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The title of this talk is stated in a much too positive way for one to feel fully comfortable about it. Perhaps it should end with a question mark rather than a period. That is, it appears legitimate to ask: what sorts of implications, if any, does animal research have for mental health?

The simplest and most honest answer (assuming that we all know what the term mental health means, in the first place) is that we don't know. We don't know for several reasons. First of all, most animal work is not directed towards the solution of problems having practical import in the field of mental health. Typically, the animal investigator designs his experiments to answer specific questions of rather limited scope. Response alteration as a function of changes in drive conditions or reinforcement schedules, surgical intervention or drug injection, as well as other types of treatments (which may precede or follow some bit of learned or unlearned behavior) is what is studied. Such research attempts to correlate present behavior to previous history or to anatomical, neurological or hormonal structures and functions, and relevance of the findings for problems in human behavior is ancillary.

Another reason that we don't know is that we have yet to estimate the relative importance of sensori-motor conditioning in human behavior, as compared with other less well defined behavior-controlling events such as thoughts, hopes, long-term goals and the like. Sensori-motor conditioning may well dominate the behavior of infra-human forms; but its relative contribution to the total determinants controlling man's behavior may be quite another thing. The superiority of man's behavioral capacities over those of the highest mammalian forms (particularly when the inventions of language and number are considered) probably separated man from the ape by millions of "psychological light years." That is, the chasm between man and his closest "relative" may be so great that there may be little, if any, relationship between the *kinds* of laws which are *primarily* involved in accounting for the behavior of either species. Of course, those of us working with animals have found it fruitful to procede as if this were not the case, i.e., as if all behavior will be explained ultimately in terms of the same set of laws. But it is clear that although rats and men may learn a similar-type maze after about the same number of trials, and after approximately the same number of errors, the human Ss have to be blindfolded so that they cannot see the whole maze "at a glance" for this equality to hold. However, it is also clear that men do not interact with the world and with each

other as if they were blindfolded. They *do* see the whole maze at a glance and their behavior is predicted by this factor. One can equate rats and men in maze learning by blindfolding men; but no matter how often you lift the rat well above the maze so he too can see it "at a glance", he still remains rather dull in learning to run it.

Finally, we don't know because laws generated from finding at one phylogenetic level can apply, legitimately, only to the species investigated. Frequent contradictory findings reported for the same species suggest that even this sort of generalization may be a somewhat over-generous allowance. For reasons not too well understood at present, rats at different laboratories persist in behaving differently under essentially the same sort of experimental conditions. Contradictory findings may often result from slight changes in procedure, equipment or other features of the experimental design, which, nevertheless may be crucial. But, the fact that such slight changes *do* result in contradictory finding, illustrates how little we know about the factors determining even rather simple animal behavior. Well controlled parametric variations of different conditions entering into a research problem, with different species as one of the variables, appear to be called for. This, of course, is an argument for the comparative approach in psychology and suggests that generalizing results from one phylogenetic level to another (and certainly from the infrahuman level) should proceed only after determining empirically what sorts of generalizations the differences and similarities between the behaviors of rats, cats, dogs, apes and men will allow.

With these few preliminary remarks, I should like to mention briefly a special type of animal research which is undertaken primarily because it appears to have clinical implications. One of the major goals of such studies is to shed light on, or generate hypothesis about, the mechanisms underlying human pathology.

This special type of research can be conveniently categorized into three classes.

Class I included all studies which are clearly relevant to human pathology but which are not likely to be investigated at the human level because of certain moral and ethical considerations. Studies involving systematic destruction of brain tissue and other organs; and those involving unusual and prolonged control of life history fall under this rubric.

Class 2 included studies in which a systematic attempt is made to induce emotional disturbance in animal subjects. Conditioned anxiety studies and studies of conflict and intense frustration involving insolvable problems, or monotonous mild provocations exemplify this approach.

The third class of studies includes those which investigate changes in behavior as a function of therapeutic procedures such as electric

convulsive shock, ataraxic and other drugs, hormone therapy and the like.

Animal research of this special type is extremely useful to investigators interested in problems of mental health. Through such research, etiological factors, neurophysiological and hormonal mechanisms, and other casual agents which may contribute to human pathology, may be suggested and isolated for further study at the human level. In cases where further human research cannot be carried out, the results of animal research must, of course, remain suggestive.

I should like to use the remainder of my time to discuss briefly two lines of research which seem to have important implications for human pathology but which have received little systematic attention, as yet. The first stems from Pavlov's original observation of a "breakdown" in a dog forced to attempt the learning of a difficult discrimination. Similar "breakdowns" (i.e., abrupt changes in behavior) have since been reported by Maier (2), Richter (5), Masserman (3) and Liddell (1), among others, for rats, cats, sheep, goats and dogs after experiences involving insolvable problems, food poisoning, intense conflict and monotonous minor provocations, respectively. The distinguishing feature of such studies is that animals previously normal in their ability to learn and profit from experience, lose this ability following their "breakdowns." Such animals fail to learn subsequent and "easier" problems, to profit from untold punishing events or positive reinforcements, and they persist in responses which are both self-defeating and self-perpetuating—a phenomenon which Mowrer has dubbed "the neurotic paradox." Observation of such animals reveals that the effects of the "breakdown" transcend the experimental situation and show up as lassitude, jumpiness, a loss in weight, aggressiveness and shyness away from the experimental situation (1, 3). That is, the emotional disturbance is not situational specific.

These are very interesting observations and certainly merit further investigation. With few exceptions, however, "breakdown" experiences in animals have not been followed up in any systematic way. Investigators have done little more than note that they occur and have apparently shifted their emphasis to the conditioned anxiety, the conditioned avoidance, and the experimentally-induced conflict paradigms for studying emotional behavior in animals. Although such studies are certainly important for understanding the conditions under which emotional behavior of a non-pathological sort is acquired in animals, and probably also in man, they appear to have little relevance for the kinds of behavioral aberrations found in seriously "disturbed" animals or humans. If one wishes to analogize to the human level, conditioned anxiety, avoidance and con-

flict studies may have relevance in accounting for such phenomena as the learning of phobias and conflicts involving approach-avoidance behavior. But, these hardly constitute behavioral abnormalities in the pathological sense. It is as reasonable to expect a pilot who has been seriously wounded in battle to actively avoid airplanes as it is for a small child to avoid the flame which has burned him. If forced to board an airplane the pilot may become panicky or anxiety-ridden; but, once off the plane, he will be as "normal" and composed as the rest of us. Certainly, this is not the same sort of behavioral pathology that one sees in a neurotic or psychotic individual. Again, the rat which has been shocked in a chamber which offers no means of avoiding the noxious stimulus may behave in a highly emotional and distraught fashion while in the chamber; but once back in his home cage, he is indistinguishable from the rest of the colony in his behavior. How different from this is the rat (5) which starves to death in the presence of food, or the sheep which loses its gregariousness and wanders off by itself (1) following a "breakdown" experience!

A second type of research which would appear to merit investigation involves the conditioning of fear and secondary drives, generally, to internal states of the organism. In the clinic one is frequently struck by the complaints of neurotics and psychotics of an incapacitating panic, dread, or fear, the source of which they have no way of identifying. It is conceivable that such feelings may be responses to specifiable internal conditions which have been associated previously with physical or verbal painful events over prolonged periods of time. The association of pain with such internal states (as for example, with hunger, sex drive arousal, a full bowel or bladder, respiration failure and the like) could lead, conceivably, to a conditioning process such that the reoccurrence of the internal state would lead to the evocation of fear, or what are sometimes considered variants of fear, namely, disgust, jealousy, dread and the like. Under such circumstances the individual would be trapped with his anxious feelings for as long as the internal state persisted. This, of course, is a highly over-simplified picture of what might be one of the many etiological factors involved in neurotic and psychotic disorders. Whether or not such conditioning plays a role in human psychopathology remains, of course, to be shown. For the present I have some data (4) which seems to indicate that conditioning of fear to internal states is a possibility, at least at the rat level. In these studies, one of two groups of rats were always shocked in grill box conditioning chambers when they were hungry and thirsty ($N = 9$) while the other received shock only when satiated ($N = 8$). Both groups experienced hunger and thirst, or satiation, equally often. Also, all subjects lived in the chambers in which they were conditioned. The rats were given an opportunity to avoid

being shocked by learning to jump onto an escape incline inside each chamber within 30 seconds after a clicking noise was sounded. The escape incline was made of hardware cloth and was a permanent feature of their living quarters. The clicker remained on for two minutes; but after 30 seconds, shock was delivered to the floors of the chambers. The Ss received one, two, or three shock trials during conditioning days.

All animals were on a 48 hours satiation-deprivation regimen throughout the whole experiment. They were allowed access to food and water for the first 24 hours and then were deprived of both food and water for the next 24 hours. Animals scheduled to be conditioned under deprivation were shocked during the 24 hours deprivation period; those scheduled for shock trials while satiated were shocked during the satiation period. Thus, animals received conditioning trials (and later in the experiment during extinction, one extinction trial) every two days.

Within 40 to 60 trials over a two or three month period all 17 Ss learned to respond to the clicker and thus avoid the shock. This conditioned avoidance response (CAR) was then extinguished by presenting the clicker (without shock) while Ss were motivated by the state opposite from the one under which they had received shock trials. Thus, animals conditioned while hungry and thirsty were extinguished while satiated; and those conditioned while satiated were extinguished while hungry and thirsty. The conditioned avoidance response was extinguished within 5 to 9 single daily trials, many of the Ss failing to jump onto the incline altogether. The next step in the experiment was to continue extinction, but now under the same internal state which was previously associated with shock. Under these conditions the extinguished response reappeared and persisted for several days. That is, with the re-association of the clicker with the internal state previously associated with pain-shock, the extinguished response, presumably motivated by conditioned fear, reappeared and persisted for some time. Latency of jumping onto the escape incline and length of time on the incline (i.e., duration) were used as measures of response strength, and the results were highly significant at the .001 level of confidence.

These results are predictable from learning theory and from the principle of drive stimulus generalization. Their importance lies in the demonstration that learned fear which is "extinguished" under motivational conditions which are different from those which were in force during conditioning, is poorly extinguished, if at all—even though the overt responses, presumably motivated by the fear, no longer occur. Thus, it appears that overt behavior, originally motivated by fear, may be extinguished in the absence of extinction of the fear itself; and that fear can reappear when the internal condi-

tions present during initial conditioning reoccur (whether by accident, illness, design or other circumstances).

These findings have been verified and extended in another experiment (4) in which neither escape from shock nor avoidance learning was permitted. In this experiment evidence of differential conditioning of fear to the internal state associated with six or twenty-four hours food and water deprivation, or with satiation, was indicated, in part, by greater frequency of defecation in the conditioning chambers, by less activity in a circular alley, and by greater food and water intake when the drive previously associated with pain was in force than under other drive conditions.

We are currently extending this work to include other internal conditions than those associated with deprivation or satiation. In a study which is just getting under way, we are associating pain-shock with androgen-induced sex drive in castrated rats. In this study our aim is to determine whether or not the sexual responsiveness of castrates (given androgen) to a receptive female is altered by associating pain with the sex drive.

To summarize, I should like to review the major points I have attempted to make in this paper. The first is that the principal contribution of animal psychology to problems in mental health is to generate hypotheses potentially testable at the human level, and to point out directions which human research might profitably take. The second is that animal psychologists contribute by investigating problems having obvious clinical implications but which are not likely to be investigated at the human level. The third is that if animal studies are to have relevance for the understanding of pathological behavior in humans, it is incumbent upon investigators to seek a model, or paradigm, which produces behavioral changes in their animal subjects which appears to most closely approximate pathological behavior in humans.

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