An Overview of Schizophrenia

Joseph Zubin

Biometrics Research, New York State Department of Mental Hygiene

Read before the Semi-annual Staff Development Conference of the Division of Mental Health, State Department of Hospitals, State of Louisiana at the Fontainbleau Motor Hotel, New Orleans, Louisiana, June 20, 1972, 9-9:45 A.M. Grand Ball Room Mezzanine Floor.

Let me first thank you for the opportunity to open this semi-annual Staff Development Conference. Despite the distance separating New Orleans from New York I have close ties here. I have known Bob Heath since our collaboration on the Columbia Greystone Topectomy Project in the late 40's, and I have much in common with several others of your eminent men.

It is relatively easy to be the opening speaker at a conference such as this since the entire field is virginal and I can wander as I please. However, since much of the territory has already been staked out — the biochemical, hereditary, psychodynamic, psychogenic and that of communication, I shall touch these bases relatively lightly and delve more deeply into the areas untouched in the forthcoming talks. I shall also try to devote myself primarily to those areas in which considerable progress is now going on, leaving the relatively static areas alone.

Most of this audience are too young to recall the pre-drug era, but those this side of 50 will recall that the psychiatric landscape was quite different in those days. Custodial care was the mode, psychosurgery was in bloom, ECT flourished and insulin was in flower. Those were heroic days. Life seemed simpler, diagnosis was unnecessary, psychotherapy
for psychotics in doubt and psychoanalysis very rewarding, at least to analysts. Our hospitals were overcrowded, community psychiatry not even a pipe dream and sensitivity training unheard of. The air was still clean, our language still pure, our noise level bearable, our youths still tractable, and women still unliberated. With the advent of the drug era, all hell broke loose! The air became dirty, language foul, noise level unbearable, youth impossible, psychoanalysis declined, diagnosis again became necessary because our choice of therapies multiplied, our hospitals began to empty, and our entire environment and culture became threatened by pollution. Yet, there seems to be a balance in nature -- a law of conservation of pollution -- for, when our air and language were clean, noise level low, and drugs were still medicine, sex was a dirty word, and now, with sex clean, ...

While psychopathology was undergoing these tremendous upheavals, it became more and more difficult for us in the trenches -- clinics and laboratories -- to maintain our equilibrium and hold on to some schemas which could render our work meaningful. I entered psychology when psychometrics was in its hey day and soon became interested in personality. I weathered the waves of factor analysis, psychoanalysis, and projective techniques as they swept over personality measurement, and searched each movement for some guide to deeper understanding of the mainsprings of personality and its deviations. I found these movements either too static, too dynamic, or too unreliable and invalid, respectively. Together with the late Carney Landis, I even tried the effect of antihistamines on personality measurement before the current drug era began. Realizing that at least modern personality theory arose historically out of psychopathology, I turned my attention to abnormal psychology and began a career of teaching and research in which I
have remained for the last 40 years or so. Here too, I weathered the storms raised by fever therapy, focal infection theory, psychoanalysis, psychotherapies of the direct and non-direct varieties and somatotherapies. I have pondered much over each of these movements, tried my hand at their evaluation and as you perhaps know, found each of them wanting when compared to properly chosen controls. This is not to say that I demonstrated their invalidity. Quite to the contrary, much clinical evidence accumulated which claimed validity, but, unfortunately, none held up under the scientific scrutiny of available techniques. I concluded that each therapy may be good for some types of patient, but it was their wholesale application to unselected patients which nullified their efficacy. Whether the drug therapies will fare any better remains a moot question. My experimental work in psychopathology was quite satisfying, but it was my teaching that gave me the most trouble. I could carry on experiments with mental patients without too much difficulty and with some exciting results, but my students kept on demanding some rationale for what I was doing. Curiosity as a guide for research didn't seem to be enough, for there were too many things to be curious about, and how does one select from the plethora of possibilities? Besides, what framework was there for integrating the new findings, or were they to remain isolated findings without reference to the accumulated knowledge of the works of others. Furthermore, genetics and biochemistry, fields of which I was ignorant, were demanding attention and I had to do some fast reading to keep ahead of my students. This is when I turned to reading philosophy of science and where in the works of Reichenbach, I finally began to realize the need for scientific models as guides for my work.
A model is a reconstruction of nature for the purpose of studying a given phenomenon. It is built by a process of abstraction defining a parsimonious set of parameters which are intended to leave intact the essential aspects of reality while removing distracting elements. These models give rise to hypotheses which can then be probed for their tenability through direct observation of nature. The purpose of models is, therefore, to 'improve' on nature by simplifying it in order to engender further research in a systematic way and thus add to the corpus of certitude of science. There are those, however, who eschew scientific models and instead prefer to confront nature with all its vicissitudes directly without interposing the screen of simplification which scientific models attempt to provide. As Blackburn pointed out, the alternative to the conventional scientific approach is the sensuous approach which stipulates that "there is other knowledge besides quantitative knowledge and there are other ways of knowing besides reading the position of a pointer on a scale."

He goes on to point out that the sensuous and the objective approach show a complementarity similar to that shown by the wave and particle theories of light. Both deal with the same phenomenon, yet produce different views of their nature and indicate the essential dependence of our observations on the tools we use for making them. Both Newton and Goethe, for example, were right regarding the nature of color and the complementarity of physics and art. Yet there is a difference between the phenomenological and scientific view which should not be overlooked. Science deals with public events while phenomenology deals with the still private events. In my own thinking, I have often likened our own biometric approach, for example, to the holding up of a mirror to the phenomena under investigation, capturing a mirror image
of what the phenomenologist perceives and in this way make it available to the view of others besides himself. But it is only an image the phenomenologist would say, not the real thing that he perceives and it captures only part of the phenomenon, overlooking what is still shrouded in darkness. However, let us not forget that the phenomenologist's reality is also only a model. What the mirror image captures for the scientist, however, becomes a public event subject to the scrutiny of others.

Psychopathology is rich in phenomenology and there are any number of systems for classifying and diagnosing the observations made on patients. However, when it comes to etiology, our ignorance is abysmal. What does one do when faced with such total ignorance? The only thing left to do is to imagine "as if" causes and test them.

In developing such models, it is important to bear in mind that one can sometimes find a circumscribed model that will satisfy a given set of conditions but which will not add to understanding causes. Thus, models can be constructed which will predict the tides rather accurately for a decade or so, but which have no value for explaining the causes producing the tides. Such mechanical models may be of extreme immediate value economically or socially but are of little value in scientific understanding of the causes of phenomena. The amphetamine model for psychosis may be a case in point and despite its usefulness in clarifying dopamine -- nor-adrenaline relationships to behavior, it may divert us from the basic search for etiology of schizophrenia, since it is an analogous rather than a homologous model.

What have been the models which have served psychopathology in the past? Historically, scientific models reflect the Zeitgeist rather closely. Thus, when primitive religion held sway, demonology served to explain
psychopathology; when astrology reigned supreme, the stars affected the humors of the body to produce psychopathology, and when Descartes' machine models were popular, defects in hydraulic pumps or in watchmakers' models of human beings served as the paradigms of psychopathology.

When the brain began to be regarded as the organ of the mind, at the turn of the century, brain anatomy was searched for the cause of mental disorder giving rise to neuropathology as one of the first scientific endeavors in psychopathology. Thousands of post mortem brains were sacrificed to the search for the never present lesion. In the first third of this century with the rise of bacteriology, models provided by the infectious diseases held sway giving rise on the positive side to the conquest of general paresis and on the negative side to Henry Cotton's focal infection theory and to Lowenstein's implication of the TB bacillus in schizophrenia. There soon followed the biochemical models for mental disorder in the wake of the discovery of inborn errors of metabolism. Finally with the advent of information and communication theory, brain models again came to the fore, but this time, not as anatomical models, but as data processing models. One of the popular current models for the brain is that of a computer and psychopathology results when the computer is overloaded or otherwise interfered with. But psychopathology is an ancient craft, and has adjusted to all kinds of invasions of its domain. We have lived with primitive religion, medicine men, alchemists, astrologists, early anatomists, phrenologists, theologians, philosophers; in modern times we have had commerce with neuropathologists, physiologists, psychologists, psychoanalysts, neurologists, geneticists, biochemists, neurochemists, biologists, and social scientists -- and finally even with psychiatrists. The decade of the 50's belonged to
biochemistry, that of the 60's will certainly mark psychopathology's
communion with neuropsychopharmacology and the late 60's and early 70's
its flirtation with biometrics. Psychopathology can live with all of these,
it welcomes them, and will eventually domesticate them. It will absorb
them even as ancient China absorbed all its invaders. Eventually, they
will all have enriched psychopathology by their impact and a $ balanced
science will emerge in which their residues will become distinguishable
only historically as is the case with the residue left by Anglo-Saxon,
Latin, and Norman French in modern English.

What are the scientific models that are now occupying the area of
psychopathology, and what are their accomplishments?

I have elsewhere (Zubin, 1969) described these models in great detail.
Here let me briefly review them for you and indicate the nature of their
contributions. First, we must draw a distinction between descriptive
approaches and etiological models. We have now had 34 centuries of description
dating back to the sacred Hindu sources of the Ayure-Veda and to some of the
most incisive delineations of psychopathology through the literary efforts
of the world's great writers and poets. However, description never cured
anyone, nor did it throw light on causes, but it did help define the field
of inquiry. We are now cashing in on the multiplicity of observations of
mental deviation through the centuries by systematizing them, through the
use of structured interviews and developing dimensions for depicting the
behavior of the mentally disordered in objective, reliable and often valid
fashion.

When it comes to the etiological models, we have not made as much
progress yet.
In the few instances where the etiology of a disorder was discovered, as was the case with general paresis, pellagra with psychosis, PKU, galactosemia, Down's Syndrome and other type of mental retardation, the disorders either disappeared or were taken over by other disciplines, leaving only the disorders of unknown origin for psychopathology. Nevertheless, even at the risk of sawing off the very limb we stand on, we must provide, if not demonstrable causes, at least presumed causes in the form of models. It is too late in the age to try to reduce etiology to a unidimensional framework such as that provided by genetics or ecology, since the evidence is so clear that multiple etiology will be found to be the rule rather than the exception. Nevertheless, it is important to push each unidimensional model as far as we can, before inquiring into its interaction with other models. We are somewhat in the same position as a statistician faced with a problem in the analysis of variance. He knows full well that none of his main effects operate independently and is fully aware that he must look at their interactions, but before he can do so, he must first separate out the main primary effects. This is how we will proceed. We will formulate each one of our models independently and then look for their interactions.

There are six such models now in the field: The ecological, developmental, learning and conditioning, hereditary, internal environment, and neurophysiological.

The ecological model simply stated assumes that the sources of man's mental disorders are to be sought in the characteristics of the ecological niche which he occupies and that all of us are vulnerable to noxious niches. According to this model, the ecological forces impinging on the niche, determine the particular nodal point occupied by the individual and thus determine his behavior. It occupies a position in psychopathology similar
to the one occupied by the Marxian model politically. Among the contributions of this model have been the establishment of the importance of such factors as population density and crowding, socioeconomic level, educational level, occupation, degree of neighborhood disorganization, minority or majority status in the community, degree of isolation, and similar ecological factors in the productions of not only mental disorders but also of physical disorders. Just how these factors bring about a mental disorder x is still a moot question but from the point of view of the ecologist, the ubiquitous tubercle bacillus for example is no more important in the production of TB than lack of genetic resistance, the noxious environmental factors and emotional stress which make possible the proliferation of the tubercle in body organs. All are necessary, but not sufficient if one or the other is missing. It is likely that mental disorders behave in similar fashion.

The developmental model stipulates that the origin of mental disorders is to be sought in the transitional stages which man passes through as he develops from fertilized ovum to foetus to neonate, child, adolescent, adult, presenium and sanium stages. Should there be lacking any of the needed nutrients or supplies that are required in passing from one level to the next, or should noxious events intervene e.g., toxemias of pregnancy, perinatal difficulties, foetal or neonatal anoxia, isolation and deprivation in childhood, the probability of mental disorder is increased.

The learning and conditioning model stipulates that one learns to become mentally ill even as he learns to become mentally well. According to the strict behavioral view, there is no need for assuming an underlying disorder which produces deviant behavior; instead the deviant behavior itself is the disorder and it is learned through experience, and not produced by an
underlying disease. Since the abnormal behavior is learned, the usual methods of acquisition of normal behavior will describe the paths to abnormality.

The hereditary model for the etiology of psychopathology is so well established in our midst as a result of the work of Rudin's school, Kallmann's Laboratory, Essen-Moller's work and his colleagues, and the more recent work of Slater, Shields, Katy, Rosenthal, Schulsinger and Mednick, and our own
Dr. Heston, to name only a few, that I am not going to spend any time discussing its importance, except to point out as a challenge, rather than as a conviction that strictly speaking there are probably no genetic disorders nor any environmental disorders in our field. We designate a disorder as genetic when we are still ignorant of its environmental parameters and vice versa. In most so-called genetic disorders, the hereditary component is necessary but not sufficient for producing a disorder. Similarly, for most so-called environmental disorders, the noxious environmental parameters are necessary but not sufficient. It is the interaction of both hereditary and environmental factors that is both necessary and sufficient for producing a disorder.

I can conceive of a genotype that could survive and maintain its health and prosper in the most inhospitable niche which our planet or any other planet can offer. By the same token I can conceive, at least in theory, of an ecological niche which could maintain the health and well being of even the most deficient genotype who could hardly survive in the usual niches on our planet.

It is true that inherited conditions like family amaurotic idiocy (Tay-Sachs Disease) or galactosemia lead inevitably in our planet to a disorder and early death, and hence, this disease might claim that its genetic loading alone is sufficient to produce the disorder. But let us not forget that, like in PKU, the environment may also play a part and it is at least conceivable that in some environments where the oxygen tension, air composition and nutritional source would be different, the propensity for Tay-Sachs disease or Huntington's Chorea (like the propensity for PKU in an environment free of phenylalanine or for galactosemia in a milk-free environment) would remain dormant.
Long ago Darwin and his followers pointed out the role of the ecological forces in gradually selecting for survival those individuals whose genetic endowment suited the niche and eliminating those whose genetic endowment was ill-suited. A similar selective process operates on the inborn behavioral repertoire of man. Here too, the ecological forces select out for survival, those behaviors which the niche reinforces and cast into oblivion those which are unreinforced or punished. Unfortunately, some ecological niches tend to reinforce inborn behavioral tendencies which lead to psychopathology. In this way the gradual selective effect of reinforcement shapes both normal as well as abnormal individuals.

This principle of selective evolution under the pressure of the environmental forces even holds for the selection of ideas and models since only those which find reinforcement in society tend to survive and even useful ideas or concepts often have to bide their time in obscurity until they are rediscovered.

As far as the model for the internal environment is concerned it stipulates that the sources of mental disorder are to be sought in the milieu interieur — in the metabolic and biochemical characteristics of the organism especially its body fluids, enzymes and metabolic products. But here again, we must remember that the biochemical agents, like the genetic, may be necessary but not sufficient. However, this topic is to be discussed by your own expert, Dr. Heath.

We now come to the last model, the neurophysiological model, which simply stated stipulates that the sources of psychopathology are to be sought in deviations in brain function, especially the electrophysiological aspects of its functioning. In our own laboratory we have been concerned more with the behavioral aspects of brain function with special reference to information
processing. Our original hope had been that the sources of psychopathology could be mapped by examining the patterns of behavior which patients exhibit in the way they process information under psychological testing. The different patterns that they would exhibit on these tests would then be used to identify the various types of disorders objectively as Kraepelin had suggested.

To our great dismay, the various clinical psychological tests ranging from intelligence to personality to projective tests turned out quite disappointing because they were so heavily dependent in many instances on the idiosyncratic reinforcement history of the individual as well as on his ecological niche. Even the Rorschach test turned out to be nothing more than an interview, which, when analyzed for its content, yielded results comparable to those of other interviews which are so heavily determined by culture. Consequently we turned to the classical response categories — the physiological, sensory, perceptual, psychomotor and conceptual.

This concludes my introductory comments on the application of scientific models to psychopathology. Now let's take up each of these models in turn and show their applicability to the investigation of schizophrenia.

There are a variety of ways of viewing schizophrenia — as a disease, like G.P. or TB; as a functional disorder, like hysteria or hypertension; as a syndrome, like the Laurence Moon Beadle Syndrome in endocrinology; as a reaction pattern, like an allergy; as a response of a vulnerable individual to a stressor; and even as a non-existing mythical label applied by either a hostile environment (Laing) or a greed-driven profession (Szasz). Which of these views one chooses to adopt will determine not only how he views the patient, but also how he treats the patient and how the patient will far.
What can one do to bring some system or order into a field that appears so chaotic? This is the theme of my paper, and how well I treat it will depend no so much on what I say, as on the readiness with which you are willing to explore with me views and attitudes which may be either acceptable or unlikely in your estimation, antithetical to your beliefs, and perhaps already rejected by you because of your personal experiences, clinical insight or research evidence. Yet, such is the current state of the field, that even long discarded notions rise to challenge us and long cherished concepts fall into discard.

Why is the field of schizophrenia so difficult to deal with?

The primary difficulty inheres in the fact that unlike some physical disorders, like TB there is no palpable cause for the behavior which we observe except the behavior itself.

Given the fact that all we have to go on is the behavior itself, without the help of any demonstrable underlying lesion or dysfunction, we must fall back on description of the behavior for detecting, diagnosing, treating or preventing the disorder. However, even in conditions like TB where the causes of the disorder are known and palpable, no two patients present an identical picture. Even though the identical lung lesion may be present in two patients, their reaction to the lesion, their behavior, their outlook and their actual wellbeing will depend upon many other factors including the ecological niche they occupy in society, their personality etc. Hence, even when we can separate out the focal disorder from its peripheral surround, we do not always observe the same total phenomena for two patients suffering from the same core disorder. Let us call the core disorder - the disease and separate it from the total picture presented by the patient by calling
it the illness (including both the focal and the peripheral aspects). We may conclude that though for a given disorder, the focal aspects are invariant for example, presence of tubercle bacillus, lesion in the lung, low genetic resistance, and an emotional milieu favorable to development of tubercle bacilli,—nevertheless, some patients will remain hopeful, energetic, outgoing and thus present a good prognosis while others will be depressed, present hopeless and lethargic and a poor prognosis. When we come to schizophrenia, even if we were sure that the focal disorder were identical from patient to patient, the illness would still be variable because of the premorbid personality and the ecological niche which the patient occupies. But unfortunately, we have no way of separating the disorder from the illness in schizophrenia as we can in TB, and consequently we are unable to specify the core disorder which is a sine qua non for schizophrenia. It may also be, that much or all of our treatment is aimed at the periphery rather than at the core of the disorder and that is why so many different therapies are found to be efficacious for some patients and not for others. Added to this problem, is, of course, the problem that there may be more than one type of schizophrenia all masquerading under the same label. Well, what can one do when faced with such a chameleon condition?

The first step that one can take, despite the difficulties already outlined, is to describe the behavior in as meticulous a fashion as possible in a reliable and valid way. Thirty-four centuries after schizophrenic behavior was first noted in the ancient Hindu Ayure-Veda text (The Caraka Samhita), we still diagnose on the basis of a description of observed behavior and to make sure that a colleague understands the case we give him a case history to read. There are no X-rays, no Wassermans, no biochemical tests,
no karyotypes that can be presented as evidence for the presence of schizophrenia that is acceptable in court today. Even if this were not the case, description of disorder is necessary to delimit the field of inquiry if we are to understand each other.

However, description is not enough; for 34 centuries abundant descriptions of psychopathology have never produced a cure, nor particularly aided the search for etiology.

We must first realize that clinical diagnosis, which is heavily dependent upon the descriptive approach, has suffered through the years from the attempt to combine it with imaginative but unproved etiological elements. This has produced a certain number of diagnoses based on "faith" rather than on evidence, and has given diagnosis a denigrated status. An additional factor was the lack of utility of diagnosis for selection of therapy and prognosis until the armamentarium of the therapies expanded in the 1940's to 1960's. We are now seeing a revival of interest in diagnosis because it has become useful again. Faced with the impasse produced by the failure of psychological clinical testing to provide an independent criterion for the diagnosis of schizophrenia as it did succeed in doing for mental retardation, psychologists and psychiatrists returned to clinical observational techniques and the interview to provide a more viable approach to diagnosis.

This movement developed in several stages. For the 1944 edition of the book, Outlines for Psychiatric Examinations, I assisted Dr. Nolan D.C. Lewis in systematizing the mental status examination and prepared a mimeographed sheet to accompany the examination which provided for checking significant items in the record. This was used for a while, but World War II produced
such a shortage of man power that it was abandoned. In the meantime, rating scales for assessing patient behavior came to the fore, spearheaded by the Malamud-Sands Rating Scale (1947) and followed soon after by those of Wittenborn (1955) and Lorr (1953). Because of our own interest in verbal behavior and the interview, our Biometric Research Unit began to develop systematic structured interviews. Each question in the interview was accompanied by several possible response items. The presence or absence of each specific item could be recorded by the interviewer as he proceeded with his interview. The mental status schedule, which had been the psychiatrist's mainstay, was converted into this kind of systematic structured interview in our laboratory, yielding both high reliability in its scoring of the items as well as considerable validity. There are three types of interviews: (1) a non-probing approach -- SCI (Burdock and Hardesty, 1969); (2) a medium-probing -- MSS (Spitzer et al., 1964) and PSS (Spitzer et al., 1970); and (3) a deep-probing -- MRC schedule (Wing et al., 1967). (SLIDES)

On the basis of a combined schedule comprised of non-probing, medium-probing, and hard-probing items, our Project on Diagnosis of Mental Disorders in the United Kingdom and the United States, headed by Dr. Barry Gurland in the United States and Dr. John Cooper in the United Kingdom, was able to demonstrate that the much heralded differences in the incidence of hospitalized functional mental disorders (schizophrenia versus affective psychosis) in the United States and the United Kingdom were little more than labelling differences (Cooper et al., 1969; Gurland et al., 1969).
To summarize, I have outlined the variety of approaches available for investigating schizophrenia, beginning with the descriptive approach and ending with the variety of causal approaches now available. Having defined the problem descriptively we can now proceed to investigate its various aspects which I will do in the second lecture tomorrow. In it I will stress the importance of culture dependent, culture fair and culture free techniques now available for investigating the ramifications of the schizophrenic problem.