Some Scientific Models for Psychopathology

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There are a variety of approaches that have been used in the past for detecting, diagnosing, prognosticating, and evaluating psychopathology. Among these may be mentioned the psychiatric approach, the psychological approach, phenomenological approach, clinical approach, and many others. The biometric approach, which I represent, was hardly heard of in the halls of psychopathology before the end of World War II. Biometrics is a science which applies measurement to living organisms. The biometric approach to psychopathology is essentially an attempt at a scientific evaluation of the behavior of mental patients, including their characteristic thoughts and emotions, as well as overt behavior. The scientific evaluation of the behavior of mental patients now falls into the interstitial area between the social, biological, medical, and physical sciences, and there is no one discipline in which the objective measurement of this behavior can be pursued regardless of territorial rights. Biometrics research is trying to provide a platform for the study of the behavior of mental patients with whatever measures become available. While no justification for introducing objective measures is necessary, one need only point to the three or four billion dollars spent annually for mental disease in this country and the two-thirds of the budget of New York State spent on mental hygiene to indicate the importance of measurement in this field.

The birth of biometrics is a reflection of a ferment that has characterized the field of psychopathology during the past decade and which has brought about the current revolution in our midst. This revolution has two aspects: management and research. I have elsewhere discussed the revolution in management. Here I shall direct my comments to the revolution in research. That such a revolution has occurred is attested to by the ten-fold increase in funds for research, the entrance of such disciplines as neurochemistry, sociology, anthropology, biometrics--fields which had rarely crossed the thresholds of psychopathology previously--and the birth of so many new journals in the field. I am not going to sit in judgment over the value of these new approaches; I shall merely try to report their impact.

In order to discuss adequately the current revolution in research it is necessary to make use of scientific models through which progress in science is usually described. While research in the laboratory and the clinic rarely if ever follows the sequence laid down by philosophers of science, communicating the results of experiments must conform to a medium of communication which is readily understood. The scientific model seems to be such a medium. The definitions, assumptions and dimensions specified by the scientific model permit the generation of testable hypotheses which can then be subjected to further experimentation to
test the tenability of the scientific model. What are the assumptions, definitions and dimensions which underlie psychopathology? An examination of the dimensions used in the classification of mental disorders might serve as a starting point.

Man has always been impressed by the manifestations among his own kind of peculiar mannerisms and speech, hallucinations, catatonic rigidity, unusual attitudinizing, stupors, anxiety, depression, cyclic variation in mood, motor and sensory impairments and psychological defects in the cognitive sphere, memory and intelligence. Originally even blindness and deafness were regarded as psychopathological. It is interesting to note however that even as far back as 1400 B.C., the Fragments from the Ayure Veda (see The Caraka Samhita, 1949), ascribing deranged mental functions to malevolent devils, give a description in which the schizophrenic stands out boldly against other types of ailments in the following terms: "one who is gluttonous, filthy, walks naked, has lost his memory and roves about in an uneasy manner." Toxic confusions due to injection of poisons, alcoholism and other exogenous conditions were differentiated from schizophrenic-like behavior, elation, depression and mental deficiency. The organic-functional distinction, our first dimension, was already in existence 3300 years ago! As soon as a mental disorder is traced to some organic cause, however, it ceases to belong to the psychiatric fold and is handed over to internal medicine or neurology as was the case with general paresis, pellegra with psychosis and will probably be the case with phenylpyruvic oligophrenia. Only diseases of unknown origin tend to remain in the psychiatric domain.

The second dimension which emerges from our analysis is the hereditary-environmental continuum. There is a long history of attempts at discovering the hereditary basis of mental disorders as opposed to their environmental causation.

The third dimension emerging in our analysis is the multiple causation versus unitary causation of mental disorders. This raises the issue whether the mental diseases constitute different disease categories or whether they are reflections of a unitary deficiency, namely inability to adapt to society. The evidence today seems to be quite clear that at least certain types of illnesses have such varying courses and outcomes that to put all mental illness into one grab bag would be a return to primitive classification systems.

The fourth dimension in our analysis is the question whether to regard mental disorders as diseases or as reaction patterns. This rests upon the assumption of continuity between normal and abnormal in the development of mental disorders. According to this approach the personality of an individual is a primary focus and whether or not he will develop an illness will depend on life's vicissitudes. The relation between personality and psychopathology has been studied for a long time but thus far very few facts about the relationship between them have emerged. There are three points of view on the matter. The first would identify personality with psychopathology and thus make the personality of the schizophrenic, for example, his psychopathology and his psychopathology his personality. The second view would indicate that personality develops of its own right and that psychopathology comes in as a blight and interferes with this development. This is the basic view of Adolf Meyer's reaction pattern approach. The third point of view is that personality and psychopathology are independent and that anyone can become a schizophrenic or a neurotic regardless of the personality that he has had up to the point when the disease developed. At the present time the data on premorbid personality is so sparse and so scattered that each of these three points of view is equally tenable and therefore we must make room for reaction patterns as a possible description of
mental disorder. However, for a reaction pattern approach to maintain itself it will have to include the variety of data that have been gathered through genetic investigations which seem to indicate that not everyone who is exposed to similar vicissitudes seems to develop similar personality deviations.

Another dimension which is implied rather than spelled out in our nosology is the exogenous-endogenous dichotomy. This is reflected in the reactive vs. endogenous depressions as well as in the reactive vs. process dichotomy in schizophrenia.

The final dimension we might discuss is the acute versus chronic dimension or deterioration versus intactness. This has had a long history. It goes back to the days of Morel in the beginnings of the nineteenth century when degeneracy was regarded as one of the characteristics of mental disorders. This influenced Kraepelin so much that he tried to include deterioration as part of the diagnostic pattern. This, too, has left its imprint on our nosology. Whether deterioration was part of certain disease processes or whether it resulted from the type of custodial care available is still an open question. With today's greater tolerance of deviant behavior and higher and higher release rates we probably will see less and less reference to deterioration as a possible outcome.

Each of these continua, the organic-functional, hereditary-environmental, unicausal vs. multicausal, disease vs. reaction pattern and acute-chronic dimensions, have become embedded in our current nosology. Although there is no generally accepted evidence for or against the existence of any of these dichotomies, they enter subtly into the making of diagnoses, and are at the bottom of much present day confusion when descriptive categories are mixed with aetiology.

The changes in behavior accompanying mental disorders have been attributed to a variety of aetiological factors, and around each of these a suitable scientific model can be erected. In reviewing this field of aetiology, the following scientific models seem to dominate: (1) social-cultural model (2) developmental model (3) conditioning or learning model (4) genetic model (5) internal-environment model and (6) neurophysiological or brain function model. Until recently, the first three models—social-cultural, developmental and learning were the most prominent. In recent years, genetics, internal-environment, and brain function models have become more popular.

Before introducing the aetiological models it might be well to point out that at the present time the most generally accepted approach to psychopathology is the naturalistic approach of description of behavior. This is always the forerunner of more specific models, since without a knowledge of the phenomena no aetiology can be understood. We have had several hundred years of description by astute observers and much knowledge has been gathered. On the basis of these observations, a rather loosely-developed structure has been developed, in which observation and aetiological inferences have been intertwined. It is time now to undertake a critical evaluation of the tenability of the structure and of the validity of the inferences.

The social-cultural model is built on the assumption that all mankind is vulnerable to mental disorders and that given sufficient deprivation, stress-producing loads, or other alterations in our environment, our behavior will be altered to the point where our ability to continue living normally as independent individuals in society is endangered. The evidence for social-cultural-environmental pressures as aetiological agents come largely from studies of socio-economic
status, isolation, educational and social deprivation and social-cultural uprooting in immigration or migration or rapid acculturation. Even the most sanguine environmentalist will not be satisfied with merely pointing to the above mentioned factors as "causal" agents, but will try to determine just how these malignant factors bring about their deleterious effect. While the story is far from told, there is already sufficient evidence to at least question whether these factors "cause" mental disorders.

The early ecological studies which demonstrated a negative correlation between socio-economic status and rates of certain mental disorders such as schizophrenia were found to suffer from the well known "ecological fallacy" of attributing the correlation between rates to the correlation within people. In some of these studies, the association between mental disorder and area of residence was traceable to the proportion of individuals living in isolation, regardless of whether the area was high or low on the socio-economic ladder. Since so many more "loners" lived in the poorer districts, it appeared as if the poorness of the district produced the illness, while in fact, the high rate of disorder was fed by the high proportion of "loners." Whether the "loners" drifted into the poorer areas is not always very clear. Recent studies from England, however, tend to lend credence to the "downward drift" hypothesis. Morrison (1959) has recently shown that though the distribution of occupations of schizophrenics in their premorbid state was below that of the distribution of their normal peers in the general population, the distribution of the occupations of their fathers was not different from that of the rest of the population.

In a more recent study, Goldberg, E. M. (1963) following upon Morrison's work, found that:

"The main evidence of individual downward drift is the ability of schizophrenic patients to win places at grammar schools, though they end in semi- or unskilled jobs. The employment histories showed that in their adolescence many patients pursued varied careers, a considerable proportion aiming at professional or technical jobs; they still fitted broadly with the career expectations of their home environment.

The discrepancies in social performance between father and son could be mainly attributed to the disease process. Patients whose illness had an insidious onset at adolescence did not attain any professional or technical skills; those whose illness started acutely before admission dropped in social class shortly before admission; while those who were mentally subnormal as well as schizophrenic did not achieve any level of skill at all.

This social drift appears to affect the highest and lowest social classes most severely. Only one patient out of 13 grammar school boys attained social class I or II status, and over half of those in social class V had dropped out of the labour market by the end of the survey. On the other hand, two thirds of the patients in social classes III and IV survived in jobs requiring a moderate degree of skill."

The fact that economic depressions and wars do not increase mental disorders significantly and the fact that when age, sex and ethnic group are controlled, even immigrants do not show markedly higher rates than the general population, lead one to conclude that the aetiological claim for the social-cultural forces
is not proven. A more thorough review of the evidence which has been presented elsewhere leads to the belief that social-cultural forces may elicit a mental disorder or may even occlude it, but cannot cause it unaided. However, much more research is required to transform this belief into fact.

Despite this conclusion, the fact remains that the detection, diagnosis and even rehabilitation of the mentally ill today rests on a social-cultural framework, deviations from which identify the mentally ill, and return to which constitutes the basis for improvement. Barbara Wootton (1959) has formulated this issue in the following way:

"...the anti-social behavior is the precipitating factor that leads to mental treatment. But at the same time the fact of the illness is itself inferred from this behavior: indeed it is almost true to say that the illness is the behavior for which it is also the excuse. But any disease, the morbidity of which is established only by the social failure that it involves, must rank as fundamentally different from those of which the symptoms are independent of social norms.

This distinction will, moreover, still remain even if we reach the stage, as we very well may, when every mental process has its known physical accompaniment, and when our present dualistic language, along with the distinction between 'organic' and 'functional' disorders, can be discarded. For even then it will still be true that some abnormalities are deplored because they cause fever or boils, others because they induce a disregard of property rights—even though it may be shown that the latter no less than the former, are associated with happenings in the stomach, the liver or the brain, and can be cured by suitable pills, injections or electric shocks. Even in this case a social judgment is still implied in the decision to rank the thieving tendency together with its bodily concomitants as symptoms of disease or dysfunction; for if it had not been for their social consequences, these physical concomitants would never have been reckoned as abnormal at all. In a sense, therefore, the effect of extending physical 'explanations' to cover all forms of aberrant conduct would be to infuse into certain conceptions of physical health elements of value-judgment comparable to those which already bias the terms in which mental health is defined. Long indeed is the road to be travelled before we can hope to reach a definition of mental-cum-

physical health, which is objective, scientific and wholly free of social value-judgments; and before we shall be able, consistently and without qualification, to treat mental and physical disorders on exactly the same footing."

In summarizing the social-cultural model, we might point out that the current revolution in management in psychopathology with regard to hopefulness of treatment, reduction of patient population, rehabilitation etc., is to a considerable extent a social-cultural change involving change of attitude on the part of patient, family and therapist. Hence, though social-cultural forces may not be so important in aetiology of some disorders they are of great importance in detection, treatment, and rehabilitation.

The developmental model for aetiology is built on the assumption that mental disease develops as a result of some specific deprivation or interference during a critical period in development when the specific deficit or interference is
crucial. Evidence for this model is afforded by the recent investigation of Pasamanick (1961) and his collaborators on the role of intrauterine events on the continuum of 'reproductive casualty'. They postulate that certain untoward events such as intercurrent illness, toxemia and other interference with the foetus during the first 9 months of life will produce mental and physical disability ranging from still birth, through live births and epilepsy, cerebral palsy, mental deficiency and finally even those who appear unscathed at first may not escape entirely but develop such lesser ailments as reading disability.

One of the most exciting events in the developmental area has been the investigation of the impact of early experience on subsequent personality and its deviations. While the evidence from human infants remained controversial, the evidence from animal studies seemed at first to yield data which appeared too closely in keeping with Freud's hunches to give much comfort to those who had refused to accept his clinical surmises. Gentling of animals in their infancy following the credo of tender love and care, actually produced less emotionally unstable adults, but to the great surprise of most investigators, shocking the infants was equally effective and most recently, Theodore Schaefer et al at Columbia (1962) has found that merely lowering the temperature a few degrees is equally effective. Thompson's (1962) demonstration that emotional mother rats give rise to emotional pups, Harlow's (1962) demonstration that monkeys raised on surrogate mothers and not permitted to play with their peers, tend to develop poorly in the psychosexual sphere, and Melzack's deprived dogs (1957) are still other triumphs for the developmental model as a possible causal factor in emotional disorders.

The conditioning or learning model postulates that the source of the deviant behavior of the mental patient is to be sought in his reinforcement history. An example of this theory is Bateson's (1956) double-bind model in which the mother's ambivalence in her relationship to her offspring produces ambivalent behavior and other types of deviation in him which we recognize as schizophrenia. While Bateson's double-bind model has aroused considerable interest in psychodynamic circles, it has thus far defined experimental testing of any of its hypotheses. Several more experimentally founded models have been provided by psychologists. Thus, Sarnoff Mednick (1958) bases his approach on the evidence that the early or acute schizophrenic conditions more quickly, and shows greater stimulus generalization (less steep gradients). These are related to the higher level of arousal which is attributed to early schizophrenia.

Since higher arousal leads to greater conditionability, and also to wider generalization in laboratory experiments, it is plausible that the higher arousal state of the schizophrenic may account for his more rapid conditioning and wider generalization. An additional finding is that the early schizophrenic takes longer to recover from stress-producing loads. This triad of events—greater arousal, wider generalization and slower recovery from stress, produces a vicious circle, increasing the early schizophrenic's vulnerability to stress and brings about the high levels of anxiety often seen in early cases. As this triad of conditions continues, the patient grows more and more tense, less adjustable and finally settles into a state of chronicity. Pursuing this model further, Mednick postulates that the high level of anxiety and fear is sometimes fortuitously relieved by some tangential event or thought. Since these intrusions have a tension-reducing value, they become part of the patient's repertoire of defense against excessive anxiety. In this way, there is no need for the concept of repression. Simple reinforcement of a competing act of thought is sufficient. The tangentiality of the schizophrenic and the intrusions in his
thinking may then have two sources; (1) wider associative trend because of the wider stimulus generalization and (2) the anxiety relieving aspect of an intruding thought.

Questions may be raised why the neurotic does not develop psychotic behavior, since he too has a high level of activation (anxiety). Perhaps the answer to this question is to be sought in the quicker recovery from stress or in the lesser tendency to generalize, both of which may break the vicious circle which is established in the schizophrenic. Some of the confusion in this field could be reduced if a distinction could be established between operant and respondent conditioning, the former showing, at least for the early cases, quicker conditionability and quicker extinction but wider generalization (Mednick, 1958; Salzinger et al., 1960), while the latter, on the other hand, may be slower on both accounts. (Shipley*, 1934) A further separation between early and chronic schizophrenics or between process and reactive schizophrenia is also necessary to reduce the confusion in this area.

The genetic model no longer needs to beg for admission thanks to the work of the Hübhin school as exemplified by Kallmann's investigations. The studies of monozygotic and dizygotic twins have recently undergone further scrutiny by David Rosenthal (1962, 1961, 1949).

He found that when the rates of concordance among twins are examined in successive admissions rather than in resident populations the rate of concordance for the former is much lower than for the latter. This is attributed to the tendency for more severe cases to remain in the hospital and thereby cause an inflation of the concordance rate. Furthermore, in discordant monozygotic twins, as compared to concordant, fewer relatives were found to have schizophrenia. In general, in discordant pairs, the affected member suffered more often from reactive than from process schizophrenia and generally had a better prognosis. The question now is no longer whether genetics in important, but how does it bring about its effects. The discoveries of the genetic underpinnings of phenylketonuria, mongolism and other types of mental deficiency lead one to hope that the actual genetic deficiency in the functional psychoses may soon become known. On the other hand, the tendency for schizophrenic females to have more frequently schizophrenic mothers than schizophrenic fathers, and mutatis mutandis for schizophrenic males, point to the importance of such environmental factors as: is it sex-identification, or is it genetic linkage?

It should be recognized, however, that strictly speaking, there are no exclusively genetic or environmental disorders. All disorders are both genetically based and environmentally elicited. Without the hereditary-environmental interaction, no disease, in fact no development at all, would be possible. What then is meant by a hereditary disease and by an environmental disease? PKU is a hereditary disease in our particular social-cultural-physical environment because of the presence of phenylalanine in our diet. Had our diet been free of this substance the phenylketonurics in our society would never develop mental deficiency and, in fact, PKU would never have been discovered. On the other hand, if only the poor, or only the mountaineers had developed this illness, and the error of metabolism were unknown, we would have regarded this illness as environmentally

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*It is not clear whether Shipley investigated chronic or early cases.
produced. Cassell, J. (1957) has pointed out that in the North Carolina county which he is studying, and which is poverty-stricken, diet-poor, and otherwise environmentally deprived, there is a high prevalence of three diseases--schizophrenia, tuberculosis, and suicide. He contends that these three illnesses are equivalent in his population, elicited by the poor environmental conditions and regards them as environmental diseases. In passing I have suggested that he give them one comprehensive name--"Schizotubercide." In two of these illnesses, tuberculosis and schizophrenia, we have already obtained tentative evidence for a hereditary component. In the third no evidence for a hereditary component has yet been discovered.

Thus, a disease is regarded as hereditary in our particular SCP (Social-cultural-physical) environment when the specific genotype of the patient is associated with a high probability of the development of the disease, but when the specific environmental factors required for eliciting the disease have not yet been established. In so far as at least some of the offspring may inherit the genetic defect and transmit it, the hereditary component of the illness must be recognized. On the other hand, a disease is regarded as environmental if most genotypes in our SCP environment will develop the illness when they are exposed to the specific environmental agents which have been identified as etiological--viz.--the particular virus in measles; the double bind, if eventually verified, in schizophrenia, and the pattern of the factors precipitating suicide, if indeed suicide be the end product of a disease.

Thus, a disease is regarded as definitely hereditary if we already have evidence of the presence of the specific genetic component and have not yet discovered the specific pattern of environmental components required for eliciting it. On the other hand, a disease is regarded as environmental if we have found the specific pattern of environmental components but have not yet discovered the pertinent genetic factors. In diseases where both the hereditary and environmental etiology is known the question never arises. In the end, however, all diseases will probably be found to require both a genetic as well as environmental component. Apparently, heredity is no more the cause of an illness than the automobile is the cause of an automobile accident. The interaction between specific hereditary and environmental factors required for the emergence of an illness must be sought if we are to detect the vulnerability of an individual before the illness overcomes him.

Genetics may be viewed as a biochemical mechanism in which the genes serve as precursors for the production of certain enzymes whose absence prevents the organism from prospering. There is, therefore, considerable hope that an investigation of the internal environment of the body may reveal the particular metabolic deficiency or excess which characterises the patient. A particular error of metabolism may, of course, be inherited or acquired. A considerable amount of effort has been spent in the attempt to relate schizophrenia to metabolic error. Certain fractions of schizophrenic blood have produced metabolic changes and changes in such behavior as rope-climbing in rats as well as transitory changes in the psychomotor behavior of normal human subjects. Presumably similar fractions from the blood of normals do not produce such changes.

Perhaps the most exciting new development, originating at McGill, is the discovery of the ability to manipulate behavior directly through implanted electrodes. This has given new significance to the work of the neurosurgeons who
carried out psychosurgery in the late 40's and early 50's. The attempt to detect deviations in the neurophysiological substrate of mental patients through the means of electrodes implanted in various portions of the brain, by recording evoked potential or by introducing current through these electrodes, and the recording of evoked potentials from the intact brain through the scalp, are opening up new possibilities for the detection of the neurophysiological anomalies correlated with deviant behavior.

What kind of a mechanism can be suggested which might explain mental disorders from a neurophysiological point of view? Whether we assume a genetic basis, or some acquired metabolic imbalance, how can such mechanisms explain, for example, the types of behavior included under the rubric of schizophrenia. One of the models most attractive to behavioral investigators implicates three aspects of the central nervous system function—sensory afferents, the reticular activating system and cortical association activity—or, if you wish, sensory input, alertness or arousal, and memory storage. Without entering into specific brain localization problems, it seems that, as far as sensory thresholds are concerned, and as far as retrieval from memory storage is concerned, schizophrenics are for the most part not notoriously different from normals. Whether they differ from normals in level of alertness or in lability is difficult to decide. Mednick, as noted earlier, postulates a difference in level, the schizophrenic showing a higher level of activation. My own inclination is to assume a difference in lability. This may explain the inordinate variability in response of schizophrenics. It is not that their responses to stimulation are intrinsically more variable, but since response varies with level of alertness, given a continuously varying level of activation, the response will vary accordingly.

But even if the level of alertness were not at fault, it is still possible that the three systems—input, storage and alertness, though each be intact in itself, may nevertheless interact differently in the case of schizophrenics. An interaction difference may explain such findings as greater latency in simple reaction time when the signal is switched from one modality to another, difficulties in sorting or in categorizing behavior, loss of set, etc. That the cortical-reticular interaction is an important component in the evaluation of incoming information has been demonstrated in a variety of animal experiments. The interaction between sensory input and memory storage in the encoding and decoding of neural information, has been pointed to by Elkes. (Folch-Pi, 1961). He states, following Neisser (1963), that two types of information processing may be distinguished—that which deals with serially presented single inputs and that which deals with simultaneously presented multiple inputs. The distinction is between asking questions one at a time and letting each answer determine the next question, or asking all the questions at once. Response to the latter may be a function of the multiply-connected "reticular mixing pool" and any negative or positive deviation in this substrate may account both for the failings of schizophrenics as well as for the successes of geniuses respectively.

The final scientific model is the epidemiological model, which seems to be a super-model including each of the above models as partial factors in the explanation of the mental disorders but requiring careful field studies to determine the relative role of each of them. Mental disorder is conceived as the end result of a series of probabilistic events, each of which must occur in interaction with others to produce the disorder, although the threshold value for each factor may differ from person to person and from one disorder to another. Thus,
two people may have inherited the same predisposition, but because of differential stress, nutritional or deprivalional factors will not both develop the illness. The virtue of epidemiology is that it takes in all possible factors ranging from radiation, paranatal existence, genetics, to social-cultural environment, etc. Thus, the epidemiological model both permits and requires the weighing of each of the submodels in the total picture of causation; the difficulties of assessing their relative importance, and of devising studies which will not overlook some of the factors, are too well known to need re-emphasizing here.

While many of the social-cultural, developmental, learning, genetic, biochemical and neurophysiological claims remain somewhat in doubt, they nevertheless lend credence to the possibility that the mental disorders will be found to be characterized by either the deviations which are now postulated by these models, or by deviations of the same general scope that have not yet been postulated. Whether they are the cause or the effect of the disorder remains to be seen, but the testing of the hypotheses generated by these models depends to a large extent on the detection of some type of deviant behavior which characterizes the patient.

In the wake of the epidemiological studies that blossomed forth in the last decade, it has become quite clear that our knowledge of psychopathology is limited to a small section of the population of the mentally ill. Most of our techniques for diagnoses have been developed for middle class or upper class patients. We have now gone as far as we can in providing these social classes with assessment techniques suited to their needs and with therapies suited to their tastes. It is tragic that we are entirely unequipped to deal with the fifth of the nation caught in the throes of poverty. It is easy enough to develop specific techniques suitable for each social class and ethnic grouping, it is true—but how are we going to establish comparative norms? We must proceed along two lines. First, we must develop and explore culture-dependent techniques such as interviewing, observational techniques, various testing procedures which will provide norms for the various groups and detect deviants from these norms. Secondly, we must provide some culture-free or culture-fair techniques. In a recent paper I had made the suggestion that, for the latter, the answer may lie with the response of the subject which occurs during the first second following stimulation. This first phase of response may occur so fast that cultural-social factors may not be able to make themselves felt. The startle pattern, for example, (Landis & Hunt, 1939) occurs in the first 300 milliseconds following a pistol shot. This initial phase is universal. At the end of this initial phase, culture takes over. The policeman may draw his gun, the New Yorker dodge into the subway, and the farmer take to the woods; but the startle pattern itself is the same for everyone. If we could find such universal behavior which is at the same time differential with regard to psychopathology, our search would succeed. The usefulness of such techniques as cross-cultural and international studies is, of course, obvious. Unfortunately, only a small portion of patients—some types of epileptics—deviate from the norm with regard to the startle pattern. There are however, other techniques in the first 1,000 millisecond range which are quite promising. Among them are certain aspects of reaction time, two-pulse thresholds, and pupillography.

Work on the pupillary response demonstrates that, far from being an exclusively protective device against excessive illumination, the pupillary system is highly sensitive to threshold manipulations of light input, to the past
history of the organism (conditioning), and to internal states of the organism such as set and motivation. For short duration light flashes, the pupillary contraction is linearly related to total light energy rather than light intensity. While the normal response of the pupil to the presentation of light is to contract, the use of light as an unconditioned stimulus and of sound as the conditioned stimulus will produce a dilation instead of a contraction at the point in time at which the light would have occurred.

Perhaps even more startling is the sensitivity of the pupil to the attitudinal state of the subject with respect to the stimuli which are to be presented. When the subject is asked to report whether a threshold light flash occurs, the pupil dilates prior to the occurrence of the light flash. This dilation is absent when the subject is asked to passively observe the light flashes but to make no report. In a somewhat similar vein are the findings on the electrical activity of the brain to light and sound stimuli. The evoked responses recorded from the scalp to light or sound stimuli are markedly altered when the stimulus which is presented is unexpected to the subject. If the subject is asked to guess as to whether a sound or a light will be presented, the evoked potential is influenced by whether the subject's guess has been right or wrong. These lines of experimentation show great promise for permitting the simultaneous study of sensory, perceptual, and conceptual functioning in mental patients.

One application of the pupillary research has been to study the effects of tranquilizing drugs. In a study comparing the effects of two drugs on patients, it was found that one drug which produced clinical improvement resulted in a reduction of the pupillary response to light stimuli whereas another drug which did not produce consistent clinical changes also did not produce consistent pupillary changes.

In the area of sensory studies per se, several lines of investigation are being followed which are aimed at developing subtle and precise measures of altered brain function. In the auditory area, the relative roles of time and intensity of sounds at the two ears in determining the position of a single sound image is being used in an attempt to assess the relative efficiency of excitatory and inhibitory neural activity. An application of this work to a schizophrenic population is in progress.

Another evidence of greater sensitivity to changes in energy input on the part of schizophrenics comes from our studies in reaction time. The Bunson-Roscoe Law or Bloch's Law, which refers to the interchangeability of intensity and duration for constant levels of energy input, is a Law which the schizophrenics apparently haven't heard about. They don't obey it. Apparently they do not integrate as well as normals and can, therefore, detect differences in the stimulus which pass unnoticed by normals.

Other evidence for the importance of psychophysiological indicators in diagnosis comes from the use of derivatives of the interview method—self-reporting personality inventories such as the Cornell Medical Index. The items dealing with self-reports of ideation and mental content are not nearly as reliable as the items dealing with self-report of internal psychophysical and somatic events (heart palpitations, sweating feet, etc.). It is not clear whether neurotics are more sensitive to normal physiological body events, actually experience deviant bodily events, or have their body events exacerbated by the normal feed-
back to which the bodily events give rise. Only careful psychophysiological measurements can cast light on this question.

In addition to the problem of assessment of the mentally ill there is the problem of assessment of mental retardation. In this field one of the chief problems is that of distinguishing the congenital mental deficiencies from socially induced mental retardation and from the mental disorders. For this purpose, psychological assessment is indispensable. We have gone as far as we can with the global intelligence tests and with paper and pencil techniques. To assess the capacities as well as the shortcomings of the mental defective, new approaches are now required. Binet, Goddard, and Terman did a tremendous service to the feeble-minded by providing the mental age scale, but their followers did a tremendous disservice by throwing out those psychophysical and experimental techniques which though measuring individual difference, did not correlate with the central intelligence factor. Physiological, sensory, perceptual, psychomotor, as well as specific conceptual techniques are needed to assess the abilities and disabilities of the patient. Focused-interview techniques are needed to evaluate his personality. With the use of these techniques, better classification, better prognosis, and treatment more suitable to the individual case will be forthcoming for the wide variety of the mental deficiencies.

At the present time, intelligence tests, standardized as they are on normal populations, give a clear picture of the defects of the patient, i.e., of the low altitude of his general intelligence, but give no idea of the breadth of his specific abilities. Furthermore, as Dr. Helen Schucman has shown, the initial test is hardly as good an indicator of future development as is the change from initial to retest. The old dictum that mental deficiency was incurable may still hold true in some instances. But as long as there is life, there is hope of learning, and while the patient may not learn as much as a normal, what he does learn may prove quite useful to him and to society, and sometimes, surprises do occur.

One of the big surprises has been the gradual disappearance of many mentally defective children in follow-up studies. Apparently, the peak for discovery and institutionalization for mental deficiency is age 14, when the school closes in on slow learners and finds them defective. After adolescence many of them can no longer be found in institutions or in treatment. Since there is no evidence for higher mortality, perhaps, slow as they are, many of them finally "learn"—for even an IQ of 50 can theoretically attain a mental age of 10, at the chronological age of 20. This may happen more often than we now suspect.

Another problem is how much the recognition of mental deficiency rests upon the social-cultural standards. Perhaps the greatest difference in this respect arises between European and American practice, the Europeans being able to retain more of their feeble-minded in the community. Thus, the Netherlands, with a population no smaller than New York State, has an institutionalization rate only 1/3rd. as high, and Amsterdam, a rate only 1/10th. as high as New York City. How the criteria for determining mental deficiency depend on the social milieu is most strikingly expressed by Böök, (1960), in the following terms: "Just where, on the slope of the (normal) curve, pleasant physiological stupidity changes into social or medical problematics is a matter of conjecture. More important than such conjectures is the fact that the malignancy of inferior intelligence is a function of technical and social developments and public tolerance."
Recent evidence of the uselessness of our current techniques when applied to underprivileged children comes from the work of Vera P. John (1963). For a long time we had accepted concrete behavior in sorting tests as an indication of schizophrenia in non-organic cases. Vera John has suggested that this so-called "concreteness" is a function of social deprivation rather than of schizophrenia, and all the work on concrete vs. abstract behavior will have to be repeated with suitable socioeconomic and cultural controls.

One of the most frequent requests made of the diagnostician is to determine whether there is any organic involvement in the clinical picture presented by the patient. This will loom larger in time, especially if we begin to deal adequately with the 1/5th of the nation in the poverty pockets. First of all, toxemias of pregnancy and other untoward gestation events which often lead to organically based congenital conditions are much more frequent in the low socioeconomic level. Secondly, brain injuries and other organic accidents are also more prevalent in this group. How to distinguish between psychogenic and organic in a milieu where social-cultural forces occlude the picture is a tremendous challenge for the clinician.

With regard to therapy, only those psychotherapists who are unashamedly mystical or sentimental will deny the need for evaluating psychotherapy. The failures of therapy are well known to the clinical community in America, but an unequivocal conclusion that therapy is valueless is far from demonstrated. There is a great difference between recognizing the problems of validating psychotherapeutic outcome and insisting that there is now sufficient evidence to establish the null hypothesis. There is a tendency current in some quarters to give up evaluating therapy and to resort instead to analyzing the process itself. This escape into process will never substitute for actual evaluation. No amount of investigation of the "therapeutic process" is going to give us the answer as to whether psychotherapy was worthwhile in the first place.

Some of the problems in evaluating outcome of psychotherapy are shared with clinical research in general, while others are unique to psychotherapy. The adequacy of controls is a universal problem. The ideal situation of matched identical twins for treatment and non-treatment groupings is no more available to pharmacologists than to psychotherapists. Many workers agree, however, that by using age of onset, duration of disease, sex, and diagnosis as the minimum essentials for comparability of two patients, a workable control situation can be established. Obviously the first three parameters are as available for the evaluation of psychotherapy as for that of any other treatment, but it is the last, i.e., diagnosis, that raises particular methodological problems. As long as the essential nature and cause of mental disease are unknown, and there continues to be disagreement among qualified persons concerning the broadest designations of mental disease, diagnosis will be imperfect and unreliable. But disputes about diagnosis can be circumvented for the purpose of a particular research project by a rigorous definition of terms or by the application of clearly outlined symptom complexes for matching of patients.

Recent work in our own laboratory has provided tools which make the life of the evaluator more bearable. By means of systematic interview in which molecular aspects of behavior can be recorded as present or absent, an objective evaluation of the traits and behaviors which characterize the patient at the time of his admission, at release and on follow-up can be readily obtained. These instruments
are: 1) The Mental Status Schedule, (Spitzer, Burdock & Fleiss, 1963) for use by psychiatrists; 2) The Structured Clinical Interview, (Furdock & Hardesty, in press) for use by clinical psychologists; 3) The Ward Behavior Inventory, (Burdock et al, 1960) for use by nurses and attendants. Two of these instruments have already demonstrated their usefulness in the evaluation of outcome of drug treatment and of intensive milieu therapy (Cole, in press; Goldberg et al, 1963).

At the present time these tools are being evaluated according to two criteria: 1) severity of illness, and 2) prognosis under specified treatment, such as psychoanalysis, behavior therapy or no therapy. It is clear that if we can specify both of these criteria and standardize our instruments against them, we can arrive at a much better basis for evaluating outcomes. Two individuals with similar prognoses should have similar outcomes under two equivalent therapies or different outcomes under non-equivalent ones.

The results obtained with these instruments have already demonstrated the value of systematic interviewing and observation. Thus, the Ward Behavior Inventory had the highest weight in the discrimination between phenothiazine and placebo in bringing about changes in ward behavior. Use of the systematic Mental Status examination has indicated that with the use of video tape it becomes possible to obtain evaluations of the same patient on the part of many interviewers. In one such study, in which some 50 seasoned psychiatrists participated, half of the group diagnosed the patient as a neurotic and the second half as a psychotic. An investigation of the profile of ratings which the patient received from the 50 diagnosticians indicated that there was a good deal of agreement regarding the degree of depression present, and similar agreement regarding hostility and other traits. Only in assessment of apathy was there any difference. The diagnosticians who called the patient psychotic rated him high in apathy, and those who called him neurotic rated him low in apathy.

An interesting conflict often arises in the mind of the clinician in prognostic studies. When the prognosis is very poor, the clinician usually redoubles his efforts under the assumption that the case he is treating has the 1 in 1,000 chance of making a recovery. This leads in some situation to undue self-adulation in success and to breast-beating in failure. In others, the failures are soon forgotten and only the successes are remembered. By providing prognostic baselines in a regular manner, the attention of the research clinician can be focussed on those with good prognosis who failed and those with poor prognosis who succeeded, while the good as well as the poor prognoses which were borne out by experience can be laid aside. Surprising failures and unexpected successes can do more in furthering our understanding and improving out prognoses than can expected successes or expected failures.

The problems of control are serious, but not necessarily insuperable. It is evident that there is no uniformity among mental health workers with respect to usage of such terms as "cured," "recovered," or "improved." But this problem is not unique to psychiatry. These terms are, after all, in the vocabulary of acute diseases like appendicitis or strangulated hernia, and are as imprecise in chronic conditions such as tuberculosis or cancer as they are in schizophrenia. Tubercle bacilli may be eradicated from the sputum and symptoms may disappear, but if the over-all life expectancy has been shortened, can the patient really be called "cured?" In cancer therapy it is customary to refer to five or ten year "cures;" the arbitrariness of such terms to an individual patient do not compromise their
use in evaluating treatments for cancer. It is clear that explicit and precise criteria for outcome are necessary in any satisfactory clinical evaluation. Freyhan's concept of "target symptom," which the therapy specifically attempts to reduce or eliminate, is an example of the type of precision that can be introduced.

The evaluation of specific goals of therapy need not depend on ratings alone. Techniques for measuring changes in such specific characteristics as flatness of affect, disordered thinking, level of retardation in depression, anxiety, etc., are either already available or are in the process of development. Thus, Salzinger et al (in press) have developed objective measures of self-referred affect through the use of reinforcement techniques. Payne (in press) has developed objective measures of thought disturbance in his tests of overinclusive thinking, and similar techniques for other target symptoms are in the making.

The criteria that have been used by psychiatrists too often cannot yield the objective data required in scientific evaluation. What we need are more definite measures of the improvement in "comfort" of both the patient and his family. Pre- and post-treatment inventorying of behavior, as suggested earlier, is one promising maneuver. Further refinement will be necessary to avoid the atomistic and highly selective profiles that such scales currently tend to give. There is, moreover, a growing body of information from a variety of reliable sources that will ultimately contribute to our knowledge of spontaneous improvement rates. Such data will provide a baseline for comparative studies of outcome.

In order to make comparisons between the various types of therapy, I suggested some 15 years ago that a center be established where a standard population of patients might be housed (Zubin, 1953). The standard population would be specified with regard to age, sex, symptoms, duration of disease, age at onset, and other pertinent variables. This population of patients would serve as the proving ground for the relative efficacies of different types of treatment. In psychiatric centers throughout the country, varieties of therapy could be tried out on comparable groups characterized according to the same variables. The outcomes of these therapies could then be evaluated and a definitive statement arrived at as to the efficacy of each treatment.

Thus far, the suggestion for a standard population, made in 1950, has not caught on. Perhaps it is too much to expect physicians to keep patients "on ice" without applying any of the apparently promising current therapies. An alternate suggestion is to establish a Central Assessment Bureau to which patients could be referred before and after therapy for an assessment by the most promising instruments now available. At the present time, the evaluation of outcome is made by those in whom a conflict of interests is, to say the least, most likely to be present—the patient, his family and the therapist. Such evaluations will no doubt continue to be made, but the provision of a neutral assessment agency, on a confidential basis, could bring about a most salutary influence in the long run. Such an assessment could at first be made on a purely advisory basis. As the agency continues to function over the years, actuarial material could be provided for the probable outcome of a given type of patient under a specified therapy. While such actuarial tables will never replace clinical judgment, any more than life tables replace the clinician's prognosis for survival of a given case, they would provide guides for choice of therapy and give each therapist a batting average against which to compare his results. Such comparisons are practically impossible now.
One comment may be made here regarding the battle for the possession of the right to practice psychotherapy. Psychopathology has an unhappy history of losing the ground it gains in hard battle by ceding newly won territory to normal psychology once the battle is over. In this way, depersonalization phenomena have led to the self-concept; phantom limb, to body image; registration, to consolidation of memory traces, etc., etc. Psychiatry, too suffers this fate. Such diseases as epilepsy, general paresis, pelagra with psychosis, phenylpyruvic oligophrenia (or phenylketonuria as it is now called) became part of nonpsychiatric medicine as soon as the etiology of these diseases became known. Only diseases of unknown etiology tend to remain permanently in the psychiatric fold. Thus, the abnormal gets incorporated into normal psychology. This will be the fate of psychotherapy. It will gradually be absorbed into learning theory and developmental theory. In this manner, all the abnormal phenomena which are attributable primarily to learning and development will cease to be regarded as abnormal and will fall into the bailiwick of the educator or remedial therapist. Only the recalcitrant cases and individuals suffering from genetically based or physiologically based conditions will remain in the hands of the psychopathologist. The battle for the possession of psychotherapy will persist only so long as ignorance about the nature of therapy and its efficacy remains. Once this ignorance is dispelled, competition will change into cooperative effort for finding the best way of administering therapy in accordance with, not law or authority, but the patient's best interests.

With regard to mental illness, the big problem is whether we should continue the freer release policy now in vogue. There are already signs that the community will not tolerate indiscriminate release. We need to develop techniques for determining whether and when a patient is ready for release. Only prognostic studies can help us here. We have already found some 150 traits which are predictive of outcome in schizophrenia (Zubin et al., 1961). In 80% of these traits, the prediction has remained unchanged since 1900, despite the fact that therapies have changed radically during the last 60 years. Apparently, the traits which the patient possesses at the time he arrives at the hospital are more important than the therapy he receives. In order to develop prognostic procedures, we must investigate the early development, premorbid history, morbid status, and course of illness. One example of a trait which we have investigated is the pattern of adolescent friendship in schizophrenics; another is flatness of affect.

This aspect of adolescence, as well as the rebellious nature of the adolescent, demanded attention because nearly half of the population is now below 21 years of age. The question has often been raised whether there is more psychopathology in our youth today than was the case in previous generations. Does the teacher of today have to deal with more psychopathology than did his predecessors? It is difficult to answer these questions definitively, but the shrinking role of the family in the upbringing of the young, a generally accepted belief, would lead one to answer them affirmatively. The gradual shift from the extended rural family to the urban-suburban family, another generally accepted belief, may have cast new burdens on the teacher by relegating occupational as well as intellectual training to the school. Until recently the family still retained the task of inculcating the mores. Recently, however, even this function has been relinquished according to some authorities.

James Coleman (1961) points out that there is little economic value in the adolescent today, and his social training comes largely from his peer group. As
both father and mother vanish into the world of work, adolescents have to develop their own society, and without proper guidance, such societies can often go astray. The house he lives in is no longer a psychological home. It is more like a boarding house. The school is becoming more and more a total institution for the adolescent, using Goffman's term, in which his entire life seems to be centered. Coleman has recently suggested that perhaps the adolescent of the future will perform live apart from his family in special boarding schools, as was the case with the British nobility. The separation between the generations is of such magnitude that adults have to deal with adolescents collectively rather than individually. The added sophistication of modern adolescents, expressing itself in lack of concern for adult authority, impatience with adult control, disdain for adolescents who remain subject to parental authority, smoking, drinking, and sex-play, all add up to a problem with which some agency has to deal. The school is the most likely candidate. In the midst of this turmoil are some adolescents who are not strong enough to find their own bearings and in whom the age-typical conflicts may precipitate illness. How to cope with this situation is one of the issues facing the sentient educator, and it is to him that society is looking for a solution.

Why has the adolescent problem loomed so large recently? As the result of the tremendous rise in the birthrate in the middle 1940's, our facilities for caring for the emotionally disturbed and mentally retarded children, who appeared shortly thereafter, suddenly became inadequate. In the meantime, parents of less severely retarded children had also become sensitized to the possibilities of treatment, and came for help. This, at least in part, is the explanation for our present astonishment as to where all of these children who need care came from. Another factor is the prolongation of life and the reduction of infant mortality. Some children who in former years would have succumbed in infancy or in childhood now survive as mentally defective or emotionally handicapped children and adults.

Increased tolerance of the mentally handicapped has been a remarkable development. The impact that the organization of parents of retarded children has had in the improvement of care is one of the most striking phenomena of our day. The fact that the parents of the most severely retarded are quite often found among our most advanced, most productive, and most talented and farsighted citizens, has helped to prevent the stigma of mental illness from attaching itself to mental defects. The random nature of much severe retardation, attested to by many surveys, has made this possible.

One of the most important factors in the socialization of adolescents is their friendship pattern. Unfortunately little is known about friendship patterns in normal adolescents and even less is known about these patterns in the abnormal. Since mental illnesses which develop insidiously seem to have a much poorer prognosis than those which develop suddenly, we examined the adolescent friendship pattern of individuals who had become schizophrenic and those who remained normal in their third decade of life. The results indicated that the mental patients differed considerably from their normal peers.

While members of the normal sample were not always satisfied with their friends, it never occurred that they had none. Of those schizophrenics who said they had had friends, a significantly greater number had friends who were either a year older or younger and significantly more had friends who came from the block on which they lived. The control group sees the period of their early adolescence as one characterized by good spirits and the optimistic feeling of
things getting better and better; the schizophrenic sample complains of the lack of good feeling and the dull sameness of the years. For them it was a time of pervasive loneliness which was keenly felt when they were with their own age-mates. They found it difficult to know what to say to friends and had so little pleasure in contact with peers that when given a choice, they preferred to be by themselves. They saw themselves as not being as good as other boys their own age and in comparison to their best friends, they felt disadvantaged in social, financial and familial matters.

If we assume that among the things friends expect of each other is the exchange of favors and confidences and that the ease with which these are made marks in part the intimacy of the relationship, then the schizophrenic is correct in his assessment of his friendships as lacking intimacy. Despite the fact that 83% of the schizophrenic sample feels that a good friend has the right to ask for help and favors, the premorbid schizophrenic not only refrained from asking for help or favors from his best friend but felt, for some vague reason, that he could not even ask for help. As a result, he reported less help actually received. While the results are not significant on the exchange of confidences, they are persistently in the same direction. The pre-morbid schizophrenic tended to reveal less to his best friend, found greater difficulty in confiding even when he wanted to, and was less often confided in by his friends.

The increase in geriatric cases is not as rapid now as it was during the 1940's, but the full impact of the population explosion and the increase in longevity has not yet been felt. How to determine which of our older population will require mental hospital care, which nursing home care, and which can live happily and productively in homes for the aged, apartments for the aged, etc., remains an important diagnostic and prognostic problem. In the studies conducted in our research group, the degree of isolation prior to admission to the old age home was found to be negatively correlated with adjustment; senile dementia cases adjusted better than functionally disordered individuals; and the "totality" of the institution with regard to the regulation of physical mobility, schedules, privileges, etc., was related to the type of adjustment made by different types of personalities. Persuasibility seemed to be higher in residents whose freedom was more curtailed.

SUMMARY

The scientific models which have been suggested for psychopathology have varied with time. The phenomenological description of the early clinicians gradually gave way to more dynamic insights under the guidance of Freud and Bleuler, who was greatly influenced by Freud. Unfortunately, the intervening variables which Freud conjured up in his model (id, ego, super-ego, etc.) did not yet lend themselves to the erection of rigorous scientific models for testing. Furthermore, Freud, whose approach was a product of the scientific models of his era, saw no place for contingency and hence every event had to have had a cause, probability and contingency playing no role. This too militated against the introduction of experimentation into psychoanalysis story of the experiment with the cadipa situation. However, statistical, immunological, nutritional, and genetic studies finally uncovered the causes of general paresis, pellagra with psychosis, mongolism and PKU. Finally direct attacks on schizophrenia and depressive states with the shock therapies, lobotomy and drug therapy broke the hold that sheer description or dynamic theory held over clinical practice. Today we see the use of behavior therapy reared through learning theory as another challenge to the
The techniques of measurement aimed at testing the tenability of the scientific models provided by psychologists and psychiatrists have also varied with time. The revolt against philosophy, led by Wundt, had its counterpart in psychopathology through Kraepelin, who had been dissuaded by Wundt from entering psychology. Instead, Kraepelin introduced Wundt's psychophysical techniques into the clinic to elicit more objective evidence of behavioral deviation in the mentally ill. He had this to say (1896):

"As soon as our methodology has sufficiently proved itself through experience with healthy individuals, it would be possible to approach the actual ultimate goal of these efforts, the investigation of the sick personality, especially of the inborn pathological disposition. In an investigation of many individuals we will always find some who deviate profoundly from the behavior of the vast majority in one or another aspect. If this deviation appears to be damaging to the mental life, and if it reaches a certain degree—which admittedly can only be arbitrarily determined—then we tend to regard it as an illness. Experience teaches us that persons with pathological traits of this kind are, on the whole, in greater danger of a general mental disturbance than those personalities (natures) whose characteristics are in the middle range. We, therefore, have first of all to investigate whether it is possible by means of psychological tests to determine individual deviations, which cannot be recognized by ordinary observation. If that succeeds, we would be in the position, through the quantitative determinations at our disposal, to establish the borderline between health and disease much more precisely and more validly than has been possible so far."

The work of Kraepelin and his followers, inspired by the newly developing experimental physiological psychology, led to the introduction of brass instruments and psychophysical methods into the clinic, but only a small reflection of these experimental advances remained, in such items as weight judgment on the Bistat and the vital index used by Spearman for measurement of mental defect. The clinical revolt against psychophysiology during the first decade of this century led unfortunately to the temporary elimination of psychophysical measurement from the clinic because the results did not relate to intelligence, which became the preoccupation of clinical psychology of the time. The second decade saw the emergence of group intelligence tests. Factor analysis won its victories from global tests in this decade. But in the thirties, a wider rebellion broke out against the use of intelligence tests (especially the IQ) as the sole basis of assessment, and personality inventories were born. These were soon devalued by the rebellion against psychometric atomism led by Gestaltists; as a result, sorting tests and abstract-concrete approaches, rigidity measures, etc., came into favor during the forties. In the fifties projective techniques became the favorites because of disappointment in the clinic with the so-called "nondynamic" testing field. These too have passed their zenith and now observational techniques in the form of inventories and rating scales hold the limelight. Now there is a revival of interest in physiological, sensory and perceptual techniques and the introduction of sociological and anthropological considerations into the clinic. There is also a desire to improve, if possible, the culture dependent indicators of mental disorder as well as to find new culture-free or culture-fair indicators.
It is clear that each rebellion has nullified some of the gains of the previous decade, but each rebellion has had its own considerable heuristic value. With a history such as this, psychopathology, if it continues in this manner, cannot help but have a wide-ranging and flexible future.

It is unfortunate, then, that so many clinical training programs do not foresee the full scope of the discipline of psychopathology and do not fully anticipate the extent of its needs. Too often they limit—or permit their trainees to limit—their areas of training in too narrow a fashion. Among the new areas which is bound to attract attention is the predicament of the poverty-stricken fifth of a nation that is so much in need of help. Our tests, our interviews, our therapeutic techniques are all standardized on the middle or upper class. Unless psychopathology takes up the challenge, it will not be doing its job—and it may find others doing it instead. It is true that a large portion of the poverty-stricken population is unable to pay for assessment and therapy, but funds for such efforts are becoming available. It is up to our profession to be in the vanguard in breaking through the barrier that has kept one fifth of the population from educational and occupational opportunities and has thereby increased the hazard of illness. With regard to therapy, medical personnel may have to share the administration of drugs with other specialists: the need is that great. It is quite likely that the drugs needed to control depression or anxiety will eventually be as safe as aspirin. If so, this will mean that sufficient training in, and understanding of, differential physiological responses to drugs will have to be given to clinical psychologists, social workers, public health officers, nurses, and other non-medically trained personnel, for them to be in their own masters in this area.

Clinical psychiatry, psychology, and the allied mental health disciplines, as professions, have progressed more rapidly and more successfully than anyone ever expected. But it is high time now that we shake off any complacency and bend all our efforts toward meeting the scientific challenges of the 60's and 70's. In the fast moving pace of these decades, not only will diagnoses change and therapies come and go, but even the mental disorders themselves may change in nature. Preparing students to meet these changes is indeed a challenge, but unless we meet it, our students, inadequately prepared, will leave the field and the future of the mental health professions will become uncertain. The history of clinical practice leads me to hope that this will not happen.
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