I.
THE ROLE OF MODELS IN CLINICAL PSYCHOLOGY

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Nowhere else has the need for definition been felt more than in this symposium, despite the ingenuous disclaimer by our chairman that the term "models" rather than theories was used because "models" is more modest! But modesty is observed as rarely in scientific models as it is in fashion models. He did insist that the approach be pragmatically rather than theoretically oriented. This was hardly a sufficient guideline, as the papers that reached me soon indicated. They ranged from such global unlimited frames of reference as "humanistic," through behavioral and neurophysiological models, to the more technique-oriented limited scope of the perceptual-cognitive model.

In order to make myself clear, let me indicate to you the generally accepted definition of model, attempt to distinguish it from theory, and try to bring some order out of the presentations to this symposium by indicating the type of model each presentation fits into.

In trying to formulate an acceptable definition of "model" I turned to the philosophers of science, and for those of you who want to delve into this problem I recommend Abraham Kaplan's *The Conduct of Inquiry* (Kaplan, 1964). He points out that behavioral scientists can be categorized according to their conceptual or cognitive styles into six groups: (1) literary, (2) academic, (3) eristic, (4) symbolic, (5) postulational, and (6) formal. The literary style is primarily phenomenological, and is represented in this symposium by the humanistic approach. The academic style is characterized more by ideational concepts than by observational data, and is represented here by the psychoanalytic model. The eristic style has a strong interest in proof utilizing experimental and statistical data and is best represented by the Pavlovian and behaviorist approaches. The symbolic style involves mathematical formulations such as those represented by the psychometric approach, which is not represented here. The postulational method recalls Hullian models, while the formal style is the most abstract of the models. It is both symbolic and mathematical, but in addition transcends any specific subject matter depending on the pattern of relationships
holding among the symbols themselves; however, it is hardly ever used in the behavioral sciences.

Corresponding to each of these cognitive styles, a model can be built to suit the needs of a given field. In the clinical field, the phenomenological model still holds first place. But there are considerable developments in the utilization of the more advanced models, most of them eristic in nature.

Which model is to be adopted depends on the degree of development of the field and to a certain extent on the personality of the experimenter. Fields of high articulation like physics can utilize the more mathematical models. Fields like geographical exploration in the last century had to resort to description or phenomenology. Clinical psychology probably still resembles more geographic discovery than physics.

But one must remember that scientific progress stands on two major supports: discovery and verification. We know a good deal about verification but the context of discovery is still unknown territory.

Scientific models arise out of the interaction between observations and schematization—and its the interaction between these two which permits science to develop. Schematization represents the creative effort; observation can then follow to determine whether the scheme is appropriate or not to the observations. This is where statistics enter as a way of determining whether the data fit the model. Thus, the schematizers storm the beachheads while the verifiers come later to consolidate and bring order into the new territory.

Good models are those that permit new hypotheses to emerge out of their structure. For this reason, good models die young, and out of their ashes, phoenix-like, rise improved structures which fit the observations better. Only the untestable models never die. They don't even fade away, unfortunately! (Zubin, J. 1952)

With all of this introduction, where do we stand with regard to models in clinical psychology? For 34 centuries, ever since the Ayura Veda of the ancient Hindus, we have had phenomenological or descriptive models. These are important since they delimit the territory in which we operate. But description is not enough, despite the claims of our behaviorists. We must take the next step and determine what are the causes that bring about clinical problems. If we do not know the causes, we must postulate them and derive hypotheses for testing them. In the search for aetiological models, the following come to mind: ecological, developmental, learning, hereditary, internal environment, and neurophysiological. (Zubin, J. 1966a & 1966b). Space permits only a brief sketch of each.

The ecological model is built on the assumption that all man-
kind is vulnerable to mental disorders, that given sufficient deprivation and stress-producing loads or other alterations in our environment, our behavior would be altered to the point where the ability to continue to live normally as independent individuals in society is endangered. Under this model certain expectancies exist with regard to social-cultural norms, and an individual who deviates from these social-cultural norms is regarded as potentially mentally ill. The techniques which are necessary to determine the presence of such deviation are primarily culture-dependent techniques, and we shall turn to them when we discuss the tools that have been developed for this purpose.

The second model, the developmental model, and the third, the learning theory model, are built on the assumption that mental illness develops as a result of some specific deprivation or interference during the critical period of development or that the source of deviant behavior of the mental patient is to be sought in his reinforcement history. In contrast with the social-cultural model which looks for deviations from expected social-cultural norms which may be indigenous to each culture, the expectancies derived from the developmental model and from the learning theory model are not culture-determined in the same sense, and may be regarded as culture-fair. By this, we mean that various expected behaviors from which the person deviates have trans-cultural translations or equivalents and we can thus speak of greeting behavior and its deviations or of bereavement behavior and its deviations or of the response to reinforcement and its deviations. While the actual response to these forms of stimulation are not identical from culture to culture, it is possible to establish the correspondences across cultures and hence we can call them culture-fair.

Finally, we have the genetic model, the internal environment model and the brain function model which indicate that something is awry either genetically or internally or in the brain functioning of the mentally ill person. These are sometimes subsumed under the name of “medical model.” These deviations may be in a sense culture-free, insofar as an individual who is vulnerable to an illness may yield indications that he is deviant in regard to his functioning in one or more of these three areas regardless of his social-cultural milieu. These give rise to culture-free indicators, which will be discussed later.

In attempting to develop a systematic approach to the assessment of normal and patient behavior, a cataloguing of the types of measures available and the methods for eliciting them experimentally was developed. (Burdock, Sutton & Zubin, 1958) It consists of a Mendelejeff-like table in which the responses of the individual range from physiological, sensory, perceptual, psychomotor, to conceptual to constitute
one coordinate, and the methods for eliciting these responses constitute the second coordinate. It is clear from this analysis that as one moves from the physiological to the conceptual level, more and more prior experience and learning becomes involved and the memory storage of the brain is involved. To the extent that the physiological level of response is not as dependent on memory storage derived from the cultural milieu as the conceptual, it is perhaps easier to find less culture dependence in the physiological responses and in the next level—the sensory responses. In our search for culture-free indicators we have limited ourselves largely to the physiological and sensory level, especially those that occur in the first 1,000 milliseconds following stimulation. The culture-dependent techniques are to be sought largely on the conceptual level, or on the perceptual and psychomotor level, if the encoded memories of past experience are involved. The culture-fair technique cannot be brought into relationship with the other two types of techniques because the culture-fair technique involves a translation from one culture to another of the stimulus as well as of the elicited response without altering the importance of deviations in the behavior in question with regard to psychopathology. The behavior itself could range throughout all the levels from the physiological to the conceptual.

Which of these models is a man to live by? First of all, we must understand that models are not guides for research except in a very vague general way. The chief purpose of a model is communication. After the experiment is done, after the hunch is tried out, we must find some way of connecting up our findings to the corpus of certitude in the field. This is where the model comes in. It permits us to relate our findings to those of others and in this way makes communication possible. Second, models are only scaffolds which are to be torn down once the structure is stabilized. One must not fall in love with his model as some passionate scientists have done, and above all one cannot take them home or marry them. For then all objectivity is foredoomed! Thirdly, there is no point in arguing about which model is better. It is all a matter of personal preference. Some prefer to deal with holistic approaches, and science benefits from all of these approaches as long as they arouse controversy that can be settled through observation leading to increased knowledge.

The scientific models described above can be grouped into three groups from the points of view of culture and past experience. The hypotheses emanating from the ecological or social-cultural model can be tested at this time only by means of culture-bound techniques such as the interview. Even though we have provided objective systematic structured interviewing techniques, we cannot eliminate the cultural bind in
which interviewing finds itself. On the opposite end of the spectrum the hypotheses arising from genetic, internal environment, and neurophysiological models can be tested by techniques that are relatively culture-free—such as pupillography, reaction time and evoked potentials. Midway between these two extremes we have the developmental model and learning theory model whose hypotheses can be tested by techniques which, though culture-bound (greeting behavior, grieving behavior, etc.), nevertheless have cross-cultural equivalence.

Which of these models should one adopt? Which one is likely to pay off? These are very difficult questions to answer. In daily clinical work, the effect of these models is probably minimal, since much of diagnosis and therapy is still based on rule of thumb. In the evaluation of clinical work and in the futherness of clinical knowledge, the models do make a difference. Furthermore, in teaching and training, the models also make a difference. In my own teaching, I begin with the assumption that we do not know enough to choose between these models and therefore can eschew none of them. It is my belief that the clinical psychologist of the future will have to become equally adept in the techniques provided by each of these models. Only in that way can he keep up with developments and be prepared to take advantage of the break-throughs likely to occur. Just where the lightning will strike, no one can tell, but those who are ready for it will be the ones who make the advances. We might follow the example set by some of our medical colleagues and demand examinations of practitioners every so often.

In this symposium, Dr. Brown has given us an example of a phenomenological model—humanistic—which is persuasive and gives us a picture of man as a total indivisible unique organism, creative rather than determined, more dependent on his system of values than on his instincts. It is useful as a guideline for clinical practice; but as Dr. Brown points out, it is better for generating hypotheses than for testing them. It belongs essentially in the context of discovery rather than in the context of verification and it is essentially culture-bound and literary in style.

Dr. Guttman provided an example of the academic type of model—psychoanalysis—which is primarily ideational rather than experimental. This point of view is best illustrated by the plaintive eulogy made by friends of Robert Boyle:

"When Robert Boyle died in 1691, Christian Huygens and Gottfried Wilhelm von Liebnitz commiserated that he had wasted his talents trying to prove by experiments what they knew to be true in the light of reason—that he was more interested in observation than in reasoning and had left no unified body of thought" (Hall, Marie B., 1967).
This reminds me of the only contact that I ever had with Freud. As a graduate student several of us tried to test the Oedipus hypothesis. We prepared a test of parental preference and discovered that, up to age six, the Oedipus hypothesis seemed to be tenable. We reported it to Freud only to receive as our reward a post card bearing the message, "Quite American, but I cannot see what you can prove with your statistics." Later on, relating this to one of the outstanding psychoanalysts, I was told, "Suppose you came to the Pope with a proof that God exists. What do you suppose he would say?" It wasn't until I rummaged through the remnants of Freud's library at the Psychiatric Institute, which represented Freud's early library, left behind in Vienna when he fled to London, that I found what seemed to me an answer to my puzzlement. Freud's model of causation was based on Mach's early models of physical causation. There was always a prime mover; one mechanical arm moved another; there was no room for contingency. Everything had a cause and chance was not to be countenanced in his scientific thinking. That is why he had no use for the statistical testing of his Oedipus hypothesis.

My attempts to weigh psychoanalysis on the scales of scientific inquiry are, however, puny in comparison to the sweep of ego psychology in relation to collective life, as Dr. Guttman envisions it. But its sweep, though personally moving and cosmic in scope, belongs to philosophy rather than scientific psychology. At best it is prescientific in its stress on subjective fundamentals.

Dr. Ullman presents us with a model based on learning theory, and stresses its freedom from personality theory and from the need for diagnosing and classifying patients. Like other learning theorists who have applied themselves to the clinical field, he stresses that the source as well as the maintenance of disordered behavior is to be found in the reinforcement contingencies of the individual. It is an appealing model and embodies what perhaps is experimental psychology's greatest contribution to clinical work—behavior therapy. The claims, however, for its omnipotence reminds one of the similar claims of psychoanalysis. Thus far, the most one can say is that for phobias it seems to work, for other neuroses it is questionable, and for psychoses probably inapplicable except for rehabilitation of "recovered" patients.

Dr. Reitan has provided us with a good, careful description of the neuropsychological model, which limits itself to brain damage as its essential core and points out, for example, the distinction to be made between a reading deficit based on temporal-lobe damage and one based on a phobia. Striking as this distinction is, we must not lose sight of the fact that a phobia does not exist in the mind alone; it also has a neuro-
physiological substrate. We had thought for a long time that hysterical anesthesia was mainly "mental" but Hernandez-Peon (1963) has shown that the evoked potentials to a tactual stimulus from the anesthetic area do not exist—the stimulus does not register in the cortex.

In order to extend the neurophysiological model from neurology to psychology we must include dysfunction as well as lesions, and I have no doubt that Dr. Reitan would agree. How to measure the neurophysiological characteristics of schizophrenics, manic depressives, etc. is the new frontier. We are no longer looking for lesions or disorders in neural conductivity, but for differences in function. Evoked potentials seem to be a new weapon forged for this battle.

We now come to Dr. Gillis' more specific model, the perceptual-cognitive model, a most interesting approach. I had difficulty in integrating it into this discussion, because of its narrow gauge compared to the wide-gauge models discussed earlier.

I wish we could demonstrate not only that schizophrenics can do worse than normals, but where they can do better. That they can do worse has been known for a long time, and though the experimenter can blame it on multiple vs. singular cues or on personal vs. impersonal values of the stimulus, it may be more parsimonious to blame it on lack of attention, motivation, or on reinforcement history—all too well-known as determinants of schizophrenic behavior. If, however, we can demonstrate that the schizophrenic does better, then motivation, etc., fall away as explanations. It has been our experience that as we control more and more closely the testing situation, insure against lack of motivation, and limit the options for the way a task can be performed, the differences between schizophrenics and normals tend to diminish. (Zubin, J. & Sutton, S. 1969)

The whopping big differences come from the situation where the above mentioned variables are not controlled. But this does not dismay us, since the differences, though small, that do persist may tell us why the schizophrenic behaves differently. If he really processes information input differently from the normal, it should be detectable in a manner independent of motivation and attention.

To sum up: I agree with Dr. Reitan's comment that there is no single model suitable for all seasons. Each experimenter selects his own in the light of his interests and personality, and each type of illness or disorder is contributed to by each of the models. The question is, how important is each model in a given situation, how important in a given case. The only answer we can give today is to turn to the epidemiological model, which can hopefully give relative weights to each model. It has been instrumental in casting light on the sources of general paresis, pel-
lagra with psychosis, PKU, and the integration of ecological considerations in the mental disorders, as was the case in the Midtown study and similar studies, and it bids well to cast light on the disorders whose sources are still unknown.

REFERENCES


