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Discussion of "Psychological Research and Mental Health"

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Discussion of "Psychological Research and Mental Health"

By JAMES T. FREEMAN

First, let me say that I am favorably impressed by the amount and variety of active research represented on the program today. There are, of course, certain areas represented which, with my interests and experiences, I feel more compelled to comment upon. I would like to discuss these first and more extensively while leaving some topics to be mentioned only briefly.

From my standpoint, the paper by Dr. Otis raises some issues of a rather fundamental nature. The possible relations between infra-human and human behavior may always be counted on to raise some lively issues—but I sometimes wonder in so doing if the basic point is not frequently missed. Dr. Otis, in the best tradition, seems properly diffident and cautious in extending the findings from one phylogenetic level to another. Some of the parallels comparing the maze behavior of the rat with humans (with the gap of many "psychological light years") may have tended to divert us from an important point which may very well be expressed as follows: psychologists do not necessarily study animal behavior for the sake of studying animals *qua* animals but as simply a matter of experimental convenience and/or expediency, if you will. There is nothing sacred about the behavior of the rat, pigeon, chimpanzee or man for that matter. For the purposes of my argument, the title of Dr. Otis' paper could have just as well read "The implications of the experimental analysis of behavior for problems in mental health." Behavior is a property of all organisms and analysis of these properties requires the deliberate simplicity of the experimental world of which lower organisms are merely a convenient part. In short, the analysis of animal behavior would seem to be less a luxury or a matter of taste than an out and out necessity imposed by the exigencies confronting the scientist in his experimental analysis of a natural phenomenon.

It would seem that the implications of biological science are serving to present an increasingly unitary and continuous view of life and modern behavioral science seems, indeed, to be giving an assist in making dualistic interpretations of nature less and less tenable. For that matter, we could well take our cue in the use of animal research from the various medical disciplines in which the implications and applications to the health of the human population are now largely taken for granted. If our view of nature be essentially correct, we should then expect neither no more nor no less successful application of the results of animal studies to "mental health" than was the case say of the development and subsequent application of the Salk vaccine. I am confident that Dr. Otis is aware of these issues as indeed I believe he implies in his paper. I am impressed, however,

that he has perhaps understated his case somewhat and that he might be upon somewhat better ground than he cares to admit.

Dr. Otis does comment several times upon the somewhat discomfiting lack of reliability of many studies. However, I am more often impressed with the orderliness and uniformity of behavioral results once we have rid ourselves of possible hidden and priori premises as to what behavior "should be like." Data at the level of behavior once we have met these conditions, appear to me more often than not, remarkably reliable. In those cases where discrepant results appear, I am frequently not surprised considering the somewhat elaborate and circuitous frameworks within which such results are often couched. In order to obtain reproducible results, however, the researcher must frequently face the criticism of "over-simplifying" his analysis and he is then placed in the incongruous position of having to apologize to his critics for the orderliness of the data! The point, I believe, has implications for the extension of animal research to human behavior. It would not be altogether surprising if we occasionally found that data obtained at a lower level of analysis were not perfectly applicable to a human situation. The frequent charge that animal results cannot be successfully applied to other situations rests, I think, upon a rather fundamental misunderstanding of the methods of science and upon certain presumptions of the status of two research fields. In order to legitimately make such an assertion two things would need to be known for anything like an "experimentum crucis" even to be specifiable. These two things are: (1) a great deal more knowledge about the behavior of lower organisms and (2) a great deal more knowledge about human behavior. In the meantime, the best we can do is to further our knowledge as any experimental science proceeds to do with the expectation that such knowledge may, from time to time, prove helpful in certain practical affairs without expecting as a necessary property any point for point correspondence between two different levels of analysis until both levels may be clearly and rigorously specified. We can see examples in many other fields where the concepts of basic science may be extended and applied engineering-wise to improved understanding and control of nature.

Finally, there are one or two specific questions I have about Dr. Otis' research. These questions are primarily technical and perhaps we may discuss them more fully afterwards. Specifically they are: (1) In the case of the internal S^D's of human Ss which appear to have previously been correlated with punishment, just how do these arise in a given situation—they are probably not spontaneous so they may be considered as response-produced cues and if so, what are the possible S^D's for these responses in such a chain?

Also, with respect to the performance of the experimental animals—is there any control for "spontaneous recovery" using appetitive

drives only in such a situation. To what extent may we regard this as spontaneous recovery or the fact that previous extinction was not indeed anywhere nearly complete? I would like to know, just how much avoidance behavior was obtained over and above a certain amount of possible spontaneous recovery?

Next, I have just a few comments about the remaining papers for I suspect my time is beginning to run short. In the case of Dr. Heilbrun's paper, I am certainly well agreed with his statement as to the need for establishing a well-defined body of relationships in the prediction of behavior particularly in the sphere of normal behavior as this may have implications for the understanding of more unusual forms of behavior. I am interested in the results obtained with anxious and non-anxious Ss with respect to counseling in the presence or absence of cues. I fear, however, that I would best leave the specific questions until later.

I was much interested in Dr. Cohen's presentation, especially the work on operant verbal behavior since we are presently doing some similar work on the effects of different types of reinforcing stimuli—a point which he has discussed in the case of different patient groups. This work strikes a responsive chord since it seems to parallel in some respects the work being carried out at The Metropolitan State Hospital at Waltham, Mass., under the aegis of the Harvard Medical School, work with which I am sure Dr. Cohen is familiar. I think such work as this is important—i.e., an attempt to try to specify what events are reinforcing under what conditions. In this way we may ultimately hope to catalog reinforcers—a task which can certainly become tedious but which seems nonetheless essential to workers in this area.

Apropos of the "botanizing" of reinforcers, Dr. Cohen later alludes to hypothalamic stimulation as an unconditioned stimulus which appears to be without inherent reward or punishment properties—in which case I would like to know just what are the inherent reward or punishment properties of any reinforcer defined independently of their reinforcing effects upon behavior. Certainly we may design experimental situations in which traditional notions as to the properties of rewards and punishment appear to be violated as, I believe, Dr. Otis might have mentioned. When Dr. Cohen mentions that "approach or avoidance is dictated only by the conditions of the experimental design" I must confess that this struck me as a bit elliptical and left me curious as to just what those conditions of experimental design were.

And finally, in connection with Dr. Knopf's paper, which is less in line with my experience, there are one or two points I would like to comment upon. When the problem is raised with respect to dealing with parents, relatives, etc., in attempting to convince them

of the efficacy of a certain practice, I got the impression that this was somehow unique to the hospital setting.

Actually, I think the problem of "culture inertia" to various "engineering" changes is a problem common to those who would attempt to apply behavioral science to the fields of education, industry, etc., and not particularly unique to any single technology.

Dr. Knopf's comments relative to the small samples of patient population seem to be indicative of present concern over large samples of Ss. It would seem, however, that a case could still be made for the intensive systematic study of a relatively few cases, especially when these cases are necessarily difficult to come by. When some of the difficulties of studying psychotic behavior (especially obtaining any kind of reliable response measures) were mentioned, the present work by the Harvard group at Waltham occurred to me where it has been reported that operant response measures have been successful in about 80 per cent of the chronic population as compared to about 40 per cent with the standard clinical tests. I am very interested as to whether or not any such type of research is presently being attempted at Iowa City. It would seem to me that such procedures would appear to have a great deal of promise.

Once again, let me say that I enjoyed all the papers and I think we may well take pride in the amount and quality of active research represented today.

Discussion of "Psychological Research and Mental Health"

By LEONARD WORELL

Although the papers in the symposium deal with quite diverse topics, there are at least two points of similarity. First, it is apparent that all the participants felt somewhat uncomfortable about and restricted by the concept of mental health. This is, of course, understandably related to the absence of a clear definition of the term. Now, it seems to me that there are at least three ways in which the concept may be employed, two of which have been used by varying participants.

First, some seem to assume that we have a bad concept which defies definition, or that we more or less have some agreement among ourselves as to what we mean, or that a definition is unnecessary. Dr. Heilbrun appears to favor the latter by his statement that "all research has mental health implications" which I believe encompasses more research than I would be willing to concede, while Dr. Otis seems to lean in the direction of a rough consensus existing among ourselves, about which I also have considerable reservation. Aside from specific disagreements, however, it is my belief that what this amorphous approach to mental health represents is the conviction