A discussion of descriptive psychopathology should occupy first place in a conference in schizophrenia, since none of the research findings we now have can be guaranteed "hard" so long as we cannot agree on the material with which we are dealing. Thirty-four centuries after schizophrenic behavior was first noted (in the ancient Hindu Ayura Veda), 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.

So much of our effort is bound to go to waste both nationally and cross-nationally if the terms we use and the field we delineate are not the same (or if not identical, at least translatable) for all who work in the vineyard. The lack of a common descriptive framework has probably impeded the understanding of schizophrenia more than any other factor--including lack of funds. However, description is not enough, since no amount of it
ever produced a cure, nor has having had descriptions of psychopathology for 24 centuries helped much etiologically. However, perhaps we are now at last beginning to develop observational techniques, systematic interviews, functional analyses of behavior, and testing procedures that can establish an objective biometric framework for psychopathology that is becoming less dependent on the observer and more on the observed!

It might be well to indicate the relation between the descriptive aspect of psychopathology and etiological approaches.

Because of our ignorance of the causes of schizophrenia, the descriptive approach has thus far been the most useful. Once we attempt to investigate etiology, we must resort to our imagination. In other words, we have to invent "as if" etiologies in the form of scientific models. At the risk of transgressing against the present ground rules for dealing only with hard facts, I will nevertheless make this sortie into fantasy, because the hard facts of today are often the fantasies of yesteryear -- as well as being the myths of tomorrow!

What are the scientific models that have been proposed? Elsewhere I have isolated the following: (1) ecological, (2) developmental, (3) learning, (4) genetic, (5) internal environment, and (6) neurophysiological. (Show Slide 1).

The ecological approach searches for etiology in the ecological niche which the patient occupies and indicts the following factors as possible etiological agents: socioeconomic status, educational level, ethnic origin, physical surroundings, etc. We are still lacking a taxonomy of environmental forces which may be the culprits in the development of schizophrenia.
Though this model is unfortunately not represented separately in this conference, it is no doubt dealt with by my colleagues.

The developmental model searches for cause in the transitional stages which man goes through in passing from foetus to neonate to childhood, adolescence, adulthood, and the senium. Disturbances at these various levels of development or in the interfaces between levels are the sources of the disorder.

Toxemias of pregnancy in the foetus, insufficient stimulation in the neonate, absence of peer relationships in adolescence, poor heterosexual adjustment, inappropriate mental and vocational adjustment, are examples of the kinds of variables examined in this model. Unfortunately, the critical periods which the organism goes through are often crucial for normal development, and no amount of subsequent corrective effort can remedy the deviation. Dr. Mishler's contribution will no doubt deal with such problems.

The learning theory model stipulates that schizophrenia like normality is learned behavior. A functional analysis of behavior is necessary to determine what were the contingencies in which the behavior in question developed and what are the contingencies which maintain it now. Once we understand the sources of this behavior and/or the reinforcement which maintains it, we are well on the way to its elimination. This model too is not represented here directly, but it is my impression that Dr. Payne's contribution would probably fit into this model.

The genetic model needs little explanation. Simply stated its main tenet is that unless you inherit the propensity for schizophrenia you can
never be one! Dr. Schulinger's contribution will deal with this area and
I am looking forward to hearing the present state of our knowledge.

The internal environment model has been with us since the days of
Kraepelin and earlier and the biological basis of schizophrenia is one of
the most active research areas of today. Dr. Weil-Malherbe will no doubt
bring us up to date on the present status of the field.

The neurophysiological model, simply stated, is that the processing of
information in the schizophrenic is different from what takes place in
normals. While Dr. Holzman may not admit to being a neurophysiologist, his
contribution to perceptual disorders in schizophrenia seems to belong in
this general purview.

Scientific models are merely scaffolds for building bridges across gaps
in knowledge. These bridges are usually called hypotheses. Some of them
are made of paper and collapse as soon as any evidence is placed on them.
Others are made of iron and can withstand the weight of a considerable
amount of evidence both pro and con before they collapse. Some are so
strong that they persist for a long time and become theories expressing
permanent laws, though time may erode them as was the case with some of
Newton's Laws.

Unfortunately, few of the bridges we have built in psychopathology have
withstood the weight of evidence. Excluding general paralysis, pellagra with
psychosis, PKU and some other mental deficiencies, we have little to show
for our efforts at discovering etiology. In fact, we are in the peculiar
position of losing to general medicine or some biomedical specialty the
disorders whose etiology become known, retaining only the disorders of
unknown origin. But any hypothesis worth its salt should be testable. It's
the untestable hypotheses that clutter our thinking and remain permanently
suspended in the thin air without confirmation or disconfirmation.

What tools do we have for testing the hypotheses emanating from our
various models? These tools developed in time. For example, it was not
until the electron microscope was invented that we were able to photograph
the constituents of chromosomes and thus lay a fundamental basis for
genetics. Our models fall into three clusters with regard to techniques
for testing hypotheses. The ecological model depends primarily on culture-
dependent techniques such as the interview for its data and hypothesis-
testing. The genetic, internal environment, and neurophysiological models,
while utilizing whatever tools seem suitable, can be relatively free of
culture-dependent tools and adopt culture-free tools -- chromosome counts,
biochemical tests, electrophysiological measures, etc. The developmental
and learning theory models utilize culture-fair techniques in which the
tools, though reflecting the local coloration of the culture, can nevertheless
be translated across cultures. Thus, greeting behavior or mourning behavior,
for example, though dependent on local culture, has its counterpart across
cultures and deviations in greeting or mourning behavior can be detected
cross-culturally. Similarly, incommunicability of patient speech can be
detected cross-culturally by culture-reflected techniques.

Let us now turn back to the descriptive approach and see what
advances have been made in this area, what the significance of these
advances might mean for therapy, and how the advances can be integrated
into teaching.
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 expended in the 1940's to 1960's. We are now seeing a revival of interest in diagnosis because it has become useful again. The revival arose as a result of several developments. First, faced with the impasse produced by the failure of psychological clinical testing to provide an independent criterion for diagnosis of schizophrenia as it did succeed in doing for mental retardation, psychologists and psychiatrists returned to clinical observational techniques and the interview and began to fashion a sharp-shooting rifle out of this blunderbus.

This movement developed in several stages. For the 1944 edition of the book, *Outlines for Psychiatric Examinations*, I had 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. Meanwhile rating scales for assessing patient behavior came to the fore, spearheaded by the Kalanud-Sands Rating Scale. Wittenborn's and Lorr's soon followed. Because of our own interest in verbal behavior and the interview, our Biometric Research Unit began to develop systematic structured interviews that contained specific items to which a yes or no answer could be recorded by the interviewer.
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 by Drs. Burdock, Hardesty and Spitzer in our laboratory, yielding both high reliability in its scoring of the items as well as considerable validity. This movement developed in three stages: (1) a non-probing approach -- SCI (Durdock and Hardesty); (2) a medium-probing -- NSS (Spitzer and Burdock) and PSS (Spitzer and Endicott); and (3) a deep-probing -- MRC schedule (Wing, et al.)

Examples of each of these techniques are shown in the following slides. (Show slides of SCI, PSS and MRC).

An example of what such systematic interviewing can accomplish in the teaching area is given by the following video-tape experiment (Katz, et al.).

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 U.K. and U.S., headed by Dr. Barry Curland in the U.S. and Dr. John Cooper in the U.K., was able to demonstrate that the much heralded differences in the incidence of hospitalized mental disorders in the U.S. and U.K. were little more than labelling differences. (Show Slide).

An example of the need for better diagnostic instruction in the U.S. is shown by the following analysis based on a paper by Dr. Curland. By means of our structured interviews, a group of patients was found in both the U.S. and the U.K. whose profiles indicated that they suffered from morbid depression and nothing else! They scored above the mean on the depression scale, and showed no symptoms which would counterindicate a diagnosis of an affective disorder according to D.I. II or standard textbooks in the USA. These were contrasted with a group consisting essentially of schizophrenics. They had to
score below the mean in depression but score high on blunting of affect, incomprehensibility of speech, or delusions of control. Alcoholics, drug addicts, and organic disorders were excluded from both groups. These two groups each constituted about 10 to 20 percent of all the admissions in both the U.S. and U.K. hospitals.

In New York, only 21% of the depressed group was given the diagnosis of depression, while 60% were given the diagnosis of schizophrenia and 19% given the diagnosis of "other". In the U.K. the picture is reversed, 60% were given the diagnosis of depression, 14% the diagnosis of schizophrenia, and 26% the diagnosis of other. Apparently, in both countries, morbid depressives are mislabelled to some extent, but the proportion mislabelled is fully 79% in the USA. but only half as much (40%) in the U.K.

What effect does this mislabelling have on treatment and outcome? In New York, only 50% of this essentially depressed group were treated for depression, while in London more than 84% were thus treated. In other words, half of the morbid depressives in New York missed out on the treatment of choice, while only 15% of the London patients missed their opportunity to receive proper treatment. There is some evidence that the New York psychiatrists were sensitive to the presence of depression in this patient group, but their failure to make a diagnosis of depression rather than schizophrenia has produced a situation which denied the best treatment for at least one group of patients.

The lesson to be gained from this study is that we must make greater efforts to develop a more sensible classification system which will redound to the best interests of the patient's treatment. This can be done in two ways:

1. improve our descriptive approaches so as to yield more reliable and valid
categories; (2) utilize the advances provided by the aetiological models to fortify the descriptive classifications. As an example of this method, let me show you how a simple reaction time experiment provided us with a better classification of schizophrenia.

We were interested in investigating the effect of a subject's shifting attention from a light stimulus to a sound stimulus (and vice versa) on the reaction time. We hypothesized that the retardation in reaction time brought on by shifting modalities would be longer for schizophrenics. We innocently asked our psychiatric colleague at a state hospital to select newly admitted schizophrenics for us to test and also asked him to survey the normal controls which we selected from a state employment agency and other sources. By the time the experiment was finished, we had developed our new systematic structured interviews and naturally thought that we could obtain measures of psychopathology by these instruments. We interviewed these patients and subjected the results to a computer analysis yielding a diagnosis. To our great surprise, half of the patients were not diagnosed as schizophrenic and half of the controls were not diagnosed as normal.

An examination of the reaction time scores indicated that a comparison of the purified schizophrenic group and purified normal group gave a much clearer separation; and furthermore, the variability in both groups dropped considerably. We can now proceed further to take schizophrenics who resemble our pure group in their profiles and examine them on our reaction time studies to see if we can validate this finding -- and if we find deviants among this group, to see how they differ from the rest. This iterative method of going back and forth between laboratory measures and interviews can finally yield us much more homogeneous subgroups for research and,
hopefully, a better selection of treatments.

But we must not lose sight of the fact that getting reliable diagnoses is not enough. They must prove their worth in follow-up. If, regardless of treatment, patients attain the same fate no matter what label is attached to them, we have wasted our time. Perhaps the present day labels will give way to clusters of patients that cut across today's categories. But until that day arrives, and we are certainly working toward it, we must do the best we can with what categories we now have, but use them to advantage, and at the same time scrutinize them for improvement. This is where research and training meet head on -- and where progress lies.

Summary:

In searching for the hard facts in descriptive psychopathology we have pointed out that there are now available systematic structured interviews for capturing objectively and reliably the behavior (including thought and feeling) of mental patients which can lead to better classification and choice of treatment. As a result of our cross-national study, we discovered a group of patients who are essentially depressed but who are denied proper treatment in this country because they are labeled schizophrenic. This holds true to a much lesser extent in the U.K.

By employing systematic interviews in the selection of patients for experiments bearing on etiology, we can take advantage of the iterative method of working back and forth between the laboratory and the clinic to improve the results of both.
Emerging Trends in Descriptive Psychopathology and Cross-cultural Studies

(Addendum to Position Paper on Descriptive Psychopathology and Cross-cultural Studies)

Joseph Zubin

Biometrics Research
New York State Department of Mental Hygiene


The position paper dealt with "hard facts" or established trends in the area of descriptive psychopathology and cross-cultural studies. We will now turn our attention to some trends peeking over the horizon whose future is still in doubt but which seem to have considerable promise.

Descriptive Psychopathology

There seem to be at least four recognizable trends in this area:

1. new observational and interviewing methods,
2. new dimensional structures underlying the anatomy of psychopathology,
3. new typological approaches, and
4. new developments in assessing normative aspects of patient behavior.

With regard to new observational techniques and interview methods, we have already pointed out the development of the systematic structured interview. Similar developments have occurred in the area of observation of ward behavior. These techniques have paid off in drug research because the direct observation of patient behavior in the ward through the eyes of the ward personnel (attendants and nurses) often reveals behavioral changes which go unnoticed during the interview session with the psychiatrist or psychologist. The focusing of attention on specific behaviors has another
advantage. It establishes specific targets for observation which can be repeatedly observed in time, and is essentially the basis of behavioral diagnosis following Kanfer and Saslow.

The object of behavioral diagnosis, like that of conventional clinical diagnosis, is to classify patients for the purpose of prognosis and, most important, for the purpose of treatment. In terms of the latter goal, behavioral diagnosis is directly oriented toward the elimination of undesirable behavior and the instatement of reasonable or well adjusted behavior.

Behavioral diagnosis depends more than clinical diagnosis on observation of the actual behavior in the situation where it usually takes place. Although some pertinent information can be obtained by means of the clinical interview for purposes of the behavioral diagnosis, most of the information must be obtained by having observations relating to frequency of occurrence of events surrounding the behavior. Such information is not ordinarily kept track of by the patient or even by his relatives and employers. Behavioral diagnosis therefore requires that either the diagnostician himself or some other person—in the case of neurotics the patient himself, but in the case of the sicker psychotics, the family or attendants around him—observe the patient. Behavior is analyzed in terms of the response class to which it belongs, the occasion on which it is seen to occur, and the consequence of the behavior. Such an analysis allows one to determine what in the environment evokes the behavior in question and what in the environment strengthens it. For example, the voices which a schizophrenic hears might be reported by him to occur whenever he is asked to do something he does not wish to do, and the consequence of the behavior of listening to his voices may well turn
out to be that he gets out of doing the undesirable task. Another example of a behavioral analysis of schizophrenic behavior might be to determine the conditions under which a particular patient becomes violent and the consequences of that violence. Other kinds of information which are necessary for a behavioral analysis consist of the categories of behavioral deficits or excesses, of biological and social variables, determining the behavior.

No system of classification of the kind which has evolved in clinical practice has been produced in the behavioral model; behavioral diagnosis consists of categorizing only in terms of the kinds of control exerted over the questionable behavior, and such taxonomies are hard to come by. From the point of view of behavioral diagnosis it is important to find out the stimuli which currently control the behavior in question so as to be able to derive a method of eliminating or otherwise modifying the behavior. Thus, knowing that a particular consequence is produced by the undesirable behavior allows the therapist to have that consequence be contingent on more desirable behavior or to eliminate the possibility of that kind of consequence altogether. Although behavioral analysis is not directed at non-environmental etiology, its objectivity and attempts at quantification make it a rather reliable kind of description to be related to any etiology. Thus its methodology may be useful for diagnosis directed at finding physical causes as well.

To summarize, behavioral diagnosis surveys the field of behavior of a given individual to tease out special types of deviant behavior which characterize him and then, having defined these as targets, employs the special armamentarium of behavior modification to eliminate them. Perhaps, because of
its specific character, it lends itself to direct immediate testing of its efficacy more than other systems of diagnosis. By focusing on overt behavior to the exclusion of postulated internal feelings, attitudes and emotions, it becomes more directly testable. However, it is clear that even such internal behavior as ideas of grandeur could be brought within the scope of the method. If it can be established that these ideas of grandeur are based on low self-esteem, an operant conditioning approach to raising self-esteem should extinguish ideas of grandeur. By discovering ways of immediately reinforcing self esteem whenever the patient accomplishes something good, perhaps higher self esteem can be developed. It would be a mistake, however, to classify the behaviors in terms of the kinds of therapies to which they are amenable (e.g. sensitization), since therapies come and go in accordance with the Zeitgeist. But it would be possible eventually to classify the deviant behaviors in terms of situations in which they are most likely to occur and thus provide an ecological classification for deviant behavior.

Another new development in the descriptive area is the provision of information on patient behavior based on observation of relatives and important others in the patient's life. Martin Katz has pioneered in this area with some rather striking results in which the patient's behavior in the home as perceived by relatives changed for the better in the hospital in one ethnic group and for the worse in another.

With regard to the anatomy of psychopathology as determined by factor analytic approaches, though this work dates back to Father Moore in the 1930's, we have not yet reached a consensus as to final structure. The work of Wittenborn and Lorr is outstanding in this area, but the degree of agreement
between different methods and approaches, while satisfactory, is not yet high enough to consider the matter finished. One of the problems with the earlier studies is that they were based on global observations of patient behavior over unspecified periods of time gathered in a variety of unspecified ways. With the advent of systematic structured interviews it became possible to base the interview results on more objective bases and on better specified uniform time periods of observation. The first factor analyses on such interview results by Spitzer, et al., did not elicit any startlingly new dimensions. The more recent factor analysis by Fleiss on the US-UK Combined Mental State Schedule has yielded some interesting contrasts.

The combined mental state schedule used by the US-UK Bilateral Diagnostic Project consists of nearly 700 discrete items, over 480 from the Present State Examination of Wing, et al., and almost 200 from the Psychiatric Status Schedule of Spitzer, et al.

As a first step in the analysis, the items were grouped on purely clinical grounds into 185 clusters of from one to twenty items each. A varimax factor analysis was applied to the set of correlations among these 185 clusters, with rotation only of those factors whose roots were greater than two. Subsequent analyses consisted of assigning individual items to tentative factors, correlating each item with each tentative factor, and possibly reassigning items to new factors on the basis of these correlations.

A total of 25 factors was finally derived. An item was assigned to a factor only if its correlation with that factor was .45 or more, implying that at least twenty percent of the variance of an item was shared with the
sum of the other items contributing to the factor. In exceptional cases, the minimum correlation was reduced to .40 to increase the number of items contributing to a factor. To reduce the correlations among the factor scores, an additional criterion for the assignment of an item to a factor was that the square of the item's highest correlation be at least twice the square of its second highest correlation.

Each factor is scored as the unweighted sum of the responses to the items assigned to it. For scoring, all items were dichotomized into the presence or absence of the behavior described, even though some items were originally rated on a scale describing varying degrees of severity.

Results from two reliability studies are available. In one, some fifty patients were rated by an observer watching but not participating in the interview. The median degree of agreement between the interviewer and observer, as measured by the intraclass correlation coefficient, was over .80. In the second study, about thirty patients were reinterviewed by another project psychiatrist within a week of the first interview. The median intraclass correlation coefficient, now measuring the degree of repeat-interview reliability, was almost .70.

Probably the most important result of the factor analysis was the separation into two factors of depression and anxiety. This result is in contrast to the result, for example, of Lorr, who found a single factor on the IMPS -- Anxious Intropunitive ness -- combining these two dimensions, and of Spitzer, who found a single factor on the Mental Status Schedule -- Depression-Anxiety -- also combining them.
One reason for our having found a separate factor for Anxiety is that there were over 50 separate items describing various kinds and intensities of anxiety: clearly, the more items of a certain kind one has, the greater his chances of finding a separate factor containing them. This was not the only reason, however.

Almost all the items in the Anxiety factor describe the occurrence of an anxious response or its avoidance in some specific situation, whereas there are no items in this factor describing general or nonspecific feelings of anxiety. In fact, the items of the combined schedule which describe general anxiety correlated higher with the Depression than with the Anxiety factor. Perhaps a reason others have failed to find a separate Anxiety factor is that they have not surveyed in sufficient depth the many situations in which anxiety may be evoked nor the many ways in which anxiety may be manifested.

The usefulness of separating depression from anxiety has been demonstrated in a discriminant analysis of psychotic and neurotic depressives. The psychotic depressives tended, as expected, to have higher scores on the Depression factor than the neurotic depressives. On the other hand, the neurotic depressives, on the average, scored higher on the Anxiety factor than did the psychotic depressives. This occurred in spite of a positive correlation between depression and anxiety.

Other findings from the factor analysis which are being followed up are the separation of reported from observed restlessness; the separation of retarded speech from retarded movement; the separation of delusional from nondelusional grandiosity; and the separation of delusional from nondelusional suspiciousness.
Another emerging trend is that of typology. In a sense, this is
the complement of the dimensional approach, clustering individuals rather
than items. Another way of contrasting these two approaches is to regard
them as testing the assumption of continuity vs. discontinuity in the
distribution of the observed behaviors. Whether the discontinuity inheres
in the behavior of the patient himself, in the judgment of the observer
of the behavior or in the interrelationship of behavior, are important
issues which have been discussed elsewhere (Zubin, in Barton & Katz).

Whether the typological approach is to be preferred to the dimensional
approach is a pseudo-problem. They should be used simultaneously to find
more homogeneous groups of patients and having found them, then proceed to
find better clusters of items characterizing the dimensions on which the
clustering was based. In this iterative fashion we may eventually arrive
at greater homogeneity in both. The relation between such homogeneous sub-
groups and our classic diagnostic categories will be most interesting. This
audience need not be reminded, however, that ad hoc clusters based on drug
studies, like ad hoc factors based on such studies, have a very short half-
life. We need more sophistication in finding the best classificatory
system for behavior, and perhaps limiting such studies to patients only is
a major fault. The need today is to develop either new scientific models of
a substantive sort or new mathematical models with specific assumption
regarding the distributions of measures, before clustering or dimensional
analysis can make any progress (see Fleiss & Zubin, 1969).

Another emerging trend is the recognition that counting of liabilities
in patient behavior is not enough, just as looking and at the liability side
of a business ledger is not enough. We must find the assets which turn a
gifted individual, despite his psychopathological liabilities into a
creative leader. Perhaps one of the most promising leads in this direction
is the work of the Sjöbring school at Lund. The fact that the Sjöbring
dimensions seem to distinguish between psychopathology (of the lesional
variety, as they call it) and personality and that premorbid personality is
not predictive of breakdown, is a challenging finding which should make
all of us eager to learn the method and to apply it. The finding that it
does predict the occurrence of cancer is such a striking fact that it too
should challenge our curiosity.

In summary, I have selected these emerging trends because descriptive
psychopathology, like the aetiological approach, can not afford to stand still,
and is apparently teeming with life. While description is only a pre-
liminary step in the search for causes, its interaction with causal research
in an iterative way as described in the original position paper can rebound
to the benefit of both.

One of the purposes of descriptive psychopathology is to lay down a
basis for prognosis. We have already seen that premorbid personality, at
least as measured by the Sjöbring method, is not prognostic of the occurrence
of illness. Could it be predictive of outcome of illness? The only finding
that has stood the test of time in prognosis is that good premorbid
personality leads to good outcome and poor premorbid personality leads to
poor outcome. This has prompted me to assume the role of devil's advocate
and deny any value whatsoever to prognosis. I adopt the stance that
duration of mental disorders like that of physical disorders is self limited
either by death, spontaneous recovery, or in some cases by therapeutic intervention. When the illness is terminated — as it is in all cases — the patient returns to his premorbid level. If that was good to begin with, he is said to have recovered. If his premorbid level was poor, what has he got to come home to? He will not be regarded as recovered even if the disease is finished and he may have a recurrence of another episode because of his inability to adjust. In other words, all illnesses have a good outcome, but the evaluation of the "cured" patient is dependent on his premorbid status. From this point of view, therapy takes a new stance. It has to study the premorbid status very carefully and try to develop means for altering the premorbid personality for the better. This should be the goal of all therapy.

Cross Cultural Trends

Regarding the emerging trend in cross-cultural studies, it is interesting to note that one of the barriers to such studies is beginning to fall. This barrier has consisted of the absence of any taxonomic approach to the factors that underlie the ecological forces shaping the niches men occupy. Our description of the ecological surround in terms of socio-economic status, educational level, occupation, overcrowding, ethnic origin, poverty, and other socio-cultural variables has not paid off. We are beginning to analyze these gross ecological features into more meaningful components. Thus, even within the same socio-economic class, the structural interconnectedness of family members, neighborhood members and even random gatherings may differ considerably and the amount of communication within such groups may vary.

Dr. Henner has begun to examine the network of interconnectedness in small neighborhood groups in New York, Vermont and London and has applied the
-11-
close technique to measure the degree of communicability in such small groups. The role of community disorganization as measured by Leighton, the role of isolation as measured by Bennett, and the role of persuasibility as measured by Hovland and associates.
dimension is separated from the depression dimension; (4) new typological approaches which, though still in their infancy, can serve in an iterative way to purify clustering of items by applying them to clusters of people and vice versa; (5) new approaches to the assessment of behavior independently of the assessment of psychopathology.

One may ask: Descriptive Psychopathology — who needs it? Isn't it sufficient to treat each patient as a unique individual and in this way eliminate the need for classification? The answer is that we need better description leading to better classification because we now have such a wide armamentarium of effective therapies that it will soon be regarded as malpractice of the severest type to blandly mislabel a depressed patient as hebephrenic and thereby deprive him of the opportunity to benefit from anti-depressants or to mislabel a manic as catatonic and thereby deprive him of lithium.

It may be argued rather cogently that the use of diagnosis to help in prognosis is a vain hope since the prognosis for all mental disorders is essentially good. The only reason why some patients still look sick even after their illness has left them is that they have returned to the poor premorbid state of functioning which characterized them before the illness developed. A person who was a misfit before becoming schizophrenic or neurotic, or for that matter, tuberculous, does not appear any better even when the acute phase of the disorder is gone. In his case, only an improvement of his premorbid personality can lead to the kind of adjustment which would permit him to be considered no longer ill.

In the cross-cultural field, a more systematic analysis of ecological forces is emerging which can analyze the currently used gross taxonomic
factors into more sensitive components by observing the network of interconnectedness and the degree of mutual comprehensibility in the dyads of the network, the degree of cohesiveness in the community.