ON THE CIRCULARITY OF THE LAW OF EFFECT

PAUL E. MEEHL
University of Minnesota

In his recent review on "The History and Present Status of the Law of Effect," Postman (19) lays considerable emphasis on the problem of "circularity" which he sees as crucial in the formulation of the law. He says:

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*. He carefully defines 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." 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 adsurdum* 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. 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 (19, p. 496).

And a little later Postman says:

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 O.K. reaction can be inferred only *ex post facto*, after learning has taken place. But here we are again impaled on the horns of the dilemma of circularity (19, p. 497).

And still further:

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 neohedonistic 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 (19, p. 501).

Throughout Postman's paper this problem is constantly lurking behind the scenes even when the author does not single it out for specific mention. I am in complete agreement with Postman's final remark that "at the present state of our knowledge the law of effect as a monistic principle explaining all learning has not been substantiated," and Postman performs a service by emphasizing this problem of circularity in his discussion of the "law." I am inclined, however, to think that he has settled the question of circularity somewhat too easily, and that his settlement of it has an effect upon much of his argumentation. I gather from the above quotations that Postman looks upon any definition of effect or reinforcement in terms of the resulting change in response strength as "circular," where that word has a pejorative sense. If he is right in this it is very serious. While the law of effect has many difficulties, I do not believe that "circularity" is among them. To show this is the aim of the present paper.

I shall consider the problem of circularity in the law of effect as identical with the problem of circularity in the definition of *reinforcement* in instrumental conditioning. I take it that Postman does the same, since in the first quotation above he cites a passage from Hilgard and Marquis' *Conditioning and Learning*, where the two problems are considered together and with free interchange of the two terminologies. These authors say:

It is apparent that no definition of effect provides an independent measure of the strength of reinforcement. The degree of satisfaction, of complacency, or of tension reduction has not been objectively determined. The strength of reinforcement can be given comprehensive definition only in terms of the amount of learning resulting from it. This is, of course, a circular definition, if strength of reinforcement is to be used as a factor determining degree of learning. A partial escape from circularity is achieved by the fact that a stimulus such as food which is found to be reinforcing in one situation will also be reinforcing in other situations, and with other animals (9, p. 83).

Writing in 1948, however, Hilgard states concerning Thorndike's "operational" definition of satisfiers and annoyers:

> These definitions are not circular, so far as the law of effect is concerned. That is, the states of affairs characterized as satisfying and annoying are specified independently of their influence upon modifiable connections. The law of effect then states what may be expected to happen to preceding modifiable connections which are followed by such specified states. The objection that Thorndike was lacking in objectivity in the statement of the law of effect is not a valid one (8, p. 24).

Hilgard is willing to let the concept of reinforcement (effect, satisfaction, reward) be introduced on the basis of behavior, but only because there are behavioral criteria of seeking and avoiding other than the effect of reinforcement upon *modifiable* connections. Whether this restriction is necessary needs to be considered carefully.

Skinner dismisses the whole problem in two sentences:

> A reinforcing stimulus is defined as such by its power to produce the resulting change. There is no circularity about this; some stimuli are found to produce the change, others not, and they are classified as reinforcing and nonreinforcing accordingly (22, p. 62).

Spence (23) takes essentially the same tack in his recent discussions of secondary reinforcement. The stimuli which impinge upon an organism may be divided, he says, into two classes: those which produce an increment in response strength, and those which do not. It seems from the several preceding quotations that there is a lack of agreement as to whether or not the law of effect or the principle of reinforcement involves an unavoidable circularity, or, if it does not, how circularity is to be avoided. In what follows, I make no claim to originality, since the essence of my development is contained in the previous quotations, together with the work of Tolman. But I feel it worthwhile to bring the arguments together in one context, and to show that the problem merits somewhat more extended treatment than is usually given it. Without claiming to present a definitive solution, I shall indicate the general direction which I believe the solution might take, and in the process introduce certain distinctions and terminological proposals which I feel might clarify our discussion and experimentation.

The Meaning of Circularity

It must be pointed out that there are two meanings of the word "circular" in common use. We have on the one hand circularity in *definition*, in which an unfamiliar term is defined by using other terms which are (directly or ultimately) defined by the term in question. There is no question of circularity in this sense in a definition of the

Skinner-Spence type. Let us accept as a crude preliminary formulation the following: "A reinforcing stimulus is one which increases the subsequent strength of responses which immediately precede it." The words *stimulus*,[1] *strength*, *increase* and *response* are all definable without any reference to the fact or theory of reinforcement. The definitions of these terms, particularly the term "response," present terrible difficulties; but I do not know of anyone who maintains that they involve the notion of reinforcement. Words such as these are current in the vocabulary of many kinds of psychological theorists who do not accept the Law of Effect as a principle of learning and in the absence of any indications to the contrary, I shall assume that we can tell what we mean by them. We can determine empirically when the strength of a response has increased without knowing anything about reinforcing stimuli, drives, satisfactions, and the like. It seems clear that the definition of a reinforcing stimulus in terms of its effect on response strength does not involve circularity in *this* sense.

The other meaning of the word circularity refers not to meanings (definition of terms) but to the establishment of propositions. We speak of *proofs* as being circular if it can be shown that in the process of establishing (proving) a proposition we have made use of the probandum. I am not aware that any responsible theorist has attempted to "prove" the Law of Effect in this way. It is true that those who look upon the law as fundamental are skeptical when they hear of a case of increase of response strength which does not *seem* to involve any obvious reinforcing consequences so that they begin to invent hypotheses to explain the results. There is no harm in this so long as the proposed explanations are in principle confirmable on the basis of some other experimental consequences, however remote. If an animal learns a response sequence without being given food, water, or any of the usual rewards, I suspect most Hullians would begin to talk about secondary reinforcement present in the situation. One can, of course, be careless with this kind of explanation, but there is nothing intrinsic to the concept that entails non-confirmability. The establishment of secondary reinforcing effects as explanations of a given experimental result consists in combining the facts known about primary reinforcers with facts about the animal's life history, in terms of which we understand how certain stimuli have acquired their secondary reinforcing powers. People on both sides of the present controversy over reinforcement theory are performing many different sorts of experiments in

[1] "Stimulus" will be used broadly to include "stimulus change," and stimulus configurations of all degrees of patterning and complexity.

order to confirm or disconfirm the Law of Effect. It would seem that if the law of effect *were* being treated by anyone as a consequence of definition, or established by some hidden assumption of its truth, the experiments would not be going on.

Can "Reinforcement" be Independently Defined?

Nonetheless, when we think about this definition we feel uncomfortable. I do not think we have in mind either a circularity in definition or a begging-the-question fallacy, but some sort of peculiar pseudo-circularity in which it seems to us vaguely that the law *could* be "derived" from the proposed definition, even though no one in fact seems to be trying to do it this way. The problem can be stated very simply: How can we introduce the concept of reinforcement in terms of effect upon strength, and still have a "law of effect" or "principle of reinforcement" which has the empirical content that everybody seems to be taking for granted in experimentation?

1. Suppose we reject the Thorndike-Skinner-Spence procedure of defining reinforcement in terms of response strength, and decide to define the term quite independently of the learning process. The first possibility, which we shall dismiss rather dogmatically, is to do it subjectivistically in terms of pleasure, experiences of satisfaction, and the like. Aside from the general behavioristic objections, and the specific problems of measurement created, this approach is not feasible because it leaves us without any basis for speaking of reinforcing value in the case of that very important class of motivations that are unconscious or at least inadequately verbalized in the human case; and it makes impossible the establishment of reinforcing value in the case of lower organisms. At the present time there are probably very few psychologists who would consider this alternative seriously.

2. Secondly, we might try to define reinforcers in terms of certain physical properties on the stimulus side. I shall attempt to show below that this is a procedure which *follows* the introduction of the generic notion of a reinforcer, and which at a later stage becomes very important. But no one wants to group together an arbitrary class of physical objects or stimuli and call them "reinforcers," since the aim of our concept formation is to make possible the statement of laws. The possibility of identifying common physical properties of that large class of stimuli already grouped together as "rewarding" seems very remote. Besides, we would set up these properties or sets of properties by examining the members of the reinforcing class, which we already have set apart on some basis or other; and the question is: How have we arrived at the members of that class?

3. A third possibility, seen in the work of Hull, is to define reinforcement ultimately in terms of drive reduction, that is, in terms of the inner physiological events involved. Here again, I do not suppose that anyone would be able to give even the vaguest specification of the defining property of all neural events which are reinforcing. Even for the so-called primary physiological needs such as hunger, the evidence as to their exact physiological basis is most

incomplete. No psychologist today is willing to equate "hunger" with "stomach contractions," in the light of the experimentation on visceral denervations, specific sub-hungers, and the like. In other cases, we have practically no information on the neurophysiology, e.g., the neurophysiologic basis of the reinforcing effect of the presence of another organism, the turning off of a light in the Skinner box, or the going through of "exploratory" behavior on the other side of a grill. There is some reason to suppose that certain stimuli retain their secondary reinforcing value in the absence of the primary drive (2, 16), which complicates the problem further.

These considerations force a return to the *effect* of stimuli as a basis for specifying that they are reinforcers, and this leads to the paradox. If we define a reinforcing agent by its effect upon learning, then it seems that whenever learning is effected, we know ("by definition") that we have given a reinforcement. For surely, when the organism behaves, some stimulus change occurs, if nothing else than the proprioceptive effects of responding. If the behavior increases in strength, then these stimulus changes, which were in fact preceded by the response, are reinforcers. Hence, it seems that a definition of reinforcement in terms of an increase of habit strength makes the law tautological and devoid of factual content. This train of thought, which I am sure is familiar to most readers, seems obvious and straightforward. But I believe it can be shown to be mistaken, once the law is stated *explicitly* in the way we all really think of it *implicitly* when we perform experiments or try to explain a given case of learning.

An Empirical Derivation of Reinforcement

Let us begin afresh by going to the behavior itself in a situation in which there is little or no disagreement as to what occurs. Consider a bright, inductively inclined Martian, who had never experienced any needs or satisfactions (except perhaps *n Cognizance*!) and who was observing the behavior of a rat in successive runnings in a T-maze. For the moment we shall simply consider a "standard rat," neglecting the individual differences in parameters and the accidents of personal histories that generate special secondary reinforcing properties. These refinements need to be added later, but as is usually the case will have to be added by being integrated into the whole structure of reinforcement theory, since we cannot treat everything at once. At the beginning, the Martian observes that the organism turns to the right or left with, let us say, about equal frequency. With further trials, a change occurs until finally the rat is responding close to 100% of the time by turning to the right. A Martian could obviously discover this with no notion of rewards, pleasure and the like. If he is ingenious enough to think of the possibility that the strength of a response might be influenced by the events that follow it in time, he would then proceed to investigate the changes that are contingent on this right turning.[2] He notes that when the rat turns to the right he brings about the following states of affairs on the stimulus side which he does not bring about when he turns to the left: He ends up nearer to the right-hand wall, which is painted green; he twists his own body to the right in responding; he ends up in a wooden box having knots in the wood; he ends up nearer the North pole; and to a dynamo on the other side of the campus; and he comes into the presence of a cup of sunflower seeds. These are the stimuli (stimulus changes) which are contingent on right turns. Is it possible that the gradual strengthening of the right turning is dependent upon one, some, or all of these changes following it? Our scientist from Mars would proceed to study a series of standard rats in the situation, altering the above variables systematically by usual inductive procedures. As a matter of empirical fact, he would discover that, within certain very wide limits, alterations in the first five have no effect. The sixth, the sunflower seeds, have a tremendous effect. He finds that he can alter the geographical direction, the direction of the body twist required, the wall color approached, etc.—that he can introduce all manner of modifications in the other factors; and so long as the sunflower seeds are presented, the rat will tend to go to where they are. On the other hand, if the sunflower seeds are omitted, and nothing else put in their place, a preference fails to develop as a function of these remaining differences.

But we have already greatly over-simplified. Actually, the Martian would discover that the effect of finding sunflower seeds in some cases is almost too slight to be detected; furthermore, even after a preference has been acquired, it may on some occasions fail to show itself. Now, it has already been apparent that when he comes upon these sunflower seeds, the rat behaves toward them in a characteristic way, that is, he ingests them. In seeking to understand the variability in the development and manifestation of a preference, one would notice a correlation between the strengthening of a preference and the rate, strength, and consistency of ingestive responses in the presence of the food. Identifying the same rat on successive days, it is found that on those days on which a preference already established broke down, very frequently the ingestive response in the presence of the sunflower seeds was at a very low or even zero strength. Failing to find anything varying in the maze situation itself to account for these differences, one can study the expe-

[2] Actually, no great ingenuity is involved here. Study of the events immediately *preceding* a run, e.g., the manner in which the experimenter handles the rat, what orientation he gives its head in placing it in the entry box, etc., would fail to reveal any systematic factor related to the direction of a preference. Considering this, together with the fact that before any runs have been made no preference exists, the Martian would be led to ask whether it is something that happens *after* the run (or during it) that affects the probability of a similar choice in subsequent runs.

riences of the animals between runs. Here appears a very striking correlate of both preference strength *and* the ingestive response in the maze: that which a human experimenter would call the "feeding schedule." The Martian would observe that when sunflower seeds were made available to the rats in their cages, they behave with respect to them in the same way as they do when they come upon the sunflower seeds in the goal box: namely, with ingestive responses. He would discover, again by systematic variation in these conditions, that such matters as the chemical nature of the substance made available, the periodicity of its availability, the lapse of time between when it was last available and the maze run; the rate of ingestion manifested at the moment of beginning deprivation (i.e., how close the rat was to satiety when interrupted), and so on, all exert an effect upon the maze response. By far the most intimate correlate would be the lapse of time since feeding. To quote Skinner again,

> The problem of drive arises because much of the behavior of an organism shows an apparent variability. A rat does not always respond to food placed before it, and a factor called its "hunger" is invoked by way of explanation. The rat is said to eat only when it is hungry. It is because eating is not inevitable that we are led to hypothesize the internal state to which we may assign the variability. Where there is no variability, no state is needed. . . . In dealing with the kind of behavior that gives rise to the concept of hunger, we are concerned with the strength of a certain class of reflexes and with the two principal operations that affect it—feeding and fasting (22, pp. 341, 343).

For a considerable class of stimuli found to affect choice behavior in the maze, there is a fairly well demarcated class of events in the extra-maze activities which exert an effect. Food, water, a female rat, all depend for their efficacy upon a deprivation schedule of some sort. For other stimuli, the rest of the day's activities seem of less relevance. For example, the effects of turning off a light in the Skinner box upon the lever pressing response would not depend upon a schedule of extra box illumination in any such obvious way as the effects of a food pellet depend upon the extra maze operations of feeding and fasting. Even here, at the extremes, it is likely that the schedule has some effect. Although I know of no experimental material on the point, it would be surprising if rats raised and maintained in a dark or extremely bright living cage would show the same response to light-off as a reinforcing agent. In order to keep the discussion quite general, I shall refer to *schedule-reinforcer* combinations, which will be understood to include those combinations in which almost any life-maintaining schedule is adequate. Whether there are any such does not need to be settled here. The stimulus presented is a *reinforcer*, and the presentation of it (an "event") is a *reinforcement*.

We are now in possession of a rather simple set of empirical facts. A certain stimulus, for a rat which has been under a specified schedule, for instance the sunflower seeds for a rat who has not ingested anything for 23 hours, will exert a strengthening effect. We can formulate a "law" stated crudely as follows: "In a rat which has not recently ingested sunflower seeds, bran mash, Purina chow, etc., a response of turning in a given direction in the T-maze will be increased if the fairly immediate presentation of sunflower seeds, etc., is made contingent upon that response." Similarly, we would find such a specific law to hold for thirst and water, sex and a mate, and so on. The general form of such special laws would be: "On schedule M, the termination of response sequence R, in setting S, by stimulus S^1 is followed by an increment in the strength of S.R." Such a law may be called a *situational-reinforcement* law, where the "reinforcement" is understood to stand for "presentation-of-a-reinforcer-following-a-specified-maintenance-schedule," and the term "situational" covers "response R in situation S."

Actually, in any given case, M, R, S, S^1 are classes. This is indicated by the suspicious-looking "etc." in the first "law" above. There is nothing shady about this "etc.," inasmuch as what is actually involved here is a class of operations and effects which are ultimately to be specified by locating each instance with respect to a whole complex set of dimensions. For example, Guttman (6) shows a relation between concentration of sugar solution used as a reinforcing agent and the strength of the lever pressing response. Heron and Peake (7) have studied protein as a specific component of reinforcement. There is to be discovered a vast number of such rather special laws which are comparable to the myriads of laws in chemistry concerning the solubility of substance Y in substance X and the like.

The next thing to notice is that while the schedule, reinforcement, response, and situation are all classes showing certain relations to one another, in general the schedule and reinforcer are related to one another more intimately than either is to the situation or response. The strength of a response which is maintained by food reinforcement is heavily dependent upon the feeding-fasting schedule, whereas the effect of a food reinforcement upon a response is relatively independent of, say recency of copulatory activity, so that a given schedule-reinforcement pair are "tied" to one another. But the Martian observes that the strengthening effect of a given schedule-reinforcement combination is relatively (not wholly!) neutral with respect to the response we are trying to strengthen and the situation in which we are trying to strengthen it. For a hungry rat, right turning depends heavily upon finding food; for a satiated rat, it depends very little. So the feeding schedule is intimately related to the reinforcing agent's efficacy. However, this "hungry-food" schedule-reinforcement combination seems to be capable of strengthening chain-pulling, lever-pressing, wheel-turning, marble-rolling, gnawing-through-paper, and so on through a very wide range of behaviors differing greatly in their topography and in their stimulus conditions. This leads to the question, will a certain schedule-reinforcer combination increase the strength of *any* response, in *any* setting?" This question turns out empirically to be answered in the negative, since we find at least three limitations upon the generality of a schedule-reinforcer combination as response strengthener. Leaving out the trivial case in which the response is anatomically impossible, e.g., to teach an elephant to thread a needle, we find:

1. No situation-response sequences may involve stimulus dimensions which are not discriminable by the organism. (Tolman's "discriminating capacities").
2. Some response sequences seem on the basis of their sequence, timing, or "complexity" not to be learnable by members of a given species, or subgroups within a species. It appears impossible to teach a rat a quintuple alternation problem, or to teach a human moron integral calculus.
3. There are cases in which the response we wish to strengthen is incompatible with responses at a very high (and relatively unmodifiable) strength under the schedule-stimulus combinations we are employing. For example, it would probably be next to impossible to teach a very hungry cat to carry a piece of fresh liver across the room, deposit it in a box, and return to receive food as a reinforcement. "Defensive" and "anxiety-related" responses are among the most important examples of this case.

How do we discover what responses have these characteristics? Experimentally, as we discover anything else. Let us call a situation-response combination having none of these properties *learnable.* A positive definition will be given below. What we find is that whereas learnable responses seem to differ somewhat in their "readiness" under different schedule-reinforcement combinations, this is a matter of parameters and does not invalidate the following tentative "law," which is stated qualitatively: "Any learnable response will be strengthened by sunflower seeds as a reinforcer." The general form of such a law is "the stimulus S^1 on schedule M will increase the strength of any learnable response." I shall call such a law a *trans-situational reinforcement* law. It must be noted carefully that such a law is still about a *particular* reinforcing agent, having, to be sure, a class character; but the particular reinforcing agent (and its associated necessary schedule, if any) is no longer tied to the response sequence first studied. The reinforcing property of sunflower seeds was noted first in the T-maze. The Martian will discover that white rats *can* learn to pull chains, press levers, and roll marbles. He finds that these learnable responses can also be strengthened by making the feeding of sunflower seeds contingent upon them. He makes the inductive generalization that sunflower seeds would exert this effect upon all learnable responses in the rat.

He now asks the obvious question: Are all schedule-reinforcer combinations like this? That is to say, when we study a new schedule-reinforcer combination and find it strengthens a response, can we assume that it will increase the strength of all learnable responses?

Naturally, our confidence in the general reinforcing power of any particular one will increase as we try it out on more and more learnable responses. But we do not know whether a higher-order inductive statement is justified, so long as we study sunflower seeds only or study several kinds of agents but in only one situation each.

Having found a particular reinforcer in a particular situation, we have discovered that it is trans-situational. Next we discover that all of the reinforcers that we have investigated have turned out to be trans-situational. The next induction is, "If a learnable response is followed by a stimulus which is known to be a reinforcer of learnable responses the strength will increase." A shorter way of saying this, having first defined a reinforcer as "a stimulus which will increase the strength of at least one learnable response," is simply: *all reinforcers are trans-situational.* Nothing is said as to the *amount* of strengthening. It is sufficient, in order to demonstrate the trans-situational character of a reinforcing agent, to show that it produces an increment in strength. If equal increments were required, it is probable that very few (if any) reinforcers would be trans-situational because of the varying behavior readinesses and different parameters of habit acquisitions from one drive to another and from one situation to another.

This assertion, that all reinforcers are trans-situational, I propose to call the *Weak Law of Effect.* It is not our problem in this paper to discuss whether the Weak Law of Effect holds or not. A "proof" of the Weak Law of Effect consists, as usual, of establishing inductively many instances of it in a variety of situations with our confidence increasing on the basis of the usual inductive canons. A "disproof" of the Weak Law of Effect would involve showing that a certain stimulus change acts as a reinforcing agent for one response, i.e., that the presentation of this stimulus following the response will increase the latter's strength; but that another response, previously established as learnable, cannot be strengthened by a presentation of this agent. A failure of the Weak Law of Effect to hold strictly would not be particularly serious, since one could (at the very least!) specify the exceptions and would hope to be able to generalize about them, that is, to discover empirically what are the kinds of reinforcers, or kinds of differences among situations, which reveal its invalidity. Actually, here again we have a case in which the law is stated in a qualitative all-or-none form; but the development of a science of behavior would eventually result in substituting a multiplicity of laws indicating the extent to which the reinforcing (strengthening) property generalized over various dimensions of the stimulus side, the reinforcing agent, and the "required" response properties. Assuming the Weak Law of Effect to have been established inductively, where are we now in our development? We have specific situation-reinforcer laws which state that a given stimulus is a reinforcing agent for a specified kind of response in a specified situation. As an example, we discover that for a standard rat, sunflower seeds will strengthen right turning in the T-maze. Having established several such specific situation-reinforcer laws, we find it convenient to introduce a definition, saying that a situational reinforcer is a stimulus which occurs as a term in such a specific situation-reinforcer law. Sunflower seeds are hence situational reinforcers. This definition is "arbitrary" or "conventional" in the usual sense, but clearly leads to no circularity. We cannot tell from the definition whether or not there is such a thing as a situational reinforcer, just as we cannot tell from the definition of a unicorn or of the phrase "King of France" whether such a thing exists. All we stipulate in the definition is that if a thing having certain properties turns out to exist, we will call it by this name. That there are situational reinforcers, that is to say, that we can find stimuli that do increase the strength of responses in a certain situation, is an empirical result. It is obvious that the specific situation-reinforcer laws have a perfectly good factual content (e.g., each such law could be false) in spite of the conventional character of the definition.

If our science contained nothing but a collection of such situational-reinforcer laws, we would still be in possession of valuable information. But we discover inductively that we can actually say more than this. For any given reinforcer, we discover that it can in fact be used to increase the strength of responses differing very greatly in topography from the one which originally led us to infer that it was a reinforcer, and in very different stimulating fields. It is true that there are a few special cases, as our cat with the liver, in which we cannot increase the strength of a *kind* of a response (carrying an object from one place to another) which we know from independent study this species is able to learn. But in all such cases we are able to specify an interfering response at such high strength that the behavior in question does not get a chance to be emitted, and hence cannot be reinforced. With this exception, we are able to say that a given reinforcer will increase the strength of all learnable responses of the species; although there will be quantitative differences which remain to be discovered and generalized about after much painstaking experimentation. We define a reinforcer which is of this sort as trans-situational, and from a study of numerous reinforcers we conclude that they are all of this type. The second order induction that all reinforcers are trans-situational (the Weak Law of Effect) is then made.

This last is certainly a very rich and powerful induction. It is true that to make predictions we must study at least one learnable response in order to find out whether a given stimulus change is reinforcing, and we must know for any contemplated response whether it is learnable. Experience with a given species need not be too extensive in order to get a general idea of the kinds of behavior which are possible and learnable; and once having this, we proceed to strengthen responses by means of reinforcing agents which have never been utilized before in connection with these responses. This is so commonplace that we are likely to underestimate its theoretical significance. So far as I know, no animal psychologist has the least hesitation in utilizing any of a very large class of reinforcing objects called "food" in experimentation upon practically any kind of behavior which he is interested in studying. Should he find a failure of response strength to increase, the chances of his asking what is wrong with the food are negligible. His inductive confidence in the Weak Law of Effect is such that he will immediately begin to investigate what is wrong with the stimulus field, or what requirements concerning the response properties he has imposed which transcend the powers of the organism. I am stressing this point because there is a tendency to say that since we have to study the effects upon strength in order to know whether an agent is reinforcing, we do not really "know anything" when we have enunciated the Law of Effect. I think it should be obvious from the diversity of both the class called learnable and the class of agents called reinforcing that to the extent that this law holds almost without exception, when we have enunciated it we have said a great deal.

The man from Mars might be tempted here to take a final step which would be suggested by the ubiquity of the manifestations of the Weak Law of Effect. It might occur to him that the great majority of the instances in which changes in response strength occur seem to involve the operation of the Weak Law, i.e., the presentation of a member of the reinforcing class. Perhaps it is not only true that any learnable response can be strengthened by the presentation of a trans-situational reinforcer but may it not be that this is the *only* way to increase the strength of responses (by learning)? Response strength may be increased by surgical and drug procedures, and also by maturation; but the demarcation of learning as a very general mode of response change, while it presents difficult problems, need not concern us here. Assuming that we have some satisfactory basis for distinguishing an increase in the strength which is based upon "experience" rather than upon interference with the reaction mechanism or biological growth determined by genetic factors given minimal (viable) environments, we may ask whether learning takes place on any *other* basis than the Weak Law of Effect. Certain apparent exceptions to this statement of reinforcement as a necessary condition would appear, but the Martian might ask whether these exceptions are more apparent than real. The formulation of such a law would run something like this: "Every learned increment in response strength requires the operation of a trans-situational reinforcer." I shall designate this rash inductive leap as the *Strong Law of Effect.*

It appears obvious that this also is a statement far from being experimentally empty or in any sense a consequence of definition. I have heard psychologists translate the statement "he learns because he was reinforced" as being tantamount to "he learns because he learns." Postman suggests the same kind of thing in the first quotation above. This is too easy. The expanded form which I suspect everyone has implicitly in mind when he talks about the Strong Law of Effect is: "He learns following the presentation of a stimulus change which for this species has the property of increasing response strength; and, other things being equal in the present setting, if this change had *not* occurred he would not have learned." Such a statement can clearly be false to fact, either because no such trans-situational reinforcer can be shown to have been present, or because the same learning can be shown to be producible without it in the present setting. The claim of the reinforcement theorist to explanation is (at this stage of our knowledge) of exactly the same character as "he developed these symptoms because he was invaded by the Koch bacillus, and we know that the Koch bacillus has these effects." This is not a very *detailed* explanation, because the intermediate or micro-details of the causal sequence are not given; but it is certainly neither factually empty nor trivial.

In our initial quotation from Postman, we find him saying, "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." The trouble with this remark lies in the ambiguity of the phrase "*a* learning experiment." That we cannot know what is reinforcing without having done *some* experimentation is obvious, and is just as it should be in an empirical science. But once having found that a certain state of affairs *is* reinforcing for a given species, there is no reason why a given case of learning cannot be explained by invoking the occurrence of this state of affairs as a causal condition. The definition of force does not entail the truth of Hooke's law. It is only by an experiment that we find out that strain is proportional to stress. Once having found it out, we are all quite comfortable in utilizing Hooke's law to account for the particular cases we come across. I am confident that Postman would not be disturbed if in answer to the question, "Why does that door close all the time?" someone were to reply, "Because it has a spring attached to it on the other side." There is no more "circularity" in this kind of causal accounting than in any other kind. It is perfectly true that this kind of "lowest-order" explanation is not very intellectually satisfying in some cases, although even here there is a considerable variability depending upon our familiarity with the situation. For a detailed consideration of these problems by more qualified persons I refer the reader to papers by Hospers (10), Feigl (4, 5), and Pratt (20).

I think it is obvious that this is the way we think of the Law of Effect, whatever we may think as to its truth. When an apparent case of learning in the absence of reinforcement occurs, those who are interested in preserving the status of the Law of Effect (in my terminology, in preserving the status of the *Strong* Law of Effect) begin to search for changes following the response which can be shown to be of the reinforcing sort. They do not simply look for *any* stimulus change and insist ("by definition") that it is a reinforcement. The statement that a given case of apparently non-reinforcement learning is actually based upon secondary reinforcement is essentially a claim that some stimulus change can be shown to have followed the strengthened response, and that this stimulus change has (still earlier) been put in temporal contiguity with a stimulus change of which we know, from a *diversity* of situations, that it exerts a reinforcing effect.

Abandoning the charge of circularity, a critic might offer a "practical" criticism, saying, "What good does it do to know that a reinforcer strengthens, when the only way to tell when something is a reinforcer is to see if it strengthens?" The trouble here lies in the vagueness, since the *generality* is not indicated, and this failure to indicate generality neglects the usual advantages of induction. That a describable state of affairs *is* reinforcing can only be found out, to be sure, by experimenting on some organisms utilizing *some* learnable response. But it is not required (if the Weak Law of Effect is true) that we, so to speak, start afresh with each new organism of the species and each new response. As a matter of fact, after we have considerable experience with a given species, we can generalize about the physical properties of a stimulus class. So that finally "food" means many substances which may never yet have been tried in a learning situation, and may never have been presented in natural circumstances to the members of a particular species. Wild rats do not eat Purina Chow. Here we begin to approach inductively one of the previously rejected bases of defining reinforcement, namely, the physical character of the stimulus change itself. To ask for a definition of reinforcers which will tell us beforehand for a given species which objects or stimuli will exert the reinforcing effect is to ask that a definition should tell us what the world is like before we investigate it, which is not possible in any science. It happens that the psychologist is worse off than others, because species differences, individual hereditary differences, and differences of the reactional biography make a larger mass of facts necessary in order to know whether a given agent will reinforce a particular organism. But at worst the Weak Law of Effect in conjunction with its member laws is far from useless. When I know inductively that all non-toxic substances containing sugar will act as reinforcers for organisms from rat to man and therefore that I can almost certainly strengthen all responses learnable by any of these species on the basis of the presentation of any of these substances, I know a great deal and my science has a very considerable predictive power.

An Analogous Problem in Physics

It is instructive to consider a somewhat analogous problem in physics, in the definition of "force." Once mass has been defined by some such artifice as Mach's acceleration-ratio technique, and acceleration defined in terms of time and distance, Newton's second law is a *definition* of force. I neglect here other attempts to introduce the notion such as the "school of the thread" (**18**), utilizing Hooke's law in the form of a definition rather than a law, or its modern variants, e.g., Keenan's (**13**) recent effort. Force is "that which accelerates mass." Mach's introduction of the concept of mass was somewhat disturbing to certain of his contemporaries because of a suggested circularity. Mach saw that it was the *inertial* character of mass, rather than "weight" or "quantity of matter" which was crucial in setting up the definition of force. Accordingly, he proceeds as follows:

a. *Experimental Proposition.* Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary *accelerations* in the direction of their line of junction. (The principle of inertia is included in this.)

b. *Definition.* The mass-ratio of any two bodies is the negative inverse ratio of the mutually induced accelerations of those bodies.

c. *Experimental Proposition.* The mass-ratios of bodies are independent of the character of the physical states (of the bodies) that condition the mutual accelerations produced, be those states electrical, magnetic, or what not; and they remain, moreover, the same, whether they are mediately or immediately arrived at.

d. *Experimental Proposition.* The accelerations which any number of bodies A, B, C. . . . induce in a body K, are independent of each other. (The principle of the parallelogram of forces follows immediately from this.)

e. *Definition.* Moving force is the product of the mass-value of a body into the acceleration induced in that body. Then the remaining arbitrary definitions of the algebraical expressions "momentum," "vis viva," and the like, might follow. But these are by no means indispensable. The propositions above set forth satisfy the requirements of simplicity and parsimony which on economico-scientific grounds, must be exacted of them. They are, moreover, obvious and clear; for no doubt can exist with respect to any one of them either concerning its meaning or its source; and we always know whether it asserts an experience or an arbitrary convention (**17**, pp. 243–244).

In the appendix to the second English edition, Mach replies to critics of this procedure as follows:

> A special difficulty seems to be still found in accepting my definition of mass. Streintz has remarked in criticism of it that it is based solely upon gravity, although this was expressly excluded in my first formulation of the definition (1868). Nevertheless, this criticism is again and again put forward, and quite recently even by Volkmann. My definition simply takes note of the fact that bodies in mutual relationship, whether it be that of action at a distance, so called, or whether rigid or elastic connexions be considered, determine in one another changes of velocity (accelerations). More than this, one does not need to know in order to be able to form a definition with perfect assurance and without the fear of building on sand. It is not correct as Höfler asserts, that this definition tacitly assumes *one and the same force* acting on both masses. It does not assume even the notion of force, since the latter is built up subsequently upon the notion of mass, and gives then the principle of action and reaction quite independently and without falling into Newton's logical error. In this arrangement one concept is not misplaced and made to rest on another which threatens to give way under it (**17**, pp. 558–559).

It is obvious that Mach defines mass in the way he does *so that* the definition of force by $F=ma$ will lead to the kinds of laws we want. That is, a previous "knowledge" of the law of gravity based upon a cruder notion of mass is involved historically in the formulation of such a definition. But the crucial point is that it is involved only in the context of discovery, not in the context of justification (**21**, pp. 6–7). There is nothing wrong with making use of any notions, including vague anthropomorphic experiences of pleasure, in deciding how we shall formulate definitions, since our aim is to erect concepts and constructs which will fit into the most convenient and powerful system of laws. The point is that we wish to come out with explicit notions that are free of this vagueness and which do not require any notions which cannot be finally introduced objectively. There is probably a remnant of hedonism in the thinking of the most sophisticated contemporary reinforcement theorists, and there is no reason why anybody should pretend that when he talks about rewards he does not have some faint component in his thinking which involves the projection of such pleasure-pain experiences. But this does not mean that these notions are made part of the scientific structure he erects, in the sense that either the definitions of terms or the establishment of the laws requires such associated imagery in his readers. I suggest that Thorndike's critics are in the same position as Mach's.

One might ask, why would a physicist be upset should he attend a spiritualist seance and find tumblers leaping off tables and floating through the air? If the concept of force is given simply by the relation

$F = ma$, then, if a glass tumbler undergoes an acceleration, a force must act and his definition assures him that the physical world will not surprise him. I do not think the answer to this question is far to seek. While it is admittedly a question of decision, I doubt that most physicists would decide to say that an acceleration occurred in the absence of a force. If the genuineness of the phenomenon were satisfactorily established, I do not think there would be a re-definition of the *concept* of force, but rather that the existence of "forces" on other bases than those previously known would be assumed. That is, the physicist would not say "here is a case of acceleration without a force," but he would rather say "here is a case of force not arising from the usual mechanical, gravitational, or electro-magnetic situations which I have thought, up to now, were the sole bases on which forces came into being." It is certainly no criticism of a Newtonian definition of force (I leave out the fact that Newton, while he defined force in this way, apparently also treated his second law as one of empirical content) to say that having thus defined force you cannot know beforehand what are the conditions in the world under which forces will appear. The mechanical forces involved in direct contact, the force of gravity, and certain electrostatic and magnetic forces were known to Newton. There is nothing about his definition of force which tells us that a peculiarly directed force will exist between a wire carrying an electric current and a compass needle, nor that attracting or repelling forces will exist between parallel wires each of which carries a current. The discovery of these conditions under which forces exist was an empirical contribution of Oersted and Ampere.

Similarly, the psychologist defines what is meant by a reinforcer, and proceeds to search for the agents that fall under this definition. There are undoubtedly kinds of stimulus changes of which we are as yet unaware which will turn out to have the reinforcing property. Dr. Wilse Webb (personal communication) has found in preliminary experiments that at least in one kind of Skinner box the click produced by the operation of an empty magazine will exert a reinforcing effect in an animal whose experience has never given this stimulus an opportunity to acquire secondary reinforcing properties. This is surprising to us. What are the conditions under which this will occur? Suppose it should be found that almost *any* stimulus change within a fairly wide range (avoiding extreme intensities which are anxiety-producing) would exert a slight reinforcing effect in the Skinner box or in any similar apparatus in which there is a considerable stimulus restriction and a marked constancy in the homogeneity of the visual and auditory fields. It might be discovered that when a member of this species has remained in such a homogeneous field for a period of time, stimulus *changes* (not otherwise specified) exert a reinforcing effect. Maybe the rat is "bored" and just likes to make something happen! A difficult notion to nail down experimentally, to be sure. But its complexity and the number of things to be ruled out, does not take it out of the realm of the confirmable.

Let us consider a very extreme case. Suppose in the T-maze situation a systematic increase in the strength of the right turn should be discovered for a standard rat. Suppose that the most thoroughgoing, exhaustive manipulation of the external effect of right-turning should fail to reveal any condition necessary for the effect. "No member of the reinforcing class is to be found." I think that at this point we would begin reluctantly to consider a reinforcing property of the response itself. Perhaps turning to the right is inherently reinforcing to this species. It seems, for instance, that "fetching" behavior in certain species of dogs is self-reinforcing (or at least that it has a biologically replenished reserve). The only reason for calling right-turning "self-reinforcing" rather than simply saying that it is a response of innately high strength in the species is that a *change* in strength occurs with successive runs, otherwise "turning to the right" is simply a kind of tropism. Is the "self-reinforcing" idea factually empty? Although many people would disagree with me at this point, I do not think it is. But it has factual meaning only intradermally. There is no reason why we could not study the proprioceptive effects of a right turn and find out whether, if they are cut out, the increase in response strength continues to occur. In principle we could create the proprioceptive effects of a right turn by artificial means and on that basis strengthen a topographically different response such as lifting the fore paw, wiggling the whiskers, or the like. Here there are difficulties, but I would be prepared to argue that in principle the self-reinforcing effect of right-turning is an empirically meaningful notion.

An interesting side-light is that even the Strong Law of Effect is, as stated, compatible with the latent learning experiments. I am not interested in avoiding the consequences of those experiments by shrewd dialectics, but in the interests of clarity it should be pointed out that in, e.g., the Blodgett design, the big drop in errors *does* follow a reinforcement. So long as the Strong Law of Effect is stated qualitatively and does not explicitly mention amounts and times, it would be admittedly difficult to design an experiment in which it could be refuted. A neo-Hullian interested for some reason in preserving the Strong Law of Effect might simply add a quantitative postulate. He might assume that when a response undergoes an increment in strength on the basis of a minimally reinforcing agent (that is, one in which the asymptote of the acquisition of habit strength is relatively low), then, if subsequently a strong reinforcement is introduced, the parameter in the new growth function which determines the rate of approach to the new asymptote is greater than it would have been without the original learning. Since in the Blodgett design there is evidence of acquisition of differential habit strengths during the latent phase, such a postulate would lead to a preservation of the Strong Law of Effect. The main reason that we are concerned to deal with latent learning material of the Blodgett type is that in the reinforcement theory as now formulated, the effect of a reinforcer is implicitly assumed to operate immediately.

Relationship of Reinforcement to Drive

Perhaps a comment is needed on the way in which reinforcement has been treated here as the primary notion whereas drive, need, or demand is defined in terms of it. I do not mean to imply that need or drive is not the more "basic" factor, if by this is meant that what is a reinforcer or what acquires reinforcing properties depends upon a certain relevance to need. But this manner of speaking refers to the causal reconstruction of behavior, and reverses the epistemological order. The needs of an organism are inferred from changes in behavior strength as a function of certain states of affairs. That is to say, we "get a fix" on a need by being able to induce the chief defining properties of those states of affairs to which behavior is shown to tend. I do not see how there is any possibility in proceeding otherwise at the level of molar behavior. Whether it will be feasible or desirable to hypothesize a kind of state called need in the case of all reinforcers is a moot point at present. I gather that Hull would argue it will, whereas Skinner would argue it will not. One can consider a sort of continuum of reinforcing states of affairs at one end of which it is most easy and natural and obviously very useful to speak in terms of a need, e.g., the case of food or water; whereas at the other end, e.g., the reinforcing effects of hearing a click or turning off a light, the notion of needs seems relatively less appropriate. But the *causal* primacy of needs in our final reconstruction of behavior laws must not be confused with the epistemological status of needs, i.e., the operations by which we arrive at a conception of the needs. Whether the reduction of need is a necessary condition for learning is a question that is not involved in my formulation of either the Weak or the Strong Law of Effect since need-reduction is not equated to reinforcement. This independence of the notions of reinforcement and need-reduction is seen not only in the question of whether need-reduction is (for a sophisticated organism) a necessary condition for reinforcing effect, but it is the intention of these definitions to leave it an open question as to whether a kind of event called need-reduction is involved in reinforcing effects at any stage. The alternative to this is to exhaust completely the concept of need by defining an intervening variable via a class of reinforcing agents, i.e., the organism's "need" is not specified in any way except to say that it is "whatever state" within the organism is involved in the reinforcing effect of a stimulus change known experimentally to exert such an effect. In this case, of course, a rat may be said to have a "need" to keep the light off, to be with another rat, to hear a sound, etc. Whether this is a desirable way of speaking we need not consider here.

In the preceding developments, I have avoided consideration of refinements which would be necessary to complete the theoretical picture. The most important of these is the apparent exception to the Weak Law of Effect in which a change in strength does not occur in spite of the presentation of a known reinforcing agent because certain other dominant factors are at work. As an example, we may consider the "fixation" of a response which is followed by anxiety reduction to the point that an opposing response consistently reinforced with food fails to develop an increase in strength. In any particular situation it is the task of experimental analysis to show what the relations are; as a nice example of this I may refer to the recent work of Farber (3). Of course, if the response does not have sufficient opportunity to *occur*, be reinforced, and hence develop strength, the Weak Law of Effect is not violated. Those cases in which this is not an adequate explanation must be dealt with by considering the opposing forces, leaving open the question as to whether these opposing forces can themselves be satisfactorily subsumed under the Strong Law of Effect. The case here is not essentially different from the case in mechanics where we introduce the concept of force as a dynamic concept (that is, by accelerations produced) and subsequently apply the same notions to systems which are in equilibrium. In physics, one makes use of the laws about force which are based upon the dynamical notion of it in order to explain those cases in statics in which no motion results. Whereas the detailed reconstruction of the causal system remains as a task for the future, I do not believe there are any fundamental logical difficulties involved in the notion that a reinforcing state of affairs is initially defined by an increase in strength, and subsequently the failure of such a state of affairs to exert the effect is explained in terms of the occurrence of other operations or states which oppose it.

Summary

Let me conclude by summarizing the development, using Mach as a model. For convenience I neglect here the specification of a schedule:

a. *Experimental Proposition:* In the rat, if turning to the right in the T-maze is followed by the presentation of sunflower seeds, the strength of the right-turning response will increase. (A situational-reinforcer law.)

b. *Definition:* A stimulus or stimulus change which occurs as the strengthening condition in a situational-reinforcer law is a *reinforcer.*

This empirical law together with the above definition enables us now to assert (as an empirical statement) "sunflower seeds are a reinforcer." The empirical content of this is that there is at least one response which the presentation of sunflower seeds will strengthen.

The presentation of a reinforcer is called *reinforcement.*

c. *Definition:* If the strength of a response may be increased as a function of behavior in an exposure to a situation (rather than by surgical, drug, or maturational changes), such a response is *learnable* by the organism. No reference to reinforcement is made here; we simply require that response strength be shown to increase following "experience," of whatever sort.

d. *Experimental Propositions:* Following suitable manipulation of their experiences, rats will show increases in the strength of pressing levers, pulling chains, rolling marbles, turning to the right at certain choice points, gnawing through paper, digging through sawdust, turning wheels, etc. (Expanded, this would consist simply in a long list of specific "laws" asserting the learnability of certain response classes.)

e. *Experimental Propositions:* Sunflower seeds may be used to strengthen lever pressing, chain pulling, etc. In general, sunflower seeds may be used to strengthen all learnable responses in the rat. (This asserts the generality of the reinforcing effect of sunflower seeds and is what I am calling a trans-situational reinforcer law.)

f. *Definition:* A trans-situational reinforcer is a stimulus which will strengthen all learnable responses. (We have already defined reinforcer so that it does not commit us to its generality, that is, a reinforcer is *at least* a situational reinforcer. If there are any reinforcers which exert the reinforcing effect upon all learnable responses, they are trans-situational). This definition with the immediately preceding experimental propositions enables us to say, "Sunflower seeds are a trans-situational reinforcer."

Such a collection of specific empirical laws in combination with the above general definition leads to a large set of laws such as these last stated ones so that in the end we find the following:

g. *Experimental Proposition:* All reinforcers are trans-situational. (The Weak Law of Effect.)

h. *Experimental Proposition:* Every increment in strength involves a trans-situational reinforcer. (The Strong Law of Effect.)

It seems clear that in the above sequence both the definitional and the factual (empirical) elements are present, and in a simple, commonplace form. The definitional and conventional elements appear in the specification of the circumstances under which a stimulus is to be called "reinforcing." Such a stipulation, however, cannot tell us whether any such stimuli exist. That they do exist, which no one doubts, is an empirical finding; and the numerous statements about them constitute situational-reinforcer laws which are in a sense the special "sub-laws" of effect. These are related to the Weak Law of Effect somewhat in the same way that the particular empirical laws about the properties of bromine, fluorine, chlorine, and so on, are related to the Periodic Law. That the stimuli which occur in the situational-reinforcer laws have a generality of their reinforcing power is also an empirical finding, at present less well established (the Weak Law of Effect). That all cases of learning require certain time relationships to the presentation of such general reinforcers is yet a further factual claim, at present very much in dispute (the Strong Law of Effect).

I can see no reason why any theorist, whatever his position, should find the preceding treatment objectionable as an explication of the Law of Effect. I do not see any way in which the Strong Law of Effect, which is after all the big contemporary issue, has been surreptitiously put into the definitions in such a way that what is intended as an empirical proposition is effectively made a consequence of our use of words. The status of the Strong Law of Effect and even to some extent the Weak Law is presently in doubt. Further, some of the words used in these definitions, e.g., the word "response," are difficult to define in a way that makes them behave in the total system as we wish them to. I have not tried to deal with all these problems at once, but I hope that there are no difficulties springing from the problem of circularity which have not been met. That it is difficult to untangle the learning sequence which has given the reinforcing property to some states of affairs, particularly in the human organism, is admitted by everyone. That a large amount of detailed work of the "botanizing" type, not particularly ego-rewarding, needs to be done before the special sub-laws of effect are stated in terms of quantitative relations is quite clear. Finally, it would be very nice if in some magical way we could *know* before studying a given species exactly what stimulus changes would have the reinforcing property; but I have tried to indicate that this is an essentially irrational demand. In the light of the previous analysis I think the burden of proof is upon those who look upon a sophisticated formulation of the Law of Effect as circular, in either of the ordinary uses of that word.

BIBLIOGRAPHY

1. CARR, H. A., *et al.* The Law of Effect: a roundtable discussion. *Psychol. Rev.*, 1938, **45**, 191–218.
2. ESTES, W. K. A study of motivating conditions necessary for secondary reinforcement. *Amer. Psychologist*, 1948, **3**, 240–241. (Abstract.)
3. FARBER, I. E. Response fixation under anxiety and non-anxiety conditions. *J. exp. Psychol.*, 1948, **38**, 111–131.
4. FEIGL, H. Operationism and scientific method. *Psychol. Rev.*, 1945, **52**, 250–259.
5. FEIGL, H. Some remarks on the meaning of scientific explanation. In H. Feigl & W. Sellars., *Readings in philosophical analysis*. New York: Appleton-Century-Crofts, 1949. Pp. 510–514.
6. GUTTMAN, N. On the relationship between resistance to extinction of a bar-pressing response and concentration of reinforcing agent. Paper presented at the meeting of the Midwestern Psychological Association, Chicago, Ill., April 29, 1949.
7. HERON, W. T., & PEAKE, E. Qualitative food deficiency as a drive in a discrimination problem. *J. comp. physiol. Psychol.*, 1949, **42**, 143–147.
8. HILGARD, E. R. *Theories of learning*. New York: Appleton-Century-Crofts, 1948.
9. HILGARD, E. R., & MARQUIS, D. G. *Conditioning and learning*. New York: Appleton-Century, 1940.
10. HOSPERS, J. On explanation. *J. Philos.*, 1946, **43**, 337–356.
11. HULL, C. L. Thorndike's *Fundamentals of learning*. *Psychol. Bull.*, 1935, **32**, 807–823.
12. HULL, C. L. *Principles of behavior*. New York: Appleton-Century, 1943.
13. KEENAN, J. Definitions and principles of dynamics. *Sci. Mon., N. Y.*, 1948, **67**, 406–414.
14. LENZEN, V. F. *The nature of physical theory*. New York: John Wiley, 1931.
15. LINDSAY, R. B., & MARGENAU, H. *Foundations of physics*. New York: John Wiley, 1936.
16. MACCORQUODALE, K., & MEEHL, P. E. "Cognitive" learning in the absence of competition of incentives. *J. comp. physiol. Psychol.*, 1949, **42**, 383–390.
17. MAÇH, E. *The science of mechanics* (Transl. by T. J. McCormack). Second English Ed. Chicago: Open Court Publishing Co., 1902.
18. POINCARÉ, H. *The foundations of science*. New York: Science Press, 1913.
19. POSTMAN, L. The history and present status of the Law of Effect. *Psychol. Bull.*, 1947, **44**, 489–563.
20. PRATT, C. C. Operationism in psychology. *Psychol. Rev.*, 1945, **52**, 262–269.
21. REICHENBACH, H. *Experience and prediction*. Chicago: Univ. of Chicago Press, 1938.
22. SKINNER, B. F. *The behavior of organisms*. New York: Appleton-Century, 1938.
23. SPENCE, K. W. Studies on secondary reinforcement. Address given to the Minnesota Chapter of Psi Chi, Minneapolis, April 22, 1948.
24. TAYLOR, L. W. *Physics, the pioneer science*. New York: Houghton, Mifflin, 1941.
25. THORNDIKE, E. L. *The fundamentals of learning*. New York: Teachers College, Columbia Univ., 1932.
26. THORNDIKE, E. L. *Animal intelligence*. New York: Macmillan, 1911.
27. THORNDIKE, E. L. *The original nature of man*. New York: Teachers College. 1913.
28. THORNDIKE, E. L. *The psychology of learning*. New York: Teachers College, 1913.
29. TOLMAN, E. C. *Purposive behavior in animals and men*. New York: Appleton-Century, 1932.

Received July 27, 1949.