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FOREWORD

This issue of the Menninger *Bulletin* is devoted to the papers given at a Regional Research Conference held at the Menninger Foundation October 23 and 24, 1953. The conference was sponsored jointly by the Foundation and Kansas University Medical School under the auspices of the American Psychiatric Association. It was the third of the series of research conferences in this region, the first being held in Denver in 1951 and the second at Salt Lake City in 1952.

The purpose of these conferences is to bring together those psychiatrists and colleagues from allied disciplines who are either actually engaged in psychiatric research or are interested in threading their way towards some research problem so that an exchange of thought and point of view concerning choice of research task, research philosophy and methodology is fostered.

The conference was certainly successful in bringing together a large number of psychiatrists from quite an extensive area. Approximately fifty researchers converged on Topeka for the meeting from points as far distant as Weyburn (Sask.) in the north, Houston in the south, Indianapolis in the east, and Provo (Utah) in the west. Between two and three hundred people were present at several of the sessions. Moreover the many participants responded most vigorously and effectively to the suggestion of giving a paper or taking part in the discussions. Unfortunately we did not make a recording of the discussions and only the papers are represented in this issue of the *Bulletin*.

The general theme which was originally chosen for the Conference was the evaluation of the effectiveness of the various methods of psychiatric treatment. But we deviated somewhat from this theme in order to include the fascinating presentations of Prof. Nathaniel S. Apter and Dr. Jules H. Masserman. The Planning Committee willingly shoulders the responsibility for any lack of continuity which this entailed.

We wish it had been possible to include in this issue the memorable summary and critique of the Conference which was made by Dr. John S. Benjamin, but for reasons beyond the control of anyone—including Doctor Benjamin—this could not be done in time for publication.

Donald J. Watterson, M.D., Chairman
Planning Committee
Topeka, Kansas

INTRODUCTORY REMARKS

By KARL MENNINGER, M.D.

I cannot think of any greater compliment to the work which my colleagues and I have been attempting to do over the past thirty years than the election of this commission to hold its Conference on Research in Psychiatric Treatment in our community. I speak of it as a *community*, not in the sense of a city, but in the sense of a group of psychiatrists, psychologists and others interested in our field working together for the common purpose, although administratively separated by city, county, state, and federal structures. We are proud of our psychiatric community and we are proud that it is a community in this sense. I speak, therefore, not only on behalf of The Menninger Foundation, but on behalf of the staffs of the Topeka State Hospital, the Winter Veterans Administration Hospital, our six local out-patient clinics, our several children's institutions, and our friends and supporters in the city and elsewhere who have helped to make possible the work we are doing. I welcome you as our guests and as our co-workers and I hasten to assure you that the subject of your special consideration here is one dear to our hearts.

The Topeka State Hospital, later the Menninger Clinic and still later the Winter VA Hospital and some of the other clinical facilities of Topeka were developed in every instance to fulfill a need for *treatment*. From a scientific standpoint, the assumption back of the idea that we or anyone else had an effective treatment to offer might in itself constitute a subject for research. Nonetheless, the belief prevailed, and to the best of our ability we, in the collective sense of all of us working in these institutions, attempted to supply that treatment. Sometimes we have had the illusion of success and sometimes the illusion of failure. Sometimes perhaps these were not illusions, but both the humility appropriate to good medicine, and the self-inquiry characteristic of good science cannot but have made us all reflect from time to time on the question of just what we were accomplishing.

I cannot remember just when this insidious question began to worm its way into my own consciousness. Like many others, perhaps like the public itself, I took it for granted that disease was disease and something bad, and that we doctors knew what to do about it if only we were given the chance, and, given the chance, we did it, with the occasional disappointment of a refractory patient. I am not sure when I began to realize that disease is not the foreign body, malignantly afflicting the innocent victim on which it descends, that I had some way or other come to conceive of it from my medical training. With the enthusiasm of youth, I had early in my career cherished the Christian names and surnames of all the diseases whose acquaintance I had made and of whose autochthonous

existence I had no doubt. But somewhere along the line, a score of years ago, I caught a vision of disease as a process, as a phase, as a *manifestation* of the patient's life, a natural phenomenon rather than an unnatural one, undesired though it might seem to be. From Freud I caught a vision of disease as the expression of internal rather than external warfare, of man against himself. Derived from this naturally was a somewhat different conception of the role of the physician. He was still there; he was still needed. But it was he rather than the disease which was unnatural. It was he who comes as an artificial, external agent to thwart the ways of nature, so to speak, to pit nature against itself in the interest of the amelioration and survival of the patient who has appealed to him.

I should hang my head in shame to confess that I didn't know anything about the long debates of the Cnidians and the School of Cos over two thousand years ago on this same subject. It is only recently in reviewing medical history that I have realized how time after time succeeding generations have had to relearn what was learned and forgotten before. On the other hand, there can be no doubt that there is a change now in all sciences in the directions of seeing the process rather than the type, the tendencies rather than the state. This is true in biology, chemistry, evolutionary theory, and in general medicine. We psychiatrists who should have led the way are almost in the position of bringing up the rear of the scientific procession. We still talk about schizophrenia and neurosis and the ego as if they were things, or the names of things, when it is most certain that whatever they are, this they are not.

I am not sure, either, just when it was that I became aware of the hypothetical nature of many of the assumptions upon which my early faith depended. I did not realize, for example, that there is no way in which to demonstrate philosophically that disease is bad or, in the broad sense of things, undesirable. I did not realize how strictly hypothetical and presumptive it is to assume that any doctor ever cures any illness. I did not reflect very seriously upon the extent to which *post hoc propter hoc* reasoning was used to justify procedures, the specific relationship of which could not be sustained by analysis. I was an empiricist of the blandest and blindest sort, and I was possessed of a faith that permitted no questioning of my convictions that what I had said or done to the patient was responsible for the patient's prompt and vast improvement.

But empiricists we all must be, to some extent, and there are many ways in which we can strengthen our empiricism by the time-honored methods of checking, recomparing, and otherwise evaluating the subsequent developments. To put it in plain English, we all feel within our hearts that we *have* cured some patients and helped some others. But we are also aware that some patients, with very similar afflictions, have

gotten well without any such help. Logic forces us to attempt to justify what we do by proving that under certain conditions, the chances of a patient getting back into channels which both he and those about him approve, are better under some circumstances than under other circumstances, and of course the first "set of circumstances" means *us*.

But because we do so many different things, the checking of control samples is very difficult. Even psychotherapy, in which you are particularly interested, is only one of the many things we do that effect the patient (for better or for worse). Psychotherapy itself is of many kinds, done by different kinds of people, in different ways, in different places and under different conditions. It is impressive to me to recall how miraculously effective a sedative, a mild hypnotic suggestion, or even a cup of coffee could be in certain acute mental illnesses seen during the war, in comparison with the relative ineffectiveness of enormously more intensive and extensive efforts in the same individuals a few years after the combat experience which precipitated the personality disturbance. Most of us might prefer to do our psychotherapy in our quiet consultation offices, but statistically the psychotherapy done in tents on the battlefield or even in the noisy wards of collecting stations was more effective.

Of course you will say of a condition of disorganization which has lasted three years, that while it may appear the same it is actually something quite different from what it was in its incipiency. The patient has become adjusted to it and has developed resistances against its removal or its alteration. True, but these resistances are developing in all patients all the time, and indeed our whole rationale of psychotherapy might be said to revolve about the way in which such resistances can best be relieved.

We try many ways, as I have suggested, and some of us find ourselves more skillful or more comfortable in the use of one or another of these ways. We guard ourselves against becoming too specialized, too limited, in our armamentarium, but we have our preferences. These preferences at the present time cannot be supported by facts or figures but only by impressions and predilections. As I understand it, the participants in this conference are dedicated to the notion that were we to make the effort, such factual evidence could be obtained and could be analyzed. This would enable us to replace our intuitive faith with substantiated convictions.

If that were all it did, I question whether research into methods of treatment would be worthwhile. Probably more things are accomplished in this universe on a basis of faith than on a basis of fact. I would say that it is not so much for what it might confirm as for what it might *disprove* that research into psychiatric treatment is important, and necessary. If what we believe is proved to be right, we are only a little better off. But if what we believe to be right is proved to be wrong, by which I mean not so, or

incorrect, then we have a basis for mending our ways, improving our methods, trying and discovering new and more promising ways.

And surely this is justification enough for a research that is perhaps the most difficult to set up, the most difficult to check, the most difficult even to define, of any in the whole psychobiological area. But these very difficulties only underline the great importance of it. The dedication of the members of this conference to this task is shared by small groups of workers in numerous places, including a group at the Menninger Clinic. It is one which commands the respect and gratitude not only of fellow workers the world over but of the common people of all lands who look to us not only for the best ministrations of which we are capable at the time but for constant efforts in the direction of self-improvement for the sake of those who suffer.

CRITERIA AND MEASUREMENT OF CHANGE DURING PSYCHIATRIC TREATMENT

By JAMES G. MILLER, M.D.*

It is my belief that the empirical scientific method as it is traditionally understood in the physical and biological sciences is applicable to research on psychiatric treatment, including psychotherapy. Here I use a quite delimited sense of the word "science," selecting from its many possible meanings that commonly employed by natural scientists like, for example, Conant in *On Understanding Science*. I believe science is often overrated in our culture. At least since the French Age of Reason man has been enormously impressed with the accomplishments of his relatively recently developed appendages, the frontal lobes. Today many who do not follow the traditional precise methods of the natural sciences still wish to be called scientists because they thus get prestige and recognition. This is an unfortunate situation because science is only one approach to experience. Aristotle and Plato referred to the true, the beautiful, and the good. Science gives us "the true"—facts and laws. But there is also the equally valid individualistic appreciation of nuances of feeling, the beautiful and the ugly in the world. And similarly there is the realm of values, the religious and ethical approaches to experience. Who is to say that one of these modalities of experiencing is superior to another? All are parts of the personal life of each of us. If we deal with one approach, as we shall here, we choose only a part of experience. This is legitimate and often helpful, but when we do employ such a selective approach we should acquaint ourselves well with its procedures and assumptions.

What are the rules of empirical science? First of all, it is the search for that particular sort of understanding which can be demonstrated by prediction. If your predictions arise from systematic understanding, then the better they are, the greater is your knowledge. The second assumption, which is related to the first, is that of determinism. If the present event does not completely determine the future, if the future is not the slave of the past, then prediction is never possible. The third assumption is that general laws of ever-increasing accuracy are desirable as bases for prediction and are attainable. Furthermore science is a public endeavor, directed toward convincing other informed experts of the validity of hypotheses. This public endeavor requires acceptance of certain ground rules. One of these is agreement on the dimensions to be used in measuring the phenomena. The natural sciences employ the centimeter-gram-second system. I believe that these can also be employed in the behavioral sciences and in research

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conducted in the area of psychotherapy. Another agreement is that operational definitions be used; that is, that no terminology be employed which does not directly relate to a phenomenon that can be pointed to or to some derivative notion. Private phenomenal experience can be a fruitful source of hypotheses for public testing, but before the wheel of the scientific process has made one full turn, these hypotheses must be submitted to public demonstrations of validity. These are the basic rules, the constitution of empirical science, the method which we intend to apply to the evaluation of psychiatric treatment.

Existing Methods for Measuring Change in Therapy

A number of techniques have been developed in recent years for measuring change in a patient during psychiatric treatment (including psychotherapy, shock therapy, chemotherapy, occupational therapy, and other techniques). The methodology for the evaluation of this sort of therapy is the same as for any other. This means that public evidence must be collected that there has been a change in the patient during the time he has been under therapy and that this is greater in some way than what would probably have occurred if he had not been under therapy. To accomplish this follow-up is essential. It is traditional in cancer research to require at least five years of follow-up after treatment is complete. "Five-year cure" is a phrase commonly found in articles on evaluation of surgery. Of course the word "cure" is naive in any clinical field, but it is unfortunate that this phrase is not more current in psychiatry, for it implies the follow-up which is much more usual in surgery than in psychiatry. Until recent years it has been impossible to find in the literature any controlled, systematic follow-up of the outcomes of psychotherapy. Even yet such studies are rare. It's more rewarding to most psychotherapists, apparently, to treat and try to help individual patients than it is to study them systematically year after year to see how they fare afterward. Yet without such planned observation it is difficult to see how a science can be made of psychiatry. Sometimes practical situations complicate such follow-up. The directors of many institutions where psychiatric treatment is done do not conceive of their organizations' functions as justifying the expenditure of the staff time and the money necessary to conduct follow-up studies. However, this problem has been met in surgery and other clinical fields. It should also be met in psychiatry.

The vast labor involved in complete study of lengthy psychotherapeutic cases has been a hindrance to much research. French has made the suggestion, to which insufficient attention has yet been devoted, that this dilemma be avoided by subdividing the records of such cases. By considering the content of the problems on which the patient is primarily working at various times, it may be possible to distinguish "therapeutic units" into which the

clinical course can be logically divided. Then one could make predictions concerning later behavior of the patient and the outcome of such individual parts of the total process. Such predictions could be checked with subsequent events, and such data might be of as much importance in understanding the process of psychotherapy as findings on the results of the total therapeutic course.

Control methods are important in studying psychiatric treatment. One is the method of matched controls. This is only one of several kinds of controls, but it is the simplest and most obvious. The basic design for all research in psychiatric therapy is theoretically as follows:

$$\frac{\text{A T A}'}{\text{B O B}'}$$

In this diagram the time axis is horizontal from left to right. The letters *A* and *B* represent comparable efforts to evaluate the status of the individual in some way, that is, diagnostic evaluations. The A T A′ group in the numerator are those who receive therapy. The B O B′ group in the denominator are the controls. *T* represents therapy and *O* means no therapy. It is important to evaluate the status of the patients before and after therapy, using the same techniques. In addition one must demonstrate that it is the therapy rather than something else which accomplishes the change between pretherapy and posttherapy diagnostic findings. So it is important to match this group with another group, described as B O B′, that receive no therapy. Of course, other events besides therapy go on in the lives of both groups, but in this design it is assumed that they can be randomized and are comparable for both groups.

If such a research is to have scientific value, it is important to employ large enough groups so that generalizations can be made. Admittedly any such research design constitutes an ambitious and necessarily lengthy project, but it is difficult to see how these complex matters can be studied simply.

Usually those who have employed this method have matched the two groups on several variables, like education, sex, age, and socioeconomic status. In this matching it is desirable to select those variables which appear likely to be of major importance, according to whatever theoretical assumptions you are employing in the research. The others can be assumed to be of lesser significance and can be randomized. Later studies may be conducted to discover their significance, if necessary. This is the analytic method of science: everything cannot be investigated at once.

Another technique of control, in some ways less cumbersome than the matching method, is the "own" or "wait" control. The general design for this is as follows:

$$\text{A A}' \text{ A}''$$

In this procedure no control group is used. The individual is his own control. That is, he is first given a diagnostic procedure of some sort; after a time lapse he is given another one; then he receives therapy; and finally he is given a third diagnostic study. This procedure controls personality factors more adequately than the matching method. Similarly it may be better in controlling motivation for therapy, because the control subjects in the matching method often have no desire for therapy and this attitude may be important in affecting their behavior. On the other hand the self or own control technique does not control the passage of time. It may well be that the events which occur in the patients' lives during the second period of time when therapy is going on are sufficiently different from those during the first time lapse or "wait" that they can account for the differential change that goes on between time A′ and time A″. In a large group, of course, such other effects might be randomized. It is possible to combine the own control and the matched control, having one group receive therapy while another group is waiting, and then having the first group wait while the second group is receiving therapy. Later they are all re-tested. This method is used in a project on the evaluation of psychotherapy at the Counseling Center of the University of Chicago.

Another technique of control is the statistical method developed by Fisher, called the analysis of variance. This procedure permits one to study at the same time several different factors which affect change ("sources of variance"). One of these is the therapy; others may be age, education, sex, personality type, diagnostic category, or whatever other variable your theory may suggest. This statistical method shows what percentage of the total change can be accounted for by any one of these factors which have been measured. While it does not necessitate a matched control group and does not have the shortcomings of the own control methods, it is less satisfactory than the techniques previously mentioned for other reasons. One is that individual cases cannot be studied, for the results on each are swallowed up in a set of statistics which tell only what factors have been most important in producing change. To overcome this shortcoming, one may combine this method with a series of case studies of the individuals in the group. There is another possible control procedure, so far as I know not as yet used in any research on psychiatric therapy. This is employment of the mathematics known as Markov processes. Exactly how this can be done has not yet been worked out, but this technique permits what one might call postdiction by probability. Patients would be studied only at the end of the therapeutic courses, and control cases would probably not be required. A number of personality variables might be measured at the end of therapy, and then a backward prediction might be made as to the most probable previous state at given earlier times, such as the beginning of

therapy and half way through it. If treatment could be shown to alter such postdictions in some way, that would be a valuable finding.

Diagnostic Instruments

In order to test hypotheses about the nature of change in treatment, it is essential to have diagnostic instruments to measure the status of the individual patient at various times. These are the times which we have labeled with "A" and "B" in the formulae above. In recent years Rogers and his colleagues have been developing a number of such techniques particularly for the study of client-centered psychotherapy. These are, however, of such character that they can be applied to any form of psychotherapy. A number of these involve ratings of the patient by the therapist, by outside observers, or even by himself at various points in the treatment. Ratings can be made on any psychological variable—self-satisfaction, intellectualization, acceptance of others, emotional maturity, level of anxiety, insight, appropriateness of behavior, depression, and many others.

Such ratings can also be applied to records of the therapy. Electrical recordings may be used. Many therapists have disapproved of this procedure. To some it has seemed to create unnecessary resistance in the therapeutic situation. To others it has appeared to be an invasion of the privacy of the relationship between therapist and patient. To still others it has seemed that such a partial record is only slightly better than nothing at all. Sound movies have been tried, but are expensive. Various types of ratings or scorings can be done on electrical transcriptions. For example, statements made early in therapy can be compared with those made later. By presenting these statements out of their contexts to raters, bias which might arise if they knew from what part of the total record they came can be eliminated. Such records can be scored for the whole range of psychological variables. It is also possible to do such things as count the number of phrases which indicate tension or lack of tension (as Mowrer and Dollard did in figuring their discomfort-relief quotient) or the number of references to self or to others.

All uses of rating techniques have shortcomings. Scaling presents difficult psychometric problems. Reliability among raters is often hard to achieve and if achieved it may be spurious. They may not agree as to the definition of the variables being employed. Or agreement among raters may be merely semantic; that is, they may react to the same cues in the same ways and yet not be measuring the dimensions they think they are. Many other criticisms have been made of the rating technique, and yet it is on the whole a distinct advance in research on psychotherapy.

The Q-technique of Stephenson can also be employed in evaluating psychiatric treatment. In this procedure a number of statements are prepared concerning an aspect of the process to be studied, *e. g.*, the therapeutic relationship. Perhaps 120 of these statements are made up and each is put on a separate card. Some observer is then asked to sort these cards into a specified number of piles (often 11), putting those statements which are least true in the extreme left pile and those most true in the extreme right pile and ordering the others in the piles between, according to their degree of accuracy in describing the phenomenon. Moreover, he is required to put a certain number in each pile, usually the largest number in the center pile and progressively fewer in the piles toward the extreme left and right. This results in a normal curve distribution of the cards.

Q-sorts of this type can be done on many different aspects of therapy. For example, as Rogers and his colleagues, Fiedler, and others have shown, the therapist might sort the cards to describe the personality of the patient at the beginning of therapy; every ten sessions throughout it; at the end; and after a follow-up interview a year later. Or he might distribute them to describe the patient's attitudes, ideals, or behavior. Or he might sort the cards to characterize his own philosophy or ideals, or his own goals for the patient. The patient might be asked to describe his conception of himself at various periods in therapy; or he might make a sorting to describe his ideals or his social behavior. Members of his family or friends who know him well might also do Q-sorts on his behavior. It is possible to avoid the limitation that all of these mentioned so far are subjective reports, by giving projective techniques or other tests to patients at various periods throughout therapy and having the test scorer make a Q-sort to describe the findings from the tests, using the same cards perhaps as the therapist or the patient or his friends might use. The scorer would not need to be told at what point in therapy a certain test was given, and consequently bias from such information would not enter into his evaluation of the findings.

It is possible to correlate the scores of these various sortings and so show numerically how much change occurs from one sort to the next. Factor-analytic studies can also be done to indicate clusters of traits or aspects of behavior which change together in therapy. So it might be shown that acceptance of oneself as one is, acceptance of others, and warm social behavior all increase at the same period in therapy that repression of hostility to the mother disappears on the Rorschach test. One of the strong points of this whole method is that it provides statistics which can be applied to the individual case.

Another technique for measuring the status of the patient is Osgood's semantic differential. The therapist or the patient or others are asked to rate given words along various dimensions. For example, the rater might be asked to indicate where he would place the word "mother" on a line at one end of which is written "good" and at the other "bad," or on a line

between "warm" and "cold," or on a line between "up" and "down." It is possible also for him to rate other words like "self," "others," "wife," along similar scales. If this is repeated throughout therapy and follow-up, it is possible by Osgood's statistical methods to measure the change in feeling tone toward these various terms with the progress of therapy and afterward.

Of course, projective techniques are constantly being improved and multiplied in number. They are available to evaluate process and outcome in psychiatric therapy. More and more often such methods are being fitted to particular situations, rather than employed in standard form. This is scientifically sound and potentially productive so long as identical methods are used with the experimental groups and whatever control groups they are compared to.

Physiological techniques exist which can measure autonomic responses throughout therapy. For example, the pneumograph measures breathing rates, which might well diminish as anxiety lessens in therapy. Similarly one can measure pulse rates, blood pressure, blood flow in the capillaries, the psychogalvanic response for changes in microsweating of the skin, and many other physiological functions which have potential psychodynamic importance.

Methods of quantifying social interactions are also developing rapidly, particularly among those working in group dynamics. Not only are there social adjustment questionnaires but also there are observational methods which score social responses in standard social situations or in small, informal groups. Also there is diagnostic psychodrama. It is distinctly likely that these will in the future be used much more extensively in the evaluation of psychiatric therapy.

The Criteria of Change

Numerous, often conflicting evidences of change or success in psychiatric treatment have been employed. Frequently these criteria are implicit rather than explicit in the minds of clinicians as they decide whether to continue or to terminate therapy. Often a therapist does not realize that there is a conflict among them in his own mind. Not long ago I had the opportunity to talk with a rather sophisticated psychotherapist, who spent half time in private practice and half time as a prison psychiatrist. I discussed with him the criteria by which he determined when therapy was ended. He thought that they were identical for all his patients, but as we considered further it became apparent that ordinarily he stopped treating his private patients when they were satisfied with the outcome or when he felt he could go no farther, but he ended his therapy of prisoners when they became easily manageable by the warden and his guards. Even though

he used these quite different criteria, he admitted he had not previously been aware of the conflict.

A good many of these criteria may be listed: (1) Self-satisfaction of the patient. (2) Adequate "internal adjustment" of the patient, according to some theory of personality. (3) Adjustment to society at large. (4) Satisfactory work adjustment. (5) Adjustment to home environment, a common criterion in child psychiatry. (6) Disappearance of symptoms, often decried as a partial or naive evidence of success, but sometimes the only one attainable. (7) Psychological changes which are considered desirable by the society. (8) Intrapersonal adjustment congruent to the therapist's ideal of adjustment, which may be quite personal for the therapist and not commonly held in society. (9) Social adjustment congruent to the therapist's ideal.

And there are others. All of these, whatever they are, are capable of objective, scientific measurement, potentially in terms of a single, embracing theory of behavior. Efforts to develop such a general theory of behavior have often been made, and are still being carried on by my colleagues and myself in the Committee on Behavioral Sciences at Chicago, as well as by others. Nevertheless such a theory does not now exist, and since it is certainly desirable to make the best approximation to deriving criteria for effective therapy from theory, I suggest that the currently available body of theory which is most likely to be profitable for this purpose is that of psychoanalysis. Seven evidences of success in psychiatric treatment, derived from Freudian theory, which are potentially testable objectively, are: (1) Important memories, attitudes, and feelings which before therapy were unconscious (unavailable to awareness and unreportable) become conscious and can be verbalized after therapy. (2) The resolution of conflicts. (3) Flexibility of adjustive mechanisms. (4) Suitability of the mechanisms of ego defense. (5) Effective handling of anxiety. (6) Working through of the Oedipus situation. (7) Resolution of transference.

The testing of the effects of treatment using control methods and diagnostic techniques like those outlined above, to measure aspects of the therapeutic process derived from systematic theory about the nature of the change occurring, can lead eventually to scientific evaluation of psychiatric care comparable to validation studies of somatic therapies now to be found among the researches in surgery and clinical medicine.

THE USE OF MATCHED GROUPS IN THE EVALUATION OF CONVULSIVE AND SUBCONVULSIVE PHOTOSHOCK*

By GEORGE A. ULETT, M.D.,† GOLDINE C. GLESER, Ph.D.,† BETTYE M. CALDWELL, Ph.D‡ and KATHLEEN SMITH, M.D.†

For the past three years we have been interested in the evaluation of a new type of treatment in psychiatry, namely: photoshock. This treatment consists of administering a small dose of a convulsant drug, following which, cerebral stimulation is induced by an intermittently flashing light, resulting in either a prolonged subconvulsive myoclonic reaction or a generalized grand mal seizure according to the amount of drug used. The convulsive seizures produced by this procedure are milder and have a more gradual onset than those seen with conventional electroshock therapy thus making it seem a more desirable treatment for depressed patients of the older age group. Our initial experience with grand mal seizures produced in this way suggested that the post-treatment EEG alterations and mental confusion might be less than with conventional shock. The subconvulsive treatment produced by photic stimulation gave promise of a method free from the hazards attendant upon generalized seizures. Also, the use of shock treatments of different severity makes possible the study of the *modus operandi* of shock treatments in general.

Some preliminary work with this treatment procedure has been reported from the laboratories of Gastaut,[1, 2] and O'Flanagan,[3] but no attempt was made to evaluate carefully the results of such therapy.

Preliminary work in our own laboratory[4] was also concerned with developing a standardized practical technique for producing photoshock treatments. We found that Azozol (4-cyclohexyl-3-ethyl-1:2:4-triazole) produced a consistent reproducible lowering of convulsive threshold when given in a single dose and at a constant speed of injection. The amount of drug required varies from individual to individual, so that the dose must be determined by increasing the amount gradually from day to day until the desired clinical result is obtained. The actual convulsive or subconvulsive treatment is not produced by the drug alone, but is triggered by a light source flashing intermittently on the patient's closed eyelids. The rate of flash which is usually effective is from 14 to 20 flashes per second. In producing convulsive treatment, the light stimulus is continued until the seizure commences (20 to 60) seconds. For subconvulsive treatments the

* This investigation was supported by a research grant (M-560 C) from the National Institute of Mental Health, U. S. Public Health Service.

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light stimulus is applied for brief periods of time in order to produce a generalized myoclonic reaction usually accompanied by alteration of the brain-wave pattern. In this kind of treatment, stimulation is continued as long as the drug is effective. At times a second dose is necessary, as the effect of the drug wears off rapidly. Inasmuch as anxiety was produced in some patients during subconvulsive stimulation, the pretreatment administration of various sedatives was investigated. Sleep, induced by intravenous sodium seconal given 15 to 20 minutes prior to treatment, was found to be satisfactory in allaying any apprehension. The use of this drug necessitates an increase in the amount of Azozol used for treatments.

In order to obtain an adequate evaluation of the photo-shock therapies, much consideration was given to the research design. The many problems encountered in such research have been discussed frequently, but few attempts have been made to overcome these difficulties. Evidence as to what the results would be for corresponding groups with other treatments or with no treatment at all is lacking. Comparison of the treatment results of different investigators is of questionable validity because of wide differences in the population studied, methods of handling patients, length of treatment time, and differences in methods of evaluation. It has been well established that the recovery rate for any particular diagnostic group is affected by such factors as age, length of illness, or the number of previous attacks; thus a comparison of various treatments must take these into account. This is a difficult feat unless groups are equated for these factors and we, therefore, attempted to design our evaluation study so that it would be complete in itself. That is, each treatment procedure and a control procedure was applied to groups matched on such basic factors, and the procedures themselves were equated in all possible details except the specific difference being investigated. Thus our design required: (a) four groups of matched patients assigned respectively to convulsive photo-shock; convulsive electroshock, subconvulsive photo-shock, and control; (b) random assignment of individuals into these groups unbiased by clinical state; and (c) equating the number and total length of time of treatment and the hospital care of the patients apart from the experimental procedure.

Let us consider further the reasons for imposing these requirements on our research design and also some of the practical difficulties encountered in fulfilling them. The three major considerations to be discussed here are: equivalence of groups, equivalence of procedure, and necessity for a control group.

Equivalence of Groups

An alternate to the matched group method would be to assign patients at random into the various treatment categories when they are admitted to

the hospital. If one truly randomized such placement, the resulting groups should not differ significantly from each other with regard to factors which are independently distributed. However, such a method would necessitate an impractically large number of patients to insure randomness on all of the many factors which might be related to recovery. Also, unfortunately, many of the important factors are not independent. For example, young patients are more likely to be suffering an acute first attack of psychosis; most chronic patients will have had previous treatment. Also, it is very likely that there is interaction between such factors and the results of treatment, *i.e.*, the effects are not additive.

The most ideal arrangement would be a factorial design by which the interaction among the various factors and the methods of treatment could be evaluated. However, with four treatment methods, only four diagnostic categories, and four dichotomized factors (*e.g.*, sex, age, chronicity, previous treatment) one would need 256 *selected* patients in order to fill each cell of the design with a sample of one. Thus, with replication one would need two or three times this number. This would necessitate a broader and more extensive population of patients from which to draw than is available to most research workers in any limited period of time. It appeared, therefore, that the only feasible solution was to match the individuals in each group on as many variables as possible, and to place certain limitations on other variables. In this way it should be possible to draw definite conclusions about the relative value of the various treatment methods on a defined sample of patients, even though only very tentative conclusions could be drawn on the relationship of treatment results to other factors. However, study of the subgroups under each treatment might also reveal further information in this area.

As finally defined, the study is limited to individuals with those types of mental illness which are thought to respond best to the shock therapies, namely; psychotic depressions of the manic-depressive, involutional, senile, and reactive varieties, together with first attack acute catatonic schizophrenic and schizo-affective reactions. Depressed patients with chronic brain syndrome and marked deterioration are excluded. The factors on which the patients are matched are: diagnosis, sex, age (within 10 years), whether or not they have had previous attacks, type of previous treatment, and amount of education. The method by which placement is determined is that an individual not matching any previous subject is assigned at random to one of the experimental groups while one who matches a previously treated patient is assigned at random to the three remaining groups. The matching and assignment are done by one of our staff members who has no clinical contact with the patient and thus is not biased by knowledge of clinical factors which might otherwise influence the type of treatment

selected for that patient. Inasmuch as ours is a hospital restricted to acute cases, there is some homogeneity in the type of patient seen, especially with regard to chronicity of disease and duration of the attack for which the patient is hospitalized.

Although simpler than a factorial design, and requiring fewer subjects than either a factorial or a random selection, the procedure of matched groups also has its drawbacks. Out of a total yearly hospital admission rate of 1600 we have, in 12 months, been able to obtain only 75 patients suitable for our research. In addition, almost 20 per cent of these were dropped from the study after being started, for various reasons which included signing out against advice, and medical factors not related to the mental disease or its treatment. We have found that matching presents many difficulties which are not apparent in *a priori* contemplation. Perhaps the major one of these is that of diagnostic labeling of patients, which has been recently re-emphasized by Kline[5] and others. Also it is only possible to match on a very few of the many factors which may be related to recovery.

Equivalence of Procedure

As described above, the subjects are divided into four groups, a control and three treatment groups. In order to minimize the effect of extraneous factors upon the treatment results, the handling of patients in all groups is kept as uniform as possible. All patients are treated in the morning, three times a week for a total of from 12 to 15 treatments. Just prior to treatment, all patients (including controls) are sedated by means of intravenous sodium seconal which produces a light stage of sleep, and are then brought to the treatment room. At this time electroshock treatments are administered to members of the appropriate group in the usual manner, and the photoshock and photo-subconvulsive groups receive intravenous Azozol and flicker stimulation while members of the control group are given only photic stimulation. Aside from these treatments, no therapy other than routine ward care is given to the subjects.

The question arises as to whether true equivalence is obtained best by giving all patients the same number of treatments, by continuing treatments until clinical recovery, and/or setting a maximum number of treatments to be given in case clinical recovery does not occur. Many investigators continue treating until recovery occurs yet it would seem that the evaluation of different treatments would yield more comparable results if the number of treatments and overall time were kept relatively constant. For one thing, it is often said that one of the advantages of convulsive therapy is that it shortens the period of hospitalization, and that perhaps as many would recover under routine hospital care but with a much longer

period of hospitalization. Thus time is an important factor, and hence the only meaningful comparison of therapeutic results is that based on a given interval of "treatment" time.

On the other hand, it must be recognized that some patients will show apparently complete recovery after a number of treatments far short of that required by the average patient. In such cases, usual clinical practice plus responsibility to the welfare of the individual patient would dictate that treatments not be continued unduly. Also, occasionally a patient may become so much worse under a particular treatment that it is considered dangerous to the patient to continue without other treatment. Thus some compromise seems necessary. We have used a range of from 12 to 15 treatments to handle such contingencies. This number of treatments is approximately that usually deemed adequate for ECT treatment of psychotic depressions. Often certain patients make sufficient improvement in 15 treatments to lead one to suspect that a small number of additional treatments might bring about full recovery, but one must remember that we are not concerned with whether a given type of treatment could make a certain patient well, but rather with the evaluation of different treatments given under comparable conditions. Of course, after psychological and psychiatric evaluation is made, such patients are given further treatment and/or hospitalization according to the judgment of the psychiatrist in charge.

Control Group

As may have been inferred from the discussion thus far, we feel that it is of utmost importance in psychiatric research to have a control group. The final proof of the value of any therapy can only lie in its ability to improve the condition of, or cure, individuals who otherwise would continue to suffer psychotic symptoms or would recover only after much longer periods of hospitalization and care. While most previous research seems to indicate that the shock therapies *do* alleviate symptoms and shorten hospitalization time, still the aforementioned difficulties of comparing data obtained at different times on different samples and with varying amounts of incidental therapy make it imperative that a control group be studied *in conjunction with* treatment evaluation.

Ideally, we would like to know what would have been the outcome, for a particular individual, under different types of therapy, but in most cases this is impossible to determine. In some rare instances circumstances make it possible to use an individual as his own control, but, in general, this is difficult to arrange. We had one case which provided us with such an opportunity: a man who had had photo-shock treatment in our laboratory during our preliminary investigation of this method. He was sent home improved, and remained fairly well for a year when his psychotic depressive

symptoms again returned and necessitated his hospitalization. This time he was placed in the control group in our series. To his knowledge he was receiving the same treatment which had relieved him previously, administered in the same way and by the same doctor. Yet at the end of the series he had shown absolutely no improvement. After re-evaluation he was placed on electroshock therapy, recovered in 13 treatments, and was discharged. For this patient, then, it seemed evident that shock treatment was effective in relieving his psychotic symptoms and that photo-shock and electroshock were about equivalent in this regard.

In general, the difficulty of using an individual as his own control lies in the time factor and in the probable interaction between consecutive treatments. Thus if only routine hospital care is administered for one month and then a particular treatment instigated which seemingly effects a cure by the end of the second month, the question still remains, "Would this person have remitted spontaneously in the total two month period?" Also, the waiting period may have had a beneficial although not immediately observable effect, which, combined with the subsequent treatment, may have helped to reduce the number of necessary treatments.

The method of a matching control series, such as we are using, is certainly not an ideal solution, but it appears to be a practical one. It suffers all the above-mentioned limitations of matching, particularly the fact that no two individuals (except perhaps twins) can be matched on more than a few characteristics. One of the major precautions that we are taking in order to insure meaningful results is that the clinical psychiatrist who is in charge of the patients and makes the psychiatric evaluation is unaware of the treatment assignment. Aside from eliminating bias, such a precaution relieves the attending doctor, to a certain extent, of the feeling of responsibility for withholding what he might recommend as "the best treatment" for a given patient. This aspect of the physician-patient relationship as related to the need for objectivity has been discussed recently by Dr. G. E. Guttentag.[6]

One important difficulty arises with the use of a control group. Occasionally a patient will become so much worse during the period of observation that the doctor in charge of the research must make the decision as to whether he has the moral right to withhold convulsive treatment for a longer period of time. This is a problem which, of necessity, must be decided for the individual case. As is well known, such exacerbations are often only temporary, although at the time the reversibility cannot be determined. The problem then remains as to what course of action should be taken in the patient's best interest. In one case, for example, a young girl with schizo-affective reaction in our control series developed catatonic symptoms, and refused food. However, these symptoms subsided in a few days, after

which she continued to improve so rapidly that she was ready for discharge at the end of the research period. This sort of thing can happen also in other than a control group. In the latter case, however, the doctor's confidence in the mode of treatment being used may enable him to continue his procedure unchanged. When other treatment is instigated under such circumstances, or when one continues longer with one type of treatment than he would with another, there is an implication of pre-knowledge of the very hypothesis one is attempting to test, *i.e.*, the relative effectiveness of the various treatments. On the other hand, if the patient's condition is very serious, the doctor is morally obligated to try any means at his disposal to save the patient. Circumstances such as these pose a serious dilemma in research on human beings and necessitate some compromise between humane considerations and experimental objectivity.

The ultimate purpose of our research, namely, the comparative evaluation of three different types of treatment, necessitates a determination and recording of the condition of the patient prior to the treatment as compared to his status at some given time following the treatment interval. We achieve our initial and final evaluation of the patients by both psychological and psychiatric examination. The psychological examination consists of a battery of tests designed to measure intellectual functioning, ideomotor retardation, amount of depression of affect, and contact with reality. The tests which are used are:

Wechsler-Bellevue (5 subtests)
Associate Learning Subtest Wechsler-Bellevue Scale
Homographs
Word Memory
Rate of Manipulation
The Rorschach
Caldwell Picture Series (5 cards)
Rosenzweig P-F Test
Level of Aspiration
Optimism-Pessimism Scale

This battery is repeated three to five days following completion of treatment.

One of the practical difficulties which we have encountered in attempting to use standardized psychological testing procedures as a part of our evaluation is that the patients vary in their testability from day to day as well as in their response to different examiners. Occasionally the psychologist encounters a patient who is untestable. Obviously, such incomplete test data make ratings difficult.

In the psychiatric examination, one of the greatest problems is that of obtaining objectivity. To meet this need various rating scales have been devised. Of these, the Malamud Psychiatric Rating Scale[7] seemed most

adaptable to our purpose. However, in an attempt to increase the comparability of ratings from one patient to another and thus to improve the objectivity of the scale, our psychiatrist defined each point of the scale in terms of observable behavior. This scale is filled out for each patient prior to treatment and at weekly intervals thereafter until the final evaluation is made. This procedure aids the psychiatrist in following the course of the patient, thus rendering more meaningful the final evaluation. Within a week after treatment the same psychiatrist writes a summary of the patient's status together with a recommendation as to discharge, continued hospitalization or further treatment. The final evaluation of improvement, on the basis of all of the above observations, is summarized in terms of a numerical rating on a continuum from "worse" to "complete recovery."

In all psychiatric evaluations it is of the utmost importance that continuity be maintained by having all ratings done by the same psychiatrist. Even with carefully defined observations, two different psychiatrists cannot be expected to make the many complex judgments required in the assessment of personality variables in an identical manner or combine them with identical weights.[8]

An important decision which must be made in judging the efficacy of any therapy is the length of time a patient must remain improved or symptom free in order that the treatment be considered effective. Inasmuch as the research problem presented here is mainly concerned with comparing one treatment with another, it seemed that the important point was to keep constant for all patients the interval between termination of treatment and evaluation. We elected to evaluate the treatment results within a week after the treatment period was over because it is our feeling that the prime purpose of the shock therapies is not to produce lasting changes in personality structure but rather to bring about a rapid alleviation of psychotic symptoms and to facilitate recovery from the acute phase of illness.

The assignment of such a role to the shock therapies is in keeping with evidence pointing to a multiple factor etiology of affective psychosis. For, as suggested by the work of Funkenstein,[9] Gellhorn[10] and others, there may be in these patients precipitation of a neurophysiological malfunctioning by some psychological stress and the restoration of normal neurophysiological balance may well remove the acute psychotic symptoms without in any way altering the pre-psychotic personality pattern which renders the patient still vulnerable to other attacks. Hence the constant maintenance of mental well-being for months or for years following shock can hardly, in fairness, be considered as a measure of the effectiveness of this mode of therapy.

The chronicity of most personality disorders is well established, and several recent follow-up studies have shown that patients who are treated not only by shock but also by the various types of psychotherapy are not

better than untreated controls at the end of five years. This indicates, not that psychiatric therapies are worthless but rather that they are similar to other medical treatments for chronic diseases such as asthma, or diabetes, useful in alleviating acute attacks of a chronic illness whose over-all therapy in terms of maintained general health is yet to be promulgated. For such reasons it has seemed wisest to us to evaluate our treatment results almost immediately after the course of therapy has been completed. However, for those patients who were sufficiently improved that no additional treatment was required, follow-up studies are being made after a three month interval, and longer studies are contemplated.

Data of this kind is certainly of value in indicating whether one treatment may have more lasting effects than another. However, we feel that the longer the follow-up study the greater the possibility that unknown and uncontrollable environmental changes will occur which can, of themselves, influence the course of mental disease for better or for worse. Only when such environmental factors are random for all groups will a comparison of long-range effects be meaningful. Thus a good follow-up study should include such sociometric data.

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DESIGN AND PLANNING IN PSYCHIATRIC RESEARCH AS ILLUSTRATED BY THE WEYBURN CHRONIC NUCLEOTIDE PROJECT

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In psychiatric research, certain well-defined steps must be taken and a discipline observed, if valid and worthwhile results are to be achieved. These steps are: First, the formulation of a hypothesis as a result either of a series of observations or from an intuitive "hunch." Next, the hypothesis must be tested by a series of planned and controlled experiments, and finally, the result of these experiments must be evaluated and the conclusions drawn and communicated.

The actual design of the experiment will depend on what is to be investigated, physical and environmental limitations, availability of resources, and the stage of development of suitable investigatory techniques in the field that is under survey.

It is desirable to design an experiment in which control material can be studied alongside material that is being subjected to manipulation, and in which clear-cut and unequivocal results can be obtained, expressed numerically, and evaluated statistically. In medical research, experiments are most frequently carried out on living persons and animals, which may show a considerable range of individual behavior even if the most stringent steps are taken to control the environment. Here, the provision of a control series selected at random from the group under review is of the utmost importance. Large samples dilute the grosser individual variations, and produce results that lend themselves more easily to statistical validation.

The vagaries of behavior and the difficulty in evaluating and expressing results are two major difficulties that hamper the medical research worker. Others are the paucity of clinical material in some of the less common conditions which may make the assembly of a large and statistically valid series impossible or infinitely protracted, and the occasional lack of a clear-cut distinction between two separate disease entities.

Diagnosis of certain psychiatric conditions is hampered not only by the lack of a sharp dividing line between one illness and another but also by our lack of unanimity as to the precise meaning of many psychiatric terms which causes communication difficulties among ourselves, and between us and our patients. A psychiatric interview is a two-way affair. The patient's thoughts, attitudes, and behavior may be modified by our handling of him,

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and our own attitudes and complexes may modify or even distort our evaluation of him.

Other difficulties arise in the evaluation of clinical recovery. The course of a mental illness may fluctuate markedly from day to day and the patient's behavior and productions may bear little relation to his true state. We can make some objective evaluation of his behavior, but how can we be truly objective about his description of his subjective sensations? What are our criteria for recovery? Do we all mean the same thing when we talk about a "social recovery"? How long must a patient remain well to be recovered? Should it be a five-year freedom from symptoms as in cancer patients? Can a recovered patient have residual symptoms if he can live and work outside a hospital?

In any new psychiatric treatment, the element of suggestion is extremely strong and is difficult to eliminate. The initial results of the new technique or shining, dial-laden box are intoxicatingly impressive, only to decline as custom and routine stifle the enthusiasm of the doctors and nursing staff, and the patients under treatment are no longer watched and studied with the original vigilance.

The object of this paper is to describe how we at Weyburn have attempted to design and run a clinical experiment in such a way as to overcome as many as possible of the difficulties that are inherent in trials and observations on psychiatric patients. Special attention was paid to selection (of treatment cases and controls), control and standardization of environment, objective evaluation of results, and elimination of semantic failures. We found it necessary to adopt a special method of assessment and to design a special assessment sheet using standard questions.

Our experiment was made at the request of the Mental Health Division of the Canadian Department of Health and Welfare in Ottawa. The Department was interested in a preparation of nucleotides that was said to produce remissions in schizophrenia when given by injection. One clinical trial had already been run, and the firm that had prepared the material was prepared to release it for sale. The Department was not entirely impressed with the results claimed, and suggested that further trials should be undertaken.

The original trial was made in 1951 by Fernand Seguin, a biochemist on the staff of the St. Jean de Dieu Hospital in Gamelin, Quebec, who suggested that injections of nucleotides might be of therapeutic value in schizophrenia. He had observed that the level of nucleotides in the cerebrospinal fluid of schizophrenics, as estimated by the ultra-violet absorption method, was greater than in nonschizophrenics.[1] This led him to believe that there was a breakdown of nucleoprotein in the schizophrenic brain which was lost into the cerebrospinal fluid. Seguin felt that the illness could be benefited by replacing the lost nucleoprotein.

A somewhat heterogeneous assortment of cases was given nucleotide therapy, but for the purpose of a preliminary appraisal of the treatment, Seguin selected 22 patients with schizophrenia who did not, so far as is known, receive any other treatment during the nucleotide trial. There were both male and female patients in the group. They had been ill for periods of from six months to 20 years, and had in most cases received treatment on previous occasions. The treatments were electroshock, insulin, both electroshock and insulin, electroshock and lobotomy (one case), and in one case the treatment was stated as "unknown."

A preparation of mixed nucleotides comprising adenylic acid, cytidylic acid, guanylic acid, and uridylic acid was made by a pharmaceutical company. The original preparation contained 5 mg. of nucleotides per cc. (the preparation used by our research group contained 31.4 mg. per cc.). The dosage schedule was not standardized. To begin with, a new preparation of nucleotides was introduced at some point in the trial. The amount given in any one day varied, as did the number of treatment days, and the total quantity of nucleotide given in a course varied from 2.1 to 18 grams. It is stated that out of the 22 patients, four were discharged after treatment, and another two left after further insulin treatment. Seguin regarded the 18 per cent discharge rate (expanded to 30 per cent later when two more patients were discharged and disregarding the two patients who later received insulin) as encouraging. Under these circumstances, it was not really possible to draw any valid conclusion as to the efficacy of nucleotides in schizophrenia.

Our group planned its research to fall into two parts. An initial trial would be carried out at Weyburn on a group of chronic schizophrenics, and trials would later be run both at Weyburn and at our sister hospital at North Battleford on patients with acute schizophrenia. It was agreed that a plan of the design of both experiments should be submitted to Ottawa for appraisal and approval by the Mental Health Division of the Department and it's statisticians before going ahead with the trial. It is with the planning and execution of the experiment with chronic schizophrenics that we are concerned at present as the experiments with acute schizophrenics are still in progress.

Since we were investigating a preparation that was believed to be of benefit in cases of schizophrenia, only schizophrenic patients were selected. We were advised that a series of 40 would be statistically valid, half of these patients being controls. It might appear that a series of 40 schizophrenic patients in a hospital of nearly 2,000 was rather small, but actually this number was not so easy to assemble. In order that conditions should be as standardized as possible, it was necessary to conduct the trial on one ward and with patients of one sex (males were used). This reduced the population of the ward by 50 per cent. Again, as this was a trial on chronic

cases, all patients selected had to have been known to have been ill for at least five years.

The upper age limit was 50 years in order to eliminate such variables as changes due to advancing years being engrafted onto the original illness, and all cases were carefully screened to eliminate organic brain disease and oligophrenia. An additional precaution had to be taken which is probably less necessary in many other centers. Because Saskatchewan is newly settled, a great many eastern Europeans constituted the hospital population and those whose command of English was poor had to be left out as the language barrier would have made assessment unreliable. The violent and the very degraded patients had also to be omitted from the series. It would not otherwise have been possible to have had the entire group living together in one ward during the eighteen weeks of the project, and we felt that keeping the group under review in identical environmental conditions was one of the most important factors in producing a varied end result.

The names of the patients selected were submitted to Ottawa where each patient was allotted a code number. Supplies of nucleotide and a similar looking control substance were prepared and boxes of vials made up, each containing enough nucleotide or control substance to suffice for a two-month course of treatment, comprising one daily injection of 4 cc., or 125 mg. in those patients who were to get nucleotide. Each box carried a patient's number so that each patient had his own package. The key to the numbers were kept in Ottawa, but no one at Weyburn had any idea as to what patient was to receive nucleotide or which the control material.

Owing to scarcity of accommodation, 30 additional chronic psychotic patients had to remain on this ward. The transfer was carried out ten weeks before the administration of the nucleotide commenced to give time for any improvement due to environmental changes to be complete before the preliminary assessments were started. The ward population was divided into two groups to facilitate the use of a comprehensive occupational and recreation program. The staff allocation was in the ratio of one nurse to every eight patients (on an eight-hour shift basis) and the same nurses were kept on the ward during the trial. Although the staff were encouraged to commend and praise patients who performed some task well, material rewards were not used as incentives, the issue of tobacco, candies and other comforts being allotted equally.

During the ten preparatory weeks, the staff got acquainted with their patients and learned how they could best be handled. During the trial they carried out regular assessments on the patients and turned in a report every third day, on an average, covering the personal appearance, toilet and social habits, activities and attitudes of each patient. The ward was organized on the "Total Push" principle used by Dr. Derek Miller and Dr. John Clancy in their work on the Reiter Stimulator in 1952. Doctor Clancy, who was in charge of the ward, worked out the ward plan and supervised it while the trial was in progress.

The assessments by the medical staff were carried out by Drs. Osmond, Smythies, and Lucy. These doctors only came on the ward on the occasions that the assessments were made, that is just before the treatment commenced, and thirty and sixty days afterward, and had no contact with the patients at any other time. In this way it was hoped that they would not become biased by observing any apparent changes in the patients in the interval. They avoided discussing the progress of the patients with Doctor Clancy. This system we also owe to the work of Miller and Clancy. The actual method of assessment was discussed at some length with our research psychologist, Mr. Ben Stefaniuk. We decided that each interview should be very brief and should not exceed fifteen minutes. Objections may be raised that it is difficult to evaluate fully a patient's deeper thought content in this short period. We did consider this aspect of the matter, but we decided that other aspects were of even greater importance.

First, it seemed desirable to finish all the assessments, a total of 120 each time in a single day, so that the patients could be interviewed under fairly standard conditions. If the interviews had been made longer, more sessions would have to have been arranged in order to avoid impairment due to fatigue on the part of the examining doctors. Second, it was decided that a set of standardized questions should be asked in order to avoid semantic errors, and third, that with the short interview a very large number of variables introduced by the interview technique of the individual doctor could be avoided.

The results of the assessments were recorded on a form designed by Ben Stefaniuk who drew considerably from Malamud's[2] clinical rating scale in which a theoretical baseline or average of normality was postulated and scored as unity with attributes indicating deviations from normality both above and below being scored up to plus five and minus respectively. In our form, we used a positive five point scale only. We did not use Malamud's baseline as we had not adequate means of working out the patient's deviation from his prepsychotic personality.

Our assessment scale had 38 separate attributes which fall into seven basic categories. These were: appearance, behavior, perception, memory, insight, thought, and mood. The total score during treatment should suggest an improvement in that particular category. As a result of a model interview that was carried out before treatment was begun, the categories were arranged in such an order as to permit the assessor to score them in the shortest possible time. A score of five in any item indicated extreme bizarreness, deterioration or disintegration.

The definition of the terms, and the weighting given to each numerical category were fully discussed at a series of meetings prior to each of four revisions of the form. These meetings included the psychologist, the three assessors, the ward doctor, and any other interested staff member. No element or factor was allowed to be entered in the form if complete agreement was not reached. In doubtful categories, definite rules were laid down both for questioning and for ascribing a numerical score. In addition to the numerical score, the assessors were encouraged to add a verbal weighting about any item about which they might be doubtful, and if necessary add a short summary of their total impression of the patient. Doubtful scorings were discussed with the three assessors by the psychologist and a definite scoring method accepted to be used in future assessments.

Minor difficulties that arose were due to such things as doctors confusing the forms, and failing to score one or other of the items. Patients were taken into one of three rooms for interviewing and the same room was occupied by each assessor during each assessment session. The psychologist worked out a series of descriptive profiles to illustrate the amount of change that occurred in each of the seven descriptive categories. So far we have not received the statistical evaluation for the significance of these changes when a total score was given to each category. In the trial assessments, the method was useful in showing up those categories in which doctors found it hard to agree in interpretation and scoring.

A short instruction manual for using the forms was written and issued. This was studied by the assessors just before each evaluation was done.

Results

As far as the ward organization and the administration of the nucleotide was concerned, the project ran extremely smoothly. A number of patients complained of pain at the site of the injection and developed a slight pyrexia on the fifth day of treatment. The injections were reduced from 4 cc. to 2 cc. for three days then the full dose was restored on the ninth day for these patients.

No significant improvement was observed during the treatment or after its completion, with one exception: a young man who was paroled a few weeks after the experiment was completed. There was some over-all improvement in communicability and alertness, but this was also observed in the 30 nonparticipating patients, who incidentally objected to being excluded from the injections.

It was felt that the improvement in communicability and sociability was fully accounted for by the fact that the patients had been moved to a more active ward. Improvements of this kind are the rule when any new measures are adopted in a mental hospital. Whether there was a significant increase

in hallucinations, as was previously observed by Miller and Clancy[3] when a group of schizophrenic patients was subjected to increased activity, has not yet been fully analyzed. However, the statisticians have discovered a high positive correlation between appearance, behavior, memory, thought processes and insight, and in insignificant negative correlation between these and hallucinations. We feel that this observation needs further investigation as it has important social implications and may call for some revision of our attitudes as psychiatrists.

In their report on our findings, the statisticians criticized our method of sampling, and suggested that we should not have confined our treatment to cases of schizophrenia only, and that our sampling could have been improved. Our reply to this is that our mandate was to test the efficacy of a certain amount of nucleotide therapy in the remission of schizophrenia and that selection was necessary as these patients were mentally ill, and it was important to have a wardful of patients that could be relied on to undergo a treatment for two months.

It seems that the statisticians preferred to assess the patient's total improvement on the grand total of all the scores. We had not considered that such a method would be informative. We felt that only the changes up and down in each of the seven main categories could be taken and compared. It has been seen though that the statisticians did make some comparison between the different categories in working out the correlation coefficients.

This project has been useful in introducing us to experimental design, and showing us how to proceed in the future. It has also given us a method of objective clinical assessment which we believe to be of particular value in work of this nature. We have learned to put on a "combined operation" in which nurses, a psychologist, several doctors, and statisticians took part. It has underlined the importance of teamwork and shown that only a well-integrated team can be expected to make investigations of this sort.

In the future, closer cooperation between the psychiatric team and the statisticians will be needed, and for this it is necessary that we should each learn a little more not so much about the technical execution of our separate jobs, as about the particular problems and difficulties with which we may be faced.

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RESEARCH APPROACHES TO CHRONIC SCHIZOPHRENIC REACTIONS: METHODOLOGICAL PROBLEMS

By NATHANIEL S. APTER, M.D.*

During the past three years I have had the unusual opportunity of collaborating with a number of preclinical and clinical representatives† of the medical sciences in studies on chronic schizophrenic patients. The eventual goals of this work are to learn: whether psychodynamic constellations discernible in schizophrenic reactions can be meaningfully correlated with different classes of biological data; how clinical psychiatric methods derived from psychoanalytic psychopathology may provide further direction to the biological sciences in studies of schizophrenic disorders; and, how the biological sciences may contribute further to the clarification of the various schizophrenic reactions. We have approached these problems by attempting to produce significant changes in the internal physiological environments of chronic schizophrenic patients. By modifying some of the physiological processes involved in cerebral metabolism, behavioral changes may be expected to emerge in some patients.

The alterations in behavioral patterns thereby induced comprise the psychiatric data which are regarded as restitutive or regressive mechanisms available to the chronic schizophrenic patient within the limits of the procedure employed. The variations produced as a consequence of the physiological processes are then compared with the more enduring behavioral patterns before the internal milieu was shifted and after the effects of such shifts have terminated. At the same time, a number of physiological, experimental-psychological and clinical-psychological observations are carried out. In this way, there are available for comparison various classes of data collected prior to, during, and after a particular procedure, designed to alter some internal physiological processes, has been used.

The immediate gain for the psychiatrist is access to an experimental method within biology without relinquishing clinical psychiatric perspectives derived from psychoanalytic psychopathology. One of the significant advantages of the clinical psychiatric method, namely, the detection,

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† Principal collaborators have been the following: Drs. Delbert Bergenstal, Ward C. Halstead, Charles Huggins, Morris Lipton, Roy Whitman and members of the Division of Psychiatry, University of Chicago; Drs. Alfred P. Bay and Theodore Tausig of Manteno State Hospital; Dr. Carl Pfeiffer and members of the Department of Pharmacology of the University of Illinois; and, Dr. Hudson Hoagland and Gregory Pincus and members of their staff of the Worcester Foundation for Experimental Biology.

recognition and interpretation of behavioral changes occurring within the individual patient, is retained. Maintenance of this perspective is necessary in order to emphasize that the variety of clinical pictures in the schizophrenic reactions may represent different etiological factors at work.

This presentation will consist of a discussion of the theoretical background and implications of this research approach, and, an initial attempt at describing the dimensions for a conceptual model of schizophrenic reactions. Subsequent reports will deal with: the effects of bilateral adrenalectomy on chronic schizophrenic reactions; the effects of various corticosteroids on such patients; experiences with biochemical convulsants of the unsubstituted hydrazide group in convulsant and subconvulsant doses; psychodynamic constellations and their bearing on questions of reversibility and irreversibility in schizophrenic reactions; and, personality development in schizophrenic patients.

The Development of the Research Approach

The literature contains many examples in which changes in behavioral patterns as a consequence of alterations in the internal milieu were utilized for studying personality functions. A classical example is the work of Hollos and Ferenczi.[1] They were able to establish connections between irreversible lesions of the central nervous system and the emotional and psychological disturbances in patients with general paresis. As psychoanalytic work advanced, a reciprocal method was described by Benedek and Rubenstein.[2] Benedek, having recognized regular variations in preoccupations of female patients undergoing analytic treatment, was able to correlate these recurrent changes in psychoanalytic data with shifts in hormonal dominance as reflected in the microscopic examination of vaginal scrapings. The theoretical background for the development of the research approach used in our work on chronic schizophrenic patients is a derivative of these two approaches.

Using combined psychiatric and experimental psychological techniques, we first studied patients with irreversible changes in the central nervous system.[3, 4] Later, we had the opportunity to examine patients with reversible disorders.[5] This work was undertaken to determine whether or not early modifications of ego functions discernible by psychiatric methods could be correlated with experimental psychological tests providing evidence of impairment of cerebral functions, especially of the frontal lobes. The principle involved is that activities of the ego undergo degradation when cortical functions are disturbed and are reconstituted as these functions improve. Observations of the degradative and restitutive mechanisms provide data for the understanding of ego-development and deterioration in accordance with the laws of Hughlings Jackson on the evolution and dis-

solution of central nervous system functions. In this connection we have adopted the analogy of Alexander.[6] "When we speak of the ego we refer to the organ system whose anatomical and physiological substratum is made up of the highest integrative centers of the central nervous system. The functioning of this organ system can be studied either by the methods of anatomy and physiology or by those of psychology. When we use the term 'ego' functions, we are referring to the psychological approach. If we study these functions by the method of anatomy and physiology, we speak of 'cortical' or 'subcortical' functions."

A syndrome of significant personality change in patients with otherwise uncomplicated hypertension was correlated with evidence of brain impairment from experimental psychological techniques. The syndrome is discernible before neurological or electroencephalographic evidence of brain damage can be elicited. From the psychiatric viewpoint, following an acute anxiety reaction some hypertensive patients were observed to relinquish a number of personality patterns when there was little clinical evidence of organic brain damage. However, a battery of objective tests especially designed to test frontal lobe function yielded evidence of impairment. The shift in adaptive behavior was thus attributed to cerebral impairment.

From the standpoint of ego-psychology, the series of events leading to the modification in adaptive behavior was conceptualized as occurring in the following stages: (1) prior to the intervention of brain damage, the ego functions were not encumbered by the effects of cerebral impairment; (2) the catastrophic reaction to the injury of the basic sensory equipment, integrating mechanisms and motor control was acute anxiety expressed through the cardiorespiratory system; (3) defense mechanisms such as withdrawal, rationalization and identification were invoked as early methods of dealing with the newly acquired cerebral defects; (4) denial, guilt, reaction formation, substitution and regression were used later; (5) when the primary defenses of the personality were replaced by the second and third order measures, the ego appeared reconstituted. When the final step had been taken, the alterations in neurotic patterns were recognizable as features of the newly adapted ego which had suffered irrevocable loss of some of its most highly developed integrative functions.[4]

As far as reversible states, the medullary tumors of the adrenal gland make available for circulation large amounts of hormones which affect cerebral functions either directly or through changes in the vascular system. These tumors may exert their effects for relatively long periods of time, months and years, before they produce irreversible damage. When they are surgically removed, a gradual restoration of cerebral functions and accompanying personality changes take place. Preoperative and postoperative findings are then compared and comprise the data for correlating behavioral with biochemical and physiological changes.[5] In another study, preoperative and postoperative studies on a patient with a cortical adrenal tumor showed similar findings. Prior to surgery, the patient was utilizing a variety of defensive techniques in response to the abnormal amounts of corticosteroids and difficulties in adapting to the external environment. Schizophrenic, depressive, and organic brain features were discernible on psychiatric examination. The Rorschach and experimental psychological examinations tended to corroborate the clinical impression. Following removal of the tumor, the patient's ego was reconstituted with the premorbid neurotic pattern gaining prominence. The clinical evidence for ego reintegration was matched with laboratory findings of the psychologist.

This method of investigation permits comparison of behavioral and biological data prior to and after operative removal of a gland known to be producing large amounts of hormones and thereby modifying the internal environment. Since comparative data on the same patient are used, each patient serves as his own control. This is essentially the approach we have used for chronic schizophrenic patients. The cerebral changes in essential hypertension, pheochromocytoma, and cortical adrenal tumors are, however, experiments found in nature. The use of pharmacological agents to modify the physiological processes in schizophrenia requires further explanation. For this, a discussion of the theoretical considerations which are used to support the analogy between biological adaptation and shifts in dynamic patterns is required.

Theoretical Considerations in Analogizing Between Dynamic Patterns and Biological Adaptation

The highest instinctual representations as expressed in the activities of the ego are derived from biochemical and physical processes. They are then transmitted through higher and higher integrative levels until they become manifest as behavioral expressions of the highest integrative centers by the central nervous system. What becomes available for the examination to the psychiatrist are the dynamic patterns or the adaptive mechanisms used in the interpersonal context. The adaptive mechanism, whether reality-adjusted, or neurotic technique, or psychotic maneuver, serves the psychic economy under the existing circumstances. Furthermore, a particular dynamic pattern recurs in characteristic fashion as long as it serves psychic economy. When alterations in adaptive techniques or changes in dynamic patterns occur, they appear as a consequence of significant changes in the internal or external environments, or both.

Modifications in dynamic mechanisms occur under the following circumstances: (1) When the external circumstances become so stressful that the

adaptive behavior usually used no longer protects against the mounting anxiety. The anxiety thus aroused may lead either to destructive or constructive re-adaptation. Under the influence of anxiety, the organism may be constrained to a regressive pattern or propelled toward the synthesis of a new and more appropriate behavioral response with subsequent extension of ego functions. On the other hand, external circumstances may be supportive so that the anxiety associated with customary adaptive methods is allayed. The usual dynamic patterns may then be relinquished. If the individual uses this type of experience only to allay anxiety, the ego functions are further restricted, *e.g.*, in increasing and persisting dependency. If the interpersonal relation is integrated as a corrective emotional experience, ego development is advanced. (2) Modifications in behavioral patterns may also be anticipated when pathological anatomical changes occur anywhere in the body. The organism then responds either temporarily or permanently by regressive behavioral changes or by denial of the internal change. (3) In special circumstances when the primary executive organ of the ego, the highest integrative centers of the central nervous system, undergoes impaired functions, alterations in the behavior occur as a result of interplay of factors among which are the newly acquired defects, attempts to compensate for them, and, reactivation of latent conflicts. (4) When metabolic processes undergo modification, either through illness or, normally, during critical phases of growth and development and during biological rhythmical activity, these changes are reflected by partial reorganization of the personality functions. (5) In our clinical experience, we have multiple opportunities to see how ordinary events and difficulties reactivate infantile conflicts in especially predisposed individuals. The adaptive pattern may then be replaced by neurotic, psychotic, or psychophysiological reactions. (6) In our therapeutic work, changes in behavioral patterns may follow when a program provides a corrective emotional experience with or without opportunities for the patient to understand the nature and origin of the maladaptive pattern.

These considerations led us to believe that we could expect modifications in behavioral responses in chronic schizophrenic patients when their internal environments were significantly altered. Despite the chronicity of the schizophrenic state, it was anticipated that the manner in which the homeostatic equilibrium is achieved would be reflected in variations of behavioral patterns.

A Conceptual Model of Schizophrenic Reactions

The schizophrenic reaction is regarded as a disorder of adaptation discernible by psychiatric methods in terms of the patients' disabilities to engage in interpersonal relations, disorders of thinking, dissociation of

affect, bizarre motility and speech patterns, and variations in the clinical course. The reaction may be temporary, recurrent, or permanent. It is regarded as an adaptive process available to every member of the human species. The schizophrenic reaction, furthermore, is capable of various degrees of reversibility or irreversibility. The quality of reversibility or irreversibility is related to the time of onset of the psychosis. The reversible or irreversible aspects are also related to another quality of time, namely, the duration of the particular episode. Another consideration is the psychodynamic formulation of the patient's problems which depends upon the clinical psychiatric information available and the body of psychoanalytic psychopathology.

This sweeping concept of schizophrenic reactions is consistent with the biological concepts of disease and clinical psychiatric methods. It has, furthermore, a prototype in biology. I refer to analogies in the evolution of one sub-species to another formulated by Sewell Wright in 1932.[7] A recent editorial[8] in the *American Journal of Psychiatry*, has also suggested the analogy to species, subspecies, and genus.

The research model conceived for the initial phase of our work has a number of dimensions already referred to, but now more explicitly described. The first dimension is conceived as a broad spectrum of schizophrenic disorders represented by the various clinical pictures with varying degrees of disruption of personality functions especially in relation to reality testing. Presumably there are parts on this spectrum which represent schizophrenic reactions that are primarily determined by constitutional or biological factors and others which are prepotently determined by psychological or experiential events. There is a continuity among the primarily biological and the primarily experiential. While for the most part the schizophrenic reaction is regarded as a maladaptive process, there is clinical evidence that for some, the maladaptive process is temporary and permits further expansion of personality functions.[9] Or, not only is there healing with defect but there is also improved adaptation through temporary defect.

The second dimension is concerned with the quality of reversibility or irreversibility. When there are reversible changes, their quality and duration are to be evaluated. Since we have selected to work with chronic schizophrenic reactions, we have accumulated some evidence during the past year of what constitutes irreversibility and what the nature of reversibility, albeit temporary, consists of. The observation that apparently irreversible schizophrenic patients sometimes remit after a prolonged period of hospitalization has not enabled us yet to differentiate clearly between irreversible and reversible signs by clinical psychiatric methods. This problem refers, of course, to the concept of process psychosis. Whether or not

process psychosis is to be correlated with predominantly biological or primarily psychological etiology, is not yet answerable.

The third dimension of our research model is one of time. Time, however, has two qualities. In one sense it refers to the time at which the psychosis manifests itself in the patient's life. The other quality refers to the duration of the illness at the time of examination.

The final dimension made possible by clinical psychiatric methods is that of the psychodynamic estimation of the patient's problems. The scrutiny of the patient's verbal and nonverbal productions and attempts to understand them in the light of psychoanalytic psychopathology have provided us with interviewing techniques which enable us to approach deeply regressed patients. The information which we gather as a result of this psychiatric technique casts important information about the patient despite the fact that we are, at the present time, not able to differentiate psychogenetically influenced patterns from the defensive maneuvers used to allay anxiety. It may be that certain genetic-dynamic propositions cannot be correlated with clinical types in schizophrenic reactions. This hypothesis, however, has not yet been subjected to the tests which Kraepelin set the example for in his careful descriptive and long follow-up studies. This dimension is added to determine the relative usefulness of these concepts in schizophrenic reactions. The psychopathological concepts which we use for psychotic studies were not obtained by classical analytic techniques but derived from studies of neurotic patients, or patients before they became psychotic. In this sense, we may find that a different order of psychopathology may be applicable to psychosis. The analogy suggested is the physicists' attempt for many decades to use the laws of macrophysics for microphysics. Eventually they found that the laws were different.

These are the dimensions of our research model. Its design is to utilize clinical psychiatric data only and to restrict concepts to the biological field. This does not mean that the ancillary disciplines can be disregarded. It does signify that there is yet to be developed a research approach utilizing biological principles alone. It further implies that until the clinical psychiatric methods are exploited within the realm of biology, many interdisciplinary attempts are premature. The model is designed so that eventually the findings of other disciplines may be introduced within the biological framework. Attempts at correlation will then be made in terms of the particular adaptive stage so that the data collected can be more clearly defined.

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PSYCHOTHERAPY—A REVIEW AND AN INTEGRATION

By JULES H. MASSERMAN, M.D.*

In *Principles of Dynamic Psychiatry*,[8] I stated that psychotherapy, like other arts and sciences, had passed through two of its stages of evolution—the mystical and the taxonomic—and was now entering its final "dynamic" phase. Since an infinity of statements, all true in some sense, can be made about any subject this, of course, left our knowledge of psychotherapy in a state of incompleteness measureable by the number infinity minus one.

But what is more disconcerting is that a dialectically antithetical statement can be made with equal validity: namely, that man has always been more or less keenly aware of his desires, capacities and limitations and has therefore in every age epitomized their dynamic interplay in his poetic fantasies, whether these took magico-religious or scientific form. Thus in every developed culture, from the Euphrates to the Arctic, man has projected three categories of gods, representing his own triune nature. The first of these categories was comprised of gods of blind, subterranean passion and fury, called variously Seth, Sin,† Ahriman, Dionysos, Siva, Loki or Beelzebub. To counteract these, there have been demigods more helpfully and rationally regulative of man's behavior here on earth: Amon, Osiris, Zoroaster, Apollo, Brahma, Thor and their beneficent kind. Above all these, however, towered awesome beings who laid down harsh and incontrovertible edicts as to the conduct of the universe: Ra, Ahura Mazda, Zeus, Vishnu, Wotan or Jahweh—They who must be obeyed because They can reward or punish without reason or appeal. Thus we have in man's most ancient personifications the prototypes of what Freud regarded as the forces that govern man called, in what he recognized were neomythologic terms, the Id, Ego and Superego of man's own psyche.‡

Nor were our early ancestors far remiss in their empiric understanding of how to alleviate man's conflicts and anxieties. From the ancient temples of Egypt to modern religious shrines, havens of refuge were provided for the harrassed, the weary and the fearful, and in these hospices nearly everything we now know was then also applied to help troubled men. For instance, Greece and Rome had quasi-religious sanatoria dedicated to Asklepios the Therapist, a demigod who was taught medicine by the centaur Cheiron, himself equipped for the problems and burdens of medicine by

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† The evil god of Babylon, worshipped every seventh day (Sin-day).

‡ "It may perhaps seem to you as though our theories are a kind of mythology, and in the present case not even an agreeable one. But does not every science come in the end to a kind of mythology?"—Freud.[9]

being a horse with the head of a man. Parenthetically, Asklepios and his daughter Hygeia were eventually slain by Zeus for serving (Gr. *theraps* = servant) mankind all too well. Be their (and our) fate as it may, the composite course of therapy pursued in the Asklepiad sanatoria would proceed about as follows:

First of all, the patient professed his adherence to the cult of the Temple and was received into its faith and protection. Next, he was treated by regulating his food (dietetics), his ablutions (balneotherapy) and his exercise (calisthenics), while at the same time his physical status was being improved by the proper application of wet, dry, heat, cold or massage (physiotherapy) and by administering measured potions of sedative, nepenthic and other drugs. In some of these sanatoria, according to Scribonius Largus and Pliny the Elder, shocks to the head from electric eels seem to have been used in intractable disorders, whereas trepanation and cortical operations were by then a familiar derivative of traditional Egyptian surgery. With most patients, however, emphasis was placed on group therapy through music, dramatics and guided discussions, supplemented by individualized sessions in which the patient, reclining on a couch as was the custom of the well-to-do of the day,* hired a philosopher to hear and resolve his perplexities. The topics discussed could well have been the vegetative (psychosomatic), animal (id) and rational (ego-superego) components of the patient's conduct, Plato's ideas as to the hidden role of Eros in his dreams, and how to reorient his daily life so as to re-establish sanity (*sanos* = health) of mind and body. All this occurred in what we moderns like to call the "mystical phase" of psychotherapy.

Nor, in considering the taxonomic efforts in our field, can we justifiably maintain our superciliousness about "old-fashioned systems of classification" in psychiatry. For instance, after all his research into the cerebral physiology of individual learning, Pavlov was forced to adopt a classification of temperaments almost identical with that of Hippocrates and Galen —a system based in turn on a concept of regulatory body humors much like our modern idea of the role of hormones. For that matter, we are now beginning again to appreciate that nosologists like Kahlbaum, Mayer-Gross, and Kraepelin were masterful clinical observers still capable of teaching important facts about human behavior to many of our starry-eyed youngsters impatient to plunge headlong—and without a life-belt—into what they like to call "depth psychology." All in all, taxonomy is an essential phase in our organization of knowledge, and the term is not synonymous with sterility of thought and futility of effort.

* Cf. Socrates' free-association therapy of the reclining Strepsiades in Aristophanes' comedy *The Clouds*. Strepsiades did not want to pay his creditors and planned to capture the moon so there could be no monthly bills.

Finally, we may venture to admit that much of mysticism and unscientific empiricism remains operative in the theory and practice of modern psychiatry. On the basis of a few clinical observations, often superficially interpreted, we still starve, choke, electro-coagulate or slice up irreplaceable brain tissue with a crudity strikingly out of proportion even with our present limited knowledge of the finesse and complexity of cerebral functions. And when the final results of such procedures are undeniably adverse, we say *post hoc* that the patient must all along have belonged to some category of untreatable "mental disease."

Equally illogical, though perhaps less immediately harmful to our patients, is the seductive use of typically mythological thinking in lieu of more precise formulations and operational deductions. This is exemplified in attempts to explain fundamentals of human behavior on the basis of highly selected parables such as those of Narcissus or Oedipus, without recognizing (a) that if the complex interrelationships among other inhabitants of these fables (such as the nymph Echo and Narcissus' lover Almeinas, or Laius, Jocasta, Chryssipus et al *v.* Oedipus) were analyzed, nearly every human relationship would also be epitomized, whereas (b) other, more ancient myths (such as the Egyptian saga of Seth, Isis and Osiris or the Mesopotamian legend of Gilgamesh, Engidu, Ishthar and Uta—Napishtim) are paradigms of filial loyalty, fraternal devotion and social sacrifice that are, culturally speaking, as highly significant to the mores of Western man as are Greek exemplifications of autistic, self-seeking, or murderous rivalry. And so too, in seminars supposedly devoted to the discussion of unconscious dynamics, we are sometimes treated to serious accounts of how in one case "the ego bribed the superego" while "really being in secret alliance with the id," whereas in another instance the "id masqueraded as the superego" and thus "gained an advantage in a bitter battle with the ego" in which it also succeeded in "splitting" the latter neatly in two—all this until a casual visitor might think he were really listening to a quasi-Homeric tale of how three Fates plotted and fought among themselves inside some poor mortal's skull for the control of his body. I am not opposed to poetic license in exposition, but perhaps even in our modern thinking the bright seductive spirit of mystery and fable still shines through the thin, drab Mother Hubbard of pseudoscience in which we pretend to clothe her.

Research and Discovery

Psychiatry, as an objective science of man's behavior, has existed scarcely seventy years, but must, then, we admit that it has added so little to the lore of the ancients as to man's knowledge of man? The answer lies in the deeper meanings of the terms *re-search* and *dis-covery*: if these are sensed, we have indeed, in the last seventy years, partially re-examined man's behavior and uncovered anew some of its determinants. These questionings have occurred both in the laboratory and in the clinic, and each locale has been the scene of invaluable clarifications and reintegrations. We can here survey only with the utmost brevity what some of these contributions have been.

Animal Studies in Psychotherapy

There is one incontrovertible advantage to laboratory work: if one asks intelligent questions the answers one gets from electrons, from chemical compounds and from animals, though bewilderingly more complex in that order, are nevertheless relatively operational and thereby less subject to obfuscation by a prejudiced observer. The problem is, then, to put the answers in their larger contexts—a task particularly difficult in the vast sciences of behavior. Let us review some of the data.

Shenger-Krestovnikova, one of Pavlov's students, first demonstrated in 1913 that experimental neuroses can be induced in dogs by subjecting them to adaptational stresses beyond their integrative capacities ("ego span"),[14] and it was Pavlov himself who advocated the use of bromides as the sole therapy of such states. This work was continued along orthodox Pavlovian lines by Gantt, and extended to pigs and sheep by Liddell.[4]

Beginning some twenty years ago, my associates and I have been particularly interested in elaborating these studies and reintegrating them with psychoanalytic theory and with clinical psychiatry.[9] Since our results have been reported elsewhere,[4-13] I shall note here only that we were able to confirm as "biodynamic principles" various basic analytic postulates such as (1) the relationship of so-called instincts to basic physiologic needs, (2) the importance of individual experience in shaping later patterns of behavior, (3) the role of frustration in eliciting seekings for displaced or substitute satisfactions, and finally, (4) the etiologic importance of motivational conflicts in causing deviations of animal behavior analogous to anxiety states, phobias, compulsions, somatic dysfunctions, regressions and even hallucinatory and delusional observations in man. Even more germane to our present interests, however, were the many methods we tried of alleviating these experimental neuroses once they were established. Of the many investigated, some eight general techniques were successful to varying degrees and in various combinations. In briefest summary, and with their clinical connotations mentioned only *pari passu*, they were these:*

Change of Milieu: A neurotic animal given a prolonged rest (three to twelve months) in a favorable home environment nearly always showed a diminution in anxiety, tension, and in phobic-compulsive and regressive behavior. However, these

* This portion of the article, with modifications, is reproduced from a previous brief summary of the therapeutic significance of the animal experiments.[5]

neurotic patterns were prone to reappear when the animal was returned to the laboratory, even though it was not again subjected to a direct repetition of conflictual experiences. To draw a human analogy, a soldier with severe "combat neurosis" may appear "recovered" after a restful sojourn in a base hospital, but unless his unconscious attitudes are altered, his reactions to latent anxiety recur cumulatively when he is returned to the locale of his adaptive conflicts.

Satiation of a Conflictful Need: If a neurotically self-starved animal which had refused food for two days was forcibly tube-fed so that its hunger was mitigated, its neurotic manifestations correspondingly decreased. Hippocrates is reported by Soranus (perhaps apocryphally) to have utilized a parallel method in human psychotherapy. Hippocrates, it seems, was once called into consultation to treat a strange convulsive malady which was keeping a recent bride virginal. Discerning, after a private interview, that she was torn between strong sexual desires neatly balanced by fear of injury, Hippocrates advised the husband "to light the torch of Hymen" with or without the patient's consent. The results of the therapy are not recorded.

Forced Solution: A hungry neurotic cat was prevented from escaping from the apparatus and instead was brought mechanically closer and closer to the feeder until its head was almost in contact with a profusion of delectable pellets. Under such circumstances some animals, despite their fears, suddenly lunged for the food; thereafter, they needed lesser degrees of mechanical "persuasion" until their feeding-inhibition disappeared altogether, carrying other neurotic generalizations with it. This method is a variation of the Hippocratic one mentioned above, but entails a greater degree of activity on the part of the patient. In some ways, the "therapy" is akin to pushing a boy afraid of water into a shallow pool. Depending on his capacities for reintegrating his experiences (in analytic terms, his "ego strength"), he may find that there was, after all, no reason for fear—or he may go into a state of abject terror and thereafter hate not only water, but pools, swimming—and all future therapists. Because of the latter eventuality, ruthless force is generally considered a dangerous method in dealing with neurotic anxieties.

Example of Normal Behavior: An inhibited, phobic animal paired for several weeks with one who responds normally in the experimental situation will show some diminution in its neurotic patterns, although never to the degree of complete "recovery." In like manner, problem children do better when they have an opportunity to live with "normal" youngsters in an environment that favors "normality"—although more specific individual therapy is nearly always necessary to complete the "cure."

Re-education by a Trusted Mentor: As noted, a neurotic animal, perhaps by the very virtue of its regression to earlier patterns of relationship, becomes exceedingly dependent upon the experimenter for protection and care. If this trust is not violated, the latter may then retrain the animal by gentle steps: first, to take food from his hand; next to accept food in the apparatus; then to open the box while the experimenter merely hovers protectively; and finally to work the switch and feed as formerly without further "support" from the therapist. During its "rehabilitation" the animal not only re-explores and resolves its motivational conflicts but also masters and dissipates the symbolic generalizations that spring from this nuclear "complex": *i.e.*, its inhibitions, phobias, compulsions and other neurotic reactions.

This, indeed, may be the paradigm for the basic processes in much clinical psychotherapy. The neurotic patient channelizes his needs for help toward a therapist upon whom he transfers his dependent and other relationships. The therapist then utilizes this "transference" with optimal patience and wisdom to guide and support the patient as the latter re-examines his conflictful desires and fears, recognizes his previous misinterpretations of reality and essays new ways of living until he is sufficiently successful and confident to proceed on his own. Whether this be called re-education, re-training, re-habilitation or psychoanalysis depends more on the context of the problem, the necessity for thoroughness in anamnestic review and symbolic analysis and the art, skill and effectiveness in the utilization of the fantasied and actual interpersonal relationships involved than on any fundamental differences in the essential dynamics of the respective procedures.

Physio-pharmacologic Methods: As has thus far been indicated, some of the vectorial processes of psychotherapy can be isolated in principle and demonstrated operationally in the laboratory. There remains, however, the fact that various physical methods such as the use of drugs, electroshock, have also proved clinically useful in the treatment of certain behavior disorders. We can here give only the most cursory supplementary review of further experiments dealing with this subject.

Action of Various Drugs: Preliminary tests of the effects of various sedative and narcotic drugs on normal animals showed that, in general, such drugs disorganized complex behavior patterns while leaving relatively simple ones intact. Thus, in one series of experiments an animal was taught in successive stages (1) to open a food box, (2) to respond to food-signals, including signs reading FOOD or NO FOOD, (3) to operate the signal-switch, (4) to work two switches in a given order, and finally (5) to traverse a difficult maze to reach one of the switches. If the animal was then drugged with a small dose of barbital, morphine, or alcohol, it would become incapable of solving the maze but would still work the food-switches properly; with larger doses, it could "remember" how to work only one switch; with still larger doses, earlier stages of learning would also be disintegrated until finally it lost even the simple skill required to open the foodbox. Conversely, as the animal recovered from its intoxication its learned responses were reconstituted in their original order. If the animal was then made neurotic by an adaptational conflict, it developed a new set of highly intricate and elaborate reactions; *i.e.*, various inhibitions, phobias, compulsions, somatic dysfunctions or even sensorial disturbances. These, too, proved relatively more vulnerable to disintegration by the sedative drugs than did the simpler, preneurotic behavior patterns, so that if a neurotic animal was given barbital or morphine, its anxiety reactions and inhibitions were significantly relieved. In effect, instead of crouching tense and immobile in a far corner or showing panic at the feeding signals, it could respond to the latter by opening the box and feeding (in a somewhat groggy but comparatively effective manner) as though, for the time being, its doubts and fears were forgotten.

Drug Addiction: In one variant of these studies in which alcohol was used as the nepenthic drug, the animals which experienced relief from neurotic tensions while partly intoxicated were later given an opportunity to choose between alcoholic and nonalcoholic drinks. To our surprise (and, it must be confessed, subdued delight) about half the neurotic animals in these experiments began to develop a quite unfe-

line preference for alcohol; moreover, in most cases the preference was sufficiently insistent and prolonged to warrant the term "addiction." In further proof of its neurotic basis, the induced dipsomania generally lasted until the animal's underlying neurosis was relieved by the dynamic methods of therapy described above. It seems redundant to discuss the human analogues to these experimental observations.

Tension-relieving Effects: In still another series of experiments we observed that the administration of hypnotic drugs (including alcohol) so dulled the perceptive and mnemonic capacities of animals that they were, while thus inebriated, relatively immune to the neurosis-producing effects of traumatic experiences. In this connection it may be recalled that many a human being long ere this has been tempted, through subversive experience, to take a "bracer" before bearding the boss, getting married, flying a combat mission, or facing other presumed dangers.

Effects of Cerebral Electroshock: In briefest summary, we found that when the ordinary 60-cycle current usually employed clinically was passed through the brain of the animal, the resultant shock acted like an intoxicant drug to disintegrate complex and recently acquired patterns of behavior, whether these were "normal" or "neurotic." Unlike most drugs, however, electroshock produced permanent impairment, however subtle, of future behavioral efficiency, even though this could not be correlated with pathological changes in the brain detectable by present methods. Weaker or modified currents (*i.e.*, the direct square-wave Leduc type) produced lesser degrees of deterioration in our animals, but also had less effect on their neurotic behavior. All in all, these experiments supported the growing conviction among psychiatrists that electroshock and other drastic therapies may be useful in certain relatively recent and acute psychoses, but that the cerebral damage they produce, however subtile, makes their indiscriminate use replete with temporarily hidden cost and potential danger.

Lobotomy, Topectomy, Thalamotomy: Obviously, any cerebral operation will (a) produce a transient general disorganization of perceptive and reactive patterns, and (b) result in a more circumscribed hiatus in the patient's responsive capacities—both effects being of possible therapeutic import. Indeed, recent studies by a number of workers, especially Bard,[1] Pribram,[15] Rioch[16] and their respective associates (as masterfully reviewed in a recent volume by Fulton,[3]) have revealed exciting new possibilities for altering basic patterns of behavior by specific cerebral lesions. Thus, section of the head of the caudate or under Area 13 in the posterior orbital gyrus may counteract otiosity and release spontaneity and responsive activity, although the latter may sometimes take the form of vicious rage. Conversely, lesions in the ventral thalamic-cingulate-hippocampal-amygdaloid circuits of the "visceral brain" may tame and quiet even dangerously aggressive behavior, though perhaps at the cost of peculiarly regressive patterns in which the animal tastes everything within reach yet fails to learn from adverse experience. Some of these findings are being tested clinically by Scoville, Dax and Radley-Smith, Grantham and others in cerebral operations designed specifically for various forms of psychotic behavior, with currently promising results.

Work in this field in our own laboratory indicates that circumscribed lesions in the thalamus and in the amygdalae may disintegrate experimentally induced neurotic patterns and overbalance the corresponding organic loss in adaptive skills by a sufficiently wide margin so that, from the standpoint of survival in a protected milieu, the animal is undoubtedly benefited. Concurrently, we have uncovered one qualification that may eventually outweigh this in basic significance: namely, *that the effects of apparently identical lesions in different animals may vary with the preceding experiences of each*—a circumstance that underlines once again the necessity for dealing with each organism, from the standpoint of both etiology and therapy, as a unique, dynamic entity. In effect, each person behaves differently from every other because (a) he was differently constituted at birth and (b) because he has had different experiences; ergo: (1) he will react uniquely to any given cerebral lesion (2) he will then need rehabilitative therapy specially tailored to fit his frame and modes of action, hide his defects, and best utilize his remaining capacities for optimal adaptation.

Man's Principle Distinction from Other Animals: his Unique Delusions and Ur-Defenses

These then, are the leads culled from comparative and experimental psychology, rich in their implications of future contributions to clinical theory and practice. However, we cannot leave the subject of psychotherapy without paying obeisance to a sacrosanct belief held by some of us that whereas the organs, physiology, and nervous system of other animals are disconcertingly like ours, we differ from them so essentially that studies of their behavior are interestingly irrelevant to the problems of psychiatry. May I submit then, that men *do* differ psychologically from animals, but that the differences consist mainly (a) in the complexity and versatility with which human beings elaborate both "normal" and "neurotic" behavior, and (b) in their possession of several transcendent articles of faith (or delusion) which animals, at least, never verbalize. I have dealt with these Ur-defenses rather extensively elsewhere.[6, 9] They are:

1. *The Ur-defense of personal Invulnerability, Power and literal or vicarious Immortality*, rooted in primary narcissism and never completely surrendered.

2. *The Ur-defense of the Perfect Servant or System* expressed consciously in the belief that one can impose order and security on a universe of chaos and danger (a) by the intercession of omnipotent and omniscient Beings who can be controlled by wheedling, bribery or command much as one once controlled one's parents, or (b) by the invention of mutually exclusive scientific or philosophic systems.

3. *The Ur-defense of "Faith in Humanity,"* derived from the almost equally illogical presumption that because one's mother at one time loved and cherished one, the rest of mankind must be almost equally provident

and indulgent. This expectation of survival through dependence, when combined with ancillary erotic yearnings and experiences, determined many of our interpersonal relationships, including those in psychotherapy.

These are, of course, but bare statements that, taken alone, may sound dogmatic, unconvincing and perhaps strangely disturbing to some. And yet reflection will reveal how many transference phenomena and verbal and other therapeutic maneuvers resonate with these mystic, irrational but universally wishful and therefore ubiquitous beliefs, and consequently how explosive the result is of any attempt to traduce or abolish them either in an individual or in a society. While it may be too cynical to propose outright that therapy consists in re-establishing these and other of man's essential delusions in proper working order, wise psychiatrists eventually learn—along with wise teachers and ministers—that truly to help a man, one must help him rebuild his own universe of *useful* fact and fancy and, largely on his own terms, his own faith in himself, in his fellowman and in his personally conceived God.

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BOOK NOTICES

The Life and Work of Sigmund Freud. By Ernest Jones, M.D. $6.75. Pp. 428. New York, Basic Books, 1953.

To write a biography of a contemporary colleague who has already taken his place as one of the greatest men who ever lived is a task that only another great man would dare undertake. Dr. Jones has himself contributed many things to science and to the world but this is undoubtedly his masterpiece. It represents enormous research, selection, investigation and organization, and demonstrates astute and penetrating judgment and interpretation. Its content is clearly indicated by its title. Its clarity and beautiful diction are in keeping with its scope and purpose. No one but Ernest Jones could have written it. And the scientific world, particularly psychiatry and psychoanalysis, will be eternally grateful to him. For every psychoanalyst, for years to come, this book (and no doubt its succeeding volumes) will be imperative reading.

For the first time, perhaps, Freud ceases to be an oracle, a wizard, a saint, a super-father, a Messiah, an obsessional neurotic, or God knows what I and others have made him; he becomes a human being with peculiar incentives and peculiar gifts, who happened upon discoveries which he had the persistence and the brilliance to work away at until they could take sufficient shape for others to pursue the ultimate goals by similar methods. (K.A.M.)

The Collected Papers of Otto Fenichel, First Series. By Hanna Fenichel and David Rapaport. $6.50. Pp. 408. New York, Norton, 1953.

We must be grateful to Drs. Hanna Fenichel and David Rapaport for the careful editing and publication of the collected papers of the late Otto Fenichel, the first series of which has now become available. Fenichel's wide scope of interests, his technical and theoretical mastery of psychoanalysis, his deep devotion to psychoanalytic work, are well documented in this volume covering his work from 1920 to his untimely death in 1946. His papers are milestones in the development of psychoanalysis, and one may hope that many present-day students will find in this collection stimulus toward a renaissance of the erudite and creative phase of psychoanalysis of which Fenichel was such an important and dedicated leader. (Rudolf Ekstein, Ph.D.)

On Aphasia. By Sigmund Freud, translation by E. Stengel, $3. Pp. 108. New York, International Universities Press, 1953.

This essay of Freud's, first published in 1891, has been unobtainable for many years. This, the first translation into English, has been made by a competent psychiatrist, neurologist, and psychoanalyst who read a paper at the London International Congress of Psychoanalysis on the strong influence of Hughlings Jackson's genetic and regression principles upon Freud's thinking. Stengel points out that projection and cathexis, terms introduced by Meynert, were first employed by Freud in this essay. Freud's preference for concepts implying dynamic processes rather than static conditions is conspicuous throughout the book. (K.A.M.)

The Interpersonal Theory of Psychiatry. By HARRY STACK SULLIVAN. $5. Pp. 384. New York, Norton, 1953.

The material of this book is derived from the lectures and notes of Harry Stack Sullivan which he prepared shortly before his untimely death. Inasmuch as Sullivan's conceptions changed from time to time, this can be considered as his latest statement regarding those aspects of human behavior with which he was most concerned. For many years he was preoccupied with his studies of schizophrenia which in turn led him to reflect intensively regarding the earliest developmental phases of life. It is primarily these studies which are presented here.

The core of Sullivan's theory is summarized in the introduction by Mabel Blake Cohen as "the psychiatry of interpersonal relations or as the study of communications between persons, or as the operational approach to psychiatry in which the psychiatrist plays the role of participant observer. It rests on the propositions that: (1) a large part of mental disorder results from and is perpetuated by inadequate communication, the communicative process being interfered with by anxiety; and (2) each person in any two-person relationship is involved as a portion of an interpersonal field, rather than as a separate entity, in processes which affect and are affected by the field."

In his studies, particularly of language, symbols and communication, Sullivan drew heavily upon anthropology and the other social sciences. Although he stated that behind much of his theories lay the discoveries of Freud, there is very little evidence of this. Except for his acceptance of the concepts of conscious and unconscious processes one can find little psychoanalytic theory. In fact, a number of psychoanalytic postulates are flatly rejected. He declares that dreams under no circumstances can or should be dealt with directly in psychotherapy because there is an impassable barrier between the "covert operations when one is asleep and covert operations and reports of them when one is awake."

That Sullivan was a gifted observer, creative theorist, skilled clinician and effective teacher is unquestioned. However, it is regrettable that he chose to ignore, if not reject, the work of so many other psychiatrists essentially sympathetic to his approach. (Lewis L. Robbins, M.D.)