Discussion

by JOSEPH ZUBIN, Ph.D.*

This session has dealt with the following issues of a technical nature: (1) the preparation of instruments for detecting changes in the behavior of mental patients, during and after treatment; (2) the development of designs for placebo and double-blind studies; (3) a consideration of the importance of immediate discharge versus status 5 or 10 years later; and (4) the development of statistical methods for the evaluation of therapeutic results.

It is interesting to note that each of these developments indicates that the resistance to the introduction of scientific methods in the evaluation of therapeutic outcome is decreasing. This was not the case 10 years ago when I read a paper on these topics before the Association for Research in Nervous and Mental Disease. Our present chairman, Dr. Cameron, then indicated his disagreement with such approaches by the following statement: "I should also like to comment on Dr. Zubin's paper because it is in some sense a very key matter of this whole question of experimental design ... I am going to say very frankly, that I do feel, as far as Dr. Zubin's paper is concerned, that what one sees here is an attempt, a valiant attempt, no doubt, but an attempt to transfer to our field the system of experimental design, designed experimentation, shall we say a philosophy of science, which really has its applicability in other fields altogether. I don't think we have frankly accepted the fact that our data are intensely variable and that we are dealing with emergent situations and not static situations. We are trying, therefore, to say this is how something should be in our field rather than how something is in our field in research."

I am glad to see that both Dr. Cameron and I have shifted positions with regard to this issue. I am not as vehement about introducing the most rigorous exacting methods and he has come a good step forward in accepting methods of control which are workable.

The first paper in this session, from Biometrics Research by Dr. Burdock and Mrs. Hardesty presents one of the fruits of the endeavor to develop tools for the objective evaluation of the behavior of the mental patient from the time he enters the hospital, during the course of therapy, and to be able to determine whether or not they displace psychiatric medication which provide concrete data for a more accurate time to determination of the behavior. Interpretation of these results in studying group therapy reveals that it is quite clear that the general intuitive feeling is evident in several lines of research. These tests have not been developed for use in the various therapeutic modalities, or the behavior of the patients released from the hospital, but they are couched in terms of changes in the behavior of the patient.

In order to continue the discussion of techniques for measurement of behavior in management of patients released from the hospital, and the effect of these techniques on the behavior of the patients, I incorporate all the problems and data of the course of the following.

In his scholarly article, Lehmann has made use of the classic study of the efficacy of psychotherapeutic techniques. The results of many studies about its effect. If the effect is to be determined, Lehmann's conclusion is that the main psychologic function is to improve the perceptual and cognitive capacities of individuals who are handicapped with impaired tolerance for further discussion.
of therapy, and on follow-up. These tools are in no way intended to
displace psychiatric judgment and evaluation. Their purpose is to
provide concrete reference points which can be used from time to
time to determine what objective evidence exists for any changes in
behavior. Interpretation of these data, of course, will depend upon
many other factors which the clinician has to bear in mind. However,
in studying groups of patients for the effects of certain treatments, it
is quite clear that these instruments may either support or contradict
global intuitive clinical judgments, as in fact has already become
evident in several studies. It is interesting to note that psychological
tests have not been very useful for the measurement of changes under
the various therapies. Apparently the underlying dimensions of person-
ality or the behavioral trends which these tests tap are not as crucial to
the detection of changes in behavior as are specific observational tech-
niques such as those provided by observational inventories. By the same
token the global psychiatric categories also are not as useful and unless
they are couched in behavioral terms, may not give indications of
changes in the behavior of patients after the various treatments.

In order to cope with the new release policies that now seem to be
developing in all the state hospitals, it is necessary to develop special
techniques for measuring outcome. In previous years, before the revolu-
tion in management and treatment had occurred, the proportion of pa-
tients released from the hospital was a sufficient index of the efficacy
of a therapy. Now with our revolving door policy in which patients go
out and come back again, there is need for a more appropriate mea-
sure, such as the Outcome Index that Dr. Burdock has proposed which
incorporates all movements in and out of the hospital through the
course of the follow-up period.

In his scholarly review of the literature of placebo techniques Dr.
Lehmann has made only one omission that I could detect, and that is
Jellinek’s classic study of the effect of the placebo reactor on comparisons
of the efficacy of drugs. It is interesting to inquire why it is that in so
many studies about 30 per cent of the patients usually react to placebos.
If the effect is truly random, why should it be 30 per cent? Dr. Lehm-
mann’s conclusion that individuals who manifest well integrated autono-
mic functioning show improved performance on psychomotor, percep-
tual and cognitive tasks under placebo conditions and conversely
individuals who manifest poorly integrated autonomic functioning react
with impaired test performance under placebo conditions, deserves
further discussion, particularly as the correlation between autonomic
variables and test performance involves a time parameter. I would like to suggest in the light of a variety of experimental results that the taking of a placebo induces an emotional response. If the emotional response is euphoric, involving expectancy of improvement, then the placebo will partake of the nature of an effective agent. On the other hand, should the emotion generated be either neutral or dysphoric, the placebo effect will not be there. It is well known that an emotional response has to represent the integration of both physiological components as well as conceptual components. While an emotion-like state can be engendered by injecting adrenaline, it is not a true emotional state unless the individual himself supplies a conceptual component (based on previous experience) for making it into an emotional state. By the same token the effect of a conceptual component can be reduced by blocking the physiological component of the response. This was done by Schacter et al., in a study in which in one group the conceptual component of euphoria and humor was prevented from interacting with physiological activation by injecting chlorpromazine to reduce the physiological arousal, and in another group the physiological component was facilitated by giving epinephrine to enhance the arousal. The epinephrine group, of course, laughed heartily at the funny film which was exhibited but the chlorpromazine group did not. The placebo apparently will be successful to the extent that it is accompanied by an expectation, conscious or unconscious, of improvement. A negative attitude, on the other hand, will lead to opposite effect. This is the reason why improvement occurs in variables which involve measurement of time since practice under influence of a beneficent mood will improve speed. Accuracy, since it is more dependent upon ability, may not benefit to the same extent.

In dealing with therapeutic results one is often faced with the problem of the adequacy of the clinical judgment of improvement. In several studies it has been found that although the clinician failed to see any significant signs of recovery, objective records of the behavior of the patient before and after treatment indicated definite results in favor of an active agent over and above a placebo. It must be borne in mind that better recording of observations by clinicians may turn up information which is now lost in the global approach towards evaluation.

The question of ethics, which is always raised in connection with control groups and placebo groups, seems to be a spurious one since if we knew the efficacy of the drug we would not be engaged in its investigation in the first place. Only drugs whose efficacy is in doubt are brought into a more rational alternative. While it is wise to test both new drugs as well as old through serendipity, there will be times when new drugs are developed as we move in a more rational way. Probably the selection of outcome and the clinical acumen as well as the methodology of the research workers will make it possible to judge the drug and then make it possible to judge of the therapeutic potential of the drug.

Dr. Freyhan's plea for coming as it does in the midst of a welcome relief from war and the coming of the objective evaluation of syndromes, especially disease categories, makes it possible to judge of the therapeutic potential of the drug. It would be especially pertinent and interesting if discussed by Dr. Freyhan. It is, of course, the relative merits of the long-term outcome become important morbid as well as correct as to immediate outcome. The course of the illness in the individual can also be predicted much more accurately.

A focus on target behavior is one which Dr. Freyhan emphasizes as psychotherapy. It is clear that in behavior therapies in particular substitution therapy must be more efficiently applied.
are brought into a clinical trial and for them there is no other alternative. While it is true that most of the discoveries of the past, both new drugs as well as new methods of treatment, have come through serendipity, at least in pharmacology the day seems to be dawning when new drugs will not be discovered by accident alone, but will be developed according to specific models laid down by prior knowledge. Therapeutic techniques too will be discovered and evaluated in a more rational way. While we must not kill the goose that lays the golden egg by requiring rigorous evaluation the very day a new therapy is born, once it has passed the preliminary stages it ought to be subjected to as rigorous an evaluation as science can permit. Probably the selection of patients for the therapeutic trial and the evaluation of outcome are the two tasks which require both a high level of clinical acumen as well as methodical scientific inquiry. For the latter, the research worker is absolutely essential.

Dr. Freyhan's plea for a more objective way of evaluating therapy, coming as it does from bedside rather than outside psychiatry, is a welcome relief from the contentions of clinicians who declare that objective evaluation is impossible in psychiatry. Instead of focusing on disease categories, he proposes to focus on symptoms or patterns of symptoms (syndromes) as targets for therapy. Since there is greater agreement on symptoms than on diagnostic labels, such a procedure makes it possible to take evaluation out of the domain of the private judgment of the therapist and into the area of public scrutiny. This would be especially true if such techniques as the behavior inventories discussed by Dr. Burdock in this session were used to buttress the clinical records now in use. From this point of view, arguments about the relative merits of studies of immediate outcome vs. studies of long-term outcome become trivial. From objective information on the premorbid as well as on the morbid characteristics of patients, predictions as to immediate outcome can be made, and if objective records of the course of the illness during treatment are collected, eventual outcome can also be predicted, and these two prognoses need not necessarily be the same.

A focus on target symptoms is not limited to the types of therapies which Dr. Freyhan discusses—somatotherapy (including drugs) and psychotherapy. It is also the basis for the newly rediscovered behavior therapies in which methods derived from learning theory constitute the therapeutic approach. It is significant that objective criteria in therapy must at the present time be based on relief of symptoms
rather than on cure of disease. Perhaps a combination of behavior therapy with somatic therapy may prove more efficacious for some types of patients than either method alone. While it is possible that in the final outcome life experience may play a more important role than therapy, perhaps prognosis of ultimate outcome could contain predictions for a benign milieu as well as for a malign milieu, each specified for the individual patient as a conditional probability. Such studies would require considerable effort and time, since we do not yet know all the pertinent variables, but there are signs of a gradually developing recognition of the relevant factors.

In view of Dr. Freyhan’s hardheaded attitude at the end of his paper, it is a little surprising on rereading his introduction to see that he was far more critical of follow-up studies than he probably intended to be. There is no reason why follow-up studies cannot be developed to follow a biological model as well as a psychogenic model of the etiology of schizophrenia. In follow-up studies based on the biological model, stress would be placed on genetic, biochemical and developmental-maturational factors, while in follow-up studies based on the psychogenic model, early infantile environment and social-cultural factors would be stressed. Both types of follow-up can be carried out with, of course, different results for different diseases. Perhaps a combination of the two models into an epidemiological model might be superior to testing each model separately, but this remains a matter of strategy rather than of basic differences.

I have often wondered why the practical clinician has such a strong distaste for evaluative studies. I suppose it is based on the same aversion we have for auditors or tax collectors. But there may be an even deeper reason which I seem to sense even in some of the better clinicians. This has something to do with giving the patient the benefit of the doubt even in the face of statistical improbability. It is poor medicine to diagnose the patient as suffering from the most malignant state which the symptoms entitle him to, especially if it is a hopeless diagnosis. Instead, good clinical practice dictates the assumption that perhaps a less malignant process is at work so that there is some hope of saving the patient. Since there is always some chance of improvement even in diseases of very high mortality, the clinician may very well assume that he is treating the exceptional case with good prognosis. Conversely, it is fool-hardy after the autopsy to continue believing in the benign nature of the illness which has already killed the patient. We must separate clinical from scientific strategy. While it is true, that therapy should prove adequate. This may prove to be the case, but in a more gradual and systematic fashion than Freyhan suggests.

Dr. Greenhouse and his associates should become “rational healers.” They should be no exception in a field in which rigorous application of the rigorous application of the scientific method is a key to progress. While I agree with their concept of combining “rationality into process” we need more objectivity in the feelings about the disease. The process of discovery is worthy of study as much on the biological basis of evaluation of therapy as it is on the therapist. The process of evaluation is much easier, since the disease is usually more clearly defined and more easily measured.

The ubiquitous clinician is one of the most formidable factors in the evaluation. While he may have a vested interest in variability, he does not always have an interest in eliminating it by study of its etiology. It is often therapeutic to go at a disease by artificially increasing variability. The groups may have natural subgroups which can be identified and cleaved in a given therapeutic direction. This may greatly in need of improvement and may serve a good purpose in specific circumstances. Different populations may be identified by different variables is found.

It is notable, however, the the clinician should be trained in evaluative studies to avoid the peril of becoming too dependent on subjective impressions. The clinician should be trained in evaluative studies to avoid the peril of becoming too dependent on subjective impressions. The clinician should be trained in evaluative studies to avoid the peril of becoming too dependent on subjective impressions. The clinician should be trained in evaluative studies to avoid the peril of becoming too dependent on subjective impressions.
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true, that therapeutic optimism may bias the clinical practitioner, he should prove adamant against such bias when he evaluates outcome. This may prove too much for some clinical investigators, but they perhaps ought not to engage in evaluation. When clinical strategy is separated from scientific strategy a more objective approach such as Dr. Freyhan proposes becomes possible.

Dr. Greenhouse's paper is a model of clarity and precision and should become "required" reading for all clinicians (as well as non-clinicians) engaged in evaluation. The principles he has laid down bear no exceptions. One should, however, remember that these principles apply only to actual experimental studies—in the preliminary browsing type of research, these rigors can be relaxed. Perhaps it is the rigorous application of these principles to even preliminary hypothesis searching that the clinician (Rapoport, for example) objects to.

While I agree heartily with Dr. Greenhouse's critique of the "flight into process" when evaluation becomes difficult, I do not share his feelings about the worthlessness of investigating "process" for its own sake. The process of interaction between patient and therapist is worthy of study in its own right, but it should not masquerade as evaluation of therapy. By studying this interactive process we can learn much about the behavior of patient and therapist and it may even throw light indirectly on evaluation.

The ubiquitous variability which characterizes mental patients, is one of the most disturbing stumbling blocks which interferes with evaluation. While Dr. Greenhouse points the way to reducing this variability, he does not pay enough attention to the possibility for eliminating it by finding homogeneous subgroups within the population under study. Perhaps increasing the magnitude of the effect of therapy as a way of overcoming variability may not serve this end. Suppose the group consists of two subgroups in each of whom the therapy goes in opposite directions. Increasing the effect may only serve to increase variability, unless the group is subdivided into its natural subgroups. The techniques for finding the natural lines of cleavage in a given population are not yet fully developed and are greatly in need of further study. While randomization and controls serve a good purpose, a sample made up of portions drawn from different populations in which contrary trends in relationships between variables is found, can rarely yield significant results.

It is notable, however, that this symposium on Evaluation of Psychiatric Treatment stands out in bold relief against the symposia of
yesteryear. Slow, but steady, progress is being made both in the
types of goals that are set for evaluation and in the methods used for
attaining these goals as well as in the technique for objectifying clini-
cal change.

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