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Pavlovian Conditioning and American Psychiatry

This symposium took place at a meeting of the GROUP FOR THE ADVANCEMENT OF PSYCHIATRY at the Berkeley-Cartaret Hotel, Asbury Park, New Jersey, on Sunday, November 4, 1962. Henry W. Brosin, M.D., President of GAP, presided. The moderators were Charles Shagass, M.D., Professor of Psychiatry, State University of Iowa; and Morris Lipton, M.D., Associate Professor of Psychiatry, University of North Carolina.

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DISCUSSION

2. Ogden Lindsley, Ph.D.

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This discussion will be given from the point of view of a researcher in a specialized field, which is free operant conditioning. However, in the course of conducting this research, I have had 10 years' experience with chronic psychotics in a state mental hospital.

First, I want to point out that research scientists must speak, and, if you will, think or respond in two different languages simultaneously: (1) the language of their method or of their research technique; (2) the language of the field in which they are currently trying to apply it. This is merely to obtain problems of clinical relevance with which to work and then to be able to manipulate variables according to appropriate methodological designs. However, one trouble with all of this speaking is that words are charming and suggestive, but they are never compelling. This is relevant to Dr. Razran's history of the development of Russian physiology. If you remember, the span was from 1863 to 1903. Sechenov's words did not seem to do anything for 40 years until the development of Pavlov's method.

Development of Pavlov's Method

I also want to point out that Pavlov's method was empirically determined. He did not react to Sechenov's words, unless you consider Sechenov's general descriptions of how to conduct decent research through observation and so forth as being relevant. Pavlov's method was developed quite by chance, when he observed in his gastric investigations that the dog salivated when the man who delivered the food powder was there without the food powder. This

“psychic” secretion attracted his interest. What he did learn from Sechenov was how to go about studying this thing. He killed 62 dogs before he developed a fistula to get saliva out of the dog’s mouth without contamination and with a pretty good representation in terms of amount. That was during a time when dogs were very valuable in Russia, and many people thought he had gone off his rocker, or something of that kind. But all that Pavlov was trying to do was to develop an appropriate method. The reason he was working with an intact dog was that he had been convinced one should not confound variables and that most of the earlier gastric work had been conducted in animals under anesthesia. He wanted to develop techniques which would provide gastric secretions from animals at the time they were alert and about to ingest. I think the 40-year lag is interesting. The words existed, but the method had not been developed, and the words themselves did not compel the development of the method.

In 1898, Thorndike’s method, using the cat in the puzzle box, to study “animal intelligence,” was also independent of all these words about conditioning.

In 1927, Ivanov-Smolensky started studying a new kind of behavior which the Pavlovians call the “orienting reflex.” With this method a child could squeeze a bulb and release a little tray which would permit a piece of candy to drop in front of the child. The stimulus here comes after the response. It is consequential. That is, I think, the only difference between the operant and classical conditioning. In classical conditioning the stimuli precede the responses; in operant conditioning one studies the responses in terms of what effects they have, or their consequences.

It was not until 1938 that we had a really useful method for investigating the operant conditioning. Between Thorndike’s work of 1898 and Skinner’s work in 1938, there were all kinds of talk about conditioning, but no really new methods. Skinner’s method was empirically determined. He was trying to build an automatic maze, and he ended up with a box with a lever.

I think it is significant that it takes about 40 years for a new method to come about. One reason is that words are so charming

and so suggestive that they blind us to the chance events by which we might empirically determine methods. In about 40 years the words lose some of their charm when we get a little perspective on them. My point is that I think we need more than verbal rapprochement for apparently logical interpretation of a problem; we need methods.

Signal Systems and Psychiatry

This brings me to my reactions to Dr. Bridger's really excellent and brilliant interpretations of the problems of psychiatric patients in Pavlovian terms. I don't know what you do about this. It is a nice teaching device, and may help to develop some clinical techniques, but I doubt that it will help to eliminate psychoses, because words are really not as powerful as new methods.

One reason that we have more words than data is that words cost very little, and that is why we make so many. Data are very dear, and that is why we have bought so little. To clarify the point further, I don't know of any method whereby I can determine whether a response of a patient belongs to the first or second signalling system. From a theoretical point of view, Pavlovian, Freudian, or other, I can interpret any response as belonging to the first or second signalling system. However, I cannot separate the two with any methodological device. What I have done is to manipulate people, not the patient. I have manipulated researchers. I have got them to agree with me. I can do that, but the psychotic still has his psychosis. I suspect that when we do get methods to analyze what this first and second signalling system may be, we will find not two systems but a continuum of systems. I base that prediction on the observation that any method which has been able to measure two previously conceived digital areas has shown them to be not black and white but all shades of a dynamic, shifting continuum.

To make things even more difficult, given responses are liable to change the signal systems on which they depend. This will be influenced by the environment and by the frequency with which they have been emitted in the past. For example, driving a car is

a complicated response. It is difficult before you learn it; it is a little less difficult immediately after you have learned it. At those times, it may be severely affected by anxiety or alcohol. On the other hand, after 10 years of driving a car in New York City, a taxi driver can drive his car beautifully under severe anxiety stress conditions or while under the influence of a drug or alcohol. Something has happened to this response functionally which topographical description does not handle.

Characteristics of Free Operant Conditioning

I should like to point out several ways in which I think free operant conditioning differs from those of other experimental conditioning. First, the method differs from earlier methods in that observation or recording is continuous. Our recording device can follow any response frequency of which the subject is capable. No behavior can occur without being measured with this kind of recording technique. It thus has great methodological power when intermittent phenomena are studied, and I can assure you behavior pathology is highly intermittent. If we have found anything in 10 years, it is that the levels of severity of patterns of psychoses shift from minute to minute, from hour to hour, from day to day, from week to week, and from month to month. A spot check every six hours yields variable data which are almost useless for research purposes. Without continuous recording, one obtains shifting patterns, with which it is difficult to work.

A second point is that we study behavior by its consequences, so we can move to much more complicated behavior without measuring corollaries of the behavior, but actually measuring the behavior itself. Suppose, for example, we were analyzing psychotherapy and recording it; listening and looking at the behavior of the patient simultaneously and continuously by giving him a switch. The faster he operates the switch, the more loudly he hears his therapist. We don't know what this will reveal, but we do know this is the process of psychotherapy. This may not be what happens therapeutically, but this is how you "inject" the therapy. This is the method of treatment, the technique whereby you give the agent

to the patient. It would be extremely difficult to measure the looking and listening behavior of a person by means of the salivation response. One might measure some correlate of a frightened look, but would not measure the looking *per se*.

A third characteristic of free operant conditioning involves its ability to measure functional properties of behavior more closely than any other method. This big move toward functional measurement and description of behavior has not really been covered in the history that was given to us. It probably starts with John Dewey and the early Chicago functional school. Skinner did part of the job when he made the point of defining a reward as that which reinforces or rewards. Most psychologists became irritated at that point and said, "You said nothing." However, the freedom allowed by Skinner's use of the word, reinforcement, permits workers like myself to deal very easily with two individuals, of whom one works to punish his wife and the other to avoid punishing his wife. In both cases we have a reinforcing stimulus. We have a reward, and it should follow all of the properties of food to a dog, or the general properties of all rewards. The difference between these two individuals is perhaps of clinical interest, but it does not disturb my methods or my interpretative descriptive systems because I have defined them functionally.

Behavior, Psychiatry, and Research

This leads me to a major problem that we face, which I call "topographical punning." For example, what is crying? Is it tears rolling down from the eyes? Psychologists, myself included, continually make the error of thinking that if we have a crying man we have an unhappy man. Even worse, if we have a crying dog, we have experimental unhappiness. If we have a dog who stands in the corner for four hours, we like to think we have induced experimental catatonia. I call this a response topographical pun and I think all human behavior eventually will have to be described in terms of its effect on something else. The crying in which we are interested is the crying that interferes with all other behavior. Crying that co-exists with high production is an interesting thing, but

not of clinical importance; however, crying that prevents ingestion, sleep, work, all normal responding, is a severe medical problem if it runs on for three or four days. By the same token, if you take out the crying, you still have this problem, which you call acute depression.

I think psychiatry is today in the same dynamic, pregnant state of growth and expectancy that physiological medicine was in one century ago when Claude Bernard made his excellent and successful appeal for experimental medicine. We do not need more chemicals. We do need more clinically relevant methods of objective and culture-free measurements of behavioral pathology. Psychiatry does not need researchers applying classical dog techniques to man. It does not need clinicians applying Pavlovian terms to clinical problems. It does not need researchers applying operant pigeon techniques to man, or clinicians applying Skinnerian verbology to clinical problems. It needs a new group of researchers and clinicians who can speak each other's languages—this can't be a one-way street—and who can respond accurately to each other's symbols or language, and who will mutually develop what we hope to be our new language. These researchers should not make the errors that both Pavlov and Skinner have made; rather, they should approach the new problems with the same creativity that Pavlov and Skinner used in approaching their problems. In other words, they should not imitate Pavlov's dog technique but they should imitate the technique he used to develop the dog technique.