EVALUATION OF THERAPEUTIC OUTCOME IN MENTAL DISORDERS*

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It is generally agreed that the group of patients undergoing a given therapy cannot alone serve as a criterion for the evaluation of that therapy. Before concluding that a given therapeutic procedure is effective, it must be contrasted with the expected outcome in a comparable group of untreated cases. To be truly comparable, the two contrasted groups would have to consist of matched identical twins, a twin of each pair being placed in the treated and his co-twin in the untreated group. Since such experimental designs are generally impossible, criteria must be sought which render groups at least approximately comparable. Many workers agree that the minimum essentials for the comparability of two patients are: (a) age of onset; (b) duration of disease, (c) sex, and (d) diagnosis. At least one scientist, Penrose, does not include diagnosis as an essential criterion and several authorities require comparability with regard to symptoms, premorbid characteristics, occupation, racial descent, and a variety of other factors.

Because of the complexity of the problems that one must face in considering outcome of therapy of mental disorders, it might prove profitable to survey the methods used for evaluating therapy in other chronic diseases, such as cancer. The parallel problems involved in evaluation of therapy in both cancer and mental disease can be seen in a classic contribution published a quarter of a century ago by Professor Major Greenwood (17).

The most desirable outcome of therapy—recovery or cure—was found by Greenwood to be as difficult to define in cancer as we find it in schizophrenia. The difficulty apparently arises from the fact that

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the words "recovery" and "cure" are terms borrowed from the acute
diseases, where it is simple to determine whether a cure has occurred
or not. In appendicitis or strangulated hernia, had the operation not
been performed in time, the patient would have most likely died within
a few days. After the performance of the operation, the expectation of
further life in the patient is quite similar to that of the general popula-
tion of the same age, sex, and occupation. The definition of cure here
requires no further discussion. In a chronic disease such as pulmonary
tuberculosis, the patient will not usually die within a week if he does
not receive treatment immediately. Life may be prolonged for months
and years. What is to be the definition of "cure"? Suppose the symp-
toms are relieved, and the bacilli have disappeared from the sputum. Is
this a cure? If an examination of the after-histories of such patients
shows that on the average they have lived as long as a random sample
of persons of the same age, sex, occupation, etc., nobody could object to
the use of the word "cure." Follow-up studies of such patients indicate,
however, that their life expectancy is much shortened when compared
to the general population. Actually the death rate in those discharged
as much improved is at least seven times as high as in the general
population. It follows that on the average "cure" in tuberculosis can
never, under present conditions, be as good as "cure" in acute disease.
In the case of cancer, some surgeons define a cure as the survival with-
out signs of local recurrence or metastasis within three to five years
after operation. Greenwood suggested that a numerical measure of
cure must take account of three quantities: (a) the average duration
of life of a person of the same age as the patient, not known to be
suffering from the disease; (b) the average duration of the life of a
person suffering from the disease and left untreated; (c) the average
duration of life of a person suffering from the disease and treated in
the manner of the treatment under investigation. If a therapy in ques-
tion has any advantage in terms of survival of the treated over the
untreated, it must meet these three criteria.

The first problem that the cancer specialist meets is that of deter-
mining the time of onset of the disease. Even if the organs of the body
were continuously open to inspection, the exact moment when the dis-
ease begins could not be determined. The only criteria of onset, both
physical and subjective, in many types of cancer are applied not by an
expert but by the patient. In some conditions such as cancer of the
tongue or breast, something more tangible is available for determining
onset of the disease. By comparing the life span of patients who are
treated within a certain period from the day of onset with the life span
of comparable individuals not suffering from cancer, an evaluation of
he effectiveness of a given therapy can be reached. Thus, the normal expectation of life of a woman aged 55 is 18.87 years; the expectation for untreated cancer of the breast beginning at this age is 3.25 years. The expectation of a woman operated on under the best conditions is 12.93 years, giving her an advantage of 9.68 years of life or 51 per cent points increased longevity over the untreated case.

Even in cases where the life span is unaltered by the operation, certain advantages may accrue to the operated case. In addition to the survival rate after operation there are at least two more factors to be considered; the comfort of the patient, and the comfort of his family. While the survival rate can be readily determined, the comfort factor is hard to evaluate. Thus Daland and his collaborators (6) have demonstrated that the duration of life from the onset of symptoms to death in cases of cancers of the rectum treated with colostomy is no different from that of untreated cases. However, the comfort of the treated patients and their families is, of course, a significant factor in making the operation desirable.

When we try to apply the criteria for onset and for recovery and improvement to the field of mental diseases we immediately meet with several difficulties. It was difficult enough to transfer these concepts from the field of the acute physical diseases to that of the chronic physical diseases. When the next step is taken of making the transfer to the field of the mental diseases a variety of difficulties arises. These difficulties stem from three basic sources: (a) the essential nature and cause of mental disease is still unknown in the majority of cases; (b) there is often disagreement of opinion among those qualified to know, concerning even the broadest designations of mental disease, e.g., somatogenic versus psychogenic conditions; (c) there is no uniformity of opinion with respect to usage of such terms as "cured," "recovered," "improved," etc. (28).

In the case of physical disorders the goal of therapy is usually self-evident. The corresponding goal in mental disorder is more difficult to define since we do not know the level to which the patient is to be returned. Should it be to his premorbid level; should it be to the level which he is now capable of attaining when one takes into consideration the disease process and its after-effects; or should it be to the average level of the general population who never had the illness? The following criteria have been proposed by various workers for gauging the goals of therapy: (a) Ability to withstand pressure of external events; (b) capacity to tolerate uncertainty, deprivation and frustration; (c) absence of blindly compulsive activity; (d) freedom from symptoms; (e) increased productivity; (f) adjustment and pleasure in social and
in sexual life; (g) good interpersonal relationship and sufficient insight. It is clear that such criteria cannot constitute the objective framework required for scientific evaluation.

We have already pointed out that in the case of cancer, one of the primary questions to be considered in evaluating therapy is the time of the beginning or onset of disease. Difficult as its determination is in cancer, it is still more difficult to establish in mental disorders. But the corresponding question regarding termination of the disease, although relatively simple in cancer, is quite difficult to establish in mental disorders. In addition to the usual clinical procedures for determining whether the patient is recovered or improved, patients in a limited number of cases end therapy against medical advice either by "elopement" from the hospital or by ceasing to visit the clinic or physician's office while still sick. Not all patients whose therapy terminates in this way come to a bad end. In at least one type of therapy—psychoanalysis—the matter of termination is incorporated into the therapeutic procedure itself. The noncomparability of the end-point in the various types of therapies and various types of patients renders comparison of data hazardous. Accepting this hazard, however, let us examine the various methods for evaluating outcome. The method of survival utilized in cancer and tuberculosis is not strictly applicable to mental disorders. To be sure the hospitalized mentally ill as a group do not live as long as their normal peers. The death rate for all the hospitalized mentally ill is about eight times as high as the death rate for the corresponding normal population, and varies from twice as high for diseases of "psychogenic" origin to 12 times as high for general paresis. Thus the mere fact of having had a mental episode seems on the average to militate against normal longevity.

But we cannot determine whether successfully treated patients achieve a longer life span than their untreated colleagues, because no data are available on the survival rate of the discharged treated cases. There are some incidental data in a few follow-up studies which yield a smaller mortality percentage of the treated than the untreated—in a five to 10 year follow-up study by Finiefs (11) and the short follow-up study of Malzberg (30)—but such data must be analyzed with regard to age, sex, etc., before they may be regarded as conclusive evidence that treated patients survive their untreated colleagues. Such a breakdown of the data is unfortunately not available. Furthermore, mere prolongation of life without alleviation of mental symptoms is hardly a goal that would be acceptable.

In order to evolve a method for determining the actual outcome of therapy aside from the survival aspect we need more definite measures
come in mental disease. Let us now examine the various studies in the literature which have dealt with the problem of evaluating the outcome of psychiatric therapy.

The data provided by the early records of state hospitals and similar institutions provide information on the outcome of non-specific therapies during the first two decades of the present century. If we define spontaneous improvement as recovery from a disease without the application of a planned therapeutic method or for reasons unknown and at the time undiscoverable so far as the physician is concerned (28), we might regard the data provided by the annual reports previous to the discovery of the shock therapies as good bases for determining "spontaneous" improvement rates. One word of explanation is required here. It is not our intention to intimate that the treatment given mental patients before 1930 was always unplanned or not calculated to bring about improvement. We do think that the statistical reports of that period can provide a first approximation to a base line against which the newer types of therapies may be more adequately evaluated. In other words, before a new type of therapy is accepted, be it a somatic therapy or a psychotherapy, it should be demonstrated to do at least as well as the pre-1930 type of treatment. The criteria for improvement which generally obtained before 1930 were as follows: "Recovered" indicated patients who had regained their normal mental health so that they might be considered as having practically the same mental condition as they had previous to the onset of the psychosis. "Improved" referred to patients who had shown any degree of mental gain short of recovery, while "Unimproved" referred to patients who had shown no mental gain. Discounting the studies which dealt with immediate outcome, without follow-up, we have a few studies which had long term follow-up. Perhaps the best known is that of Fuller (13) which gives a 15 year follow-up of first admissions admitted to the civil state hospitals of New York between 1909 and 1928 and their subsequent history in terms of discharge and death. What this means with regard to the complete accounting for all patients is somewhat doubtful since there may have been a small number of patients who were readmitted to other states or into private institutions, whose whereabouts might not have been traced. The data covered the period from October 1, 1909 to June 30, 1928, and consisted of 7,200 patients equally divided into 1,200 in each of the following six categories: manic-depressive psychosis, dementia praecox, senile dementia, psychosis with cerebral arteriosclerosis, alcoholic psychoses, and general paralysis. In addition, 2,400 successive admissions, regardless of diagnosis, were followed up in the same way. By the end of the 15 year
of the improvement in "comfort" to both the patient and the family. These may be put in terms of the criteria mentioned above. Theoretically, we may regard the patient as improved when he returns to his premorbid occupation and continues in it as if no interruption had occurred, pursuing all his goals in life in a manner no different from that of his peers in age, sex, occupational, economic, and social level, etc. Since at present an evaluation of these factors is highly subjective, dependent on clinical judgment and intuition rather than on objectively demonstrable factors, it will be difficult to establish such evaluation on a firm footing. Furthermore this evaluation is based primarily on the judgment of individuals who are closely involved in the outcome: the patient, his family, and his therapist. To say that these individuals are at best somewhat prejudiced either positively or negatively is a sheer understatement.

What can one do in an attempt to establish a more objective system of evaluation? Ideally, it might be well to base the judgment with regard to outcome of therapy on rating scales which would evaluate the pretreatment status and the post treatment status of an individual to determine the degree of improvement that he exhibits after the given treatment. Such ratings have indeed been proposed by Malamud, Wittman, and Wittenborn, among others. Unfortunately these ratings can cover only certain aspects of behavior and these only in an atomistic way. There is much need of research in determining the type of rating to be made, how it is to be made, and how to combine many ratings so as to get an adequate picture of the individual patient both before and after treatment.

In the recent series of topectomy studies three new attempts at rating have been made: (a) a rating of social improvement made by Heath (19); (b) a rating with regard to the malignancy of the condition before treatment and after treatment made by Hoch (21); and (c) a series of anchored rating scales on a specially devised controlled interview by Garrison (14). Before such instruments can become really useful in evaluating therapy they will have to be reduced to the common denominator of "Basic English" so that it will be possible to make comparisons between the reports of different individuals who have used the same therapy. Secondly, these rating scales will have to be anchored so that the use of terms such as "improved" will have an exact and precise meaning. Until these scales are more nearly perfected and attain a wide acceptance and use, we shall have to depend on such unsatisfactory types of evaluations, as being paroled or not paroled, or being in or out of the hospital after a given therapy. These last two are the only comparable criteria at present available for evaluating out-
follow-up, 41 per cent of all the patients fell into the category of discharged and not readmitted. That means that of all the psychoses about 41 per cent were improved and stayed improved at the end of 15 years. At the end of five years about 37 per cent were in this category. Since the changes from the fifth year to the fifteenth year follow-up are so small, we might perhaps take the five year figure as our point of reference because most studies of the newer therapies for which we are seeking base lines do not have follow-ups longer than five years. Furthermore, in periods longer than five years we must begin to consider the mortality rates that are normal to this group, which would complicate further analysis.

For dementia praecox, 30 per cent fall into the category of discharged and not later readmitted by the end of five years, while 35 per cent fall into that category at the end of the fifteenth year. For manic-depressive psychosis the figures are 57 and 66 per cent, respectively. Fuller's New York State figures for schizophrenia were corroborated by the five and ten year follow-up study in Rhode Island (13).

Practically the only comparable data for neuroses are derived from Ross's study (44) of the 1,186 mental cases who were patients in the Cassel Hospital. In this small private English hospital where neurotic patients received a variety of psychotherapy, (hypnosis, persuasion, and a modified form of analysis) 45 per cent of the cases treated were called "well" at the end of one year and an additional 25 per cent were term "improved" so that the total of 70 per cent may be said to have been ameliorated by the end of one year. At the end of three years 40 per cent were still well and 10 per cent remained improved. After five years only 34 per cent were known to be well and 6 per cent remained improved. This study was based on follow-up made mostly by questionnaires and consequently may present a more optimistic picture than would have been obtained had the total sample of patients been reached. In America no similar study is available except that of Landis (25) which showed that psychoneurotics admitted to the New York State Psychiatric Institute and Hospital present data similar to that of the Cassel Hospital. Landis carefully excluded from his data all cases that received intensive psychoanalysis. It may be concluded that without any specific therapies there seems to be an expectancy of something like 40 per cent improved and not later readmitted during five years for unselected psychotics and a similar percentage for psychoneurotics who received the various types of psychotherapy, exclusive of intensive psychoanalysis.

Before turning to analysis of the effectiveness of specific therapies,
let us inquire into the over-all effect of therapeutic effort on general incidence of mental disease and its prevalence. An analysis of the data for the last half of the 19th century in Massachusetts by Goldhamer and Marshall (16) indicated that the incidence of mental disease 50 years ago, in the age groups below 50, is no different from what it is today. Whether the increase in the older age groups represents a real increase, or only an apparent one, reflecting an increased tendency to hospitalize this age group, cannot now be determined. From this we may conclude that within the last century there has been no basic change in the incidence of mental disorders, at least not enough to reduce the incidence of hospitalized mental disease in the population. Goldhamer and Marshall stated that there seemed to have been many more severely disturbed psychotic cases admitted in the last century than at the present time. This may reflect the present day tendency for earlier hospitalization but does not necessarily indicate a change in the hospitalization rate.

Have the improvement rates changed considerably in the last hundred years? It is difficult to make basic evaluations of data collected in different centuries by different methods. Considering discharged patients as at least improved, we note that the Royal Edinburgh Hospital has a ratio of discharges to admissions ranging from 32 to 38 per cent during the period 1813-1934, with a more recent period (1904-1934) showing a rate of 32 per cent.

Similar data are provided by Penrose (40) on the Ontario Hospitals. From 1882-1921 the discharge rate per 100 admissions was quite constant, hovering around 45. From 1922-1938 it rose steadily to 60, probably as a result of wider application of therapy to general paresis. From 1938, with the introduction of shock therapy, it rose to 70 in 1940 and declined again to 60 in 1944. Furthermore, the improvement rates as measured by this index are quite similar numerically in most European countries. Since 80 to 90 per cent of all discharges occur during the first year of hospitalization, the above mentioned ratio is a defensible though not perfect method for estimating outcome. We may conclude, then, that neither first admission rates nor improvement rates varied significantly during the last century.

What has been the impact of modern therapies on the discharge rate? In reviewing the results of shock therapy and modern developments in psychotherapy, against the background of earlier rates of "spontaneous" improvement, we must bear in mind a series of important qualifications. First, the samples of patients treated by these various new methods are not chosen at random from the general patient population. A certain amount of selection occurs with regard to admission
of patients in many institutions and with regard to the selection of patients in a given institution for the various types of therapies. It is well known that the state hospital population is not as highly selected as is the private hospital population. Consequently, even a random sample of state hospital schizophrenics, for example, is likely to be made up of a group of individuals which is quite different from a similar number drawn from a private mental hospital. Patients who are treated in the clinic present different characteristics from those treated in resident hospitals, and psychiatric patients who are treated by psychiatrists in private practice, or by general practitioners, certainly are not of the same variety as those who are hospitalized. Wholesale comparisons of such diverse groups against the base lines provided by the Fuller study, for example, are untrustworthy. Second, a considerable number of people who are mentally ill take their troubles to other than medically recognized therapeutic agencies, so that the number of people whose complaints are relieved by such methods as are afforded by Christian Science, faith healing, astrology, and the like, is also a factor to be borne in mind. If there has been a shift from these non-medical treatments to medical, improvement rates would be expected to change. In other words, the problem of determining whether the data are comparable from one situation to another is a difficult one, since the samples of treated patients vary in nature and distribution with regard to severity, age, etc.

Third, the spontaneous improvement rate to be expected regardless of the effectiveness of the therapy under examination is to be borne in mind. The spontaneous improvement rate, does not represent the zero point of therapeutic effort. A therapy that does not reach the spontaneous level described above, may still be effective above the level of no therapy in individual cases. Fourth, the degree of expectation raised by therapy in the mind of the patient, the attitude of the family, and the therapist himself is an important factor. One is reminded of the study of the General Electric Company conducted with its workers at the Hawthorne plant (43). The introduction of certain improvements in illumination raised the production rate in this factory. After a short while the improved illumination was reduced to its original level. The level of production did not drop accordingly. Apparently the mere fact that the company was doing something for the workers was in itself sufficient to raise the level of production.

A fifth factor that one must take into consideration is the intercurrent diseases or accidents that sometimes bring about improvement in mental patients. A sixth, is the oscillation in the well-being of mental patients who improve apparently for no detectable reason and who
later suffer a relapse. A seventh factor is a possible change in the nature of the patient population itself. It is quite likely that with the introduction of shock therapies for acutely ill patients the number and caliber of patients coming for help may have changed somewhat in favor of the less disturbed and more incipient cases who formerly never came to the hospital for help. A comparison of the results in such cases with the average run of state hospital cases would, of course, be invalid. An eighth factor is the interval of the follow-up, since it has been demonstrated that rates of improvement and relapse depend on time. A ninth factor, in addition to the usual background variables, such as, age, sex, and type of onset, is the duration of the disease. It has been thought that duration of disease before treatment may be a factor in the improvement rate, on the theory that the longer one waits, the less are the chances of improvement. It should be pointed out that one of the necessary consequences of postponing treatment is that many of the patients who would have improved spontaneously during the long wait, do improve, and that the residue of patients who have waited two or three years for treatment consists of a differentially selected type of patient, perhaps of a different subtype of psychosis. Hence, it is well to bear in mind that comparing cases of three years’ duration with those of only one year duration is invalid not only for obvious reasons but also for the reason that the two groups of patients are probably quite different in the basic nature of their disease processes. Finally, one must bear in mind the effect of the duration of the disease on the recovered patient himself. To expect a patient who has been sick for three years, for example, to return to his premorbid level after treatment is perhaps too ambitious a program since it is likely that a severe psychosis may have left some scarring. For this reason it is necessary to compare the treated patient with the spontaneously improved patients of the same variety, instead of with his own premorbid level or with the general population.

There have been many studies varying in size and scope which have attempted to evaluate the outcome of various therapies. We have selected several of these on the basis of their experimental design, to indicate something of the nature of these evaluation studies. The first of these was conducted by Malzberg (30) for the New York State Department of Mental Hygiene on the outcome of 1,000 insulin-treated schizophrenic cases selected from several state hospitals. In each of the hospitals a comparable control group equated for sex and for type of schizophrenia was established. There was no control for age nor for duration of disease. The results of this study indicated that the immediate outcome showed an improvement rate of 65 per cent, which was
Therapeutic Outcome in Mental Illness

lowered to 49 per cent about a year later by relapses. The comparable
group of patients treated with nonspecific therapy, that is, with pri-
marily custodial care, showed an improvement rate of 22 per cent.
Whether this differential between the treated and untreated will per-
sist after five years will not be known until a follow-up study is made.

Finieis (11) reported a study of outcome of treatment in 949
schizophrenics, of whom 503 were treated with insulin and/or con-
volutive therapies while 446 received no specific treatment, so constitut-
ing a control group. In general, following the specific therapies 37.5
per cent were improved and stayed out of the hospital for at least five
years, while of the untreated group only 14.1 per cent were in that
category. When, however, the follow-up for those who had been out of
the hospital for ten years or more was considered, no great advantage
was found in the treated group. Apparently, the longer the follow-up,
the less the discrepancy between treated and untreated.

Another study by Penrose (34, 37) utilized actuarial methods in
the evaluation of the outcome of therapy. The actuarial method as used
in establishing the effects of toxins, drugs, or other therapeutic pro-
cedures is widely accepted in the field of experimental somatic therapy.
In clinical practice it is difficult to apply this method, since we do not
have standard tables of expected recovery rates based on such impor-
tant criteria as age, sex, length of illness, etc. In order to utilize con-
trols for all such factors a large group of cases would have to be
evaluated.

Penrose postulated that by controlling for sex and age and neglect-
ing diagnosis one could really equate the control group with the
treated group and get comparable results. The control group was
selected by matching for age at first admission, sex, interval between
first admissions and special treatments, and interval since such treat-
ment. Penrose’s criterion for outcome is a simple one, namely, whether
or not the patient in question is still on the books of the hospital at a
given time after treatment. By developing actuarial tables giving the
chances of any patient’s still being on the books of the hospital after a
given period subsequent to his first admission, he was able to determine,
whether the treated group had a deficit or an excess of patients on the
books after the lapse of five years. Since this study was conducted in
Ontario, Canada, it was quite possible to include almost all the patients
who had ever been in the hospital since hospital admissions are cen-
tralized in this province. There are, nevertheless, several difficulties
attached to this method. First, Penrose discarded diagnostic categories
entirely and matched his groups on the basis of age and sex, and dura-
tion of illness. He argues plausibly that age and sex are sufficient indi-
cation of the type of diagnosis which will be used for a large group, particularly since there are established age norms for the various diagnostic categories. The possible inclusion in the control group of the small number of epileptics and feeble-minded in the younger age groups and of seniles in the older, however, introduces certain difficulties in evaluation. But a more important difficulty arises from the fact that only those that were on the books of the hospital were available for evaluation. Whether the patients out of the hospital were alive or dead, was not known to Penrose. If, however, the patients out of the hospital were to have a higher mortality rate than the patients in the hospital, a speciously favorable effect for the therapy would be produced since the dead patients could not reappear on the books of the hospital. In view of this, his conclusions would err on the optimistic side. On the basis of this study Penrose concluded that after a five year follow-up there is no advantage in electric shock treatment and insulin treatment over the expected improvement rate in untreated cases. As far as leukotomy is concerned, the data are not too clear yet. In other words, spontaneous improvement is just as capable of releasing patients from the hospital as the special therapies that are now in use. It should be pointed out, however, that even in Penrose's conclusions, pessimistic as they are, the advantage lay with the special therapies, since the patients receiving such treatments were discharged from the hospital earlier, so providing a saving to the hospital in addition to the relief of human misery which the treatments effected.

Freudenberg (12) criticized these results by pointing out that the diagnostic grouping based simply on age and sex would include many defectives, seniles, and alcoholics in the control group who would ordinarily not receive shock therapy since shock therapy would be unlikely to produce any good results in such individuals. Whether the presence of alcoholics, who may have generally a high discharge rate, tended to raise the rate of discharge despite the fact that defectives and seniles who generally have a low discharge rate were also included, is debatable. At all events the fact that the control group is not a pure control is an important consideration in any critical evaluation of Penrose's thesis.

In a further discussion of Penrose's paper, Mayer-Gross (31) reported a study of 50 cases treated in 1940 and 1941 with a three year follow-up of voluntary patients at the Maudsley Hospital. This group was controlled for age, sex, diagnosis, and duration of illness. The insulin treated group was consistently ahead throughout this entire period and remained so at the end of the 36 months. At the end of
this three year follow-up only 8 per cent of the treated cases were still in the hospital whereas 31 per cent of the untreated cases were still in the hospital. What a five year follow-up of these patients will show remains to be seen.

A recent study by Karagulla (25) which compared the results of ECT in depressive states with those obtained by more conservative methods (sedation, psychotherapy, occupational therapy) indicates that “the percentage rate of recovery does not vary greatly whether patients are treated conservatively or with electric convulsion therapy. Such slight differences as may exist are statistically insignificant.” Furthermore, this author failed to find any evidence for the generally assumed hypothesis that ECT decreases duration and prevents recurrence of depression. He also stated that though a greater proportion of cases are discharged as “recovered” in the treated group they have a higher incidence of relapse than similar cases in the control group.

The results from most other studies evaluating psychiatric therapy are quite contradictory and confusing. One special factor should be mentioned. When a rather small hospital or medium-sized hospital conducts a survey, the patients with a poor prognosis in such hospitals tend to be placed in the control group and thus increase the discrepancy between the improvement rate in the treated and untreated cases.

Attempts have been made by Wilder (48) to summarize the data on outcome in psychotherapy up to 1946. For psychoneurotics treated in general hospitals, between 60 and 70 per cent were found to be recovered or improved after three to four years from time of discharge. For out-patient clinics, the figures are a little lower, namely, 50 to 60 per cent after three years. For nonpsychiatric clinics, the rate of improvement varies with the length and duration of treatment and seems to average about 30 to 40 per cent.

With regard to outcome of psychoanalysis it should be realized that data for this therapy are not comparable with those of other therapies, since the cases are highly selected on such bases as economic status, intellectual level, and suitability to the analyst. Furthermore, a problem arises with regard to the discontinued treatment cases. If these are included, the per cent cured and improved ranges from 50 to 60, while if these are not included, the rate is closer to 90 per cent. These data are for clinics. For psychoanalysis under the conditions of private practice, no data are available, but the report on 30 referrals to psychoanalysts made by Kessel and Hyman (26) indicates an improvement rate (cured and improved) of 60 per cent. Some analysts unofficially place their improvement rates at a much lower figure.
informal estimate made unofficially ranges from 11 to 25 per cent cures and an additional 50 per cent improved. Whether these unofficial estimates include discontinued treatment cases is not known.

For psychiatrists in private practice who do not utilize the full psychoanalytic approach, but use many of its concepts, the results after a three year follow-up are said to be about 50 per cent improvement (48).

The most challenging study in recent years is that of Denker (9) who reports the outcome in 500 consecutive cases, taken from the files of the Equitable Life Assurance Society, of persons claiming complete disability due to psychoneurosis. These were patients who had been ill at least three months before claim was submitted and had been totally disabled for at least that period. The treatment given these patients consisted of being regularly seen by their family physicians who utilized sedatives, tonics, suggestion, and reassurance. By the end of the first year 44.6 per cent had recovered sufficiently to return to work and have their disability payments cease. By the end of the second year 71 per cent were in that category; by the third year, 81 per cent; by the fourth, 86 per cent; and by the end of the fifth, 90 per cent. Thus at the end of five years only 10 per cent were still disabled. These data compare favorably with the best results from psychotherapeutic procedures for psychoneurotics, only here nothing approaching psychotherapy in the technical sense was employed.

Since there has been a tremendous growth in the utilization of personal counseling services, it might be well to cast an evaluative look on the degree of success which such programs attain. The Veterans Administration recently conducted a follow-up study of 393 cases treated by personal counselors (4). Each veteran in this group was rated by his training officer for the degree of improvement shown in his adjustment to his training program or his job about six months or more after the beginning of counseling. The training officer had no knowledge of what had gone on in the counseling. Of the total group 41 per cent were rated as much improved, 42 per cent as somewhat improved, and 17 per cent as not improved. No control data on uncounseled veterans are given. It is reasonable, however, to conclude that the rate of improvement exhibited by these veterans is strikingly similar to the rates of improvement obtained in the other therapies.

**Summary and Conclusions**

The present day methods for evaluating outcome of therapy in mental disorders leave much to be desired. The first difficulty comes from the fact that the data usually reported are not satisfactory for evaluation. The second essential difficulty arises from the fact that a
given therapy may not be suitable for one patient but be suitable for another. Prognostic tests are needed for forecasting the type of therapy most suitable for a given patient.

In order to make comparisons possible between the various types of therapy now in vogue, it is highly desirable that a center be established where a standard population of patients might be housed. The standard population should be specified with regard to age, sex, other background factors, symptoms, duration of disease, age at onset, and other pertinent variables. This standard population of patients would serve as the proving ground for the relative efficacy of the various types of treatment now in use. In various centers of the country specific varieties of psychotherapy, somatotherapy, or psychoanalysis could be tried out on comparable groups possessing the same characteristics as the standard population. The outcome of these therapies could then be evaluated on the basis of similar patients and a definitive statement be arrived at regarding the efficacy of each of the treatments. After a five year follow-up on such a standard population, a shift might be made to another age range or pattern of characteristics and the same procedure repeated. In this way, a series of standard studies indicating the relative merits of various types of therapies could be carried out, and the value of many of the controversial psychologic tests determined. Without such standard reference points, both the therapist and the scientist are lost on a dark sea without a compass or a chart.

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