

## PUNISHMENT<sup>1</sup>

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FIRST, an introduction: I will attempt to achieve three goals today. (a) I will summarize some *empirical generalizations and problems* concerning the effects of punishment on behavior; (b) I will give some demonstrations of the *advantages of a two-process learning theory* for suggesting new procedures to be tried out in punishment experiments; and (c) finally, I shall take this opportunity today to *decry some unscientific legends* about punishment, and to do a little pontificating—a privilege that I might be denied in a journal such as the *Psychological Review*, which I edit!

Now, for a working definition of punishment: The definition of a punishment is not operationally simple, but some of its attributes are clear. A punishment is a noxious stimulus, one which will support, by its termination or omission, the growth of new escape or avoidance responses. It is one which the subject will reject, if given a choice between the punishment and no stimulus at all. Whether the data on the behavioral effects of such noxious stimuli will substantiate our common-sense view of what constitutes an effective punishment, depends on a wide variety of conditions that I shall survey. Needless to say, most of these experimental conditions have been studied with infrahuman subjects rather than with human subjects.

### SAMPLE EXPERIMENTS

Let us first consider two sample experiments. Imagine a traditional alley runway, 6 feet long, with its delineated goal box and start box, and an electrifiable grid floor. In our first experiment, a rat is shocked in the start box and alley, but there is no shock in the goal box. We can quickly train the rat to run down the alley, if the shock commences as the start-box gate is raised and persists

until the rat enters the goal box. This is *escape* training. If, however, we give the rat 5 seconds to reach the goal box after the start-box gate is raised, and only then do we apply the shock, the rat will usually learn to run quickly enough to avoid the shock entirely. This procedure is called *avoidance* training, and the resultant behavior change is called *active* avoidance learning. Note that the response required, either to terminate the shock or to remove the rat from the presence of the dangerous start box and alley, is well specified, while the behavior leading to the onset of these noxious stimulus conditions is left vague. It could be any item of behavior coming *before* the opening of the gate, and it would depend on what the rat happened to be doing when the experimenter raised the gate.

In our second sample experiment, we train a hungry rat to run to the goal box in order to obtain food. After performance appears to be asymptotic, we introduce a shock, both in the alley and goal box, and eliminate the food. The rat quickly stops running and spends its time in the start box. This procedure is called the *punishment procedure*, and the resultant learning-to-stay-in-the-start-box is called *passive* avoidance learning. Note that, while the behavior *producing* the punishment is well specified, the particular behavior *terminating* the punishment is left vague. It could be composed of any behavior that keeps the rat in the start box and out of the alley.

In the first experiment, we were teaching the rat *what to do*, while in the second experiment we were teaching him exactly *what not to do*; yet in each case, the criterion of learning was correlated with the rat's receiving *no* shocks, in contrast to its previous experience of receiving several shocks in the same experimental setting. One cannot think adequately about punishment without considering what is known about the outcomes of both procedures. Yet most reviews of the aversive control of behavior emphasize active avoidance learning and ignore passive avoidance learning. I shall,

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in this talk, emphasize the similarities, rather than the differences between active and passive avoidance learning. I shall point out that there is a rich store of knowledge of active avoidance learning which, when applied to the punishment procedure, increases our understanding of some of the puzzling and sometimes chaotic results obtained in punishment experiments.

But first, I would like to review some of the empirical generalities which appear to describe the outcomes of experiments on *punishment* and passive avoidance learning. For this purpose, I divide the evidence into 5 classes: (a) the effects of punishment on behavior previously established by *rewards* or positive reinforcement, (b) the effects of punishment on *consummatory* responses, (c) the effects of punishment on complex, sequential patterns of *innate* responses, (d) the effects of punishment on discrete reflexes, (e) the effects of punishment on responses previously established by punishment—or, if you will, the effects of punishment on active escape and avoidance responses. The effectiveness of punishment will be seen to differ greatly across these five classes of experiments. For convenience, I mean by *effectiveness* the degree to which a punishment procedure produces *suppression* of, or facilitates the *extinction* of, existing response patterns.

Now, let us look at punishment for *instrumental responses or habits previously established by reward or positive reinforcers*. First, the outcomes of punishment procedures applied to previously rewarded habits are strongly related to the *intensity* of the punishing agent. Sometimes intensity is independently defined and measured, as in the case of electric shock. Sometimes we have qualitative evaluations, as in the case of Maier's (1949) rat bumping his nose on a locked door, or Masserman's (Masserman & Pechtel, 1953) spider monkey being presented with a toy snake, or Skinner's (1938) rat receiving a slap on the paw from a lever, or my dog receiving a swat from a rolled-up newspaper. As the intensity of shock applied to rats, cats, and dogs is increased from about .1 milliamperes to 4 milliamperes, these orderly results can be obtained: (a) *detection and arousal*, wherein the punisher can be used as a cue, discriminative stimulus, response intensifier, or even as a secondary reinforcer; (b) *temporary suppression*, wherein punishment results in suppression of the punished response, followed by

complete recovery, such that the subject later appears unaltered from his prepunished state; (c) *partial suppression*, wherein the subject always displays some lasting suppression of the punished response, without total recovery; and (d) finally, there is *complete suppression*, with no observable recovery. Any of these outcomes can be produced, other things being equal, by merely varying the intensity of the noxious stimulus used (Azrin & Holz, 1961), when we punish responses previously established by reward or positive reinforcement. No wonder different experimenters report incomparable outcomes. Azrin (1959) has produced a response-rate *increase* while operants are punished. Storms, Boroczi, and Broen (1962) have produced long-lasting suppression of operants in rats.<sup>2</sup> Were punishment intensities different? Were punishment durations different? (Storms, Boroczi & Broen, 1963, have shown albino rats to be more resistant to punishment than are hooded rats, and this is another source of discrepancy between experiments.)

But other variables are possibly as important as punishment intensity, and their operation can make it unnecessary to use *intense* punishers in order to produce the effective suppression of a response previously established by positive reinforcement. Here are some selected examples:

1. *Proximity* in time and space to the punished response determines to some extent the effectiveness of a punishment. There is a response-suppression gradient. This has been demonstrated in the runway (Brown, 1948; Karsh, 1962), in the lever box (Azrin, 1956), and in the shuttle box (Kamin, 1959). This phenomenon has been labeled the gradient of temporal delay of punishment.

2. The conceptualized *strength* of a response, as measured by its resistance to extinction after omission of positive reinforcement, predicts the effect of a punishment contingent upon the response. Strong responses, so defined, are more resistant to the suppressive effects of punishment. Thus, for example, the overtraining of a response, which often decreases ordinary resistance to ex-

<sup>2</sup> Since the delivery of this address, several articles have appeared concerning the punishment intensity problem. See especially Karsh (1963), Appel (1963), and Walters and Rogers (1963). All these studies support the conclusion that shock intensity is a crucial variable, and high intensities produce lasting suppression effects.

perimental extinction, also increases the effectiveness of punishment (Karsh, 1962; Miller, 1960) as a response suppressor.

3. *Adaptation* to punishment can occur, and this *decreases* its effectiveness. New, intense punishers are better than old, intense punishers (Miller, 1960). Punishment intensity, if slowly increased, tends not to be as effective as in the case where it is introduced initially at its high-intensity value.

4. In general, resistance to extinction is decreased whenever a previously reinforced response is punished. However, if the subject is habituated to receiving shock together with positive reinforcement during reward training, the relationship can be reversed, and punishment during extinction can actually increase resistance to extinction (Holz & Azrin, 1961). Evidently, punishment, so employed, can functionally operate as a *secondary reinforcer*, or as a cue for reward, or as an arouser.

5. Punishments become extremely effective when the response-suppression period is tactically used as an aid to the reinforcement of new responses that are topographically *incompatible* with the punished one. When new instrumental acts are established which lead to the old goal (a new *means* to an old *end*), a punishment of very low intensity can have very long-lasting suppression effects. Whiting and Mowrer (1943) demonstrated this clearly. They first rewarded one route to food, then punished it. When the subjects ceased taking the punished route, they provided a new rewarded route. The old route was not traversed again. This reliable suppression effect also seems to be true of temporal, discriminative restraints on behavior. The suppression of urination in dogs, under the control of *indoor stimuli*, is extremely effective in housebreaking the dog, as long as urination is allowed to go unpunished under the control of *outdoor stimuli*. There is a valuable lesson here in the effective use of punishments in producing *impulse control*. A *rewarded alternative*, under discriminative control, makes passive avoidance training a potent behavioral influence. It can produce a highly reliable dog or child. In some preliminary observations of puppy training, we have noted that puppies raised in the lab, if punished by the swat of a newspaper for eating horsemeat, and rewarded for eating pellets, will starve themselves to death when only given the

opportunity to eat the taboo horsemeat. They eagerly eat the pellets when they are available.

It is at this point that we should look at the experiments wherein punishment appears to have only a temporary suppression effect. Most of these experiments offered the subject *no* rewarded alternative to the punished response in attaining his goal. In many such experiments, it was a case of take a chance or go hungry. Hunger-drive strength, under such no-alternative conditions, together with punishment intensity, are the crucial variables in predicting recovery from the suppression effects of punishment. Here, an interesting, yet hard-to-understand phenomenon frequently occurs, akin to Freudian "reaction formation." If a subject has been punished for touching some manipulandum which yields food, he may stay nearer to the manipulandum under low hunger drive and move farther away from it under high hunger drive, even though the probability of finally touching the manipulandum increases as hunger drive increases. This phenomenon is complex and needs to be studied in some detail. Our knowledge of it now is fragmentary. It was observed by Hunt and Schlosberg (1950) when the water supply of rats was electrified, and we have seen it occur in approach-avoidance conflict experiments in our laboratory, but we do not know the precise conditions for its occurrence.

Finally, I should point out that the attributes of effective punishments vary *across species* and *across stages in maturational development* within species. A toy snake can frighten monkeys. It does not faze a rat. A loud noise terrified Watson's little Albert. To us it is merely a Chinese gong.

I have sketchily reviewed some effects of punishment on *instrumental* acts established by *positive reinforcers*. We have seen that any result one might desire, from response enhancement and little or no suppression, to relatively complete suppression, can be obtained with our current knowledge of appropriate experimental conditions. Now let us look at the effects of punishment on *consummatory acts*. Here, the data are, to me, surprising. One would think that consummatory acts, often being of biological significance for the survival of the individual and the species, would be highly resistant to suppression by punishment. The *contrary* appears to be so. Male sexual behavior may be seriously suppressed by weak punishment

(Beach, Conovitz, Steinberg, & Goldstein, 1956; Gantt, 1944). Eating in dogs and cats can be permanently suppressed by a moderate shock delivered through the feet or through the food dish itself (Lichtenstein, 1950; Masserman, 1943). Such suppression effects can lead to fatal self-starvation. A toy snake presented to a spider monkey while he is eating can result in self-starvation (Masserman & Pechtel, 1953).

The interference with consummatory responses by punishment needs a great deal of investigation. Punishment seems to be especially effective in breaking up this class of responses, and one can ask *why*, with some profit. Perhaps the intimate temporal connection between drive, incentive, and punishment results in drive or incentive becoming conditioned-stimulus (CS) patterns for aversive emotional reactions when consummatory acts are punished. Perhaps this interferes with vegetative activity: i.e., does it "kill the appetite" in a hungry subject? But, one may ask why the same punisher might not appear to be as effective when made contingent on an *instrumental* act as contrasted with a consummatory act. Perhaps the nature of operants is such that they are separated in time and space and response topography from consummatory behavior and positive incentive stimuli, so that appetitive reactions are not clearly present during punishment for operants. We do not know enough yet about such matters, and speculation about it is still fun.

Perhaps the most interesting parametric variation one can study, in experiments on the effects of punishment on consummatory acts, is the *temporal order* of rewards and punishments. If we hold hunger drive constant, shock-punishment intensity constant, and food-reward amounts constant, a huge differential effect can be obtained when we reverse the order of reward and punishment. If we train a cat to approach a food cup, its behavior in the experimental setting will become quite stereotyped. Then, if we introduce shock to the cat's feet while it is eating, the cat will vocalize, retreat, and show fear reactions. It will be slow to recover its eating behavior in this situation. Indeed, as Masserman (1943) has shown, such a procedure is likely, if repeated a few times, to lead to self-starvation. Lichtenstein (1950) showed the same phenomenon in dogs. Contrast this outcome with that found when the temporal order of food and shock is *reversed*. We

now use shock as a discriminative stimulus to signalize the availability of food. When the cat is performing well, the shock may produce eating with a latency of less than 5 seconds. The subject's appetite does not seem to be disturbed. One cannot imagine a more dramatic difference than that induced by reversing the temporal order of reward and punishment (Holz & Azrin, 1962; Masserman, 1943).

Thus, the effects of punishment are partly determined by those events that directly precede it and those that directly follow it. A punishment is not just a punishment. It is an event in a temporal and spatial flow of stimulation and behavior, and its effects will be produced by its temporal and spatial point of insertion in that flow.

I have hastily surveyed some of the effects of punishment when it has been made contingent either on rewarded *operants* and instrumental acts or on *consummatory* acts. A third class of behaviors, closely related to consummatory acts, but yet a little different, are *instinctive act sequences*: the kinds of complex, innately governed behaviors which the ethologists study, such as nest building in birds. There has been little adequate experimentation, to my knowledge, on the effects of punishment on such innate behavior sequences. There are, however, some hints of interesting things to come. For example, sometimes frightening events will produce what the ethologists call displacement reactions—the expression of an inappropriate behavior pattern of an innate sort. We need to experiment with such phenomena in a systematic fashion. The best example I could find of this phenomenon is the imprinting of birds on moving objects, using the locomotor following response as an index. Moltz, Rosenblum, and Halikas (1959), in one experiment, and Kovach and Hess (1963; see also Hess, 1959a, 1959b) in another, have shown that the punishment of such imprinted behavior sometimes depresses its occurrence. However, if birds are punished prior to the presentation of an imprinted object, often the following response will be energized. It is hard to understand what this finding means, except that punishment can either arouse or inhibit such behavior, depending on the manner of presentation of punishment. The suggestion is that imprinting is partially a function of fear or distress. The effectiveness of punish-

ment also is found to be related to the critical period for imprinting (Kovach & Hess, 1963).

However, the systematic study of known punishment parameters as they affect a wide variety of complex sequences of innate behaviors is yet to be carried out. It would appear to be a worthwhile enterprise, for it is the type of work which would enable us to make a new attack on the effects of experience on innate behavior patterns. Ultimately the outcomes of such experiments *could* affect psychoanalytic conceptions of the effects of trauma on impulses of an innate sort.<sup>3</sup>

A fourth class of behavior upon which punishment can be made contingent, is the simple, discrete reflex. For example, what might happen if a conditioned or an unconditioned knee jerk were punished? We are completely lacking in information on this point. Can subjects be trained to inhibit reflexes under aversive motivation? Or does such motivation sensitize and enhance reflexes? Some simple experiments are appropriate, but I was unable to find them in the published work I read.

A fifth class of behavior, upon which punishment can be made contingent, is behavior *previously established by punishment procedures*: in other words, the effect of passive avoidance training on existing, active avoidance learned responses. This use of punishment produces an unexpected outcome. In general, if the same noxious stimulus is used to punish a response as was used to establish it in the first place, the response becomes strengthened during initial applications of punishment. After several such events, however, the response may weaken, but not always. The similarity of the noxious stimulus used for active avoidance training to that used for punishment of the established avoidance response can be of great importance. For example, Carlsmith (1961) has shown that one can increase resistance to extinction by using the same noxious stimuli for both

purposes and yet decrease resistance to extinction by using equally noxious, but discriminatively different, punishments. He trained some rats to run in order to avoid shock, then punished them during extinction by blowing a loud horn. He trained other rats to run in order to avoid the loud horn, then during extinction he punished them by shocking them for running. In two control groups, the punisher stimulus and training stimulus were the same. The groups which were trained and then punished by different noxious stimuli extinguished more rapidly during punishment than did the groups in which the active avoidance training unconditioned stimulus (US) was the same as the passive avoidance training US. Thus, punishment for responses established originally by punishment may be ineffective in eliminating the avoidance responses they are supposed to eliminate. Indeed, the punishment may strengthen the responses. We need to know more about this puzzling phenomenon. It is interesting to me that in Japan, Imada (1959) has been systematically exploring shock intensity as it affects this phenomenon.

Our quick survey of the effects of punishment on five classes of responses revealed a wide variety of discrepant phenomena. Thus, to predict in even the grossest way the action of punishment on a response, one has to know *how* that particular response was originally inserted in the subject's response repertoire. Is the response an instrumental one which was strengthened by reward? Is it instead a consummatory response? Is it an innate sequential response pattern? Is it a discrete reflex? Was it originally established by means of punishment? *Where*, temporally, in a behavior sequence, was the punishment used? How *intense* was it? These are but a few of the relevant, critical questions, the answers to which are necessary in order for us to make reasonable predictions about the effects of punishment. Thus, to conclude, as some psychologists have, that the punishment procedure is typically either effective or ineffective, typically either a temporary suppressor or a permanent one, is to oversimplify irresponsibly a complex area of scientific knowledge, one still containing a myriad of intriguing problems for experimental attack.

Yet, the complexities involved in ascertaining the effects of punishment on behavior *need not* be a bar to useful speculation ultimately leading to

<sup>3</sup> Since the delivery of this address, an article has appeared on this specific problem. See Adler and Hogan (1963). The authors showed that the gill-extension response of *Betta splendens* could be conditioned to a previously neutral stimulus by a Pavlovian technique, and it could also be suppressed by electric-shock punishment. This is an important finding, because there are very few known cases where the same response can be both conditioned and trained. Here, the gill-extension response is typically elicited by a rival fish, and is usually interpreted to be aggressive or hostile in nature.

experimentation of a fruitful sort. The complexities should, however, dictate a great deal of caution in making dogmatic statements about whether punishment is effective or ineffective as a behavioral influence, or whether it is good or bad. I do *not* wish to do that. I would like now to speculate about the data-oriented theories, rather than support or derogate the dogmas and the social philosophies dealing with punishment. I will get to the dogmas later.

### THEORY

Here is a theoretical approach that, for me, has high pragmatic value in stimulating new lines of experimentation. Many psychologists today consider the punishment procedure to be a special case of avoidance training, and the resultant learning processes to be theoretically identical in nature. Woodworth and Schlosberg (1954) distinguish the two training procedures, "*punishment for action*" from "*punishment for inaction*," but assume that the same theoretical motive, a "positive incentive value of safety" can explain the learning produced by both procedures. Dinsmoor (1955) argues that the facts related to both procedures are well explained by simple stimulus-response (S-R) principles of avoidance learning. He says:

If we punish the subject for making a given response or sequence of responses—that is, apply aversive stimulation, like shock—the cues or discriminative stimuli for this response will correspond to the warning signals that are typically used in more direct studies of avoidance training. By his own response to these stimuli, the subject himself produces the punishing stimulus and pairs or correlates it with these signals. As a result, they too become aversive. In the meantime, any variations in the subject's behavior that interfere or conflict with the chain of reactions leading to the punishment delay the occurrence of the final response and the receipt of the stimulation that follows it. These variations in behavior disrupt the discriminative stimulus pattern for the continuation of the punished chain, changing the current stimulation from an aversive to a nonaversive compound; they are conditioned, differentiated, and maintained by the reinforcing effects of the change in stimulation [p. 96].

The foci of the Dinsmoor analysis are the processes whereby: (a) discriminative stimuli become aversive, and (b) instrumental acts are reinforced. He stays at the quasi-descriptive level. He uses a peripheralistic, S-R analysis, in which response-produced proprioceptive stimuli and exteroceptive stimuli serve to hold behavior chains together. He rejects, as unnecessary, concepts such as fear or

anxiety, in explaining the effectiveness of punishment.

Mowrer (1960) also argues that the facts related to the two training procedures are explained by a common set of principles, but Mowrer's principles are somewhat different than those of either Woodworth and Schlosberg, or Dinsmoor, cited above. Mowrer says:

In both instances, there is fear conditioning; and in both instances a way of behaving is found which eliminates or controls the fear. The only important distinction, it seems is that the stimuli to which the fear gets connected are different. In so-called punishment, these stimuli are produced by (correlated with) the behavior, or response, which we wish to block; whereas, in so-called avoidance learning, the fear-arousing stimuli are not response-produced—they are, so to say, extrinsic rather than intrinsic, independent rather than response-dependent. But in both cases there is avoidance and in both cases there is its antithesis, punishment; hence the impropriety of referring to the one as "punishment" and to the other as "avoidance learning." Obviously precision and clarity of understanding are better served by the alternative terms here suggested, namely, passive avoidance learning and active avoidance learning, respectively. . . . But, as we have seen, the two phenomena involve exactly the same basic principles of fear conditioning and of the reinforcement of whatever action (or inaction) eliminates the fear [pp. 31-32].

I like the simple beauty of each of the three unifying positions; what holds for punishment and its action on behavior should hold also for escape and avoidance training, and vice versa. Generalizations about one process should tell us something about the other. New experimental relationships discovered in the one experimental setting should tell us how to predict a new empirical event in the other experimental setting. A brief discussion of a few selected examples can illustrate this possibility.

### APPLICATIONS OF THEORY

I use a case in point stemming from work done in our own laboratory. It gives us new hints about some hidden sources of effectiveness of punishment. Remember, for the sake of argument, that we are assuming many important similarities to exist between active and passive avoidance-learning processes. Therefore, we can look at active avoidance learning as a theoretical device to suggest to us new, unstudied variables pertaining to the effectiveness of punishment.

Turner and I have recently published an ex-

tensive monograph (1962) on human traumatic avoidance learning. Our experiments showed that when a very reflexive, short-latency, skeletal response, such as a toe twitch, was used as an escape and avoidance response, grave difficulties in active avoidance learning were experienced by the subject. Experimental variations which tended to render the escape responses more emitted, more deliberate, more voluntary, more operant, or less reflexive, tended also to render the avoidance responses easier to learn. Thus, when a subject was required to move a knob in a slot in order to avoid shock, learning was rapid, in contrast to the many failures to learn with a toe-flexion avoidance response.

There are descriptions of this phenomenon already available in several published experiments on active avoidance learning, but their implications have not previously been noted. When Schlosberg (1934) used for the avoidance response a highly reflexive, short-latency, paw-flexion response in the rat, he found active avoidance learning to be unreliable, unstable, and quick to extinguish. Whenever the rats made active avoidance flexions, a decrement in response strength ensued. When the rats were shocked on several escape trials, the avoidance response tended to reappear for a few trials. Thus, learning to avoid was a tortuous, cyclical process, never exceeding 30% success. Contrast these results with the active avoidance training of nonreflexive, long-latency operants, such as rats running in Hunter's (1935) circular maze. Hunter found that the occurrence of avoidance responses tended to produce more avoidance responses. Omission of shock seemed to reinforce the avoidance running response. Omission of shock seemed to extinguish the avoidance paw flexion. Clearly the operant-respondent distinction has predictive value in active avoidance learning.

The same trend can be detected in experiments using dogs as subjects. For example, Brogden (1949), using the forepaw-flexion response, found that meeting a 20/20 criterion of avoidance learning was quite difficult. He found that 30 dogs took from approximately 200-600 trials to reach the avoidance criterion. The response used was, in our language, highly reflexive—it was totally elicited by the shock on escape trials with a very short latency, approximately .3 second. Compare, if you will, the learning of active avoidance by dogs in the shuttle box with that found in the

forelimb-flexion experiment. In the shuttle box, a large number of dogs were able to embark on their criterion trials after 5-15 active avoidance-training trials. Early escape response latencies were long. Resistance to extinction is, across these two types of avoidance responses, inversely related to trials needed for a subject to achieve criterion. Conditions leading to quick acquisition are, in this case, those conducive to slow extinction. Our conclusion, then, is that high-probability, short-latency, *respondents* are not as good as medium-probability, long-latency operants when they are required experimentally to function as active avoidance responses. This generalization seems to hold for rats, dogs, and college students.

How can we make the inferential leap from such findings in active avoidance training to possible variations in punishment experiments? It is relatively simple to generalize across the two kinds of experiments in the case of CS-US interval, US intensity, and CS duration. But the inferential steps are not as obvious in the case of the operant-respondent distinction. So I will trace out the logic in some detail. If one of the major effects of punishment is to motivate or elicit new behaviors, and reinforce them through removal of punishment, and thus, as Dinsmoor describes, establish avoidance responses incompatible with a punished response, how does the operant-respondent distinction logically enter? Here, Mowrer's two-process avoidance-learning theory can suggest a possible answer. Suppose, for example, that a hungry rat has been trained to lever press for food and is performing at a stable rate. Now we make a short-duration, high-intensity pulse of shock contingent upon the bar press. The pulse elicits a startle pattern that produces a release of the lever in .2 second, and the shock is gone. The rat freezes for a few seconds, breathing heavily, and he urinates and defecates. It is our supposition that a conditioned emotional reaction (CER) is thereby established, with its major stimulus control coming from the sight of the bar, the touch of the bar, and proprioceptive stimuli aroused by the lever-press movements themselves. This is, as Dinsmoor describes it, the development of acquired aversiveness of stimuli; or, as Mowrer describes it, the acquisition of conditioned fear reactions. Therefore, Pavlovian conditioning variables should be the important ones in the development of this process. The reappearance of lever pressing in

this punished rat would thus depend on the extinction of the CER and skeletal freezing. If no further shocks are administered, then the CER should extinguish according to the laws of Pavlovian extinction, and reappearance of the lever press should not take long, even if the shock-intensity level were high enough to have been able to produce active avoidance learning in another apparatus.

Two-process avoidance theory tells us that something very important for successful and durable response suppression was missing in the punishment procedure we just described. What was lacking in this punishment procedure was a good operant to allow us to reinforce a reliable avoidance response. Because the reaction to shock was a respondent, was highly *reflexive*, and was quick to occur, I am led to argue that the termination of shock will *not* reinforce it, nor will it lead to stable avoidance responses. This conclusion follows directly from our experiments on human avoidance learning. If the termination of shock is made contingent on the occurrence of an operant, especially an operant topographically incompatible with the lever press, an active avoidance learning process should then ensue. So I will now propose that we shock the rat until he huddles in a corner of the box. The rat will have learned to *do* something arbitrary whenever the controlling CSs reappear. Thus, the rat in the latter procedure, if he is to press the lever again, must undergo *two* extinction processes. The CER, established by the pairing of CS patterns and shock, must become weaker. Second, the learned huddling response must extinguish. This combination of requirements should make the effect of punishment more lasting, if my inferences are correct. Two problems must be solved by the subject, not one. The experiments needed to test these speculations are, it would appear, easy to design, and there is no reason why one should not be able to gather the requisite information in the near future. I feel that there is much to be gained in carrying on theoretical games like this, with the major assumptions being (*a*) that active and passive avoidance learning are similar processes, ones in which the same variables have analogous effects, and (*b*) that two processes, the conditioning of fear reactions, and the reinforcement of operants incompatible with the punished response, may operate in punishment experiments.

There is another gain in playing theoretical games of this sort. One can use them to question the usual significance imputed to past findings. Take, for example, the extensive studies of Neal Miller (1959) and his students, and Brown (1948) and his students, on gradients of approach and avoidance in conflict situations. Our foregoing analysis of the role of the operant-respondent distinction puts to question one of their central assumptions—that the avoidance gradient is unconditionally steeper than is the approach gradient in approach-avoidance conflicts. In such experiments, the subject is typically trained while hungry to run down a short alley to obtain food. After the running is reliable, the subject is shocked, usually near the goal, in such a way that entering the goal box is discouraged temporarily. The subsequent behavior of the typical subject consists of remaining in the start box, making abortive approaches to the food box, showing hesitancy, oscillation, and various displacement activities, like grooming. Eventually, if shock is eliminated by the experimenter, the subject resumes running to food. The avoidance tendency is therefore thought to have extinguished sufficiently so that the magnitude of the conceptualized approach gradient exceeds that of the avoidance gradient at the goal box. The steepness of the avoidance gradient as a function of distance from the goal box is inferred from the behavior of the subject *prior* to the extinction of the avoidance tendencies. If the subject stays as far away from the goal box as possible, the avoidance gradient may be inferred to be either displaced upward, or if the subject slowly creeps up on the goal box from trial to trial, it may be inferred to be less steep than the approach gradient. Which alternative is more plausible? Miller and his collaborators very cleverly have shown that the latter alternative is a better interpretation.

The differential-steepness assumption appears to be substantiated by several studies by Miller and his collaborators (Miller & Murray, 1952; Murray & Berkun, 1955). They studied the displacement of conflicted approach responses along both spatial and color dimensions, and clearly showed that the approach responses generalized more readily than did the avoidance responses. Rats whose running in an alley had been completely suppressed by shock punishment showed recovery of running in a similar alley. Thus the inference made was that the avoidance gradient is steeper than is the ap-

proach gradient; avoidance tendencies weaken more rapidly with changes in the external environmental setting than do approach tendencies. On the basis of the analysis I made of the action of punishment, both as a US for the establishment of a Pavlovian CER and as a potent event for the reinforcement of instrumental escape and avoidance responses, it seems to me very likely that the approach-avoidance conflict experiments have been carried out in such a way as to produce inevitably the steeper avoidance gradients. In other words, these experiments from my particular viewpoint have been inadvertently biased, and they were not appropriate for testing hypotheses about the gradient slopes.

My argument is as follows: Typically, the subject in an approach-avoidance experiment is trained to perform a specific sequence of responses under reward incentive and appetitive drive conditions. He runs to food when hungry. In contrast, when the shock is introduced into the runway, it is usually placed near the goal, and no specific, long sequence of instrumental responses is required of the subject before the shock is terminated. Thus, the initial strengths of the approach and avoidance instrumental responses (which are in conflict) are not equated by analogous or symmetrical procedures. Miller has thoroughly and carefully discussed this, and has suggested that the avoidance gradient would not have as steep a slope if the shock were encountered by the rat early in the runway in the case where the whole runway is electrified. While this comment is probably correct, it does not go far enough, and I would like to elaborate on it. I would argue that if one wants to study the relative steepnesses of approach and avoidance responses in an unbiased way, the competing instrumental responses should be established in a *symmetrical* fashion. After learning to run down an alley to food, the subject should be shocked near the goal box or in it, and the shock should not be terminated until the subject has escaped all the way into the start box. Then one can argue that two conflicting instrumental responses have been established. First, the subject runs one way for food; now he runs the same distance in the opposite direction in order to escape shock. When he stays in the start box, he avoids shock entirely. Then the generalization or displacement of the approach and avoidance responses can be fairly studied.

I am arguing that we need *instrumental*-response

balancing, as well as *Pavlovian*-conditioning balancing, in such conflict experiments, if the slopes of gradients are to be determined for a test of the differential-steepness assumption. Two-process avoidance-learning theory requires such a symmetrical test. In previous experiments, an aversive CER and its respondent motor pattern, not a well-reinforced avoidance response, has been pitted against a well-reinforced instrumental-approach response. Since the instrumental behavior of the subject is being used subsequently to test for the slope of the gradients, the usual asymmetrical procedure is, I think, not appropriate. My guess is that, if the symmetrical procedure I described is actually used, the slopes of the two gradients will be essentially the same, and the recovery of the subject from the effects of punishment will be seen to be nearly all-or-none. That is, the avoidance gradient, as extinction of the CER proceeds in time, will drop below the approach gradient, and this will hold all along the runway if the slopes of the two gradients are indeed the same. Using the test of displacement, subjects should stay in the starting area of a similar alley on initial tests and when they finally move forward they should go all the way to the goal box.

The outcomes of such experiments would be a matter of great interest to me, for, as you will read in a moment, I feel that the suppressive power of punishment over instrumental acts has been understated. The approach-avoidance conflict experiment is *but one* example among many wherein the outcome *may have been* inadvertently biased in the direction of showing reward-training influences to be superior, in some particular way, to punishment-training procedures. Now let us look more closely at this matter of bias.

#### LEGENDS

Skinner, in 1938, described the effect of a short-duration slap on the paw on the extinction of lever pressing in the rat. Temporary suppression of lever-pressing rate was obtained. When the rate increased, it exceeded the usual extinction performance. The total number of responses before extinction occurred was not affected by the punishment for lever pressing. Estes (1944) obtained similar results, and attributed the temporary suppression to the establishment of a CER (anxiety) which dissipated rapidly. Tolman, Hall, and Bretnall (1932) had shown earlier that punish-

ment could enhance maze learning by serving as a cue for correct, rewarded behavior. Skinner made these observations (on the seemingly ineffective nature of punishment as a response weakener) the basis for his advocacy of a positive reinforcement regime in his utopia, *Walden Two*. In *Walden Two*, Skinner (1948), speaking through the words of Frazier, wrote: "We are now discovering at an untold cost in human suffering—that in the long run punishment doesn't reduce the probability that an act will occur [p. 260]." No punishments would be used there, because they would produce poor behavioral control, he claimed.

During the decade following the publication of *Walden Two*, Skinner (1953) maintained his position concerning the effects of punishment on instrumental responses: Response suppression is but temporary, and the side effects, such as fear and neurotic and psychotic disturbances, are not worth the temporary advantages of the use of punishment. He said:

In the long run, punishment, unlike reinforcement works to the disadvantage of both the punished organism and the punishing agency [p. 183].

The fact that punishment does not permanently reduce a tendency to respond is in agreement with Freud's discovery of the surviving activity of what he called repressed wishes [p. 184].

Punishment, as we have seen, does not create a negative probability that a response will be made but rather a positive probability that incompatible behavior will occur [p. 222].

It must be said, in Skinner's defense, that in 1953 he devoted about 12 pages to the topic of punishment in his introductory textbook. Other texts had devoted but a few words to this topic.

In Bugelski's (1956) words about the early work on punishment: "The purport of the experiments mentioned above appears to be to demonstrate that punishment is ineffective in eliminating behavior. This conclusion appears to win favor with various sentimentalists [p. 275]." Skinner (1961) summarized his position most recently in this way:

Ultimate advantages seem to be particularly easy to overlook in the control of behavior, where a quick though slight advantage may have undue weight. Thus, although we boast that the birch rod has been abandoned, most school children are still under aversive control—not because punishment is more effective in the long run, but because it yields immediate results. It is easier for the teacher to control the student by threatening punishment than by using positive reinforcement with its *deferred*, though more powerful, effects [p. 36.08, italics mine].

Skinner's conclusions were drawn over a span of time when, just as is the case *now*, there was no conclusive evidence about the supposedly more powerful and long-lasting effects of positive reinforcement. I admire the humanitarian and kindly dispositions contained in such writings. But the scientific basis for the conclusions therein was shabby, because, even in 1938, there were conflicting data which demonstrated the great effectiveness of punishment in controlling instrumental behavior. For example, the widely cited experiments of Warden and Aylesworth (1927) showed that discrimination learning in the rat was more rapid and more stable when incorrect responses were punished with shock than when reward alone for the correct response was used. Later on, avoidance-training experiments in the 1940s and 1950s added impressive data on the long-lasting behavioral control exerted by noxious stimuli (Solomon & Brush, 1956). In spite of this empirical development, many writers of books in the field of learning now devote but a few lines to the problem of punishment, perhaps a reflection of the undesirability of trying to bring satisfying order out of seeming chaos. In this category are the recent books of Spence, Hull, and Kimble. An exception is Bugelski (1956) who devotes several pages to the complexities of this topic. Most contemporary *introductory psychology* texts devote but a paragraph or two to punishment as a scientific problem. Conspicuously, George Miller's new book, *Psychology, the Science of Mental Life*, has no discussion of punishment in it.

The most exhaustive textbook treatment today is that of Deese (1958), and it is a thoughtful and objective evaluation, a singular event in this area of our science. The most exhaustive journal article is that by Church (1963), who has thoroughly summarized our knowledge of punishment. I am indebted to Church for letting me borrow freely from his fine essay in prepublication form. Without this assistance, the organization of this paper would have been much more difficult, indeed.

Perhaps one reason for the usual textbook relegation of the topic of punishment to the fringe of experimental psychology is the wide-spread belief that punishment is unimportant because *it does not really weaken habits*; that it pragmatically is a *poor controller* of behavior; that it is extremely *cruel* and unnecessary; and that it is a

technique leading to *neurosis* and worse. This legend, and it is a legend without sufficient empirical basis, probably arose with Thorndike (1931). Punishment, in the time of Thorndike, used to be called punishment, not passive avoidance training. The term referred to the use of noxious stimuli for the avowed purpose of discouraging some selected kind of behavior. Thorndike (1931) came to the conclusion that punishment did not really accomplish its major purpose, the destruction or extinction of habits. In his book, *Human Learning*, he said:

Annoyers do not act on learning in general by weakening whatever connection they follow. If they do anything in learning, they do it indirectly, by informing the learner that such and such a response in such and such a situation brings distress, or by making the learner feel fear of a certain object, or by making him jump back from a certain place, or by some other definite and specific change which they produce in him [p. 46].

This argument is similar to that of Guthrie (1935), and of Wendt (1936), in explaining the extinction of instrumental acts and conditioned reflexes. They maintained that extinction was not the weakening of a habit, but the replacement of a habit by a new one, even though the new one might only be sitting still and doing very little.

When Thorndike claimed that the effects of punishment were indirect, he was emphasizing the power of punishment to evoke behavior other than that which produced the punishment; in much the same manner, Guthrie emphasized the extinction procedure as one arousing competing responses. The competing-response theory of extinction today cannot yet be empirically chosen over other theories such as Pavlovian and Hullian inhibition theory, or the frustration theories of Amsel or Spence. The Thorndikian position on punishment is limited in the same way. It is difficult to designate the empirical criteria which would enable us to know, on those occasions when punishment for a response results in a weakening of performance of that response, whether a habit was indeed weakened or not. How can one tell whether competing responses have displaced the punished response, or whether the punished habit is itself weakened by punishment? Thorndike could not tell, and neither could Guthrie. Yet a legend was perpetuated. Perhaps the acceptance of the legend had something to do with the lack of concerted research on punishment from 1930-1955.

For example, psychologists were not then particularly adventuresome in their search for experimentally effective punishments.

Or, in addition to the legend, perhaps a bit of softheartedness is partly responsible for limiting our inventiveness. (The Inquisitors, the Barbarians, and the Puritans could have given us some good hints! They did not have electric shock, but they had a variety of interesting ideas, which, regrettably, they often put to practice.) We clearly need to study new kinds of punishments in the laboratory. For most psychologists, a punishment in the laboratory means electric shock. A few enterprising experimenters have used air blasts, the presentation of an innate fear releaser, or a signal for the coming omission of reinforcement, as punishments. But we still do not know enough about using these stimuli in a controlled fashion to produce either behavior suppression, or a CER effect, or the facilitation of extinction. Many aversive states have gone unstudied. For example, conditioned nausea and vomiting is easy to produce, but it has not been used in the role of punishment. Even the brain stimulators, though they have since 1954 tickled brain areas that will instigate active escape learning, have not used this knowledge to study systematically the punishing effects of such stimulation on existing responses.

While the more humanitarian ones of us were bent on the discovery of new positive reinforcers, there was no such concerted effort on the part of the more brutal ones of us. Thus, for reasons that now completely escape me, some of us in the past were thrilled by the discovery that, under some limited conditions, either a light onset or a light termination could raise lever-pressing rate significantly, though trivially, above operant level. If one is looking for agents to help in the task of getting strong predictive power, and strong control of behavior, such discoveries seem not too exciting. Yet, in contrast, discoveries *already have* been made of the powerful aversive control of behavior. Clearly, we have been afraid of their implications. Humanitarian guilt and normal kindness are undoubtedly involved, as they should be. But I believe that one reason for our fear has been the widespread implication of the *neurotic syndrome* as a *necessary* outcome of all severe punishment procedures. A second reason has been the general acceptance of the behavioral phenom-

ena of rigidity, inflexibility, or narrowed cognitive map, as *necessary* outcomes of experiments in which noxious stimuli have been used. I shall question *both* of these conclusions.

If one should feel that the Skinnerian generalizations about the inadequate effects of punishment on instrumental responses are tinged with a laudable, though thoroughly incorrect and unscientific, sentimentalism and softness, then, in contrast, one can find more than a lurid tinge in discussions of the effects of punishment on the *emotional* balance of the individual. When punishments are asserted to be ineffective controllers of instrumental behavior, they are, in contrast, often asserted to be devastating controllers of emotional reactions, leading to neurotic and psychotic symptoms, and to general pessimism, depressiveness, constriction of thinking, horrible psychosomatic diseases, and even death! This is somewhat of a paradox, I think. The convincing part of such generalizations is only their face validity. There *are* experiments, many of them carefully done, in which these neurotic outcomes were clearly observed. Gantt's (1944) work on neurotic dogs, Masserman's (1943) work on neurotic cats and monkeys, Brady's (1958) recent work on ulcerous monkeys, Maier's (1949) work on fixated rats, show some of the devastating consequences of the utilization of punishment to control behavior. The side effects are frightening, indeed, and should *not* be ignored! But there *must be* some rules, some principles, governing the appearance of such side effects, for they *do not* appear in all experiments involving the use of strong punishment or the elicitation of terror. In Yates' (1962) new book, *Frustration and Conflict*, we find a thorough discussion of punishment as a creator of conflict. Major attention is paid to the instrumental-response outcomes of conflict due to punishment. Phenomena such as rigidity, fixation, regression, aggression, displacement, and primitivization are discussed. Yates accepts the definition of neurosis developed by Maier and by Mowrer: self-defeating behavior oriented toward no goal, yet compulsive in quality. The behavioral phenomena that reveal neuroses are said to be fixations, regressions, aggressions, or resignations. But we are not told the necessary or sufficient experimental conditions under which these dramatic phenomena emerge.

Anyone who has tried to train a rat in a T maze, using food reward for a correct response, and shock

to the feet for an incorrect response, knows that there *is* a period of emotionality during early training, but that, thereafter, the rat, when the percentage of correct responses is high, looks like a hungry, well-motivated, happy rat, eager to get from his cage to the experimenter's hand, and thence to the start box. Evidently, merely going through conflict is not a condition for neurosis. The rat is reliable, unswerving in his choices. Is he neurotic? Should this be called subservient resignation? Or a happy adjustment to an inevitable event? Is the behavior constricted? Is it a fixation, an evidence of behavioral rigidity? The criteria for answering such questions are vague today. Even if we should suggest some specific tests for rigidity, they lack face validity. For example, we might examine *discrimination reversal* as a test of *rigidity*. Do subjects who have received reward for the correct response, and punishment for the incorrect response, find it harder to reverse when the contingencies are reversed, as compared with subjects trained with reward alone? Or, we might try a *transfer test*, introducing our subject to a new maze, or to a new jumping stand. Would the previously punished subject generalize more readily than one not so punished? And if he did, would he then be *less discriminating* and thus neurotic? Or, would the previously punished subject generalize poorly and hesitantly, thus being *too discriminating*, and thus neurotic, too? What are the criteria for behavioral *maljunction* as a consequence of the use of punishment? When instrumental responses are used as the indicator, we are, alas, left in doubt!

The most convincing demonstrations of neurotic disturbances stemming from the use of punishment are seen in Masserman's (Masserman & Pechtel, 1953) work with monkeys. But here the criterion for neurosis is *not* based on instrumental responding. Instead, it is based on emotionality expressed in consummatory acts and innate impulses. Masserman's monkeys were frightened by a toy snake while they were eating. Feeding inhibition, shifts in food preferences, odd sexual behavior, tics, long periods of crying, were observed. Here, the criteria have a face validity that is hard to reject. Clearly, punishment was a dangerous and disruptive behavioral influence in Masserman's experiments. Such findings are consonant with the Freudian position postulating the pervasive influences of traumatic experiences, permeating all

phases of the affective existence of the individual, and persisting for long time periods.

To harmonize all of the considerations I have raised concerning the conditions leading to neurosis due to punishment is a formidable task. My guess at the moment is that neurotic disturbances arise often in those cases where *consummatory* behavior or *instinctive* behavior is punished, and punished under *nondiscriminatory* control. But this is merely a guess, and in order for it to be adequately tested, Masserman's interesting procedures would have to be repeated, using discriminative stimuli to signalize when it is safe and not safe for the monkey. Such experiments should be carried out if we are to explore adequately the possible effects of punishment on emotionality. Another possibility is that the number of rewarded behavior alternatives in an otherwise punishing situation will determine the emotional aftereffects of punishments. We have seen that Whiting and Mowrer (1943) gave their rats a rewarding alternative, and the resulting behavior was highly reliable. Their rats remained easy to handle and eager to enter the experimental situation. One guess is that increasing the number of behavioral alternatives leading to a consummatory response will, in a situation where only one behavior alternative is being punished, result in reliable behavior and the absence of neurotic emotional manifestations. However, I suspect that matters cannot be that simple. If our animal subject is punished for Response A, and the punishment quickly elicits Response B, and then Response B is quickly rewarded, we have the stimulus contingencies for the establishment of a masochistic habit. Reward follows punishment quickly. Perhaps the subject would then persist in performing the punished Response A? Such questions need to be worked out empirically, and the important parameters must be identified. We are certainly in no position today to specify the necessary or sufficient conditions for experimental neurosis.

I have, in this talk, decried the stultifying effects of legends concerning punishment. To some extent, my tone was reflective of bias, and so I overstated some conclusions. Perhaps now it would be prudent to soften my claims.<sup>4</sup> I must

admit that all is not lost! Recently, I have noted a definite increase in good parametric studies of the effects of punishment on several kinds of behavior. For example, the pages of the *Journal of the Experimental Analysis of Behavior* have, in the last 5 years, become liberally sprinkled with reports of punishment experiments. This is a heartening development, and though it comes 20 years delayed, it is welcome.

#### SUMMARY

I have covered a great deal of ground here, perhaps too much for the creation of a clear picture. The major points I have made are as follows: *First, the effectiveness of punishment as a controller of instrumental behavior varies with a wide variety of known parameters.* Some of these are: (a) intensity of the punishment stimulus, (b) whether the response being punished is an instrumental one or a consummatory one, (c) whether the response is instinctive or reflexive, (d) whether it was established originally by reward or by punishment, (e) whether or not the punishment is closely associated in time with the punished response, (f) the temporal arrangements of reward and punishment, (g) the strength of the response to be punished, (h) the familiarity of the subject with the punishment being used, (i) whether or not a reward alternative is offered during the behavior-suppression period induced by punishment, (j) whether a distinctive, incompatible avoidance response is strengthened by omission of punishment, (k) the age of the subject, and (l) the strain and species of the subject.

*Second,* I have tried to show the theoretical virtues of considering active and passive avoidance learning to be similar processes, and have shown the utility of a two-process learning theory. I have described some examples of the application of findings in active avoidance-learning experiments to the creation of new punishment experiments and to the reanalysis of approach-avoidance conflict experiments.

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the nature of active and passive avoidance learning are Hebb's (1960) recent remarks in his APA Presidential Address. He said: "The choice is whether to prosecute the attack, or to go on with the endless and trivial elaboration of the same set of basic experiments (on pain avoidance for example); trivial because they have added nothing to knowledge for some time, though the early work was of great value [p. 740]."

<sup>4</sup>Presidential addresses sometimes produce statements that may be plausible at the moment, but on second thought may seem inappropriate. In contrast to my complaints about inadequate research on punishment and

*Third*, I have questioned persisting legends concerning both the ineffectiveness of punishment as an agent for behavioral change as well as the inevitability of the neurotic outcome as a legacy of all punishment procedures.

*Finally*, I have indicated where new experimentation might be especially interesting or useful in furthering our understanding of the effects of punishment.

If there is one idea I would have you retain, it is this: Our laboratory knowledge of the effects of punishment on instrumental and emotional behavior is still rudimentary—much too rudimentary to make an intelligent choice among conflicting ideas about it. The polarized doctrines are probably inadequate and in error. The popularized Skinnerian position concerning the inadequacy of punishment in suppressing *instrumental* behavior is, if correct at all, only conditionally correct. The Freudian position, pointing to pain or trauma as an agent for the pervasive and long-lasting distortion of *affective* behavior is equally questionable, and only conditionally correct.

Happily, there is now growing attention being paid to the effects of punishment on behavior, and this new development will undoubtedly accelerate, because the complexity of our current knowledge, and the perplexity it engenders, are, I think, exciting and challenging.

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