PART TWO: DISCUSSION II

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That advances in prognosis must, perforce, lag behind those made in diagnosis and therapy follows from the dependence of most of our foresight on previously established hindsight. Until suitable techniques for follow-up studies were developed it was impossible to know the eventual outcome of illness. Today, despite the availability of such techniques, the long, tedious collection of data required by follow-up studies is not attractive to the clinical research worker facing immediate problems. Furthermore, many technical problems that a follow-up study must face are not met in studies of immediate outcome. For this reason, there has been a dearth of follow-up studies and, by contrast, a plethora of studies of immediate outcome whose value was limited by the absence of information on the eventual outcome. Landis, in his 1937 review of follow-up studies, could find only one adequate follow-up investigation of the neuroses (Stockbridge) before 1930 and not many more adequate studies of the psychoses. During the last two decades, several more studies became available, and the present meeting has the distinction of almost doubling the available number of adequate studies.

It has already been pointed out that there are, at present, no truly prognostic studies, all of those now found in the literature being “hysterognostic” (retrospective). The reasons for this state of affairs are not difficult to fathom. Our ignorance of the basic causes, of the factors at work in treatment, of the “natural” history of the illnesses without treatment and of the attainable goals of treatment for a given individual prevent us at the present time from establishing a sound basis for prediction except in a few conditions or in a very limited tentative way. It is, therefore, a good omen to see at least three brave attempts at tackling this basic problem in the present symposium. Since the ultimate goal of diagnosis is selection of suitable therapy by means of which the patient can be improved, the prognosis for a given patient under a selected therapy becomes the single most important keystone in the structure of therapeutic effort.

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That military screening is a form of prognosis is aptly clarified by Hunt, Wittson and Hunt. Their data leave no doubt that military screening during World War II was effective in cutting down subsequent attrition attributable to mental disorders. But it must be borne in mind that, difficult as the task of screening was, it is relatively simple when contrasted with prognosis under civilian conditions. The primary question which the screening board had to answer was: Is this man suitable for training and subsequent military duty? Prediction of probable adjustment to the military situation is a much simpler type of prognosis than adjustment to every day civilian life. Furthermore, it seems likely that screening can detect those whose mental disorder is well developed and those in whom indications of incipient illness are present. The prediction of breakdown in those not yet manifesting any behavioral deviation, i.e., those who are pre-psychotic, is not yet possible, and most of the pre-psychotics who passed the screening tests but subsequently developed a psychosis in the service, probably could not now or ever be detected by present day tools. Perhaps, like other diseases, the presence of psychosis will never be detectable until its incipient signs become overtly apparent.

With the success obtained by screening-out the unfit, attention is now turned to screening-in the individuals who, despite their handicap, can be utilized in certain limited capacities. The manpower shortage has reached such proportions that we cannot afford to turn down even those who have any possibility of being of service. Heretofore, our chief concern was to determine the liabilities and handicaps a man possessed. We must now turn our attention to the assets as well as the liabilities. The particular liabilities which a person possesses may actually become an asset when combined with certain other capacities. Thus, the obsessive individual may prove to be of great value in painstaking accurate work, though he may break under the stress of situations where his obsessiveness contributes to frustration. The prediction of potential breakdown and the provision of techniques for avoiding it, either by rejection from military service in extreme cases or by securing a billet which will capitalize the individual's assets and minimize chances of breakdown, is the next step in screening.

Another area to which modern screening procedure might turn its attention is the task of screening within the mentally ill group. Having demonstrated the usefulness of brief interviews and inventories in
separating the ill from the well, can we now by means of similar
techniques separate out the various categories of illness from each
other? Such techniques would facilitate the work of the clinician and
shorten the process of diagnosis and thereby widen the opportunity
for the real work of the clinician in therapy.

The diagnostic-prognostic tripod stands on three legs: etiology, be-
havior and therapy. Of these, etiology is the weakest; therapy, though
somewhat strengthened by empirical success is not too far ahead; but
behavior, because of its phenomenological basis, is the most reliable.
Even though our hunches regarding etiology may be far-fetched and
our knowledge of therapy limited, our observational powers of the
patient’s actual behavior have been more fully developed in clinical
practice. Perhaps a quantification of patients’ behavior, by means of
tests, will permit a determination of the relationship between such
quantitative indexes and etiological factors, as well as therapeutic
outcome. It is, therefore, important to encourage quantitative psy-
chological, physiological, biochemical and other objective studies of
patients, since only by measurement can we ever hope to establish
sound diagnosis.

It should be noted that once the question of whether the candidate
is sick enough to be rejected is answered, the high degree of agreement
(94 per cent) between different teams of psychiatrists no longer holds.
On the question of what is the matter with the patient, only 33
per cent agreement is found between different diagnosticians. This is much
closer to civilian experience. Why there was such a high degree of
agreement on the diagnosis of personality disorders (74 per cent) is a
question which should be investigated further.

The 20 year follow-up study conducted by Rennie is indeed a land-
mark in prognostic studies. Although the follow up depended, to
an unspecified extent, on letters with all of their attendant dif-
ficulties, the results fall into line with other studies to such an
extent that they add validity to the general observation (dating back
to Esquirol) that approximately one-third recover, one-third improve
and one-third fail to improve. This may reflect either a natural con-
stant emanating from the biological roots of the illness, transcending
the environmental changes in stress and strain of the last 200 years,
or some psychological constant characterizing the process of subjective
human judgment when distributed over any three categories, regardless of the nature of the quality being judged.

The immediate outcome in general, despite Rennie's comment, does not seem to differ radically from the long-term outcome. The general improvement rate drops from 75.4 per cent for immediate outcome to 68.8 per cent for eventual outcome, but the proportion of "recoveries" rises slightly from 32.0 per cent to 34.2 per cent. Whether these changes are reliable or not is debatable, since no mention is made of the number dying during this 20 year period and what their disposition before death was. Apparently, we have here a study of the patients who survived for a period of approximately 20 years. If there is any correlation between survival and outcome, the results of this study would be biased to that degree. One also wonders whether the outcome and follow up would be more striking if those who were found subsequently to be suffering from malignant psychoses were excluded.

The problem of whether a neurosis can develop into a psychosis is revived by Rennie. About 13.8 per cent of the patients originally diagnosed as neurotic were later found to develop (or to be) psychotic. From this fact, Rennie infers that neuroses sometimes develop into psychoses. The reverse, however, very rarely takes place, i.e., psychotics do not become neurotics. The hypothesis that the 13 per cent neurotics who later were diagnosed psychotic were originally misdiagnosed, is as tenable as the hypothesis that these patients shifted from neurosis to a psychosis.

While the reaction-pattern type of model to explain the etiology of neurosis is an attractive one, it does not lend itself as readily to experimental manipulation as the disease-entity model with the potential physiological, biochemical and genetic factors that enter into the postulation of disease models. If one regards the problem, not as a search for basic etiology, but as a search for serviceable models leading to further knowledge about etiology it is the opinion of this discussant that the disease model is to be preferred.

The alternation between somatic and mental deviation, which is so often observed in neurotics and psychosomatic conditions, leads one to wonder whether it is not possible that the disappearance of the somatic symptom may not be accompanied (perhaps only incidentally) by an ACTH like secretion. We already know that excess ACTH
may give rise to mental symptoms. Is it possible that the relief of somatic symptoms is brought about by the secretion of a similar biochemical substance which, when produced to an excess, gives rise to the mental symptoms. It has been suggested that postpartum psychoses, for example, may have this origin.

Clow's study illustrates the care that must be taken in designing a suitable experimental design for the evaluation of the outcome of therapy. The prognosis itself cannot be made for an unspecified time but must be related to the period of follow up. Thus, some patients whose prognosis for immediate outcome is poor may have a good prognosis for long term follow up and vice versa. Furthermore, the point in the disease process at which the prognosis is made, is important. Criteria for prognosis of early cases are not the same as those which are to be applied in the prognosis of chronic cases. Some of the tests used in the Columbia-Greystone I study proved to have diametrically opposite significance for chronic as opposed to early cases of illness.

The fact that patients with positive family history sometimes have a better outcome than patients with negative family history is very intriguing. There are at least two possible explanations, one based on environmental considerations and the other on genetic considerations. The environmental argument is based on the greater tolerance for mental quirks that is usually found in families in which there are several instances of mental deviation. This is also found to be true of families in which there are several instances of physical defect. The genetic argument takes, as its point of departure, the probabilistic nature of genetic factors. A family with a wider prevalence of illness usually has a lesser degree of severity of illness, since, in the long run, families with severe illnesses occurring during the reproductive period of life tend to have rather low reproduction rates and tend to die out. The occurrence of a case of schizophrenia in an otherwise healthy stock might be the result of the rather rare meeting of two recessive genes which would tend to have a greater probability of giving rise to a severe degree of illness.

The role of education in prognosis is a baffling one. For early cases education is usually positively related to outcome. In chronic conditions, however, a high educational level often militates against improvement. Whether higher educational level brings with it less toler-
ance for personal mental deviation on the part of the patient as well as on the part of his usually higher level family or whether the occurrence of the illness despite the apparent highly developed personality of the patient (as evidenced by his educational achievements) indicates a severer type of illness is difficult to determine at the present stage of our knowledge. One gains the impression from Dr. Clow's paper that there is a tremendous need for better landmarks in the evaluation of the goals and expectancies of modern therapies and a need for confirmatory studies in which actual predictions are made and then followed up in the course of time.

The fact that both Clow and Rennie seem to find little if any difference between the various therapies applied to the different samples of patients raises once more the issue of what is the effective agent in therapy. Is there a factor common to all the therapies, and if so, what is its nature? Only a study of the natural history of mental disorders on a group to which only good custodial care is afforded can yield a definitive answer. Without such a baseline, we can never be sure whether a given therapy does as well as, better, or poorer than what spontaneous improvement would bring about.

Dr. Oberndorff's very sagacious analysis of the changes with time in the elusive quality of "skill in living" which characterizes sound mental health is a most stimulating contribution to the problem. One wonders whether those who found themselves lacking in this skill invariably went to a psychiatrist in days gone by and whether they even do so currently. Furthermore, it is interesting to speculate on whether the number of such individuals, relative to the general population, is increasing. There are certainly greater anxiety-producing factors in our times and perhaps the institutions for relieving such tendencies—the church, the family, etc.—are not as influential as they once were. How those who eventually become skilled in the art of living learn the technique of overcoming these hazards is an interesting question. Is it a highly individual process, or can education contribute to it? Furthermore, if skill in living is learned, the effective agent in such psychotherapy must be education. Perhaps the first fact of importance in education is that individual differences exist in nearly all aspects of life and that they place limits on potential development. Though few of us, if any, live up to our limits, no one can ever transcend them. The law of effect and of the reinforcing value of a satisfying
experience are some of the principles with which the expert educator deals. Unfortunately, most learning theory today is devoted to the intellectual or cognitive part of life. Few if any investigations have been carried out on the learning principles involved in affective or volitional behavior. Perhaps there is less difference than is now supposed between these types of learning, and the principles already developed in the cognitive field can be transferred to the field of emotion and will. But only experimentally based findings can be of help in answering this moot question.