

# THE CONTIGUITY PRINCIPLE IN LEARNING THEORY

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The last twenty years of basic learning theory has reflected a period of the supremacy of the law of effect and the discounting of contiguity. This trend started when conditioned response people began noticing that conditioning was often complicated by selective learning which did not seem to follow the Pavlovian rubric. We can perhaps identify the starting point of this era with the 1928 article by Miller and Konorski (12) on a special type of conditional reflex—later to be named the conditioned or discriminated “operant” (19), the “instrumental” conditioned response (7), and so forth. The development subsequent to Miller and Konorski has been one of rediscovering the law of effect—and relegating contiguity to a minor role either as a primitive learning principle among dualists or as a necessary but insufficient factor among the single-process effect theorists.

Now, I suspect, we are on the threshold of a rediscovery of contiguity. I think Mowrer's paper announcing his switch to dualism in the 1947 *Harvard Educational Review* (13) is one of the signs of the times. Also the 1949 Birch and Bitterman (1) critique of the effect position—and support of Maier and Schneirla (10) on dualism—I interpret as an undercurrent of dissatisfaction with the recent pre-eminence of effect as the major learning principle. Mowrer has elevated contiguity to an equal status with effect as a sometimes-sufficient condition for learning. Birch and Bitterman have hinted that their sensory integration version of contiguity will handle phenomena which even previous dualists have considered the exclusive property of effect. In each

case these authors suggest a trend toward reinstating the significance of contiguity as a sufficient condition for some learning.

At the same time there seems to be another sort of trend underway in favor of contiguity. This second trend I would say is more relevant to contiguity as a *factor* than contiguity as sufficient. It consists of the discovery that contiguity is a powerful source from which to derive learning phenomena without regard to one's position as to whether contiguity is sufficient—or only necessary. A good illustration is seen in Estes' (3) recent article in which he disowns the reinforcement problem and proceeds to deduce a mathematical model of selective acquisition based on a very Guthrie (6) set of assumptions flowing from contiguity alone. Other illustrations are seen in modern interpretations of partial reinforcement, as presented in the Jenkins and Stanley (9) review article, and in the interpretations of extinction and spontaneous recovery in Skinner's (20) very recent paper challenging theories in general and S-R reinforcement theories in particular.

Perhaps my perception of the current situation is biased, but I feel that we are getting further and further in handling the major phenomena of learning with less and less worry about reward as a factor. Perhaps we can carry it all the way and eliminate the law of effect from a general treatment of learning. I do not feel entirely optimistic about this parsimonious outcome but I think it is worth a concerted effort. And at the moment I would place the dualist position as a halfway point in such an

effort. It strikes me as an easy out—with defeatist consequences—for a current situation in which neither contiguity nor effect can make a convincing universal case.

On a less intuitive basis my objections to dualism are easily classified: (1) there appears to be strong evidence that learning—when properly defined—is a single process, and (2) the arguments of the dualists are unconvincing whether one takes a single-principle contiguity approach or whether one plays devil's advocate and takes a single-principle effect approach.

Starting with the first of these—the evidence for a single process—my point is simply that the phenomena exhibited in selective learning are so similar to those of Pavlovian conditioning that it is hard to believe in any differences in physiological process. One should of course be prepared to expect that two different physiological mechanisms will have the same overall outcome—as when temperature regulation is achieved both by perspiration and by vasodilation. But one should not be prepared to expect that two different mechanisms will exhibit point-by-point similarity in their detailed properties. Yet this is the case in a conditioned modification and a selectively acquired one. The ease with which the S-R reinforcement people have been able to appropriate Pavlov's findings attests to the similarity of the details of the two forms of learning. In such matters as acquisition, extinction, spontaneous recovery, generalization, differentiation, effects of partial reinforcement, and so forth, it makes no difference whether we are talking about a Thorndikian or a Pavlovian learning situation. The impressive fact is that we can translate from one discipline to the other by the simple expedient of interchanging appropriately the terms "reward" and "unconditioned stimulus." To me this makes it very

improbable on *a priori* grounds that there is not a common process. The notable differences are not in properties but rather in inferred experimental operations in reinforcing with an unconditioned stimulus on the one hand and with a reward on the other. This suggests looking extensively for a hidden similarity in operations or process—it does not readily suggest that there is a different process to go with each experimental operation.

In my second objection to dualists—that their arguments are not convincing—I shall not attempt to cover all dualists. Instead I will mention only present company. I am not impressed by the Birch and Bitterman argument because it hinges primarily on what I consider to be an incorrect analysis of avoidance training. Here I have to play devil's advocate and defend the law of effect. Their main objection to a single-principle effect theory is that they believe the findings of avoidance learning disprove the Hullian hypothesis that cessation of pain is a reward. Their disproof is too easy to be true. The argument runs as follows: in avoidance training *pain* and its termination is invariably correlated with failure to respond; *no pain* is invariably correlated with the correct response. Therefore, they argue, according to Hull failure to respond is rewarded by pain termination whereas performance of the correct response is subjected to extinction through non-reward. Since the facts of avoidance learning are just the opposite, Hull's notion of the reward value of pain termination is said to be disproven.

However, Birch and Bitterman have ignored entirely the factor of conditioned fear as a drive—and reduction of fear as a reward. These factors play a critical role in modern effect interpretations of avoidance learning as formulated by Mowrer (13) and others (14, 16). In this interpretation the

critical fact is that failure to make the correct response becomes very frightening because it invariably produces pain. At the same time performance of the correct response cannot become frightening because it is invariably not correlated with pain. Thus in avoidance training we create an instrumental learning situation—with fear as drive—and in which the only way to convert a frightening situation into a nonfrightening one is by lifting the paw, leaping into the air, or whatever else the experimenter might require. This can be handled by Hull (8) in drive-reduction terms or by Guthrie (4) in stimulus-change terms.

I am more impressed with the Birch and Bitterman argument that sensory pre-conditioning is an exception to the law of effect, although I am also familiar enough with effect dialectics to know that there are several ways out. For one we can postulate an investigatory drive which is satisfied by attending to any and all environmental changes. For another we can note that with strong stimuli the mere cessation of the stimulation is reinforcing to whatever response the animal makes to the stimuli. Thus we may predict that the response to  $S_1$  becomes connected to  $S_2$  and vice versa—and will mediate just about the amount of transfer actually obtained—namely, a *small* amount in most cases. Moreover, the usual smallness of transfer in sensory preconditioning appears to be inconsistent with their sensory-integration interpretation of Pavlovian conditioning. Thus the Birch and Bitterman argument can be said to backfire considerably if the question is asked as to why the phenomenon is always such a weak counterpart of direct conditioning. If all conditioning by contiguity is afferent modification, why did Brogden (2), in the original experiment on this topic, find that some subjects failed to show any

transfer, the average transfer was only 21 per cent of direct conditioning, and what little transfer was obtained initially extinguished very rapidly?

The Mowrer and Suter (15) experiment will also be attacked by single process effect theorists. For example, does a signal that precedes shock, accompanies shock, and terminates with shock become a cue for fear or a mixed cue for fear and the relaxation of fear that accompanies the termination of shock? Or, since the animal is free to move about on a grill, what evidence is there that all animals do not momentarily stop shock immediately by partial hops from the grill? Or, to be technical, what control is offered for an Adrian-type "on effect"—with an immediate and sharp pain reduction through sensory adaptation—as suggested by Miller and Dollard (11) when a cue is correlated with onset of pain?

Personally I think the Mowrer and Suter experiment is embarrassing for the effect position but not a crucial exception. On the other hand, it goes almost without saying that the experiment would not convince a contiguity theorist that one is forced into dualism; rather it is merely further support that contiguity is sufficient. Similarly Mowrer's (13) "collateral support" for dualism, while it helps make the dualistic position reasonable, does not at all show one is forced into dualism. The hypothesis that contiguity applies only to visceral-autonomic-involuntary effectors whereas effect applies only to skeletal-central-voluntary effectors is intriguing but unconvincing. Skeletal responses can be conditioned and primarily visceral responses can be selectively learned. At least this is the way I interpret eyelid conditioning and the use of rewards and punishments in teaching control of bowel and bladder sphincters.

Moreover, the voluntary-involuntary breakdown does not seem to be particularly neuro-anatomical. It is well known that many visceral responses can become voluntary, and skeletal responses which are not ordinarily useful never come under voluntary control. Few people can wiggle their ears, cross their eyes at will, or move each toe independently, although all of these can become voluntary with practice. A reasonable alternative to the Mowrer hypothesis is that while skeletal responses are continually determining whether rewards are forthcoming, visceral systems rarely get exposed to selective learning. Instead, visceral systems are reared in the constant and predictable type of environment which promotes the evolution of innate reflexes and prevents selective learning from getting a chance to occur. However, selective learning apparently can take over in the autonomic when, for example, a conflict makes acquisition of a psychosomatic symptom rewarding.

These foregoing single-process arguments can of course be as well utilized by an effect theorist, and do not particularly support contiguity except in a roundabout way. I would now like to drop the role of devil's advocate and argue that there is so much room for theoretical expansion within the framework of *any* theory of selective learning that a contiguity approach is at least as promising as the effect one.

The room for expansion comes chiefly from the fact that nobody knows what "effect" is. It is an odd law that contains a term so indeterminate in advance. It is analogous to the law of sleep which states that "sleep is produced by soporific agents." As currently used, effect is defined as anything which strengthens instrumental responses. We can collect an empirical list of such events and get a workable statement of the law of effect as fol-

lows: "If a response occurs contiguously with a neutral stimulus pattern, it will become connected to that pattern if it is followed by one of the things in the list." However, this workable law gives only a circular description of the operations producing "effect." We are essentially defining effect as any event which makes the law of effect true. Descriptively this procedure may be practical but it leaves us up in the air about the outcome with an untried event. It also makes the law of effect exceptionless and untestable from a reductionist standpoint.

The reductive alternative to this hand-book type of "law" is to seek a common factor in the empirical list of rewards. If we hit upon the right factor, we can predict the outcome when a new untried event is put in the reward position in a trial and error situation. The search for such a factor has been hampered because of one fairly common factor in the items of the empirical list collected so far. It is the property of being events which we feel are desirable from the standpoint of the organism. This leads to one of the most common and reprehensible versions of the law of effect—a version which merely says that responses with "adaptive" outcomes get fixated, and does not state the procedure for determining the adaptivity of a response. Such formulations probably constitute the most common way of conceptualizing the law of effect.

All such purposive conceptions of the law of effect are probably merely statements of the biological significance of learning. Selective learning is bound to be dominated by adaptive outcomes if the animal is left to his own devices in a maladaptive situation. He got the learning process through natural selection and it would be a surprise if it did not turn out to be a bodily process which helped him adapt to his environment. But survival-need reduction is

no more a cause of an individual instance of learning than is cooling of the body a cause of a person's perspiring when hot.

A more mechanistic, and therefore more acceptable, interpretation of effect is the drive reduction hypothesis of Mowrer (13), Miller and Dollard (11), Hull (8), and others in the S-R reinforcement group. There are numerous difficulties in Hull's use of "need reduction," but when he and his followers speak of "drive reduction"—or still more tangibly, of "stimulus-intensity reduction"—we have a principle which can predict that most learning will be adaptive but also can account for such examples as the maladaptive drug addict who acquires a habit which is adjuctive but not adaptive.

But a good mechanist is also not going to ignore the Smith and Guthrie (21) hypothesis that effect is removal from the situation. If one is endowed with a variety of adaptive reflexes, what could be more adaptive, when next thrown into a previously experienced maladaptive situation, than to do whatever got you out of it the last time you were in it? The response that changes a maladaptive situation into an adaptive one is inevitably going to be a wise choice from an adaptive standpoint—and a last-response factor may well have been incorporated into the learning process by natural selection.

The Smith and Guthrie hypothesis—which they deduce from contiguity—is a hard one to test experimentally and it has survived without real disproof for about thirty years. It has the advantage over the drive reduction position in that it accepts contiguity as sufficient and therefore does not have the awkwardness of the drive reduction position in coping with the conditioning of such responses as the knee-jerk, vaso-dilation, salivation based on acid, and so forth. Moreover, the drive reduction position

is faced with problems even in its home territory of accounting for instances of *selective learning* where the "reward" is not drive reducing in any obvious way.

Thus in a collaborative study with Dr. Thornton Roby (17) we found that hungry rats would learn responses for which reward was only a very sweet solution of saccharine and water. Hungry rats gorge themselves on such a non-nourishing solution and even after weeks of acquiring sophistication in this form of sham-feeding they readily learn to dash to the correct side of a T-maze to get a few minutes at the saccharine bottle.

In some other experiments in collaboration with Jepson Wulff and Robert Backer (18) we found that male rats reared from infancy in segregation from females readily learned an instrumental response that led to a chance to copulate with a female in heat even though copulation was always stopped short of ejaculation. That is, the "reward" for selective learning was an opportunity to initiate copulation but the female was always removed considerably before ejaculation. By some standards the "reward" would be called sexual frustration. The animals had never ejaculated before the experiment so acquired drive reduction was ruled out; and they did not ejaculate during the experiment so primary drive reduction was ruled out. Yet the response instrumental in producing the female in heat showed steady acquisition compared with controls receiving only a male companion in the goal box.

These findings seem contrary to the drive-reduction hypothesis of the nature of "effect," although they might be thought to follow the beneception mechanism of Troland (22). Perhaps the proper generalization is that the common factor is the execution of an innate and prepotent consummatory response, regardless of whether it reduces

the drive state. Whatever the correct inductive generalization, the results are more in line with a Guthrian contiguity analysis of selective learning than they are in line with a Hullian drive-reduction analysis. According to Guthrie the common factor in the empirical list of rewards is that they radically change the environment from just before to just after reward. The kinds of changes Guthrie (5) considers important indicate that it should be unimportant whether or not the animal's drive is reduced so long as the consummatory behavior remains prepotent and therefore able to take the animal "out of the situation."

I cite these experimental findings more to prove the fluid stage of our knowledge about reinforcement than to bolster the Guthrian contiguity analysis. My opinion is that Guthrie is still a long way from exhausting the implications of contiguity for instrumental learning. In the meantime my hunch is that we will eventually be forced to conclude that rewards are critical only as the means of motivating practice and performance and that the contiguity factor will turn out to be sufficient in describing the effects of practice *per se*. In terms of the main topic of our symposium my chief argument is that disproof of the universality of the law of effect does not force us into dualism as the only alternative—at least not until an all-out modern attempt has been made to see if association by contiguity is the common process both in selective learning and conditioning.

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