Taxonomy in the Mental Disorders

-- A historical perspective

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It is customary for the first speaker to introduce the symposium topic. This is not easy in this symposium because of the chameleon-like character of the mental disorders which, unlike many physical disorders, take on the local coloration of their culture. While every known culture has its share of mental disorders, their character varies so much with the milieu that it is difficult to make cross-cultural comparisons. This holds true even more when comparisons are made of different historical periods. Rather than add to a rather long list of historical surveys we begin by pointing out what have been viewed, at one time or another, as the principal dimensions of the mental disorders, and by indicating their significance for diagnosis. We will then proceed to a consideration of what the future has in store.

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. 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 Ayur-Veda, 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 glutinous, filthy, walks naked, has lost his memory and moves about in an uneasy manner." (See the Caraka Samhita, 1949.) Further, 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! Our diagnoses still rest largely on this dichotomy today. It was quite a satisfactory basis for classification as long as we regarded the patient as suffering from a disease of the soul and hence did not require any material substrate to explain his disease. With the rise of modern science and the replacement of the soul by the mind in psychopathology it has become quite clear that the distinction based upon the dualism of mind and body no longer can be maintained. Nevertheless the schism persists. 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. This has been the case with general paresis, pellagra psychosis, and will probably also be the case with phenylpyruvic oligophrenia. Only diseases of unknown origin tend to remain in the domain of psychopathology.

The second dimension that emerges from our analysis is the hereditary-environmental dichotomy. There is a long history of attempts at discovering the hereditary basis of mental disorders as opposed to their environmental causation. As we all know today, this dichotomy too is a spurious one. Present-day distinctions between hereditary and environmental diseases are based on ignorance rather than knowledge. If we have evidence stemming from family and twin investigations and from the presence of actual chromosome abnormalities but still do not know the environmental factors required for eliciting an illness, we call it a hereditary disease. By the same token we call a disease environmental if we have found the particular environmental agent that is necessary for eliciting it but have not yet determined the underlying genotype necessary for its evocation. But it is common knowledge that both heredity and environment must cooperate in the production of both health and disease.

The third dichotomy emerging in our analysis is multiple causation versus unitary causation of mental disorders. This raises the issue of 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 well-defined 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 reflects the question of whether to regard mental disorders as diseases or as reaction patterns. The latter point of view 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 the primary focus, and whether 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 has 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 account for the variety of data, gathered through genetic investigations, which seem to indicate that not everyone who is exposed to similar vicissitudes seems to develop similar personality deviations.

The final dimension we might discuss is the acute versus chronic dichotomy, or deterioration versus intactness. This has had a long history, going back to the days of Morel in the beginning 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 the disease process or whether it resulted from poor custodial care, is still an open question. With today's greater tolerance of deviant behavior, and with higher release rates, we probably will see less and less reference to deterioration as a possible outcome.

Each of these dichotomies—the organic—functional, hereditary—environmental, unicausal—multicausal, disease—reaction pattern, and acute—chronic dimensions—has 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 etiology.

In addition to the dichotomies already discussed, there is another dichotomy that adds noise to the diagnostic system—the conflicts between the diagnostician and the actuary. The pressing needs of the moment and the need for finding some wise decision regarding the disposition of a case often forces a diagnostician into a tentative conclusion regarding the diagnosis that a more research-minded colleague might refuse to accept. On the other hand, the search for syndromes and patterns by which the actuary is motivated may, if it is a blind search, lead him into a cul-de-sac of diagnostic decisions unworthy of consideration when it comes to questions of therapy or prognosis. Essentially, the diagnostician is searching for patterns. Recognition of patterns actuarially has not yet reached the state of development that some gifted clinicians have already attained. I would venture to say however that this conflict is a pseudo—conflict. We might consider the clinically gifted diagnostician as working in Reichenbach's (1938) realm of discovery and the actuarial worker as working in the realm of verification. While we already know the well-trodden road of verification, the path leading to discovery is not well defined. That is why it is important to nurture the gifted clinician so as not to dry up the well from which new ideas emerge. Unfortunately, the less gifted clinician also wishes to be a creative dweller in the realm of discovery but, lacking true creative powers, adopts a pseudo—actuarial approach in which his prior experience and intuitions become matters of faith and routine application instead of being continuously tested for their efficiency. By the same token, the less gifted actuary sometimes becomes a pseudo—clinician and applies his knowledge to cases where verification is wanting. Let us remember that behind every gifted clinician stands an actuary attempting to verify his hunches; behind this actuary stands another clinician looking over his shoulder with newly discovered insights; behind him, in turn, waits the next actuary.
to verify these new findings; and so on, ad infinitum. It has been reported in some recent investigations that very often the analytically minded actuary tends to confuse and befuddle the intuitively gifted diagnostician he teams up with. This is very unfortunate, since the need for insightful integrations of the traits that the actuary analyzes is so important for making progress in this field.

We come now to the major question before us: what is wrong with present-day diagnostic systems and why did we organize a symposium to discuss new methods for taxonomy? Why is the current system found wanting? There are several reasons for the current debacle in diagnosis that has led many psychiatrists to give up diagnosis entirely. Among these are the following: the rise of psychoanalysis and social psychiatry, the revolution in psychopathology that has occurred in the last two decades, and the rise of epidemiology that has brought comparative data from various regions and countries into focus.

Beginning with the nineteen-twenties, social psychiatry began to appear in the field of psychopathology and began to point to such factors as social, economic, cultural, educational, and ethnic differences in the diagnosis of mental disorders and in their detection as well as in their treatment. The initial detection of mental disorders is rarely made by professionals, but rather by laymen: the patient himself, his family, friends, neighbors, policemen, sheriffs, etc. Therefore communities that have different attitudes towards mental disorder will tend to segregate for attention different kinds of patients and thus produce apparent differences in the occurrence of mental disorders, even though basically there may be no such differences. The socio-economic status of a patient often is a determining factor in the diagnosis.

Not only does the culture in which the patient lives determine the kind of psychopathology that will be detected in him, but the cultural background of the diagnostician himself will determine to a large extent what he will make of an illness once it is brought to his attention. One school of psychopathology denies any specificity to mental disorders, maintaining that there is only one kind—failure to adapt or adjust to the environment. For this school, diagnosis is either impossible or of no value. At the opposite end of the spectrum is the school that maintains psychiatric diagnoses are based on pathognomonic factors or syndromes of such factors that are universally characteristic of each category of disease in all environments. Between these two extremes there are various compromises. This is one reason why the reliability of diagnoses is so low.

Another reason for the impasse in present-day diagnosis is the recent revolution that has occurred and is still occurring in psychopathology with regard to management and also with regard to research developments. Up until the 1920s the making of diagnoses was really not useful except for statistical and administrative purposes. Diagnosis was purely academic, since the available treatments were too few. No matter what the diagnosis was, the treatment remained essentially the same—custodial care or its equivalent.
With the gradual rise of the somatic therapies, beginning with the fever treatment for general paresis, and of the psychotherapies and more recently of the drug therapies, diagnosis has assumed a greater and greater importance because of the need for determining the kind of therapy best suited for each patient. It is therefore to be regretted that as the need for diagnosis gradually grew, interest in it fell to such a low point because of its previous inutility. This is one of the tragedies of the current scene. For the first time we can make use of diagnostic skills, but we do not have them because they have fallen into disuse. The reasons for their disuse are not too difficult to fathom. The interest in mental patients has gradually shifted from the mental hospital to the outpatient clinic and to the private practitioner's office. Diagnostic schemes once useful in the state hospital are no longer as useful, as became evident during World War II. The attempts to patch up the diagnostic system to meet the new needs—especially those due to the introduction of psychodynamics—turned out to be less successful than was hoped.

Further evidence for the inadequacy of the present nosological system arises from the impact of epidemiological studies of the mental disorders. One recent report by Kramer (1961) underlines the tremendous discrepancies that exist between the United States and the United Kingdom. First are the well-known discrepancies in first admissions for affective disorders and for the psychoses with cerebral arteriosclerosis, the former being more prevalent in the United Kingdom and the latter in the United States. Kramer finds in additional definitely higher ratio of females to males among the first admissions in the United Kingdom, while in the United States the opposite holds true, the males exceeding the females. It is difficult to determine the cause of these differences at this time, but labeling differences may well be responsible.

We now turn our attention to the question of how this challenge presented by the need for good diagnostic criteria is to be met. One of the great needs of today is the provision of suitable instruments. One of the most damaging condemnations of present diagnosis is the documentation of its low reliability. The evidence on this score is so great that we hardly need to add to it here. One of the reasons for this difficulty may be a reflection of the fact that we have failed to consider the social-cultural aspects of the detection and diagnostic procedure. At the present time all of our diagnostic approaches depend upon recognizing deviation from social-cultural norms. These deviations constitute the symptoms on the basis of which diagnosis is made. Some of these symptoms may be found to be universally characteristic of patients across all cultures, while others may be more culture-dependent. In the case of those categories of illness whose characteristics seem to be independent of culture there should be little difficulty in identifying similar disorders under different conditions. For those disorders manifested in behaviors that are culture-bound it will be very difficult to identify similar disorders in different cultures, since their characteristic behaviors will of course be different.

Barbara Wooton (1959) has stressed the dependence of diagnosis on social-cultural valuation in the following:

"... 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 theiving tendency together with its bodily concomitants as symptoms of disease or disfunction; 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."

But there must be more to mental disorder than simply deviation from social-cultural expectancy. Elsewhere (Zubin, 1963) we have pointed out that there must be some kind of vulnerability, either genetically based or due to early-environmental or perhaps intra-uterine conditions, which constitutes an important element in the development of the mental disorder or its occlusion depending upon the kind of environment the individual grows up in. We need, therefore, instruments for both types of evidence, the culture-bound and the culture-fair or culture-free. We need instruments to detect social-cultural deviation and we also need instruments for detection of vulnerability independent of social-cultural deviation.

Let us first turn to the former category—deviation. At the present time the chief basis for detection and diagnosis is the psychiatric interview. It is true that psychological tests are also often used but they are for the most part only secondary. It is unfortunate that this is the case, since Kraepelin himself laid so much stress on the fact that if enough suitable psychological tests become available it would be possible to detect the vulnerable personality by noting the individuals who are at the extreme ends of distributions in the various psychological performances. Although this was a promising idea, it has not yet paid off and perhaps will never pay off unless we find methods for getting at more basic aspects of psychological
functioning than our tests have thus far been able to elicit. Perhaps our tests are
too culture-bound to be of general use. For example, the Board of Education in
New York City has given up the use of intelligence tests and has substituted obser-
vational techniques and interviews. Even limiting ourselves to the interview, we
can make progress by demanding better instruments for interviewing. In our own
laboratory we have developed a series of structured interviews which are objective
in nature and which elicit the varieties of social-cultural deviation that we nowa-
days look for in the detection and diagnosis of mental disorder. The clinical inter-
view that now constitutes the basis for diagnosis is an unstructured interview varying
from clinician to clinician and unsuited to the purposes of scientific investi-
gation.

Unless a systematic procedure is introduced for collecting information, diag-
nosis will vary from observer to observer even for the same patient. In order to
increase agreement among observers, four controlled procedures are being
developed: (1) the Mental Status Schedule; (2) the Social Adaptation Schedule; (3)
the Structured Clinical Interview; and (4) the Ward Behavior Inventory.

The application of rating scales to the classification of mental patients has
a long history despite its relatively recent development. Father Moore (1933) was
one of the first to rate the behavior notes in case history material. He factor-
analyzed the results, arriving at dimensions not unlike those Kraepelin developed
earlier through clinical observation. The Malamud-Sands scale (1947) was the
next one to be developed. Since then, Lorr (1953) and Wittenborn (1955) have pro-
vided scales for psychiatrists for rating the behavior of mental patients.

The chief virtue of the new instruments lies in the structure they impose on
the interview. While we now have a variety of scales for rating the clinical inter-
view, the conduct of the interview on which these scales are based depends entirely
on the preference of the individual interviewer. In order to get comparability on
their scales it is essential that the interviews themselves be more standardized.
This we have attempted to do for the mental status, the anamnesis, the social-
adaptation, and the ward behavior of the patient. We shall describe only the
Mental Status Schedule, since the other techniques are built on the same paradigm.

This technique consists of an interview schedule for the examination and a
matching inventory of 248 dichotomous items descriptive of small units of patho-
logical behavior. The schedule contains questions arranged in a definite sequence
designed to provide for follow-up of incomplete responses. Most of the questions
are open-ended, so as to encourage the patient to reveal his own mentation. Proper-
ly administered, the interview has the "feel" of the clinical evaluation. However,
unlike the usual clinical interview, the provision of a specific schedule of questions,
a fixed order of presentation, and a uniform coverage of the same areas of psycho-
pathology with each patient make it more likely that the differences observed will
be due to actual differences among patients rather than to different interviewing
procedures. This technique has yielded reliabilities of the order of .90 or more
when several groups of patients were evaluated independently by different
psychiatrists. Moreover, it has distinguished significantly between the amount of psychopathology shown by different groups of patients, i.e. inpatients, clinic outpatients, and former inpatients examined on follow-up.

The development of techniques for the culture fair or culture-free detection of mental disorder are not as easy, because the groundwork for such techniques is only now beginning to be laid down. Elsewhere (Zubin, 1963) we have proposed that in order to sample man's capacities we must not limit ourselves, as most psychological tests do, to responses that depend upon past experience, history of reinforcement, etc. We pointed out that as one ascends from purely physiological responses to the sensory, perceptual, psychomotor, and conceptual realms, the roles played by past experience and cultural influence increase. For this reason, a search for culture-free indicators must focus on the physiological, sensory, perceptual, and psychomotor responses that are likely to depend less upon culture than to the conceptual responses. We have postulated that responses elicited within the first 1000 milliseconds following stimulation occur so rapidly that they may not be influenced by cultural factors, and we have now found several such indicators in our laboratory. Some of the behavioral techniques we have developed that fall in this category of depending less upon social-cultural norms involve (1) pupillography (2) cross-modality reaction time, and (3) measures of temporal resolution in vision and audition.

After collecting the data based upon structured interviewing instruments or culture-free techniques, the next problem is that of discovering the best methods for analyzing the data so as to yield homogeneous groups of patients that may parallel present-day diagnostic grouping or perhaps cut across them. The provision of such techniques is now in the forefront of our thinking. The rest of this paper will be devoted to reviewing such techniques and indicating in what direction they might go. One of the interesting and happy developments is that the high-speed computer makes it possible to do things now that twenty or thirty years ago would have been thought impossible because of the onerous nature of the analyses that had to be made.

While there are techniques for contrasting samples from two different populations on a variety of tests, viz. Fisher's discriminant function and the likelihood of ratio criterion for dichotomous variables (Birnbaum and Maxwell, 1959), there are no proven techniques involving more than one variable for dealing with the problem of separating a population into subgroups. The univariate problem was solved by Pearson (see Rao, 1952) by the method of moments. There has recently been renewed interest among theoretical statisticians in the problem of fractionating mixtures of homogeneous populations. Hill (1963) considers the case of univariate normal populations, Rider (1961) the case of Poisson, binomial, and negative binomial populations, Blischke (1962, 1964) the binomial case, and Boes (1963) the uniform case. No results appear to be available for the case of more than one measurement per patient, however; it seems that here, for once, the mathematicians trail the applied scientists, rather than lead the way for them. Thus, investigators in the nosology of mental disorders have had to resort to clustering methods,
measuring the similarity among patients and grouping patients in such a way that
individuals within a cluster are more similar than are individuals from different
clusters.

As a first step in this direction, Zubin (1938a, 1938b) provided the method
of likemindedness analysis, which is suitable for finding the individuals who tend
to cluster on qualitative variables such as response to dichotomous items. This
method has already proved its usefulness in a variety of situations and more re-
cently has been extended by McQuitty (1954) in several analyses.

Another method that is designed for dichotomous responses is Lazarsfeld's
latent structure analysis (1950). This method differs from likemindedness analy-
sis in that it attempts to locate subgroups of the population such that, within the
subgroups, the responses to the items are all uncorrelated among themselves.
Latent structure analysis has been applied with some success in the social sci-
ences (Lazarsfeld, 1950) and we anticipate success in its application to the men-
tal disorders as well. We know of no published results, however.

In dealing with quantitative data, these methods cannot be applied directly
unless the variables can be dichotomized without loss of too much information.
If loss of information is to be avoided, methods suitable to quantitative data must
be resorted to.

In the case of continuous measurements, a great many clustering proce-
dures are available. Perhaps the first, historically, is so-called inverse factor
analysis, or Q technique, due to Stephenson (1935, 1952). In this procedure, the
correlations between persons are used in a factor analysis, rather than the cor-
relations between variables as in classical factor analysis. Lorrr(1963) gives an
excellent criticism of this technique, and among his cogent arguments are the fact
that information on the profile level and profile scatter is lost and the fact that the
results do not really yield clusters of people, but rather clusters of traits.
There are, further, deep logical questions concerning inverse factor analysis;
what does it mean for a patient to have a factor loading derived from a cluster of
patients; what meaning does the linear model underlying factor analysis have;
what, in fact, is the meaning of a correlation between two people? Q technique
has been employed in a number of typological investigations: by Monro (1955) in
England and by Katz and Cole (1963) in the United States, for example. Before
the results of such studies are employed in forming the basis of a revised nosology,
we strongly suggest that the logical underpinnings of Q technique be meticulously
scrutinized.

In addition to the correlation between two people, there is available the dis-
tance between them as a measure of similarity. The distance is merely the
square root of the sum of squared differences on each of the variables, with the
variables converted to a common metric by division by the standard deviation.
Underlying the distance measure there is the intuitively meaningful representation
of the data as a set of points in a Euclidian space of as many dimensions as there
are variables. Given such a representation, Euclidian distance is the natural
similarity measure, as demonstrated by Cronbach and Gleser (1953).
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Another method that is designed for dichotomous responses is Lazarsfeld's latent structure analysis (1950). This method differs from likemindedness analysis in that it attempts to locate subgroups of the population such that, within the subgroups, the responses to the items are all uncorrelated among themselves. Latent structure analysis has been applied with some success in the social sciences (Lazarsfeld, 1950) and we anticipate success in its application to the mental disorders as well. We know of no published results, however.

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In addition to the correlation between two people, there is available the distance between them as a measure of similarity. The distance is merely the square root of the sum of squared differences on each of the variables, with the variables converted to a common metric by division by the standard deviation. Underlying the distance measure there is the intuitively meaningful representation of the data as a set of points in a Euclidian space of as many dimensions as there are variables. Given such a representation, Euclidian distance is the natural similarity measure, as demonstrated by Cronbach and Gleser (1953).
The difficulties inherent in the use of the distance measure are at least two-fold. First of all, there is the problem of correlations among the variables. Of the two clustering procedures employing the distance measure that have recently been proposed (Sawrey, Keller, and Conger, 1960, and Saunders and Schucman, 1962), neither faces the question of correlations squarely. The nature of the difficulty is that if the variables are correlated within the subgroups to be identified, then we would want to detect these groups with their proper contours. How can this be accomplished by ignoring the correlations, as is done in the two procedures mentioned, is not at all clear. The solution is not to estimate the correlations from the sample, however, for, as is well known, when groups having different means are combined, true within-group correlations that are positive may be estimated as negative, true negative correlations may be estimated as positive, and true zero correlations may be estimated as far different from zero, all of these possibilities depending only on the configuration of the subgroup means.

The second problem lies in the estimation of the standard deviations. The point here is that if a variable is effective in separating subgroups, then the sample deviation of this variable is inflated by an amount equal to the variability of the subgroup means. We will thus be dividing each difference on this variable by too large a quantity, diminishing the contribution to the distance of a variable that should be given the fullest weight. Saunders and Schucman (1962) seem to recognize this dilemma, for they describe the use of their technique on variables that are divided by known population standard deviations. They do not, nor do Sawrey et al. (1960), consider the problem of the bias in the standard deviation that, in the vast majority of cases, will have to be estimated from the sample.

We are not able to propose any solutions to these two problems, but we would like to conclude with the description of yet another procedure for isolating homogeneous subgroups. This method, still being perfected by the second author, takes as its point of departure a definition of homogeneity as a multivariate normal distribution within each subgroup. The method begins by testing the entire sample for normality; if the hypothesis is rejected, two normal populations are hypothesized, their parameters estimated, the sample separated into the two groups, and each group tested for normality. If this hypothesis is rejected, an attempt is made to identify three normal subgroups, and the procedure continues until a goodness-of-fit test fails to reject the hypothesis of normality. Much work remains to be done on refining this method, but we seriously believe that, if success is to be had in the erection of a psychiatric nosology based on objective numerical methods, it will be as the result of procedures based on explicit statistical models, such as latent structure or the method just outlined, and not on ad hoc decisions, no matter how seemingly objective, as to which people belong together.

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