Epidemiological Aspects of Prognosis in Mental Illness

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The juxtaposition of such an individual-centered term as prognosis with such a group-centered term as epidemiological requires, to say the least, some explanation. This is especially true when these two strange bedfellows are found together in the bedstead of mental illness. Epidemiological investigations in the mental disorders are in a very early stage of development, and prognoses in mental illness are very hazardous. Any attempt at providing an epidemiological model for prognosis may therefore be premature.

Until recently, epidemiologists of mental illness were chiefly concerned with etiology. Such factors as socio-economic status, employment, mobility, type of residential neighborhood, national origins, paranatal influences, early development, home environment, parental attitudes, and familial incidence were examined for their etiological import. But their significance for outcome of illness has not been stressed in the past. Recent experience in the care of the mentally ill in England and elsewhere has placed a new emphasis on the importance of epidemiological factors for outcome. If we adopt a psychogenic model for mental disorders,

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the type of environment to which a patient is returned is the most important factor in his rehabilitation. In terms of a biogenic model, the environment is critical because the advent of new stresses may be fatal to recovery. Even those who deny that we can cure mental illness admit that a more favorable environment will permit patients to remain in the community who otherwise would require hospitalization. In fact, the attitude of the patient and of the community toward mental illness is such a potent factor in outcome that it is reminiscent of a story about two members of parliament in a legendary country in which the attitude toward mental illness is reported to be so benign that the population has accepted hospitalization for mental illness on the same terms as hospitalization for physical illness. One M.P. in the heat of debate, told his opponent, "What you say doesn't make sense. You must be crazy!" Whereupon his interlocutor pulled a document from his breast pocket and pointing to it said, "Here is a certificate from Hospital X declaring me sane. Can you match it?"

In this paper we shall limit ourselves to the contribution of epidemiology to prediction of outcome of mental illness. It is quite fitting for this paper to be read at Kraepelin's centennial since it was he who introduced prognosis and follow-up studies into psychiatry. While his attempt to base diagnosis on outcome alone may be open to question, his heroic tour de force did much to establish order where chaos reigned. While keeping his eye on eventual outcome, he was able to disentangle the nondeteriorating from the deteriorating psychoses, and in the latter he was able to distinguish the course of organic deterioration from that of functional deterioration. Such accomplishments loom large when compared with the confusion which preceded Kraepelin, and which might recur if we discarded his system completely. The importance of prognosis, which Kraepelin urgently insisted on, remains undiminished today. The chief aim of this paper is to introduce greater objectivity in the prognostic approach.

Most clinical prognoses are purely impressionistic and their
overall accuracy is generally so low that it is not surprising to find many clinicians utilizing the prognostic category of “guarded” in self defense. Nevertheless, the clinical prognoses are in reality true predictions whereas the epidemiologic studies, being actuarial in nature, are usually not prognostic but hystero-gnostic, i.e., based on a retrospective evaluation of the past after the outcome of illness is already known. The relative merits of actuarial and clinical prognoses have recently been examined by Meehl (10) and by others (11). A careful review of this controversy leads perforce to the conclusion that whenever objective criteria for the evaluation of outcome are available, it is to be expected that the retrospectively validated studies of actuarians should prove superior to the clinical judgments, since the latter do not have the benefit of formalized regression equations and crystalized, mathematically formulated previous experience. However, when the criteria for evaluating outcome are not specified nor even definitely known, as is the case in mental illness at the present time, it is impossible to decide which method is superior.

But why is prognosis so difficult? First of all, it is difficult because prophesy is always difficult. Any attempt to predict runs up against the possibility that the conditions which characterized a previous observation are not duplicated in the future observations. But aside from the intrinsic difficulty of predicting, the field of mental illness presents us with a situation in which there is a lack of objective criteria for gaging improvement, nor is there any agreement on those criteria which have been provided. These criteria range from ability to withstand pressure of external events, capacity to tolerate uncertainty, deprivation, and frustration . . . to freedom from symptoms, . . . adjustment and pleasure in social and sexual life (15). It is clear that such criteria cannot constitute the objective framework required for scientific evaluation. A third difficulty stems from the fact that the disease entities themselves are ill defined and do not present us with homogeneous categories of individuals. A fourth diffi-
culty is the total absence of knowledge regarding the natural history of the illness and the absence of valid information regarding follow-up of discharged cases. A fifth source of difficulty is the failure on the part of the examiner to take into consideration the stage of the illness at which the prognosis is being made and his failure to indicate for what period of time in the future his prognosis is to come true. In the studies in this field immediate outcome after therapy bears little or no relationship to the eventual outcome on five year follow-up. A sixth source of difficulty is the potential role of the specific therapy or of the therapist as a possible factor in influencing outcome. What one therapist can apparently accomplish, another one sometimes finds it difficult to carry out. A seventh and final difficulty stems from the fact that the prognosticator very often fails to accept certain inherent deficiencies as residual effects of the disease in the same way that the physician in physical illness takes into consideration the residual effects produced by chronic physical illness even after its amelioration. Since mental illness is a chronic state, it is quite unlikely that the patient will return to his pre-morbid level as if no disease had occurred. The fact that the patient has aged during his hospital stay and the possible role that the degree of community and family acceptance plays in his final rehabilitation are important factors that need to be considered in any prognosis. The role of the community and the family should loom high in any consideration of epidemiologic factors in prognosis.

But why is prognosis so important today? Before the advent of specific therapies, prognosis was not so important because treatment was not specific, and selection of patients for type of care available did not depend on any prognostic features since there was one universal method of treating all mental patients, namely, custodial care. With the advent of the new therapies it becomes necessary to determine for each patient what particular therapy he is most likely to benefit from. As a matter of fact neglect of prognosis has brought about the unsavory result that
all therapies, no matter whether they be of the psychotherapeutic variety or the somatic variety tend to produce results on five-year follow-up which are no better than those characterizing patients receiving nonspecific therapy or even no treatment at all. Thus, although on immediate outcome there is a definite advantage for specific therapies, when an accounting of the five-year follow-up takes place, the temporary advantages, good as they are and important as they are, disappear, and there is no permanent advantage left for any of the therapies (12).

The paradox of superior immediate outcome in the face of insignificant gains in the long run requires an explanation. Three hypotheses are offered. First, the “specific” treatments are not specific to the illness. They merely serve to hasten the improvement in those who would eventually improve anyhow and bring about temporary improvement in the other cases, followed eventually by relapse. Secondly, the therapies may be effective as the short run prognoses would indicate, but in the long run outcome may be altered either by “spontaneous” recurrence of the illness, which has been temporarily ameliorated by the therapy, or by recurrence of the illness as a result of social factors, e.g., stresses on the individual from a “sick” family environment. A third possibility is that the specific therapies are truly specific for certain types of patients only and not for all patients. If we could by some means determine the therapy of choice for each patient and administer to him only the preferred therapy, the improvement rates in specific therapies for both immediate and final outcome would then rise and stay above the rates obtained under nonspecific therapy. In the first case, maintenance of medication would be the answer and in the second, removal from a destructive environment, or social work or milieu therapy with the family. The third possibility leads to the need for prognosis. It is evident then that in the short run evaluation of therapy, differential types and degrees of pathology must be taken into account, and in the long run, additionally, quality of home environment must be considered. The tremendous cost
of mental disease and the need not only for concentrating on those who get well under treatment but for identifying those who do not get well under current therapies, leads to the conclusion that prognosis is the method of choice. Moreover, in designing experiments for the evaluation of a new therapy, it is important to have some idea of the prognosis for each individual under nonspecific therapy, for only then can control groups be established which would be equivalent in expected final outcome. Only with such equivalent groups can a reliable decision be reached as to the efficacy of a particular therapy.

**Specific Problems in Epidemiological Prognosis**

There are at least three types of prognostic indicators in mental illness: the psychiatric or clinical prognoses, the psychological test prognoses, and the sociological or social work prognoses. Each of these sources has a time-honored history, and until recently there were very few attempts at evaluating the vast literature which is available in each of these areas. More recently, Dr. Charles Windle (13), a former staff member of our department, undertook a survey of psychological test prognosis in psychopathology. After examining some five hundred articles he found one hundred and eighty-three suitable for analysis with regard to the relationship between psychological test performance and outcome. The remaining 300-odd articles, although they dealt with prognosis, did not give sufficient information to enable Dr. Windle to draw any conclusions from their data. Even the 183 articles that were included failed to give pertinent information in many instances so that the analysis has to be regarded as only tentative. He concludes that although there is no dearth of prognostic studies attempting to relate psychological test performance to eventual outcome, many of the individual studies fail to demonstrate empirical justification for the conclusions drawn. Furthermore, comparisons among studies reveal little agreement among findings. Consequently all that we have left after this survey are a few plausible hunches but no verified
conclusions. With the possible exception of these few hunches that may eventually be found valid, it appears that previous research on prognosis with psychological tests can tell us only that it is not possible to discover reliable prognostic criteria without controlling variables to a much greater extent than has been customary.

The very minimal criteria that need to be provided in a study in order that it may be helpful in demonstrating the prognostic power of psychological tests are: (a) The conditions of the research must be specified. This means that the patient population must be described in all pertinent detail, the conditions of therapy must be stipulated, and objective criteria of outcome must be presented. (b) The experiment must deal with relatively homogeneous populations and conditions. If such investigation were done, we could eventually build up a composite picture, an accomplishment much less likely to result from studies in which these variables are uncontrolled. (c) Findings should be reported in terms amenable to statistical evaluation. (d) Findings must be subjected to cross validation. Since most prognostic studies are really hysterognostic, that is retrospective in nature, it is quite likely that many of the studies reported in the literature have capitalized on chance variations which are only pseudo-significant. Only a cross validation of these findings can help separate the wheat from the chaff. As Cochran and Cox (3) have shown clearly, studies in which differences are picked out after the completion of the investigation simply because they are so large, often lead to erroneous conclusions. The experimenter may think he is making a t-test on the 5% level when he is actually testing at the 13% level or the 40% level, etc. When investigators in this field recognize a need for minimal experimental and evaluative procedures they may hope to discover what, if any, prognostic power psychological tests may have.

A similar investigation of the prognostic value of background characteristics has been launched recently with the help of a grant from the Milbank Memorial Fund. Some 800 English and
foreign articles were found bearing upon this problem, and these have been analyzed item by item to determine the relevance of each characteristic for outcome. Here again it was found that the vast majority of these studies failed to give sufficient information about the nature of the patient and about the nature of the therapy and about many of the other pertinent factors, so that the results must be regarded as only tentative. Despite these shortcomings, however, the rather large number of studies permitted the emergence of certain consistencies in the findings. The prognostic indicators were divided into categories grouped under the rubrics of premorbid factors and course of illness. The studies in each category which indicated good or poor prognosis for hospitalized schizophrenics were tabulated. The categories were as follows: (1) Socio-economic characteristics, (2) personality traits, (3) physical characteristics, genetic factors, (4) disease history, (5) feelings and emotions, and (6) thought processes.

The results are shown in Table I.

The items leading to good or poor prognosis can be integrated under such well-known concepts as Langfeldt’s (8) nuclear vs. schizophreniform psychoses or process vs. reactive psychoses; but it is too early to test either one of these hypotheses with the data at hand.

It might be interesting to inquire whether each of these indicators possesses the same prognostic power for nonspecific therapy or custodial care as it does for some of the specific therapies. It was found that in the vast majority of instances the patient who possessed characteristics prognostic of good outcome or of poor outcome under nonspecific therapy, achieved similar good or poor outcome regardless of the specific therapy applied. For a small proportion of the items, some 10%, the specific therapy showed an advantage while for another 10%, the specific therapy showed a disadvantage, that is, patients who would have improved under nonspecific therapy failed to do so under the specific therapy in question. In 82% of the items, the direction
of prognosis is unchanged from that expected in nonspecific therapy, regardless of the type of therapy employed. This would seem to indicate that in the vast majority of instances the specific therapies do not make any great difference. We are now engaged in investigating the background information of some 200 patients with all the alleged prognostic items included. When these data are collected, it may be possible to make pattern analyses of the constellations of prognostic indicators that may be more helpful in the evaluation of outcome than single items taken one at a time.

On the sociological level, quality of home environment also bears investigation as a prognostic factor in the outcome of schizophrenia. In the summer of 1956 a pilot study supported by the Department of Mental Hygiene was undertaken to investigate the relation of home environment to prognosis. The investigation originated as a result of an observation made in a psychological prognostic study conducted under a grant from the National Institute of Mental Health. In the latter study it was noted that several patients with a "poor" psychological test prognosis had a "good" outcome, being out of the hospital one year following admission, while several others with a "good" prognosis had a "poor" outcome. It was further noted that those who failed despite good prognosis came from rather "good" homes while those who succeeded despite a poor prognosis came from rather "poor" homes. An investigation was therefore made during the summer of 1955 \(^a\) of the social backgrounds of all the patients in the prognostic project with the aid of records of preconvalescent and convalescent home evaluations by hospital social workers. The results indicated that the ratings of the home for readiness to receive the patient, as evidenced by interest, warmth, insight, and financial security, were inversely associated with patient outcome defined as in or out of the hospital at the end of one year after admission. In other words,

\(^a\) Supported by a grant from the Columbia University Social Science Research Council.
<table>
<thead>
<tr>
<th>Prognostic Indicators</th>
<th>Number of Studies Showing Positive Prognosis</th>
<th>Number of Studies Showing Negative Prognosis</th>
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</thead>
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<tr>
<td><strong>Premorbid Factors</strong></td>
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<tr>
<td>Socio-economic characteristics</td>
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<td>Married status</td>
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<td>0</td>
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<tr>
<td>Clerical or skilled occupation</td>
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<td>0</td>
</tr>
<tr>
<td>Good deportment in school</td>
<td>0</td>
<td>3</td>
</tr>
<tr>
<td>Limited education (females only)</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Possessing an immediate family</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>(females only)</td>
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<td></td>
</tr>
<tr>
<td>Personality traits</td>
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<td></td>
</tr>
<tr>
<td>Obsessive</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Cyclothymic vs. schizothymic</td>
<td>13</td>
<td>0</td>
</tr>
<tr>
<td>Good premorbid adjustment</td>
<td>8</td>
<td>0</td>
</tr>
<tr>
<td>High intelligence</td>
<td>2</td>
<td>1</td>
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<tr>
<td>Good social adjustment (friends,</td>
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<tr>
<td>heterosexual relations—dates</td>
<td>4</td>
<td>0</td>
</tr>
<tr>
<td>Religious</td>
<td>1</td>
<td>0</td>
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<tr>
<td>Physical characteristics, genetic factors</td>
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<td></td>
</tr>
<tr>
<td>Body type</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Athletic</td>
<td>8</td>
<td>0</td>
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<tr>
<td>Pyknic</td>
<td>12</td>
<td>1</td>
</tr>
<tr>
<td>Family history of schizophrenia</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>Male sex</td>
<td>2</td>
<td>4</td>
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<tr>
<td><strong>Course of Illness</strong></td>
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<tr>
<td>Disease history</td>
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</tr>
<tr>
<td>Age at admission:</td>
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</tr>
<tr>
<td>Early</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>Specifically between 20 and 40</td>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td>Previous episodes</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>Immobility during first two years of illness</td>
<td>20</td>
<td>2</td>
</tr>
<tr>
<td>Sudden onset</td>
<td>30</td>
<td>0</td>
</tr>
<tr>
<td>Brief duration of symptoms</td>
<td>17</td>
<td>0</td>
</tr>
<tr>
<td>Psychogenic precipitating factors</td>
<td>10</td>
<td>0</td>
</tr>
<tr>
<td>Diagnosis</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Catatonic</td>
<td>9</td>
<td>0</td>
</tr>
<tr>
<td>Paranoid</td>
<td>0</td>
<td>6</td>
</tr>
<tr>
<td>Hebephrenic</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>Symptomatology</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Atypical</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>Somatic</td>
<td>1</td>
<td>0</td>
</tr>
</tbody>
</table>
### TABLE I (continued)

Prognostic Value of Background Factors for Outcome of Schizophrenia

<table>
<thead>
<tr>
<th>Prognostic Indicators</th>
<th>Number of Studies Showing Positive Prognosis</th>
<th>Number of Studies Showing Negative Prognosis</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Feelings and emotions</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Anxious</td>
<td>6</td>
<td>0</td>
</tr>
<tr>
<td>Manic depressive (i.e., depression, elation, agitation, restlessness, impulsiveness)</td>
<td>15</td>
<td>0</td>
</tr>
<tr>
<td>Dull affect (apathy)</td>
<td>0</td>
<td>7</td>
</tr>
<tr>
<td>Inappropriate affect</td>
<td>1</td>
<td>7</td>
</tr>
<tr>
<td>Hostility</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>Externally directed aggression</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>Hypochondriasis</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td><strong>Thought processes</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Delusions</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intrapunitive</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Changing</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>Of reference, of influence, persecutory, bizarre, of depersonalization, somatic</td>
<td>1</td>
<td>19</td>
</tr>
<tr>
<td>Hallucinations</td>
<td>2</td>
<td>9</td>
</tr>
<tr>
<td>Confusion, perplexity, cloudiness</td>
<td>9</td>
<td>1</td>
</tr>
<tr>
<td>Flight of ideas</td>
<td>2</td>
<td>0</td>
</tr>
<tr>
<td>Disorientations of time, place, person</td>
<td>7</td>
<td>4</td>
</tr>
</tbody>
</table>

- **Course of Illness (continued)**

Patients remaining in the hospital at the end of one year following admission tended to come from better homes than those who were out of the hospital. In an attempt to explain this finding, analyses were made of the social and psychopathological characteristics of each patient in his premorbid and morbid state. Items were assigned positive or negative values on the basis of their prognostic value as reported in the literature survey, and an index of psychopathology was constructed. It was found that the patients who were out of the hospital tended to be those whose psychopathology was less malignant and that they also tended to come from homes which were rated as “poor.” On the
other hand, the patients who were in the hospital tended to have more severe psychopathology and also tended to come from homes generally rated as “good.”

This pilot study utilized for its evaluation of home environment the unstructured preconvalescent studies of hospital social workers. Further refinement of what was meant by “good,” “fair,” or “poor” environment and more careful collection of systematic data on the problem were found necessary. A group of seven social work students at the New York School of Social Work (2) undertook this task, working with 31 of the original 51 families included in the pilot study with whom it was possible to arrange interviews. They developed a set of criteria for a “good” home which included emotional factors such as warmth, freedom to change, consistency, preservation of integrated family living without undue pressure as the result of the needs of the patient, and physical environment of the home. The criterion of outcome used was changed to number of admissions to Brooklyn State Hospital or other mental hospitals since 1953.

When the home evaluations were compared with outcome it was found that outcome in both “good” and “poor” homes was worse than outcome in “fair” homes. Moreover, the pattern of frequency and duration of rehospitalization revealed that in the “good” homes there were shorter and more frequent hospitalizations, in the “fair” homes less frequent hospitalizations, whether long or short, and in the “poor” homes there were primarily short and very frequent hospitalizations. Degree of pathology was not taken into consideration in this study, but it was hypothesized that poor outcome in the “poor” homes might reflect the environmental factor.

The small size of the sample precluded more refined statistical analysis, and it was decided to test the interrelationships of degree of pathology, defined by an index based on case history items, home environment, as evaluated by a skilled social worker in a systematically structured interview with the family and convalescent adjustment, with evaluation by a skilled social worker in a structured interview with family and patient, for the entire
sample of 230 schizophrenic patients collected since 1953 at Brooklyn State Hospital. This study is now in progress.

One of the baffling problems involved in the evaluation of outcome of illness is the selection of the criterion of outcome. For the most part, the studies reported in the literature have dealt with rather vague undefinable concepts such as "improved" or "in and out of the hospital." In an attempt to develop a more basic measure of the outcome of therapy, the stability of the patient during his posthospitalization period has been investigated. The proportion of time following release from the hospital which the patient spent in the community is computed, and this becomes an index of the stability of outcome. Some patients leave the hospital never to return and they, of course, have 100% ratings. Some patients return to the hospital several times in the course of the follow-up period for longer or shorter stays, and the proportion of time spent out of the hospital is recorded as the index of stability of outcome. We have found this index to be superior to the "in and out of the hospital" index and to ratings on improvement. In a related investigation we found that immobility during the two years following first admission is one of the better prognostic indicators of eventual outcome. By analyzing the number of months of hospitalization during the two years following first admission of a patient and dividing the number of months spent in the hospital by the number of moves made into the hospital, including the first admission as the first move, an index is obtained which may range all the way from 1 to 24. This index, designated the immobility index (4), relates very highly to outcome on 5-year, 10-year, and 15-year follow-up. Several studies, including those of Kramer et al. (6), have demonstrated that the first two years following hospitalization are crucial years and that most of the patients who stay in the hospital longer than this period are chronic incurable cases. The proportion of patients released during the first two years has varied considerably over the last thirty or forty years, but the proportion of patients who have been in the hospital two years or longer who were released has remained steady over the years.
That is why we consider the immobility index based on the first two years as a very stable index of the characteristics of the patient.

Another criterion of outcome is level of adjustment of the patient in his social roles in the family or community. A study of level of adjustment as a criterion is presently in progress. In this study, a social worker interviews patient and family at specified intervals after release from the hospital and uses a standardized interview schedule in which information is obtained regarding quality and quantity of interaction by the patient in his roles in the family, work, social, and community areas. An attempt will be made to assign weights to the items of the scale in order to derive an index of convalescent adjustment. Inasmuch as the "in hospital" status may have a variety of meanings, because of difficulties of placement, etc., another criterion now being studied is behavior while in the hospital. Behavior scales have been developed for ward attendants containing 150 statements about observable "bits" of behavior. The items are so constructed as to minimize ambiguity of meaning.

At present in a study under way at Brooklyn State Hospital, patients admitted during a 3-month period were rated on admission and rated again on the same scale 6 months after admission or just prior to release if release came before 6 months. The amount of information conveyed by each item will be determined and an index of hospital adjustment will be computed.

Perhaps the most important problem facing the biometrician in this area is that of integrating the variety of measures obtained from psychological tests, psychiatric evaluation, prognostic indicators, and evaluations of family and social environment. Instead of the conventional methods used in the past, new ones must be found employing techniques suitable for the analysis of the single individual as well as for the group. Two such techniques have been utilized: agreement score technique (14), and distance function (9).

The agreement score technique consists of dividing the distri-
bution of test scores for each test at its median and considering scores above the median as plus and those below the median as minus. The number of agreements (either double plus or minuses) between each pair of patients is then computed to constitute the agreement scores. There are several methods by which these agreement scores can be further analyzed to obtain a fractionation of the sample. If a patient agrees more closely with his own group than with any of the other groups, he is regarded as well placed in his own group. If he agrees more closely with some other group, he is regarded as belonging to that group, even though his admission status is different. In this way, the number of patients in each type of admission status who showed highest agreement with their own group, or highest agreement with any of the other groups, was determined. The agreement scores correctly locate 65.8% of the patients. In other words, of the patients whose agreement scores would place them in the "First Admission" category, 51.2% were actually so classified clinically. Similarly, of the patients whose agreement scores would place them in the "Chronic" category, 81.6% were actually so classified clinically, and for the total group 65.8% are correctly classified. Conversely, of those patients clinically classified as First Admission, 80.8% are so identified by their agreement scores, while 60.8% of the patients whose clinical classification was Chronic, were so designated by their agreement scores. The overall concordance is thus 64.6%.

Mahalanobis' distance score was also computed for these data. This measure gives the distance between pairs of individuals positioned by their test scores in a hyperspace of suitable dimensionality. The smaller the distance between any pair of individuals, the greater the similarity between them.

With the distance score as a basis of classification, 50.0% of the First Admissions are classified as clinical First Admissions, 82.5% of the Chronics are classified clinically as Chronics. On the other hand, taking clinical status as the criterion, 78.6% of the First Admissions and 57.9% of the Chronics are classified correctly.
Apparently both the distance and the likemindedness scores have a tendency to misclassify Chronics as First Admissions. Both methods also show a higher degree of concordance between clinical status and agreement score for the First Admissions than for the Chronics.

In view of the variety of approaches we have presented to the problem of prognosis, it might be well to summarize the different efforts. The factors that have prognostic value can be classified into three groups: (a) individual factors, including both psychological test results and psychiatric evaluation; (b) intra-individual factors; (c) social or cultural factors. With regard to the first group, the individual factors, we have described elsewhere (1) a schema for classifying individual behavior in terms of physiological, sensory, perceptual, psychomotor, and conceptual response tendencies. This classification has been of value for differentiating the mentally ill from the mentally well and for predicting outcome.

In connection with this attempt the following quotation is of more than passing interest:

As soon as our methodology has sufficiently proved itself through experience with healthy individuals, it would be possible to approach the actual ultimate goal of these efforts, the investigation of sick personality, especially of the inborn pathological disposition. In an investigation of many individuals we will always find some who deviate profoundly from the behavior of the vast majority in one or another aspect. If this deviation appears to be damaging to the mental life, and if it reaches a certain degree—which admittedly can only be arbitrarily determined—then we tend to regard it as an illness. Experience teaches us that persons with pathological traits of this kind are, on the whole, in greater danger of a general mental disturbance than those personalities (natures) whose characteristics are in the middle range. We therefore have first of all to investigate whether it is possible by means of psychological tests to determine individual deviations, which cannot be recognized by ordinary observation. If that succeeds, we would be in the position, through the quantitative determinations at our disposal, to establish the borderline between health and disease much more precisely and more validly than has been possible so far (7).
This was written in 1894 by a man named Emil Kraepelin. There are other individual factors based on psychiatric evaluation such as type of onset, precipitating events, course of illness, duration of hospitalization, symptomatology, which are included in the prognostic index based on the survey of the literature.

The intra-individual methods involve biochemical analysis of molecular structures. The role of genetics has recently been spotlighted in the significant pronouncement that “there is no crooked thought without a crooked molecule behind it.”

The cultural and social factors involve those characteristics that deal with development, environment, and the general social organization that goes under the name of culture. We have tried to point out that in the social-cultural area there is a host of factors involving both the patient’s feelings and attitudes toward his fellows and those of his fellows toward him, a matrix in which specific factors can be defined and their patterning and grouping into syndromes arranged so as to predict outcome of mental illness.

The implications of these methods for the institutional agencies which society has developed for dealing with the mentally ill are of considerable interest. A deeper understanding of the variables that lie behind the illness can go far toward influencing our legal approaches to the problem of insanity, our approaches to the problem of hospitalization, and lastly, our attempts both at educating the general population for preventive purposes and for dealing with the rehabilitated mentally ill.

A striking example of the importance of community attitude toward treatment of mental illness is afforded by the town of Gheel in Belgium. While the story of Gheel is well known, I would like to quote some recent impressions from Alan Gregg (5):

But nothing shows so clearly the power of the layman’s attitude toward psychiatric disease as does a visit to the town of Gheel in Belgium. There even today some 2,600 insane are billeted by the state in the families of the citizens of Gheel because of a religious vow, or at least a wonderfully tenacious tradition, in force since the
thirteenth century that the citizens of Gheel would always extend this expression of human sympathy and kindness to the insane. Today on the condition of providing a separate room for the patient, a laborer or tradesman or farmer of Gheel can have an insane person as a member of his family, paid for by the state. The low incidence of crimes of violence by or against the patients in Gheel amazes most visitors. The impression I received on a visit there drew additional flavor when I learned that scarcely any native of that town but had been tended as a child by one or another of the insane of Gheel.

Gregg concludes that apparently mental disorders are more sensitive to atmosphere, sympathy, tolerance, and quiet tenacity and faith than any other branch of health and healing.

If we adopt the psychogenic model for mental illness, epidemiological factors become an important tool in rehabilitation. If we adapt the biologic model, the epidemiological factors become important in relieving or preventing stresses that may aggravate the improved patient. If we conclude that the present treatment of mental illness is purely on an ameliorative basis, epidemiological factors become the chief source of the maintenance of the patient in the community. Thus, whichever way the die falls, epidemiological factors loom large in the welfare of the patient.

The achievements of epidemiology in warding off the effect of epidemic physical disease have been notable. But these achievements have hardly touched mental disease. In fact, if anything, the increase in survival and freedom from infectious disease has probably aggravated the problem of mental disorders. It is high time that the epidemiological approach be extended to our field to see what good it can achieve.

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DISCUSSION

Dr. Isidor Chein: This paper is an admirable and lucid exposition of the major payoff question in the field of mental disease. As I get
the question, it runs something like this: What clinical syndromes at which stages of their development and in what kinds of patients respond under what conditions in what short and long range ways to which measures administered by whom?

The question may be compared to a complicated maze, each "what," "which," and "whom" representing a many-alternated choice point, with the alternatives at each choice point being obscured by a heavy curtain of fog. The choices we must make are among the variables that may be taken into account, the factors with which we might work. There are many correct pathways through this maze, each leading to its appropriate reward, a definitive and precise prediction ratio. The problem is not merely to get to one or more reward boxes, but, in each case, to get to that reward box which is appropriate to the starting point. In other words, the correct alternative at any choice point depends not merely on where we stand at any given moment, but on how we got there and which particular reward box we are heading for.

Consider the various choice points:

1. What Clinical Syndromes. Even assuming no errors of diagnosis and further assuming that all the things assigned to one nosological category belong together more closely than they belong with elements of other nosological categories, it is still almost certain that quite different things are being put into any given category. There is, therefore, no reason to assume that the elements of a given category are homogeneously subject to the same fate. Moreover, the distribution of fates is subject to the vagaries of chance in determining the relative distributions of the different types of elements in any given category at any given time in any given clinical population. It follows that we cannot afford to take current diagnostic categories at face value, but must multiply distinctions far beyond the point that would make sense in the current era of clinical practice. The problem is what distinctions to make. Needless to say, before the search is ended, the categories will be shuffled and reshuffled to make for the optimum alignments between diagnoses and treatments.

2. At Which Stages of Their Development. At the present moment we hardly have any more reliable guides than the arbitrary measure of the duration of the disturbance and the degree of discouragement we have already experienced in trying to treat it. Again the problem before us is to place our bets on what are likely to be the distinctions with the maximum payoffs.

3. In What Kinds of Patients. Here we have some guidance from the demographic and other variables that have turned out to be
significant in epidemiological studies of the distributions of various
disturbances and in studies of short range effects of various treatments.
Alas, as a measure of how much we have learned, these turn out to
be the very variables, for the most part, that I learned to recite by rote
in my first psychology class, almost thirty years ago, variables that
need to be controlled in any experiment—age, sex, race, religious and
cultural background, socio-economic status, rural-urban residence,
education, intelligence, and initial level of the criterion variables.
Most of us have learned in the face of practical limitations to honor
them in the breach rather than in the observance. To these we may
add, in our special case, such variables as ward behavior and willing-
ness to respond to therapeutic efforts.

However we behave at each of the preceding choice points, the
cross classifications define the ultimate stratification cells—"ulstriths,"
as Toops calls them—for the elements of which we want to prog-
nosticate. The more homogeneous our ulstriths, the greater may our
confidence be that we have something sensible to prognosticate about.
The less homogeneous they are, the more we have to rely on the
gods of chance that they are not heterogeneous in such ways as to
make them unprognosticable.

4. Under What Conditions. What we need here is a theory of the
environment and of organism-environment interactions that will
direct our attention to the aspects that are relevant to our prog-
nostications. Studies by my associates and myself, for example, indi-
cate that whereas familial environmental deprivations play a role
in the etiologies of both juvenile delinquency and juvenile drug
addiction, the kinds of deprivation involved are quite different in
the two conditions, and that those addicts who are delinquents
independently of their addiction have been deprived in both respects.
Or, to take another instance: In an unpublished doctoral thesis,
Robert Lane showed that while hypothesized schizophrenogenic
family relationships did indeed discriminate between middle class
schizophrenic patients and controls, they were equally characteristic
for middle and lower class patients. They were also characteristic
to the same degree for the lower class controls. One way of interpret-
ing this finding is to assume that schizophrenogenic environmental
stresses may take different forms in the middle and lower classes. The
preassumption is that we have a much better theory of the schizo-
phrenogenic middle class environment than of the parallel lower
class environment.

What is true of etiology is presumably also true of prognosis. The
time is long since past (if there ever was such a time) when we
could afford to look upon the environment, as did the researchers on the heredity-environment problem, as though it were a unidimensional variate. But this means that we have to make relevant distinctions in the environments to which our patients return.

The way of a prophet may be hard, but it is much harder if he has to prophesy without regard to the events that intervene between his prophecy and his target date. This task is easiest if his prophecies are simply conditional—in the form, “If the intervening events are so-and-so, then the outcome will be thus-and-so, but if they are such-and-such, then the outcome will be thus-and-such.”

I am afraid that time will not permit me to review the remaining “what,” “which,” and “whom.” I should like instead to refer to one difficulty that Dr. Zubin and his associates may have underestimated to some extent. This is the fact that whichever gods constructed our maze, they built into it some complicated servomechanisms as a result of which the various choice points do not operate autonomously, i.e., what we do at one determines to some extent what we will do at another.

Dr. Zubin et al. provide us with a nice case in point, especially when I give a slightly different reading to one of their reported findings. According to them, patients coming from better homes tend to have more severe psychopathology and greater likelihood of being in the hospital one year following admission than patients from poorer homes. I assume that this does not mean that better homes produce more severe psychopathology, but rather that better homes can more readily than poorer homes carry their less severely disturbed individuals without requiring hospitalization. Curiously, Zubin and his associates do not draw the inference that it makes little sense to evaluate outcome in terms of hospitalization after a given period or in terms of number or time of hospitalizations during a given period without taking into account the differential probabilities of hospitalization for a given degree of disturbance. I take it, however, that we have here an interaction between a kind of patient variable (kind of home background) and the significance of an outcome variable (hospitalization).

Other possible interactions of our choice points are also readily indicated. Hollingshead and Redlich have reported that lower class hospital patients are not as likely as middle class patients to receive psychotherapy—an at least three-way interaction involving kind of patient, kind of treatment, and kind of therapist. Similarly, one may suspect that at least part of the social class differences in relative frequency of neuroses and psychoses found by Hollingshead and
Redlich may be accounted for by a differential readiness to diagnose a particular condition in these different kinds of patient. That this may actually happen has been neatly demonstrated experimentally in another unpublished dissertation by William Haase. Identical Rorschach records were not identically evaluated when presented with social histories indicative of lower class origin and when presented with social histories indicative of middle class origin. The lower class received less favorable trait ratings, more severe diagnostic ratings, and less favorable prognoses.

The point of calling attention to these interactions is that, if we are not alert to them, we may find ourselves with all kinds of artifacts in our data, including the most dangerous artifact of them all, the self-fulfilling prophecy.

One additional point: In our concern with successful prediction, we may overlook a major root source of knowledge on which we may base better prediction formulas—intensive study of the cases in which predictions fail and the reasons for the failure.

DR. MORTON KRAMER: I would urge Dr. Zubin to look more carefully at the statement he made about epidemiological studies, prospective and retroactive type studies. I think I can point to many studies which have taken the epidemiological approach to evaluate what happened to particular individuals with particular disease entities on a prospective basis; that is, they had to be followed to find out what happened to these individuals.

For example, the recent evaluation of polio vaccine was not a retroactive type study. Nor were studies done of tuberculous individuals to find out something about mortality of individuals who were patients with particular degrees of illness. Nor have the studies on effects of fluoride for correction of dental caries been done on a retroactive basis. They were designed to use the Kingston population to find out fully what happened to different individuals subject to different kinds of stresses. I am sure in the mental health field and also all other areas of chronic illness, you can find similar studies.

DR. PAUL HOCH: I might say something about Dr. Kramer’s discussion. I think to add confusion to confusion, Dr. Kramer was speaking about experimental studies, although prospective studies may be epidemiologic.

DR. J. ZUBIN: Dr. Chein’s disagreement with me reminds me of a story I heard not long ago. The scene is a country cottage with a long front porch. At one end sit two proverbial spinsters. At the other end, sit two traveling salesmen. They haven’t yet met. Suddenly,
a hen goes running by with a rooster in hot pursuit. Just as the rooster is about to catch up with the hen, he espies a piece of bread in the road, so he puts the brakes on and turns toward the bread. The poor hen, not knowing the chase is over, continues out into the road and gets run over by a truck.

The response of the two groups of observers to this tragic scene is as follows: The spinsters: "You see, she'd rather be dead!" The salesmen: "Boy, I hope I never get that hungry!"

Now, it all depends upon the point of view from which you are viewing the scene, and I dare say there are many things that we see in our program which may not be apparent to others. I don't see, however, any real basic differences between our points of view.

For example, take the question of homogeneity. Certainly, one patient who is classified as dementia praecox, or schizophrenic, is not the same as another one classified as such, and that is why we introduced the concept of likemindedness. We wanted to see whether we could get natural groupings in our data so as to reduce the heterogeneity. If Dr. Chein knows of a better method, I certainly would want to buy it.

The whole study arose from an attempt to find out why it was that certain people who we predicted should get well did not get well, and did not go home; while others we predicted would not get well did go home.

We were very close to the individual in our investigation, and I think we agree that epidemiological studies not supported by individual investigations of each case, certainly will get you nowhere.

Now, in the long run, our real goal is not to make predictions. We like them to come true when we do make them, but that is not our real goal; that is our assumed goal. Our real goal is to understand schizophrenia and to control it eventually. I don't know of any better way than to try to predict and then to go back and see whether we made any mistakes in our predictions. We learn from our mistakes, of course, by picking up the very cases for whom we failed to predict and by finding out what happened to divert them from the course which we thought they would take. That is perhaps the only excuse we have for engaging in this activity. Simply to play the game of prophet and prognosticator, would not really justify our efforts.

As for Dr. Kramer's question with regard to the prospective and retrospective studies, I think he will agree with me that in the field of mental health we don't know of any such studies. It is in the other areas perhaps where we have had such studies.