amount of reward and amount of consummatory activity are experimentally separable conditions of learning. Learning improves as a function of sheer amount of reward: a whole kernel of corn is a more effective incentive for chickens learning a maze than one-fourth of a kernel. However, learning also improves if the amount of consummatory activity is increased for a constant reward: when the whole kernel of corn is divided into four separate quarters the maze is learned better than with the equivalent amount given in one piece. Of the two factors, amount of consummatory activity influences learning to a greater extent than sheer amount of reward (320).

The role played by amount of reinforcement has also been explained in terms of the subject's attitude rather than in terms of differential strengthening of S-R connections. Crespi has proposed a two-factor theory of *incentive-value*. He conceives of incentive-value as proportional to the distance between the subject's level of expectation (both of quantity and quality of reward) and the level of attainment. Attainment which does not reach the level of expectation is frustrating and affects learning adversely, while attainment above expectation is "elating" and improves learning. It is not the sheer amount of reward alone which is decisive nor the animal's expectancy alone but rather the relation between the two. A striking illustration of this principle is the fact that rats will perform significantly better with no incentive at all than with a very small incentive. A very small incentive serves to whet the animal's appetite, raises his level of expectation and eventually leads to frustration. In accordance with the same principle, downward shifts in the amount of incentive lead to poorer and more variable performance whereas upward shifts improve the performance (42, 42a). Crespi's conceptualization is reminiscent of the experiments on *level of aspiration* of human subjects. That the effectiveness of a given quantity of reward cannot be evaluated without reference to the subject's expectancy is also stressed by Cowles and Nissen (40) on the basis of experiments on delayed responses in chimpanzees. Similarly Nissen and Elder (208) report that increases and decreases in amount of incentive affect not only the response within a given trial but succeeding trials as well. Such perseverative effects suggest the operation of reward expectancy.

When human subjects are used, comparisons of different amounts of reinforcement yield variable and inconclusive results. Slight increases in rate of learning as a function of increases in reward—addition of small money gains to the announcement of *Right*—have been found in some Thorndikian experiments (60, 223, 280). The effects are exceedingly slight if the amount of reward varies within the same series (223) and are somewhat more pronounced if incentives are changed from series to series (280). On the other hand, promise of a reward was found as effective as actual administration of the reward (78). In situations of this type, the experimenter never knows whether an increase in reward is experienced as such by the subject. Moreover, if different rewards are presented in the course of an experiment, their effects may interact with each other. Whatever effects there are, may possibly not stamp in individual responses more firmly but rather affect the general level of the subject's motivation and thus affect learning secondarily (171).

Frequency and Pattern of Reinforcement

The simplest hypothesis relating frequency of reinforcement with strength of learning is that each reinforcement adds to the strength of the S-R connection being reinforced. This hypothesis is plausible, at least at first blush, in the light of empirical data. The percentage of conditioned responses usually increases as a function of the number of reinforcements (108), human rote learning steadily improves on successive trials (171). Indeed, it is difficult to think of learning situations in which an increase in number of trials does not result in better performance.[14] Frequency *per se* cannot be profitably considered a significant condition of learning. Rather, repeated pairings of stimulus and response allow effective conditions of learning, (such as simultaneous conditioning, reinforcement by need reduction, confirmation of expectations, etc.) to exert their effects. There would probably be little disagreement with such a general formulation of the role of frequency. Agreement ceases when the assertion is made that each individual reinforcement in a series of reinforcements contributes a differential increment (say, $\Delta_S H_R$ in Hull's language) to the existing habit strength. The theoretical question at issue is whether or not the effects of successive reinforcements are additive and cumulative. Is the law of effect a law of cumulative effect?

The assumption that the effects of successive reinforcements continuously cumulate in time is central to Hull's theoretical system (**127**). He conceives of habit strength as a monotonic increasing function of the number of reinforcements. As habit strength approaches the subject's physiological limit, the increment from each reinforcement progressively decreases in magnitude. Thus habit strength is a simple positive growth function of the number of reinforcements. Habit strength, as Hull uses the term, is a theoretical construct which can be measured only indirectly through its behavioral manifestations. There are a number of behavioral studies whose results are consistent with Hull's quantitative picture of the growth of habit strength:

1. Reaction amplitude increases as a function of the number of reinforcements (**118**).
2. Reaction latency decreases with the number of reinforcements (**235**).
3. The number of trials required for experimental extinction may be proportional to the number of reinforcements (**209**, **317**, **329**).
4. The more frequently a response has been reinforced the greater the probability that the appropriate stimulation will evoke that response (**162**, **269**, **276**, **329**).

Unfortunately not all experimental results available fit into Hull's theoretical picture. Leeper (**160**) has criticized Hull for what he considers a biased selection of illustrative experiments. There is substantial experimental evidence that partial reinforcement, i.e., reinforcement on only a fraction of the trials rather than on each trial may be at least as effective as continuous reinforcement. Thus Humphreys showed that reinforcement on 50 percent of the trials is as effective in the establishment of a conditioned response as reinforcement on 100 percent of the

[14] A notable exception is Skinner's finding that a single reinforcement results in a series of lever pressings by the rat at an optimal rate (**237**). One reinforcement is sufficient to establish an adequate response strength. Further reinforcements serve to build up a reflex reserve, which is subsequently emptied in the absence of reinforcement (extinction).

trials and that partial reinforcement may result in *greater* resistance to extinction than continuous reinforcement (**128, 129**). Humphreys believes that conditioned responses occur to the extent that the subject *expects* the reinforcing stimulus to follow the conditioned stimulus, extinction occurs to the extent that the subject no longer expects reinforcement. Partial reinforcement during the initial training period makes it difficult for the subject to shift from expectation of reinforcement to expectation of non-reinforcement. In terms of the expectancy hypothesis it is necessary clearly to distinguish between frequency of trials and frequency of reinforcements as conditions of learning. The relation between these two frequencies determines the subject's expectations and hence the course of acquisition and extinction of responses (**130**). The equal effectiveness of partial and continuous reinforcement has, however, also been ascribed to the influence of secondary reinforcement during the ostensibly unreinforced trials (**52**). Such an explanation would be in conformance with a description of the reinforcement process as continuous and cumulative. The relationship between number of trials and relative frequency of reinforcement is complicated by the phenomenon which Hovland has called "inhibition of reinforcement" (117). A massing of reinforcements results in a weakening of conditioned responses, with spontaneous recovery within a short interval of time. Inhibition of reinforcement as well as expectancy have to be taken into account in the analysis of partial reinforcement (**75, 76**).

The studies cited thus far by no means exhaust the evidence for the equal effectiveness of partial and continuous reinforcement. Brogden (**16**) showed that conditioned flexion to shock was established as readily with 40 percent reinforcement as with 100 percent. Decrease in the frequency of reinforcement had the positive effect of eliminating a great deal of the animal's diffuse and restless behavior. When food rather than shock was used as the reinforcing stimulus substantially the same results were obtained, except that there was a slight decrease in the frequency of response (CRs on $\frac{4}{5}$ of the trials) when reinforcement was applied only 20 percent of the time. Brogden concludes that application of the reinforcing stimulus serves primarily as an *incentive* to the subject, and thus a low frequency of reinforcement may maintain the conditioned response at a high level.

Mowrer and Jones (**187**), who confirmed Humphreys' findings, present an argument reconciling these results with the law of effect. The effects of a reward need not necessarily be restricted to the particular response that occurs just before the reinforcement. The reinforcement applies to preceding responses as well (though to a decreasing extent). If we think in terms of *response units* (sequences) each of which is followed by a reward rather than in terms of individual responses which sometimes are reinforced and sometimes are not, the apparent advantage of intermittent reinforcement disappears. On the contrary, in terms of a response-unit analysis, the intermittently reinforced group which has to expend more effort in order to obtain a reward gives fewer extinction responses.[15] The response-unit hypothesis of Mowrer and Jones points to the importance of considering the *temporal pattern* of reinforcement and not only the sheer frequency. Skinner's results with periodic reconditioning are a relevant case in point (**237**). When the lever-pressing response of the rat is reinforced periodically, e.g., every three minutes, the animal's rate of response not only tends to become uniform, but also the more frequent the reinforcement the more rapid is the rate. On the other hand, when reinforcement is at a fixed ratio, i.e., when the final member of a fixed number of responses is reinforced, the *less* frequent the reinforcements the higher is the rate of response. Such laws of operant behavior can be analyzed only in terms of the total temporal pattern of responses and reinforcements (e.g., Skinner's concept of reflex reserve). The importance of the temporal pattern is also illustrated by Brunswik's finding that in discrimination learning *probability of success* is an important determinant of the animal's behavior at a choice point (**21**).

In summary, it is clear that frequency of reinforcement is an important determinant of the strength of learning. However, partial reinforcement can be as effective as, and more effective than, continuous reinforcement. Reinforcements are not always simple additive units, and the temporal *pattern* of a series of responses and reinforcements gives rise to behaviors which cannot be predicted in terms of a simple monotonic relationship between frequency of reinforcement and strength of learning.

Delay of Reward and Gradient of Reinforcement

The degree to which reinforcement strengthens a response depends in part on the time interval that elapses between response and reinforcement. Experimental inquiry has been directed at two interdependent problems: (1) the difference between immediate reinforcement and delayed reinforcement and (2) the exact quantitative relationship between length of delay and strength of learning, i.e., the nature of the *gradient of reinforcement.*

Thorndike early suggested that satisfying states of affairs are the more effective the closer they are temporally to the S-R bond. "Other things being equal, the same degrees of satisfyingness will act more strongly on a bond made two seconds previously than on one made two minutes previously . . . " (**265**, p. 172). A few of the early investigations of delayed reward failed to bear out this prediction (**309**, **313**), but it is now clear that in these experiments secondary reward was not con-

[15] Mowrer and Jones (**187**) also point out that the law of effect is compatible with Humphreys' expectancy hypothesis. The fulfillment of an expectation may serve as a reward (need reduction).

trolled—the animal subjects were delayed in the *food chamber* of a maze —so that there was no effective delay of reinforcement. When these first attempts are discounted, the experimental literature shows general agreement on the superiority of immediate over delayed reward. The detrimental effects of delayed reinforcement have been demonstrated with animal subjects in maze learning (**34**, **100**, **318**), problem box learning (**222**), the formation of discrimination habits (**35**, **321**), and in the establishment and maintenance of operant responses (**210**, **211**, **237**).[16] Similar findings have been reported for delayed punishment (**32**, **141**, **192**, **308**, **311**, **321**, **323**).

Different investigators used different periods of delay but there were strong indications that the most serious detrimental effects were concentrated in the first minute of delay. Formal quantification of the functional relationship between length of delay and strength of learning are found in the writings of Hull (**127**) and Perin (**210**, **211**). On the basis of theoretical calculations, which fit a considerable amount of empirical data, Hull concludes that (1) habit strength is a negative growth function of the time separating the response from the reinforcement and (2) the asymptote of this gradient is zero, i.e., with a sufficiently long delay reinforcement becomes ineffective. In the case of the rat, the gradient reaches zero at a delay of about 30 seconds (**210**). This rather short gradient of habit strength constitutes the *gradient of reinforcement*.[17] It is important to bear in mind that the gradient of reinforcement refers to the effects of different intervals *between single responses* and that it does not apply to one of a series of responses leading to a common reward (**108**). In the case of a series of responses, e.g., in a maze, the gradient is more extended and more complex, and is generated by the "summation of an exceedingly complex series of overlapping gradients of reinforcements, in part consisting of, but largely derived from, the 'primary' reinforcements occurring at the end of the temporal period covering the behavior sequence involved" (**127**, p. 143). In other words, more and more members of a stimulus series acquire (secondary) reinforcing power, each probably in accordance with the short gradient of reinforcement. The summation and interaction of these short gradients result in the extended *goal gradient*. Hull believes that the goal gradient is an exponential or negative growth function (not, as he had originally believed, a logarithmic function). The greater the influence of secondary reinforcement, the less steep the slope of the goal gradient, i.e., the greater the temporal range over which a reinforcement can exert its influence.

[16] Skinner (**237**) found that delays are detrimental only after periodic reconditioning whereas delays up to 4 seconds did not affect the original conditioning of the lever-pressing response.

[17] The expression *gradient of reinforcement* was first used by Miller and Miles (**178**).

Hull's goal gradient hypothesis (**120**, **122**) has proved to be a powerful deductive tool by means of which a considerable amount of empirical results could be predicted, sometimes with striking accuracy. Among the findings which the hypothesis predicts and which were empirically confirmed are the following:

1. With number of reinforcements held constant, when the delay of reinforcement is short, less time is required to execute a response than under conditions of long delay of reinforcement (**6**).
2. Of a pair of alternative acts the one which is reinforced with a shorter delay is chosen (**5**, **226**).
3. Other things being equal, the greater the difference in the delays of reinforcement yielded by two alternative reactions, the more quickly the animal will learn to choose the act yielding the shorter delay (**5**).
4. When the absolute differences in delay are equal, differentiation between two short delays is achieved faster than differentiation between two long delays. For example, a 30-second delay is more readily differentiated from a 60-second delay than a 60-second delay is from a 90-second delay (**5**, **85**). On the basis of the goal-gradient hypothesis Hull has also been able to predict the effects of delays of equal relative, but of different absolute, duration (**327**).

The gradient of reinforcement in Hull's treatment is, strictly speaking, a *temporal* gradient. As Hilgard and Marquis (**108**) point out, such temporal gradients should be clearly distinguished from non-temporal gradients which refer to the spatial distance between a response and the reinforcement or the serial position of a response with respect to the reinforcement. Spatial separation or remoteness in a series, of course, implies temporal delay since it takes time to traverse the space leading to the goal or the members of a series ending in reinforcement. On the other hand, an animal delayed in a restraining compartment is in a very different situation from a subject that has to cross a runway in order to reach a food reward. The main difference lies in the fact that the spatial interval is filled with a series of acts leading to the reinforcement whereas sheer temporal delay may serve to disrupt the integration of a behavior sequence. The rat's speed-of-locomotion gradient (**122**) in a straight alley is a spatial gradient, with running speed plotted against successive segments of the path leading to the reward (**6**, **122**) or punishment (**23**, **176**). Similarly, the extensive experimental investigations of the goal gradient in maze learning (**120**, **238**, and many others) and of animals' ability to discriminate short from long paths to reward (**35**, **50**, **85**, **327**) should be considered primarily as studies of the *non-temporal* aspects of the gradient of reinforcement. In such experiments, the crucial experimental variable is the *distance* between a segment of the apparatus (runway, maze) and the locus of reinforcement. This distance involves not only a temporal delay but also a complex sequence of intervening acts.

A notable example of a spatial gradient is, of course, the Thorndike effect. The close kinship between the Thorndike effect and the goal-gradient was demonstrated by Muenzinger, Dove and Bernstone (198). In an "endless" maze (four identical mazes arranged as the sides of a square, with food boxes at the four corners) a double-winged gradient of elimination of errors was obtained. These authors believe that the goal-gradient is bidirectional in its fundamental form and that the usual backward elimination of errors in a maze reflects only the first half of the gradient The other half cannot manifest itself because the animal's activity usually ends at the goal. This analysis has not remained undisputed. Hill (109) reports that the forward gradient appears only late in learning and ascribes it to the failure of anticipatory errors to be eliminated. In a more recent investigation, however, Thompson and Dove again report evidence for a basically bidirectional goal-gradient (262).

With human subjects, gradients of reinforcement have usually been plotted as a function of serial position rather than in terms of sheer temporal delay. The various studies of the Thorndike effect are a case in point: the spread of effect is most clearly demonstrated when frequency of repetition is plotted against distance from reward in terms of response units (15, 270, 331). On the other hand, when time alone is considered, a 6-second delay is found to be as beneficial to learning as a 0-second delay of reinforcement, and there is no evidence for a temporal gradient (167). The activity filling the interval between response an reinforcement is an important factor: when an announcement of *Right* follows an interval filled by another response, i.e., if the reward refers to the next to last response, the reinforcement is virtually ineffective. Response and reinforcement must "belong" together. (Such detrimental effects of interpolated activities are, of course, well known in the study of retroactive inhibition.) The fact that the gradient of reinforcement, which is readily demonstrated with animals, cannot be easily applied to human subjects is of considerable theoretical importance. With the aid of symbols the human learner can bridge temporal gaps that are prohibitive to the animal (192). Such apparent independence of immediate reward has considerably complicated the application of the law of effect to human learning.

Strength of Drive

The modern version of the law of effect equates effect to drive reduction. The strength of the drive could be expected to affect the operation of the law in two ways: (1) strength of the drive could be one of the determinants of the speed of acquisition and (2) performance (habit evocation) may vary with the strength of drive.

What is the influence of the strength of drive at the time of learning? An experimental investigation by Finan (73) showed that with the number of reinforcements constant an instrumental act is established more strongly when the drive is strong than when it is weak. A stronger drive yields a stronger habit although the relationship is by no means linear. This generalization has, however, not remained unchallenged. In Hull's theoretical system, the course of acquisition (building up of habit strength) is described as independent of the strength of drive at the time of learning. Habit strength is a joint function of number of reinforcements, the time for which the conditioned stimulus has been acting before the occurrence of the response to be learned, the time interval between response and reinforcement, and the magnitude of the goal object (127, 144). Drive strength is *not* one of the variables of which habit strength is a function. In a recent study Kendler (145) has justified this omission of drive strength as one of the determinants of habit strength. In one of his experiments, animals learned a bar-pressing response under different degrees of thirst deprivation. The animals were, however, equated in strength of motivation during extinction. The results show that different drive conditions at the time of learning had not influenced the amount of habit growth. Kendler also showed that a group of subjects which had a low drive strength during learning but received a large number of reinforcements established a stronger habit than a matched group which learned under high motivation but received a smaller number of reinforcements. The results reported by Finan and Kendler are contradictory and the role of the strength of drive during acquisition must remain open.

There is general agreement, on the other hand, that the strength of drive is an important determinant of performance. According to Hull reaction-evocation potentiality is a multiplicative function of habit strength and drive strength. This hypothesis receives experimental support from Perin's finding that for a given number of reinforcements, resistance to experimental extinction is an almost linear function of the number of hours of food deprivation at the time of the extinction procedure (209). In his investigation of operant conditioning, Skinner (237) found that rate of response is considerably affected by the animal's drive: over a wide temporal range of food deprivation, increase in drive leads to faster rate of emission of responses (emptying of the reflex reserve).

Although performance varies with strength of drive, there are some indications that appropriate stimuli will evoke a response even when the subject is to all intents and purposes fully satiated (209, 330). Extrapolation from Perin's theoretical curves relating resistance to extinction to drive strength predict this result. But here, too, the last word has not been spoken. Koch and Daniel (147) report zero or close to zero reaction potential immediately after satiation. Although the general dependence of performance on strength of drive may be considered as well established, the precise quantitative nature of this relationship as well as the problem of interaction of different drives (**144**, **145**) is still in need of further experimental analysis.

The Place of the Law of Effect in Learning Theory

Parametric studies such as those described in the preceding section derive their main significance from the contribution which they can make to general learning theory. We shall now consider the role played by the law of effect in theoretical interpretations of learning.

Theories of learning can be classified in more than one way. One can distinguish between molar and molecular theories, according to emphasis on specific movements as against stress on acts and their outcomes; or one can pit modern associationism against configurational theories. The role assigned to reinforcement by reward and punishment provides another criterion of classification. There are clear-cut lines of division separating systematic points of view according to the role assigned to the law of effect: (1) effect may be considered to be a principal condition of all learning, (2) effect may be rejected as a condition of learning but considered an important determinant of performance, (3) the essentials of the learning process may be conceptualized without reference to effect, with reward and punishment assigned a subsidiary role and credited with only indirect influence on learning.

The Law of Effect as a Principal Condition of all Learning

The law of effect could not become the pivot of a comprehensive theory as long as it was restricted to the narrow universe of multiple-choice ("trial-and-error") learning. It was only with its application to the facts of conditioning that the law of effect could become a unifying principle around which a systematic theoretical structure could be built.

For a considerable period of time students of effect and students of conditioning failed to make contact with each other. In the field of classical conditioning stimulus *substitution* was the primary principle of explanation. To the extent that substitution could not account for all the empirical data, auxiliary hypotheses such as the principle of dominance and such concepts as set and attitude were introduced (**105**, **106**). On the other hand, Thorndike firmly maintained the distinction between learning by selection and fixation (formation of S-R bonds) and conditioning, which he termed "associative shifting." Thorndike never believed that the study of conditioning could throw much light on the nature of learning: "The conditioned reflex is one type of learning that manages, even more completely than maze learning, to conceal the true nature of the learning process in a mass of special conditions" (**267**, p. 85). In the light of this opinion it is not surprising to find that Thorndike has almost completely ignored the facts and theories of conditioning in all his writings. Students of conditioning, on the other hand, could not indefinitely ignore the problem of effect, for the crucial role of incentive in the establishment and maintenance of conditioned responses was an incontrovertible experimental datum.

Paralleling Thorndike's dichotomy between connection formation and associative shifting, several two-fold classifications of learning situations have been made: classical and instrumental conditioning (**108**); Type S and Type R conditioning (**236**); conditioning and success learning (**228**); quantitative and qualitative conditioning (**216**); conditioning and motivated learning (**74**). Such classifications reflect important differences in experimental procedures under which learning can take place, but, as Hilgard and Marquis (**108**) have emphasized, they do not represent pure types of learning which necessarily require principles of explanation as different as substitution and effect. In any given experiment, both types of learning may take place, and the classification of an experiment will largely depend on which aspects of the response the experimenter emphasizes and measures. A classical conditioning experiment emphasizes stimulus substitution and homogeneous reinforcement; an instrumental reward or escape experiment dramatizes the principles of effect and heterogeneous reinforcement. "It is a common error to permit the reference experiment to dramatize a particular process, and then to suppose that the experiment represents a pure case of the process dramatized" (**108**, p. 97).

Thus, the distinction between learning by selection and fixation (heterogeneous reinforcement) on the one hand and learning by stimulus substitution (homogeneous reinforcement) on the other does not preclude a unified conceptual scheme which makes a law of effect the basic principle of all learning. It is the virtue of Hull's theoretical analysis that these two types of learning are subsumed under a common principle: they are both reduced to the operation of the law of primary reinforcement (Hull's formulation of the law of effect). The basic principles and generalizations of Thorndike and Pavlov are combined and unified in a single theoretical structure (**246**). Hull's law of primary reinforcement makes temporal proximity to a reinforcing state of affairs a condition without which learning cannot take place. Both selective learning (the Thorndike situation) and conditioned-response learning are special cases of the operation of this law. In the case of simple selective learning, one of many possible alternative reactions occurs in temporal proximity to need reduction and hence is differentially reinforced. Such a receptor-effector connection may or may not be of super-threshold strength at the beginning of an experiment. The conditioned response also depends on temporal proximity to reinforcement but in this case a *new* receptor-effector connection is almost invariably established: the connection between CS and UR. Hull concludes that "the differences between the two forms of learning are superficial in nature, i.e., that they do not involve the action of fundamentally different principles but only the differences in the *conditions* under which the principle operates" (**127**, p. 78). When the CR is considered basically akin to selective learning, the behavioral laws discovered in conditioning experiments can be applied to, and integrated with, the phenomena of selective learning. The principles of conditioning in conjunction with the law of primary reinforcement can be used in the deduction of more complex forms of learning.

Although the law of primary reinforcement is closely akin to Thorndike's law of effect, there is an important difference which Hull has made explicit. As the law has been repeatedly stated by Thorndike it is both a law of motivation and a law of learning. In a review of Thorndike's work Hull (**123**) raised the question whether motivation (striving) produces the learning, or learning produces the motivation, or whether some third and still more basic process produces both. Thorndike's formulation seems to imply that striving is to be considered primary: he defines a satisfier as that which an animal strives to attain or does nothing to avoid. Hull, on the other hand, considers learning ("strengthening") primary and derives striving from the principles of conditioning and the primary law of reinforcement (need reduction) as basic assumptions. In his own words, "states of affairs which organisms will strive to attain are reinforcing agents, not because they will evoke striving, but they evoke striving now because at some time in the past they were potent reinforcing agents, thereby joining stimuli and responses . . . which constitute the striving" (**123**, p. 822). The point is that Hull is willing to assume as originally given only those reinforcing agents which satisfy basic biological needs and are linked with the organism's survival, and he then derives other motives or strivings with the aid of the principles of conditioning. In Thorndike's writings, on the other hand, we find no such hierarchy of motives. The role of motives (wants, interests and attitudes in learning) is described as twofold: (1) they determine what response a situation shall evoke, and (2) the satisfaction of wants strengthens S-R bonds (**276**). It is to the question of the *origin* of these wants, interests and attitudes that Hull addresses himself, seeking to derive them with the aid of conditioning principles.

The gap between learning situations in which biological need reduction occurs and the myriad of learning situations in which there is no such immediate primary reinforcement is bridged by the principle of *secondary reinforcement.* According to this principle, any receptor activity which regularly precedes a primary reinforcement will itself gradually become a reinforcing agent. The sight and smell of food as well as other stimuli emanating from a feeding compartment, such as the click of the mechanism releasing pellets into a Skinner box (**22, 237**), are sources of secondary reinforcement. A stimulus such as a buzzer or tone which has been regularly associated with shock also acquires reinforcing properties (67, 69, 74). The reinforcing power of token rewards, i.e., rewards which may subsequently be exchanged for a primary reward such as food may similarly be explained in terms of derived reinforcement (39, 65, 179, 207, 319). The effectiveness of social rewards and punishment calls for similar explanatory concepts: a practically unlimited chain of higher-order conditioning must be assumed (127, 177).

Recently Spence (247) has suggested that it is the particular stimulus *pattern* at the time of reward which acquires secondary reinforcing properties. This reinforcing power is generalized to preceding stimulus patterns according to a temporal gradient. In this conceptualization the vexatious problem of backward action is eliminated.

It is in Hull's theoretical system that the law of effect has come to its full theoretical fruition: it has become a primary postulate with whose correctness a complex structure of deductions and theoretical interpretations must stand or fall.

The Law of Effect as a Law of Performance

It is possible to accept the law of effect in a *descriptive* sense, i.e., to recognize that rewarded responses are usually repeated in preference to non-rewarded ones, and yet to deny that the law of effect is a law of *learning*. Several writers, under the leadership of Tolman, have taken this position (64, 159, 287, 289, 292, 294, 315). A rigorous distinction is made between the *acquisition* and *utilization* of habits. The acquisition of habits depends on the formation of *cognitive* patterns within the organism which reflect the stimulus relationships in the environment. Such cognitive patterns Tolman has described as "sign-gestalt expectations" or "hypotheses" (287, 288, 290, 291). Organisms come to accept one event as a sign or "local representation" of another event (293). Learning occurs when the subject has built up an expectation that a given sign in the environment will, via a behavior route, lead to a certain significate or outcome. These expectations result from the organism's commerce with the environment and their acquisition is governed by such conditions as frequency, recency, and perceptual laws of stimulus organization (sign-gestalt formation).[18] Differential reward is not considered a determinant of learning. Experienced reward does, however, play a role as a determinant of performance or utilization of habits. From a set of alternative responses to a stimulus (sign) that response is *selected* and *performed* whose consequence is most "demanded," i.e., most rewarding in terms of the momentary motivational state of the

[18] In building up expectations the organism essentially reacts to the relative probabilities that signs in the environment will be followed by certain outcomes. A reinforcement theory stressing reaction to probability has been elaborated by Brunswik (21).

animal. Even though knowledge and need (learning and performance) are thus distinguished, behavior is always a joint function of both. When a subject moves toward a goal he must (1) have a need for that particular goal, and (2) have the knowledge that a given piece of behavior on his part will lead to that goal. Hence needs and knowledge constitute an interdependent pattern or field (**315**). In this theoretical account, then, the law of effect is rejected as a law of *learning* and relegated to a secondary role as a condition of the moment-to-moment utilization of habits that have been acquired independently of effect. Only if motivation remains constant can behavior be accurately predicted on the basis of the law of effect.

Tolman calls his account of learning and performance a field theory[19] which may be applied to substitute stimulus (CR) learning, trial-and-error as well as more complex forms of learning such as "inferential" and "inventive learning" (**290**). The learning theory put forward by the chief exponent of field theory, Kurt Lewin, (**161**) is closely akin to that of Tolman. Lewin sharply distinguishes between learning as *change in knowledge* or cognitive structure (differentiation of unstructured areas, restructurization) and learning as *change in motivation* (changes in valences and values). Changes in cognitive structure are ascribed in part to the same type of "forces" as govern perceptual fields, in part to the impact of the needs, values, and hopes of the subject. Among the complex of forces governing changes in movitation reward is only a minor factor, for "forces governing this type of learning are related to the total area of factors which determine motivation and personality development " (**161**, p. 239). Throughout his treatment Lewin emphasizes the need to distinguish the motivational from the cognitive problems, to study their separate laws, and then to determine the role of each type of factor in different learning situations. In this connection it should be noted that the distinction between learning and performance is not limited to the field-theoretical approaches. In Hull's account, a parallel distinction is made between the principles governing habit-formation and the principles governing habit use. Whereas the concept of *habit strength* describes the degree of acquisition, the construct of *effective reaction potential* refers to the degree to which a habit is ready for performance.[20] Thus both S-R theory and field theory allow for the distinction between learning and performance. The difference is that for Hull motivational factors govern both acquisition and performance, whereas in the Tolman-Lewin formulation motivational factors come into play only in the utilization of habits.

One of the main experimental supports for the Tolman-Lewin view

[19] For another restatement of learning theory in field-theoretical terms, see Adams (**1**).

[20] Effective reaction potential is a product of effective habit strength and drive, taking account of the total amount of inhibitory potential (**127**).

has been the phenomenon of latent learning. When a reward is introduced after a series of unrewarded trials in a maze, an improvement in performance occurs which far exceeds the usual effects of a single reward. A substantial part of this sudden improvement may then be ascribed to learning (formation of sign-gestalt expectations) which took place during the unrewarded trials but was not utilized in the absence of reward. Such analysis is strictly in accord with Tolman's theory. The phenomenon of latent learning was first demonstrated by Blodgett in 1929 (14) and has been repeatedly confirmed since (**49**, **51**, **102**, **103**, **295**, **304**). A recent repetition of Blodgett's experiment by Reynolds (**220**), however, failed to show latent learning. This result has, at least temporarily, detracted somewhat from the support which the latent learning experiment has provided for the field-theoretical view. To the extent, however, that latent learning has been successfully demonstrated it provides a serious challenge to any view that would make learning a cumulative function of successive reinforcements by reward. As Leeper (**160**) has pointed out, it is impossible to argue that some other reward, such as satisfaction of the exploratory drive, accounts for the learning. Such a reward should reinforce exploratory behavior and not running to the goal with few errors once a food reward is introduced. In this connection it is interesting to note that in a recent attempt to derive the facts of latent learning within the framework of Hull's theoretical system Seward (**232**) was forced to abandon Hull's postulate (No. 4) that increments from successive reinforcement summate to yield a combined habit strength. Instead, he had to assume that conditioning is independent of reinforcement and is complete in one trial when S and R are simultaneous. On the other hand, Buxton (**28**) was able formally to derive the phenomenon of latent learning in terms of field-theoretical principles.

Utilization of a habit depends on the presence of appropriate motivation (appetite for the goal object). Performance of a given act does not always depend on the presence of any *one* particular motive: different motives may be equivalent to each other in their capacity to evoke performance to the extent that the outcome remains congruent with the motive that prevails at the time of performance (**61**, **62**, **63**, **64**). Within limits, therefore, changes in motivation may leave behavioral responses (habit utilization) relatively unaffected, or lead to only partial changes in performance. Even total removal of reward may lead to an only temporary disturbance in performance rather than a permanent disintegration of a habit (**310**). When the subject is confronted with a *choice*, however, motivation may serve as the basis on which *selection among alternative responses is made*. On this hypothesis, if a subject were to learn that one route in a maze leads to food and another route leads to water, he would be expected to choose the food route when hungry and the water route when thirsty. This deduction has been repeatedly put to experimental test but the results and interpretations have thus far remained contradictory.

In 1933 Hull (**121**) performed an experiment in which he was able to establish differential reactions to the same maze environment on the basis of different drives (hunger and thirst). He trained his rats in a simple two-route maze. The animals had to traverse one arm of the maze when they were hungry and were fed in the goal chamber. On days when they were thirsty, the animals had to traverse the other arm of the maze and received water in the *same goal chamber*. Hull's animals learned this discrimination only with great difficulty: 25 training periods of eight days each were needed before 80 percent correct responses were given on the first run of a series. Hull interpreted his results to show that animals can be conditioned to internal conditions (drive stimuli). The slowness and difficulty of the training would seem to run counter to the predictions of a sign-gestalt theory. The experiment was then repeated by Leeper (**159**) who fully realized its crucial significance for a field theory of learning. He introduced an important modification. In order to make a clear perceptual differentiation of the situation possible for the subjects, he constructed a maze with *two end-boxes*, one containing food and the other water. When a rat made an incorrect choice, it entered the goal chamber which contained the reward not desired under its prevailing motivation. Thus the rat had continual opportunity to build up an expectation of "what leads to what." Whereas in Hull's experiment the rat was simply blocked on an incorrect trial, Leeper's subject acquired information about means-end relations on every trial, correct and incorrect. As a result Leeper's rats required only one eight-day period to reach approximately the same criterion as Hull's rats after 25 eight-day periods.[21] Leeper felt that his results fully support Tolman's theory and the distinction between acquisition and utilization of habits.

The results of Hull's and Leeper's experiment are reviewed in some detail to show how virtually the same experiment lends itself to alternative interpretations, one based on the law of effect and assuming summation of reinforcement, the other making learning independent of effect. Subsequent studies of the same problem have remained equally inconclusive Kendler (**146**) raised the question whether results similar to Hull's and Leeper's could be obtained if both hunger and thirst drives were simultaneously present during the training period. He found that animals learned to respond appropriately on the test trials, i.e., in accordance with the motivation prevailing during the test trials. Although this result would seem to be exactly in accord with Leeper's

[21] Hull explained the difference between his and Leeper's results by suggesting that after the first few trials Leeper's animals probably operated under both drives, thus being rewarded no matter which compartment they entered (**127**). In a reply to Hull, Leeper denied on the basis of his experimental records that such was the case (**160**).

interpretation, Kendler offers alternative explanations within the framework of an effect theory. He speculates that (1) only those drive stimuli which are reduced during the training trial are associated with the rewarded response, and (2) invokes anticipatory goal reactions as possible differential cues.

Recently Spence and Lippitt (249) have reported "an experimental test of the sign-gestalt theory of trial and error" which is again concerned with the utilization of alternative habits under different motivations. The subjects (white rats) in this experiment were motivated by thirst and given 12 days experience in a simple Y-maze. One arm of the maze always led to water. The other arm led to food for one-half of the subjects, to an empty goal box for the other half. The test trials were run under hunger motivation, with the thirst drive satiated. During these test trials the subjects continued to go down the alley leading to water and the group which had previously experienced food was not superior to the other group in learning to choose the food alley. These results are clearly contrary to what would be expected in terms of sign-gestalt theory. However, the same authors had previously reported (in abstract form) an experiment which was more in accord with Tolman's theory (248). In this case, the subjects were satiated for both food and water during the training series and found water at one end of the maze and food at the other. When made hungry or thirsty, the animals chose the alley leading to that goal which satisfied the need prevalant at the moment. At that time Spence and Lippitt concluded, "Latent learning does not occur in the situation where animals perceive the subsequent goal object while motivated for another, but latent learning does occur where complete satiation made for no particular goal-directedness." Thus the results of Spence and Lippitt do not call necessarily for abandonment of a sign-gestalt interpretation but for a modification: experience with goal objects which satisfy a need and with goal objects for which there is no need during the training period are not equally effective in learning (acquisition of expectations).

Just as the facts and interpretations of latent learning remain under discussion, the general problem of the mechanism of discrimination learning is still the cause of serious disagreement between proponents and opponents of an S-R-effect theory of learning. The experimental fact, on which there is general agreement, is that subjects can learn to choose between positive stimuli and negative stimuli simultaneously presented, i.e., between stimuli response to which leads to reward and stimuli response to which fails to lead to reward or leads to punishment (150, 154, 156, 157, 170, 205, 239, 241, 245). Even before the discrimination has been learned, the animal does not respond in a haphazard fashion but shows definite systematic response tendencies, or "hypotheses" (149, 150, 151, 152). It is around these systematic response tendencies during the pre-solution period that the main argument between "continuity" and "non-continuity" theories has been centered. According to the continuity theory, discrimination learning, like other types of learning, results from a cumulative process of building up an association between the positive stimulus cue and the response. Every time a response to the positive stimulus cue occurs and is followed by reward, the association between cue and response is strengthened; every time a response to the negative stimulus cue is made and fails to be followed by reward, the tendency to respond to this cue is weakened. Discrimination is established when the difference in the excitatory strengths of the positive and negative cues is sufficiently great to overcome other aspects of the total stimulus situation which are not consistently associated with reward or failure. In terms of this analysis discrimination learning is fully explained in terms of stimulus-response association and the law of effect. The continuity hypothesis of discrimination learning has been sponsored by Spence (**240**, **241**, **242**, **243**, **244**, **245**), and McCulloch (**168**, **169**, **170**). Spence has shown that a derivation of "hypothesis" behavior during the pre-solution period is possible in the framework of the continuity theory (**240**) as is a derivation of the special type of discrimination learning studied in "transposition" experiments (**242**). According to the noncontinuity theory as it is interpreted today, the animal learning a discrimination changes from one systematic mode of response (hypothesis) to another until the problem is solved but *practice on the unsuccessful hypothesis does not contribute to the learning of the correct association.* This view has been defended primarily by Lashley (**157**, **158**), Kreshvesky (**153**, **154**), and Haire (**97**, **98**, **99**).

Both theories have been supported by important experimental investigations. A survey of the evidence suggests that the argument is still in an inconclusive stage. The crucial experimental question is whether associations are formed during the *pre-solution period* which significantly influence the subsequent establishment of the discrimination. In a typical investigation of this problem, the significance of the stimulus cues is reversed during the pre-solution period; the cue which is to be positive on the test trials is made negative and vice versa. According to the continuity theory such reversal should slow down learning on the test trials, according to the non-continuity theory the reversal should have no effect. The first demonstration of the cumulative effect of training was presented by McCulloch and Pratt (**170**) in their study of weight discrimination by white rats. Preliminary training to the lighter of two weights slowed up learning when the heavier weight was made positive. In a study of visual form discrimination habits of chimpanzees Spence (**241**) similarly found that the establishment of a discrimination was directly dependent on the excitatory strengths of the positive and negative stimuli. The greater the relative number of reinforcements a given stimulus had received in a series of learning tasks, the easier it was to establish a positive response to that stimulus. The correlations obtained were positive, high and significant. Spence also showed that sudden learning (insight) like gradual learning, was positively correlated with the excitatory strengths of the positive and negative stimuli as determined by the number of reinforcements (**243**).

Not all the evidence has been on the side of the continuity theory, however. Using the technique of reversing the positive and negative stimuli during the pre-solution period, Kreshevsky (**154**) found that the reversal did not significantly affect the learning of the correct solution. Lashley (**157**) after repeating Spence's study of form discrimination with rats argued that evidence from positive correlations between number of reinforcements and ease of discrimination learning is inconclusive. He considers the assumption unwarranted that a high correlation between two arrays cannot exist when the values of one are in part determined by chance. In computing his own correlation Lashley omitted all trials showing systematic reaction to position, thereby changing the excitatory values of the stimuli by random measures from 0 to more than 100 percent, and yet the correlations between number of reinforcements and ease of discrimination learning were not reduced but slightly increased! Lashley then offered experimental proof against the assumption that all stimuli acting at the time of a response tend to be associated with that response. Rats who learned to respond in terms of size did not at the same time learn to respond in terms of that shape which was consistently associated with the positive stimulus. "If the animals are given a set to react to one aspect of a stimulus situation . . . large amounts of training do not establish association with other aspects, so long as the original set remains effective for reaching the food" (**157**, p. 259). Instead, Lashley emphasizes the role of perceptual organization and attention in determining which aspects of the stimulus situation will be associated with the response.

The controversy still continues. In 1945 Spence (**245**) published a carefully designed experiment in which he again used the technique of cue reversal. Great care was taken to control all relevant factors including position habit. Cue reversal significantly retarded learning and again led Spence to conclude that the development of an association between a cue and a response is a cumulative process which is independent of systematic response tendencies to other cues during the training period. Discrimination depends on the number of times the animal has been rewarded in the presence of the positive cue.

In spite of Spence's impressive results, the last word has not been spoken. Recently Lashley and Wade (**158**) published a stringent criticism of "neo-Pavlovianism." They singled out for theoretical and experimental attack two assumptions central to "neo-Pavlovianism," i.e., to modern effect theory: (1) that in conditioning all aspects of the stimulus situation are associated with the reaction, and (2) that the effects of reinforcement spread to stimuli other than those present during training (stimulus generalization). Lashley and Wade believe these principles to be contrary to fact. In their experiments, groups of subjects (rats, monkeys) were trained to react positively to a given stimulus. The stimulus would then be opposed in a discrimination experiment to another on the same stimulus dimension. Is some cases, the reaction to the initial stimulus was reinforced, i.e., the initial stimulus remained positive; in other cases, the reaction to the initially positive stimulus was extinguished, i.e., it was made negative in the differential training. The rates at which discriminations could be established under these two conditions were compared. In every case differential training was faster when the initial reaction was extinguished than when it was reinforced! The differences were consistent though not statistically reliable. On the basis of these results Lashley and Wade reassert that in discrimination relatively few, often not more than one, aspects of the stimulus are effective in the choice reaction of the animal. "A definite attribute of the stimulus is 'abstracted' and forms the basis of reaction; other attributes are not sensed at all or disregarded" (**158**, p. 81). Stimulus generalization is ascribed to failure of association. When a subject responds to a stimulus to which he was not originally trained, he does so because of his failure to attend to those characteristics which distinguish the training stimulus from the stimulus to which the reaction is generalized. On the other hand, a subject establishes a differentiation when he forms associations with such aspects of the stimuli as he had not attended to in earlier stages of his training. The burden of the argument is a denial of automatic irradiation and indiscriminate association of stimuli with responses by virtue of sheer temporal proximity to a reinforcing state of affairs.

The crucial conceptual distinction which the field-theoretical approach has introduced is between learning and performance. Though recognizing the logical status of this distinction McGeoch seriously questioned its experimental usefulness. "The only way we can know that learning has occurred is by an observation of successive performances since *learning is a relation between successive performances.* . . . Assertions that motive and effect influence performance but not learning become meaningless in the absence of quantitative demonstration, a demonstration which cannot be made without measurements of performance" (**171**, p. 599). Replying to this criticism, Leeper (**160**) pointed out that different test conditions may result in very different performances following the same learning situation. In strict conformance with McGeoch's view one would then have to conclude that any learning situation results in a large number of "learnings." A much more parsimonious approach consists in the use of such distinct intervening variables as learning and performance, or, in Hull's language, habit-formation and habit-evocation.

Another difficulty which results from the distinction between learning and performance is the conceptual gap which is left between knowledge and action, If learning consists in the building-up of expectations, how is expectation translated into action, and how can the specific character of action be predicted? The field theory predicts what a subject will come to expect but it fails to predict in any specific way what he will do as a result of the expectation. Guthrie, who has long been concerned with the analysis and prediction of particular movements, has expressed this criticism as follows: "Signs, in Tolman's theory, occasion in the rat *realization*, or *cognition*, or *judgment*, or *hypotheses*, or *abstraction*, *but they do not occasion action*" (90, p. 172). Hilgard and Marquis (108) regard this failure to predict specific action as a weakness but also see in it a source of strength. A breadth of interpretation is possible which strict conditioning theories do not allow. A variety of performances can be grouped together in terms of the purpose which they serve without regard to particular details of movement.

Rejection of the Law of Effect as a Basic Principle of Learning or Performance

It is his insistence on an analysis of the learning process in terms of specific stimuli and specific response movements which has led Guthrie (88, 89, 90, 91, 92, 93, 94) to the formulation of a theory in which the law of effect has no place as either a basic principle of learning or of performance. Guthrie distinguishes strictly between acts and movements. An act is a class of movements defined by the *end result*. The law of effect with its emphasis on the *consequences* of S-R connections thus applies primarily to acts. In Guthrie's view, however, it is to movements and not acts that the basic laws of learning must refer. The achievement of an act, i.e., of an effect or end result, is "completely dependent on the acquisition, through learning, of a specific stereotyped movement or set of movements for the accomplishment of the effect that defines the act" (94, p. 51). As to the association of stimuli and response movements, it is entirely explained in terms of one basic principle—association by contiguity. It is always the last movement or set of movements made *simultaneously* with a given stimulus that is repeated on recurrence of the stimulus. Only a single coincidence of stimulus and response is necessary to establish the association. Thus learning is accomplished in one trial but so is unlearning, for one presentation of the stimulus on which the response is not made destroys the association. Although all learning is complete in one trial, the process of acquisition proceeds only gradually because of the continuously changing nature of the components of the stimulus situation. Only when the response movements have been conditioned to the various components of the stimulus situation—exteroceptive, proprioceptive, and interoceptive—can the response be reliably evoked.

Guthrie's description of the basic processes in learning is made independently of the action of rewards and punishments. The practical efficacy of reward and punishment can be satisfactorily accounted for in terms of stimulus-response contiguity. It is always the response last associated with a stimulus which will be repeated when the stimulus recurs. A successful response either removes the organism from the stimulus situation (for example, by allowing escape from a puzzle-box) or so alters it (for example by removal of internal drive-stimuli through feeding) that no new associations with the situation can be formed. Similarly the effectiveness of punishment depends on what it makes the organism *do* in the presence of a stimulus (for example, make withdrawal movements), not on what it makes him feel. It is the function of reinforcing agents to protect the associations made. Effect prevents unlearning and allows the law of recency to operate.

Guthrie's explanation of effect in terms of changed stimulus context has been criticized on the ground that rewards may be effective even though they do not clearly alter the stimulus situation (**169**). Eating a small pellet of food, for example, does not remove the drive stimulus and yet is effective in strengthening an S-R connection. Guthrie believes that even with a small reward the stimulus situation is materially altered since the "annoyance"—the restless, excited behavior which precedes feeding—is removed though the "annoyer" (the drive stimuli maintaining the animal's search for food) persists. Removal of annoyance removes the cues for successful action and thus prevents unlearning (**92**). This reformulation is not entirely conclusive. After eating, the animal may soon again be in an aroused state, the annoyance may return, and previous responses would be expected to be unlearned. "It is obvious that something has changed when the animal ceases to run or to push levers and begins to eat, but a theory must state more precisely just what the change is which guarantees the learning of the prior response if the theory is to be verified experimentally" (**108**, p. 92).

As to experimental verification of the theory, the most outstanding work is that of Guthrie and Horton (**95**) who reported a detailed, well documented study of cats' behavior in a puzzle box. The filmed records of the animals learning to escape from a puzzle box are well in accord with Guthrie's theoretical expectations. The most striking characteristic of the subjects' behavior was its repetitiousness. The movements immediately preceding escape were distinguished by their stereotypy since removal from the situation presumably protected the last association formed. Whatever variability of behavior appeared is explained by changes in the stimulus situations which caused new associations to be established.

More recently, an experimental study based on Guthrie's theory was reported by Seward (**230**) who was interested in testing the "finality theory of reinforcement." He divided his subjects (rats) into two groups. One group was given food upon pressing a bar, the other group was removed from the experimental situation when it had made the response. Though both groups learned to press the bar, the reward group was clearly superior to the removal group. Though removal from the situation protects the last association, the total effectiveness of reward cannot be ascribed to the termination of the situation.

Stimulus-response contiguity theory has been criticized as not easily lending itself to experimental test (**108**, **246**). The continually changing stimulus elements, especially the subjects' own proprioceptive impulses, are not readily controlled and manipulated. Usually changes in the stimulus situation have to be inferred from the failure of a response to be repeated. Such inferences are, of course, circular. The main difficulty lies in the highly specific nature of the stimuli and responses in terms of which the analysis is made. The theory is distinguished by logical and consistent formulation; it exemplifies a coherent account of learning developed without recourse to a law of effect. But it is still greatly in need of experimental verification.

The Role of Effect in Complex Learning

Our survey has shown the pivotal role of the principle of effect in the construction of a systematic theory of learning. Experimental tests of the various theoretical propositions have been conducted almost exclusively in the animal laboratory (Hull, Tolman, Guthrie) or with human subjects in simple rote-learning situations (Thorndike). "Proofs" and "disproofs" of the law of effect in such experiments still leave open the question of what role effect (satisfaction) plays in more complex types of learning. The question is sharpened by two types of observation: on the one hand much learning occurs in the absence of demonstrable drive reduction, and on the other hand satisfying responses sometimes fail to be repeated (**3**). The challenge of complex adult learning which so often seems to defy the principle of effect, can be met in two ways. It is possible, with Mowrer (**185**), to defend the law of effect as a universal principle of learning despite the apparent failure of many learners to obey it—by reformulating the law of effect so as to encompass these more complex forms of learning. On the other hand, one may assert, as Allport has done (**3**, **4**), that the law of effect holds only for animals, small children, mental defectives and in some peripheral phases of adult learning, whereas different principles, such as ego-involvement and active participation, need to be invoked in the analysis of more complex adult learning. Thus, for Allport the law of effect is not a *law* of learning but merely one of the many conditions which may favor learning—a condition, moreover, which applies only to a very circumscribed segment of learning behavior. The controversy between these two views is still in progress and is well exemplified by the recent symposium on "The Ego and the Law of Effect."[22]

In attempting to extend the law of effect to learning which is not motivated by physiological need reduction the concept of *derived* or *secondary* drive plays a central role. A secondary drive is a learned drive. "These secondary drives are acquired on the basis of primary drives, represent elaborations of them, and serve as a facade behind which the functions of the underlying innate drives are hidden" (**177**, p. 19). Under the heading of derived drives come anxiety and anger and such social needs as pride, ambition, and desire for social approval. Miller and Dollard (**177**) suggest that self-induced stimulation is the basis of acquired drives as well as the basis of acquired rewards and purposes. "Most of the responses which are the basis of socially significant acquired drives and acquired rewards are internal responses . . . " (p. 55). In the course of learning, any stimulus cue which acquires the ability to reduce a secondary drive acquires reward value.

Secondary drives are learned but they may also serve as the basis for further learning since the reduction of a secondary drive is a reward which may reinforce S-R connections in accordance with the law of effect just as does the reduction of a physiological drive. By postulation of such derived drives the gap between animal learning and social learning may be bridged and analysis of the latter in terms of stimulus, response, and effect (tension reduction) is made possible. Miller and Dollard's book, *Social Learning and Imitation* (**177**) represents an attempt to apply this analysis to such social behavior as imitation, leadership and crowd action. May (**175**) has attacked the problem of war and peace in a similar conceptual framework, and Whiting (**316**) has applied stimulus-response-effect analysis to the process of socialization in a primitive society. In terms of such analyses, a complex network of acquired drives develops out of the primary biological drives, and it is through the reduction of these acquired drives that the process of socialization operates. When adult learning *seems* to proceed without obvious drive reduction we must search for the derived drives whose satisfaction makes learning possible.

The concept of derived drive has been experimentally fruitful. A series of experiments conducted over the last ten years or so by Mowrer is a notable demonstration of the extent to which this concept has produced testable hypotheses. Mowrer has made a determined effort to explore the implications of the law of effect to their limits and to erect upon the foundations of this law a unitary account of learning which

[22] *Psychol. Rev.*, 1946, **53**, No. 6 (**4**, **185**, **221**).

would have equal applicability to animal learning and to complex social learning. The concept of derived drive appears to be the touchstone of this theoretical edifice. Mowrer has demonstrated, for example, that *preparatory set* or *expectancy* may function as a drive whose reduction serves as a motivating factor in learning (**180**, **183**, **190**). A state of tension or discomfort arises not only from the presence of a basic organic need but also from the anticipation of the recurrence of one or more of these needs. Not only does the *application* of an electric shock result in tension but also the *anticipation* of the shock, and this anticipation constitutes a derived drive. This state of tension is high before the occurrence of the punishment and low immediately thereafter (**183**). Reduction of this anticipatory tension is rewarding. "Other things being equal, the greater the extent of the drop in the expectancy-tension after the occurrence of a stimulus-response sequence the greater the reinforcing or learning-inducing value of this drop" (**183**, p. 38).

Mowrer's stimulus-response analysis of *anxiety* (**181**, **182**, **190**) and *fear* (**189**) is closely related to his conception of expectancy. Anxiety is the (learned) anticipation of a noxious stimulus. This anticipation, which is a source of tension and discomfort and hence acquires drive quality, results in a variety of acts from which are selected and fixated (by the law of effect) those forms of behavior which are most instrumental in the reduction of anxiety. The postulation of anxiety and anxiety-reduction explains the seemingly paradoxical finding that a conditioning procedure which permits *avoidance* of a shock results in better conditioning than a procedure which merely allows escape from the shock or mitigation of the shock (**17**, **190**). Avoidance results in anxiety-reduction which provides reinforcement even though the noxious stimulus itself is not delivered. The effectiveness of avoidance and concomitant anxiety reduction depends in part on the temporal sequence of the noxious stimuli. If shocks are presented at regular temporal intervals, better conditioning results than with irregularly spaced presentation. When stimuli come in a regular order, anxiety mounts to a maximum as the time approaches for presentation of the stimulus, then drops. With irregular presentation, the subjects are "kept in a more or less chronic state of apprehension or suspense" (**182**, p. 510). Mowrer and Lamoreaux have also been able to show that in avoidance conditioning, the CS becomes a source of anxiety whose termination is rewarding in itself independently of the reinforcement provided by avoidance of the shock (**189**). As a result, conditioning is better if the CS (source of anxiety) terminates at the moment when the conditioned response occurs than if it ceases either before or after the response. The fact that the CS in avoidance training becomes itself a source of anxiety and thereby a motivating factor in its own right helps to establish conditioned avoidance responses which are radically different from the responses made to the unconditioned stimulus. Thus some of Mowrer and Lamoreaux's rats were trained to jump in order to terminate the CS whereas the "correct" response to the UcS was running, and vice versa. Mowrer and Lamoreaux conclude that there are two distinct sources of reinforcement in avoidance conditioning, the one tending to strengthen the connection between fear and whatever response reduces the fear and the other which strengthens the connection between fear and whatever response eliminates the situation (shock and fear combined). They believe that the recognition of fear as a secondary motive in learning has important systematic implications for the law of effect. "In this way so-called anticipatory or 'foresightful' behavior can be made to conform very acceptably to the requirements of the Law of Effect, and seemingly 'purposive' or 'teleological,' responses are accounted for well within the framework of scientific causation" (**189**, p. 48).

Even though such intervening variables (secondary drives) as anticipation, anxiety, and fear[23] help to subsume under the law of effect behavior which is seemingly foresightful or purposive, a vexatious problem remains. Why do organisms so frequently persist in behavior which is clearly punishing, or, at least, more punishing than rewarding? It is, of course, possible to argue that whatever behavior subjects persist in must somehow be rewarding or it would be abandoned. Such reasoning would, however, be clearly circular and would beg the very question at issue in discussions of the law of effect. Recently Mowrer and Ullman (**192**) have attacked this problem of "non-integrative" learning. They believe that the key to the riddle of non-integrative learning lies in the temporal pattern of rewards and punishments. Many acts have consequences which are both rewarding and punishing. According to the gradient of reinforcement, those consequences which immediately follow the act will be more influential in learning than temporally remote effects. Integrative learning occurs when remote consequences are through symbolic behavior brought into the psychological present. Non-integrative learning takes place when the organism fails to make the symbolic bridge to the future and remains at the mercy of the im-

[23] Another derived drive is *fatigue*. In extinction, fatigue is the motivation competing with the performance of the act. Not to respond reduces the fatigue and is therefore rewarding. This analysis is supported experimentally by the fact that effortfulness of task is inversely related to the number of extinction responses. Thus the process of extinction is explained in terms of effect of need reduction (**186**).

mediate gradient of reinforcement. It is the failure to react to remote punishing consequences which is the basis for the persistence of behavior in the face of punishment. This analysis holds well for the behavior of Mowrer and Ullman's rats who, indeed, showed only a very limited capacity to learn in terms of temporally remote consequences. But what about non-integrative learning in humans capable of reactions to symbols? In face of this difficulty the authors unfortunately fall back on a question-begging argument: "The fact that certain habits or 'traits of character' may persist in the face of consistent punishment, raises a more difficult problem. One possibility is that punishment, which is observed, is offset by self-administered reward, which is not observed" (192, p. 85). But on this supposition the law of effect is incapable of disproof since whatever learning occurs is *ipso facto* considered the result of reward administered either visibly by others or invisibly and (often unobservably) by the self. Mowrer and Ullman probably sense this difficulty since they admit in the same paper that "there may be more to the problem than this and that the 'strength of the total ego' seems capable in ways which have not yet been clearly analyzed, of being mobilized in support of any single part (habit) for which the going is particularly hard" (p. 85).

In the symposium already referred to, Mowrer (185) has further clarified his view of the relation between the law of effect and "ego-processes." He does not believe that the type of learning which is described as "ego-involved" poses problems which the law of effect cannot answer. For the term "ego-involvement" he would substitute the term "interest"; for "interest" he would in turn substitute "emotional arousal"; and emotional arousal (e.g., fear) is a derived drive whose satisfaction is not in principle different from the reduction of the so-called primary, or biological drives. By such successive reductions Mowrer achieves the unification of all motives which may be effective in learning and staunchly defends the law of effect as the sole true law of learning. "Learning occurs when and only when a drive is reduced, a problem solved, a satisfaction derived, but . . . this satisfaction may stem from the reduction of either a primary or secondary drive" (185, p. 332).[24] It is the mediation of effect through symbolic processes,

[24] In his most recent statement Professor Mowrer (185a) has reversed his position to some extent. He now admits that the law of effect has failed as a universal monistic principle of learning and suggests that it may be necessary to assume two basic learning processes: (1) the processes whereby solutions to problems, i.e., ordinary habits, are acquired and (2) the processes whereby emotional learning or "conditioning" takes place. Habit formation or problem solving is mediated by the law of effect whereas conditioning or emotional learning is conceptualized as a process of "association" independent of

especially self-administered rewards, which distinguishes ego-involved learning from learning which is reinforced by the reduction of physiological needs.

An attempt rather similar to Mowrer's, though less orthodox in formulation, to bridge the gap between ego-psychology and the law of effect was made by Rice (**221**). Rice admits that the law cannot be held if it means that success or satisfaction leads to (a) a perpetuation of specific responses and/or (b) the repeated choice of the same specific goal object. When normal adults repeat one of these two elements, it is usually with a variation in the other, or both response and goal are varied. But even though either specific response sequence or goal or both may be varied, it may still be true that *something* about the activity is repeated, such as the "interest" that is involved. Rice suggests a reformulation of the law of effect. Success or satisfaction does not stamp in a specific stimulus-response connection but it does confirm the learner's *interest* in the general range of problems in which he has been successful. To the old question, *what* is it that success "stamps in," Rice then offers the answer that it is "interest." Interests themselves, even the processes to which the term ego refers, are acquired and perpetuated through the operation of this modified law of effect. Like Mowrer, Rice believes that symbolic processes (self-administered rewards) play an important role in the mediation of the effect. "That core of the act which constitutes the 'interest' is the feature of it which is most likely to be symbolized and repeatedly confirmed through approval of its symbol" (**221** p. 316).

The papers of Mowrer and Rice make it clear that it is *logically* possible to reformulate the law of effect and to describe its operation in such a way as to make it universally applicable to animal and "ego-involved" learning alike. But as Allport's reply (**4**) to these papers shows, the psychologist who is primarily concerned with the analysis of ego-processes cannot be satisfied by such reductions and reformulations. Allport rejects the reduction of ego-processes to emotional arousal (secondary drives). Only certain emotional states are ego-involved and the two states should not be considered identical. Indeed, it may be the lack of emotional arousal which characterizes the smooth functioning of some ego-processes. Any law of learning based on "reduction of

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pleasure or tension reduction. Mowrer also suggests that effect learning is mediated by the central nervous system whereas emotional learning is carried on by the autonomic nervous system. This reformulation considerably curtails the sphere of operation allotted to the law of effect. It remains to be seen whether this particular dichotomization will fare any better than previous attempts at mutually exclusive classifications of the learning process.

emotional tension" can not indiscriminately be applied to learning that proceeds from ego-interests. Allport staunchly maintains that the role of ego-processes is "irreducible." There are other basic phenomena of learning for which effect theory cannot account satisfactorily. The favorable influence of motor activity and active participation resists reduction to pleasure or satisfaction. Above all, the ubiquitous directive action of *interests* must be taken into account in any comprehensive theory of learning; ". . . learning proceeds because it is relevent to an interest system: it adds to knowledge, it differentiates items within the system, it broadens the range of equivalent stimuli . . . . Pleasure attending a single response, or even concatenations of response, is not decisive" (4, p. 346). The capacity to relate environmental events to one's interest system, to discriminate between relevant and irrelevant means, far transcends the operation of effect, however broadly conceived.

In the light of such considerations Allport considers the law of effect as a secondary principle in learning. He believes that satisfaction plays a decisive role only in those organisms and those situations in which ego-processes are not involved. In less mechanical forms of learning satisfaction loses importance and can be maintained only as a question-begging concept. The symbols which are invoked as mediators of self-reward are "vague molecular constructs that taper off into a kind of aimless triviality so far as explanatory power is concerned" (4, p. 344). At best, satisfaction or success serves as an *indicator* to the learner of how well he is adjusting to his problem. But though satisfaction helps the learner perceive the situation he uses this indication in a variable manner according to the pattern of interests that comprise his ego-structure.

Conclusions

In the course of this survey we have touched on learning behavior ranging from conditioned salivation to the development of interest systems in mature adults. To all these types of learning the law of effect has been applied by some, only to be rejected by others. In spite of an ever-growing volume of experimental and theoretical papers, agreement still seems to be a long way off. The golden jubilee of the law of effect will be celebrated in the not-too-distant future, but some of the basic issues for learning theory which the law of effect had hoped to answer still remain:

1. What is the agent responsible for reinforcement? Is it association by contiguity; an OK reaction; the reduction of tension, primary or derived; the satisfaction of an interest; the confirmation of a cognitive expectation; the approval of the ego?

2. What is it that is reinforced? Is it a neural bond; an S-R connection; a perceptual organization; an interest system; the ego?

3. What is the basic mechanism of reinforcement? Is it the lowering of synaptic resistances; cortical irradiation; or are the physiological processes which will find ultimate acceptance still unnamed?

Perhaps there is no one right answer among these alternatives. Probably more than one of these principles and additional ones that remain to be discovered will be needed in an integrated theory of learning. It is safe to say that at the present state of our knowledge the law of effect as a monistic principle explaining all learning has not been substantiated. As *one* of the behavioral facts of learning, it cannot be gainsaid.

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