

# THE MISBEHAVIOR OF ORGANISMS

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THERE seems to be a continuing realization by psychologists that perhaps the white rat cannot reveal everything there is to know about behavior. Among the voices raised on this topic, Beach (1950) has emphasized the necessity of widening the range of species subjected to experimental techniques and conditions. However, psychologists as a whole do not seem to be heeding these admonitions, as Whalen (1961) has pointed out.

Perhaps this reluctance is due in part to some dark precognition of what they might find in such investigations, for the ethologists Lorenz (1950, p. 233) and Tinbergen (1951, p. 6) have warned that if psychologists are to understand and predict the behavior of organisms, it is essential that they become thoroughly familiar with the instinctive behavior patterns of each new species they essay to study. Of course, the Watsonian or neobehavioristically oriented experimenter is apt to consider "instinct" an ugly word. He tends to class it with Hebb's (1960) other "seditious notions" which were discarded in the behavioristic revolution, and he may have some premonition that he will encounter this bete noir in extending the range of species and situations studied.

We can assure him that his apprehensions are well grounded. In our attempt to extend a behavioristically oriented approach to the engineering control of animal behavior by operant conditioning techniques, we have fought a running battle with the seditious notion of instinct.<sup>1</sup> It might be of some interest to the psychologist to know how the battle is going and to learn something about the nature of the adversary he is likely to meet if and when he tackles new species in new learning situations.

Our first report (Breland & Breland, 1951) in the *American Psychologist*, concerning our experiences in controlling animal behavior, was wholly affirmative and optimistic, saying in essence that the principles derived from the laboratory could be applied to the extensive control of behavior

under nonlaboratory conditions throughout a considerable segment of the phylogenetic scale.

When we began this work, it was our aim to see if the science would work beyond the laboratory, to determine if animal psychology could stand on its own feet as an engineering discipline. These aims have been realized. We have controlled a wide range of animal behavior and have made use of the great popular appeal of animals to make it an economically feasible project. Conditioned behavior has been exhibited at various municipal zoos and museums of natural history and has been used for department store displays, for fair and trade convention exhibits, for entertainment at tourist attractions, on television shows, and in the production of television commercials. Thirty-eight species, totaling over 6,000 individual animals, have been conditioned, and we have dared to tackle such unlikely subjects as reindeer, cockatoos, raccoons, porpoises, and whales.

Emboldened by this consistent reinforcement, we have ventured further and further from the security of the Skinner box. However, in this cavalier extrapolation, we have run afoul of a persistent pattern of discomforting failures. These failures, although disconcertingly frequent and seemingly diverse, fall into a very interesting pattern. They all represent breakdowns of conditioned operant behavior. From a great number of such experiences, we have selected, more or less at random, the following examples.

The first instance of our discomfiture might be entitled, *What Makes Sammy Dance?* In the exhibit in which this occurred, the casual observer sees a grown bantam chicken emerge from a retaining compartment when the door automatically opens. The chicken walks over about 3 feet, pulls a rubber loop on a small box which starts a repeated auditory stimulus pattern (a four-note tune). The chicken then steps up onto an 18-inch, slightly raised disc, thereby closing a timer switch, and scratches vigorously, round and round, over the disc for 15 seconds, at the rate of about two scratches per second until the automatic feeder fires in the retaining compartment. The chicken goes into the compartment to eat, thereby auto-

<sup>1</sup>In view of the fact that instinctive behaviors may be common to many zoological species, we consider *species specific* to be a sanitized misnomer, and prefer the possibly septic adjective *instinctive*.

matically shutting the door. The popular interpretation of this behavior pattern is that the chicken has turned on the "juke box" and "dances."

The development of this behavioral exhibit was wholly unplanned. In the attempt to create quite another type of demonstration which required a chicken simply to stand on a platform for 12-15 seconds, we found that over 50% developed a very strong and pronounced scratch pattern, which tended to increase in persistence as the time interval was lengthened. (Another 25% or so developed other behaviors—pecking at spots, etc.) However, we were able to change our plans so as to make use of the scratch pattern, and the result was the "dancing chicken" exhibit described above.

In this exhibit the only real contingency for reinforcement is that the chicken must depress the platform for 15 seconds. In the course of a performing day (about 3 hours for each chicken) a chicken may turn out over 10,000 unnecessary, virtually identical responses. Operant behaviorists would probably have little hesitancy in labeling this an example of Skinnerian "superstition" (Skinner, 1948) or "mediating" behavior, and we list it first to whet their explanatory appetite.

However, a second instance involving a raccoon does not fit so neatly into this paradigm. The response concerned the manipulation of money by the raccoon (who has "hands" rather similar to those of the primates). The contingency for reinforcement was picking up the coins and depositing them in a 5-inch metal box.

Raccoons condition readily, have good appetites, and this one was quite tame and an eager subject. We anticipated no trouble. Conditioning him to pick up the first coin was simple. We started out by reinforcing him for picking up a single coin. Then the metal container was introduced, with the requirement that he drop the coin into the container. Here we ran into the first bit of difficulty: he seemed to have a great deal of trouble letting go of the coin. He would rub it up against the inside of the container, pull it back out, and clutch it firmly for several seconds. However, he would finally turn it loose and receive his food reinforcement. Then the final contingency: we put him on a ratio of 2, requiring that he pick up both coins and put them in the container.

Now the raccoon really had problems (and so did we). Not only could he not let go of the coins, but he spent seconds, even minutes, rubbing them

together (in a most miserly fashion), and dipping them into the container. He carried on this behavior to such an extent that the practical application we had in mind—a display featuring a raccoon putting money in a piggy bank—simply was not feasible. The rubbing behavior became worse and worse as time went on, in spite of nonreinforcement.

For the third instance, we return to the galinaceous birds. The observer sees a hopper full of oval plastic capsules which contain small toys, charms, and the like. When the  $S_D$  (a light) is presented to the chicken, she pulls a rubber loop which releases one of these capsules onto a slide, about 16 inches long, inclined at about 30 degrees. The capsule rolls down the slide and comes to rest near the end. Here one or two sharp, straight pecks by the chicken will knock it forward off the slide and out to the observer, and the chicken is then reinforced by an automatic feeder. This is all very well—most chickens are able to master these contingencies in short order. The loop pulling presents no problems; she then has only to peck the capsule off the slide to get her reinforcement.

However, a good 20% of all chickens tried on this set of contingencies fail to make the grade. After they have pecked a few capsules off the slide, they begin to grab at the capsules and drag them backwards into the cage. Here they pound them up and down on the floor of the cage. Of course, this results in no reinforcement for the chicken, and yet some chickens will pull in over half of all the capsules presented to them.

Almost always this problem behavior does not appear until after the capsules begin to move down the slide. Conditioning is begun with stationary capsules placed by the experimenter. When the pecking behavior becomes strong enough, so that the chicken is knocking them off the slide and getting reinforced consistently, the loop pulling is conditioned to the light. The capsules then come rolling down the slide to the chicken. Here most chickens, who before did not have this tendency, will start grabbing and shaking.

The fourth incident also concerns a chicken. Here the observer sees a chicken in a cage about 4 feet long which is placed alongside a miniature baseball field. The reason for the cage is the interesting part. At one end of the cage is an automatic electric feed hopper. At the other is an opening through which the chicken can reach and pull a loop on a bat. If she pulls the loop hard

enough the bat (solenoid operated) will swing, knocking a small baseball up the playing field. If it gets past the miniature toy players on the field and hits the back fence, the chicken is automatically reinforced with food at the other end of the cage. If it does not go far enough, or hits one of the players, she tries again. This results in behavior on an irregular ratio. When the feeder sounds, she then runs down the length of the cage and eats.

Our problems began when we tried to remove the cage for photography. Chickens that had been well conditioned in this behavior became wildly excited when the ball started to move. They would jump up on the playing field, chase the ball all over the field, even knock it off on the floor and chase it around, pecking it in every direction, although they had never had access to the ball before. This behavior was so persistent and so disruptive, in spite of the fact that it was never reinforced, that we had to reinstate the cage.

The last instance we shall relate in detail is one of the most annoying and baffling for a good behaviorist. Here a pig was conditioned to pick up large wooden coins and deposit them in a large "piggy bank." The coins were placed several feet from the bank and the pig required to carry them to the bank and deposit them, usually four or five coins for one reinforcement. (Of course, we started out with one coin, near the bank.)

Pigs condition very rapidly, they have no trouble taking ratios, they have ravenous appetites (naturally), and in many ways are among the most tractable animals we have worked with. However, this particular problem behavior developed in pig after pig, usually after a period of weeks or months, getting worse every day. At first the pig would eagerly pick up one dollar, carry it to the bank, run back, get another, carry it rapidly and neatly, and so on, until the ratio was complete. Thereafter, over a period of weeks the behavior would become slower and slower. He might run over eagerly for each dollar, but on the way back, instead of carrying the dollar and depositing it simply and cleanly, he would repeatedly drop it, root it, drop it again, root it along the way, pick it up, toss it up in the air, drop it, root it some more, and so on.

We thought this behavior might simply be the dilly-dallying of an animal on a low drive. However, the behavior persisted and gained in strength in spite of a severely increased drive—he finally

went through the ratios so slowly that he did not get enough to eat in the course of a day. Finally it would take the pig about 10 minutes to transport four coins a distance of about 6 feet. This problem behavior developed repeatedly in successive pigs.

There have also been other instances: hamsters that stopped working in a glass case after four or five reinforcements, porpoises and whales that swallow their manipulanda (balls and inner tubes), cats that will not leave the area of the feeder, rabbits that will not go to the feeder, the great difficulty in many species of conditioning vocalization with food reinforcement, problems in conditioning a kick in a cow, the failure to get appreciably increased effort out of the ungulates with increased drive, and so on. These we shall not dwell on in detail, nor shall we discuss how they might be overcome.

These egregious failures came as a rather considerable shock to us, for there was nothing in our background in behaviorism to prepare us for such gross inability to predict and control the behavior of animals with which we had been working for years.

The examples listed we feel represent a clear and utter failure of conditioning theory. They are far from what one would normally expect on the basis of the theory alone. Furthermore, they are definite, observable; the diagnosis of theory failure does not depend on subtle statistical interpretations or on semantic legerdemain—the animal simply does not do what he has been conditioned to do.

It seems perfectly clear that, with the possible exception of the dancing chicken, which could conceivably, as we have said, be explained in terms of Skinner's superstition paradigm, the other instances do not fit the behavioristic way of thinking. Here we have animals, after having been conditioned to a specific learned response, gradually drifting into behaviors that are entirely different from those which were conditioned. Moreover, it can easily be seen that these particular behaviors to which the animals drift are clear-cut examples of instinctive behaviors having to do with the natural food getting behaviors of the particular species.

The dancing chicken is exhibiting the gallinaceous birds' scratch pattern that in nature often precedes ingestion. The chicken that hammers capsules is obviously exhibiting instinctive behavior

having to do with breaking open of seed pods or the killing of insects, grubs, etc. The raccoon is demonstrating so-called "washing behavior." The rubbing and washing response may result, for example, in the removal of the exoskeleton of a crayfish. The pig is rooting or shaking—behaviors which are strongly built into this species and are connected with the food getting repertoire.

These patterns to which the animals drift require greater physical output and therefore are a violation of the so-called "law of least effort." And most damaging of all, they stretch out the time required for reinforcement when nothing in the experimental setup requires them to do so. They have only to do the little tidbit of behavior to which they were conditioned—for example, pick up the coin and put it in the container—to get reinforced immediately. Instead, they drag the process out for a matter of minutes when there is nothing in the contingency which forces them to do this. Moreover, increasing the drive merely intensifies this effect.

It seems obvious that these animals are trapped by strong instinctive behaviors, and clearly we have here a demonstration of the prepotency of such behavior patterns over those which have been conditioned.

We have termed this phenomenon "instinctive drift." The general principle seems to be that wherever an animal has strong instinctive behaviors in the area of the conditioned response, after continued running the organism will drift toward the instinctive behavior to the detriment of the conditioned behavior and even to the delay or preclusion of the reinforcement. In a very boiled-down, simplified form, it might be stated as "learned behavior drifts toward instinctive behavior."

All this, of course, is not to disparage the use of conditioning techniques, but is intended as a demonstration that there are definite weaknesses in the philosophy underlying these techniques. The pointing out of such weaknesses should make possible a worthwhile revision in behavior theory.

The notion of instinct has now become one of our basic concepts in an effort to make sense of the welter of observations which confront us. When behaviorism tossed out instinct, it is our feeling that some of its power of prediction and control were lost with it. From the foregoing examples, it appears that although it was easy to banish the Instinctivists from the science during the Behavioristic Revolution, it was not possible to banish instinct so easily.

And if, as Hebb suggests, it is advisable to reconsider those things that behaviorism explicitly threw out, perhaps it might likewise be advisable to examine what they tacitly brought in—the hidden assumptions which led most disastrously to these breakdowns in the theory.

Three of the most important of these tacit assumptions seem to us to be: that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli.

It is obvious, we feel, from the foregoing account, that these assumptions are no longer tenable. After 14 years of continuous conditioning and observation of thousands of animals, it is our reluctant conclusion that the behavior of any species cannot be adequately understood, predicted, or controlled without knowledge of its instinctive patterns, evolutionary history, and ecological niche.

In spite of our early successes with the application of behavioristically oriented conditioning theory, we readily admit now that ethological facts and attitudes in recent years have done more to advance our practical control of animal behavior than recent reports from American "learning labs."

Moreover, as we have recently discovered, if one begins with evolution and instinct as the basic format for the science, a very illuminating viewpoint can be developed which leads naturally to a drastically revised and simplified conceptual framework of startling explanatory power (to be reported elsewhere).

It is hoped that this playback on the theory will be behavioral technology's partial repayment to the academic science whose impeccable empiricism we have used so extensively.

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