THE PSYCHIATRIC EVALUATION INDEX

Potential Use in Assessment of the Results of Psychotherapy*

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INTRODUCTION

Many psychiatrists and other behavioral scientists espouse, apparently, the pessimistic assumption that only completely negative results have emerged from competent objective investigation of behavioral, affective, or mentational changes associated with psychotherapy. As Reznikoff and Toomey (1) have demonstrated in their comprehensive review of the many studies relating to this problem, the above supposition is simply not true. There are, in fact, numerous well-designed and adequately performed studies in the literature which clearly indicate that certain personality changes *are* associated with the process or the outcome of psychotherapeutic interaction. Unfortunately, numerous other studies indicate equally well that there are *no* such changes, or if there are, that these changes are coincidental to the psychotherapeutic process *per se*.

Nevertheless, those of us who conduct psychotherapy as part of our day-to-day professional duties are generally convinced that definite, positive, and socially desirable changes do accompany, and result from, our efforts, at least in a sufficient number of patients so that we are motivated to continue using this technique. Because of such personal clinical experience, most therapists believe that the psychotherapeutic process is a valid and helpful one, and that the conflicting results in research studies arise from inadequate hypotheses or inappropriate methods of testing hypotheses, or from all the other theoretical and practical difficulties in collecting and assessing relevant data which have been outlined by Reznikoff and Toomey.

We should like to suggest that the difficulty is not primarily one of bias, nor of poor applicability of the scientific method to this kind of problem; rather, it lies in the fact that clinical experience with psychotherapy is essentially idiographic, relating to an intensely personal longitudinal interaction between the therapist and his patient, whereas most

* This paper is one of a series, taken from a larger study of mental health problems of older persons, supported in part by a grant from the Missouri State Division of Health.

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Reprinted from AMERICAN JOURNAL OF PSYCHOTHERAPY, Vol. XVIII, Supplement 1, pages 3–14 and 61–63. March, 1964.

scientific studies have been nomothetic, relating to the broadly actuarial measurement of certain behavioral phenomena. Such phenomena may or may not be related to the personality characteristics and levels in which the clinician is interested, and nomothetic studies, at best, tend to involve assignment of phenomena to gross, concrete classes which often are related only poorly to what the clinician has in mind (even though it may be extremely difficult for the clinician to define *his* criteria of change or "improvement" in other than vague and general terms).

In this, as in all scientific investigations, it is a primary necessity to define the terms involved. The basic problem is one of clinical epidemiology: The investigator deals with a number of cases of phenomenon X, appearing in the population at risk (that is, the patients being treated). This population is then exposed to a technique, psychotherapy, and at various points along the way, and at the end of therapy, and perhaps several months or several years later, the number of cases of phenomenon X in the population at risk is measured again. Obviously it is of vital importance that the investigator first rigorously define the phenomenon X and the population at risk which is being considered, and also equally rigorously define the technique (psychotherapy) which is being used. The nature of these definitions is unimportant as long as the investigator is able to communicate them clearly, so that the conditions of the experiment can be replicated by other investigators under other situations. The independent variable, psychotherapy, and the dependent variable, phenomenon X, should then remain relatively comparable from investigation to investigation. This model does not necessarily provide for control of intervening variables, but random sampling and other competent methods of dealing with such variables—with changes brought about, for example, by environmental influences or by the self-limiting process of emotional disorder—have been discussed competently in earlier articles by Dressel (2), Gordon *et al.* (3), Grummon (4), Hobbs and Seeman (5), McNemar (6), Miller (7), Rosenthal and Frank (8), Schofield (9), Watterson (10), Zubin (11), and others.

Many ways of defining and measuring "phenomenon X" have already been tried. These have included clinical judgments, psychologic tests, physiologic measures, content analyses, and intraindividual measures. The aim of this article is to discuss the potential use of a new intraindividual measure which may well prove especially applicable to this purpose. This measure, developed by the authors as a new test instrument for behavioral research, is called "The Psychiatric Evaluation Index."

Intraindividual Measures and Q-technique

Meehl's brilliant analysis (12) has indicated that both clinical and sta-

tistical prediction have advantages and disadvantages. Although Meehl found actuarial studies to date more promising than clinical ones, Holt (13) and others have subsequently pointed out that the evidence reviewed by Meehl favored the actuarial procedures. Nevertheless, the dilemma (which of two imperfect techniques is best for our purpose) remains, but a possible escape from the horns of this dilemma involves describing the individual patient in the context of a frame of reference that is largely his own. By comparing several measures of the same individual patient on two or more occasions or from two or more points of view, data can be derived which are objective, reliable, and valid, as well as being amenable to subsequent statistical manipulation. By using factor analysis and analysis of variance, an idiographic approach can result in a description not only of the dimensions unique to a single person, but also of the factors common to a group, or to a technique such as psychotherapy.

One type of intraindividual measure which appears to be especially useful is the "Q-sort," in which the subject or rater may assign a variety of descriptive statements to several classes from "least like" to "most like" the subject, commonly in a quasi-normal distribution. This can be done by the patient referring to himself, by the therapist referring to the patient, or even by an outsider (such as a relative) referring to the patient. Comparison and analysis of two or more of these sorts at various times can thus provide considerable information useful for objective assessment of behavioral dysfunction as related to the concurrent variable of the psychotherapeutic process. As Stephenson has noted (14), such measures are "subjective" in the sense that they may involve introspection and self-observation by the patient, but they are "objective" in the sense that the data thereby obtained can be dependable, replicable, and indicative of attainable "constant relations."

Stephenson has suggested our *modus operandi*. One can begin with any *one* clinical subject, perhaps a young woman. She is provided with a Q-sample of pertinent statements, and then performs a series of Q-sorts. Eventually there will be a series of Q-sorts to consider, sorts which are specific for *this* patient in *this* particular time period. The matrix of Q-sorts can be factorized, often providing several orthogonal factors. As Stephenson has said, "It can readily be shown that the factors are as objective as the color of this young woman's eyes and the tilt of her nose. Moreover, they are specific to the case—no one else could provide them" (15, p. 102).

The use of such Q-technique methods permits tests of certain types of propositions even though only one rater or one subject is used. Stephenson, however, believes that the composition of the Q-sample should derive from the subject himself (possibly a group of statements actually made

by the subject during therapy), and have reference specifically to him alone. He also feels that the conditions of instruction should vary with each administration (for example, on one day the patient might be asked to describe himself as he is, and on another day to describe himself as he would like to be, on a third day to describe himself as he thinks the therapist thinks he is, and so on). However, we do *not* believe in the necessary correctness of Stephenson's statement: "If the method is extended to another case, and then another, even a *comparative* methodology is impossible, since the same Q-sample cannot be used for different cases (all have different histories and make different references to self)" (15, p. 102). Our experience has indicated that the *same* Q-sample, with similar conditions of instruction, can be used for a large number of subjects, providing at least some similar factors; clinical observation repeatedly indicates that all psychiatric patients have some factors in common, as well as unique factors, and a number of studies have indicated a certain communality about the psychotherapeutic process, no matter by whom it is practiced. (See, for example, references 16 through 21).

The question then becomes, whence and in what context should the Q-sample be derived. It seems to us that rational, effective programs of therapy must be based on a profound understanding of the kinds of *problems* for which psychiatric patients seek professional aid. The variety of psychotherapeutic techniques considered "most useful" suggests a prevailing lack of knowledge about the structure of the psychiatric difficulties presented by patients, and indicates a need for better information about and understanding of the dynamic psychologic problems of patients before definite therapies are formulated. An obvious and pressing task, then, would be the development of an assessment technique relevant to the objective description of the problems of psychiatric patients, as seen both by the patients themselves and by the professional persons observing such patients.

It is true that Thorne (22) and others have indicated that the use of patients' "symptoms" or "complaints" as measurement criteria is inadequate, but Mosak (23) has pointed out that to discount symptomatic changes as "superficial" may be to discard information about the real effects of psychotherapy, and Pascal and Zax have argued, "If the ultimate purpose of psychotherapy is to effect behavioral change (and it has to be), then *behavior* must be the criterion" (24, p. 330). The World Health Organization Expert Committee on Mental Health (25), in dealing with such criteria, recommended that symptoms be used as units of observation, since such units could then be organized into identifiable and useful classes of data.

At any rate, since 1926, when Adolf Meyer presented his plan for the

reorientation of psychiatric diagnosis in terms of the patient's chief complaints, clinicians have emphasized the importance of symptoms in making accurate diagnoses and prescribing appropriate treatment. Whitehorn (26) has pointed out that the patient comes to a medical facility for relief of his symptoms and assumes that if these symptoms are alleviated or adequately modified, he may be able to function successfully within the community. Rechtschaffen (27) has noted that many therapeutic techniques used in dealing with older patients are unsuccessful because they are nonreactive to the patient's *own* view of his difficulties and their origins. Magraw and Dulit have stressed the need to consider the patient's problems *as they are seen by the patient.* ". . . What is bothering the patient does more than *point* to what the patient has. In a very real sense it *is* what he has. It is the diagnosis. It is the illness. . . ." (28, p. 335).

In our preliminary survey and analysis of several hundred clinical records of patients seen at a municipal psychiatric clinic (29), it became apparent that the presenting complaints of these patients were the most obvious and objective measures of their problems. In later studies (30, 31, 32), we found that logical analysis of patients' complaints assessed by objective techniques could result in the establishment of reliable relationships between the behavior of complaining and age, sex, diagnosis, and other medical, social, and psychologic variables, and that more complex levels of criterion change could be inferred easily from such assessments. All patients referred to a psychiatrist or related therapist appeared to have some sort of individual psychologic problems, and the symptoms and complaints of these patients appeared to be significant indicators of their problems.

Early in our investigations, however, it became apparent that a major limitation in simply using unstructured symptoms from case material was that the recorded complaints of the patients are already second hand. The recording therapist who interviews the patient is, of necessity, an intervening variable. The therapist's selective perception is unavoidable, no matter how objective and accurate he tries to be. Moreover, even in the most nondirective interview, the patient's selective perception about what to tell the therapist, intervenes and causes distortion in reporting. One method of decreasing such distortion is to give each patient equal opportunity to select from an extensive group of representative complaints (or symptoms or signs) those which he feels apply to him, and to identify *degrees* of applicability. (The subject is not asked to indicate, "Yes, this complaint is like mine," or "No, it isn't," but rather to indicate which statements are "most similar" or "least similar" to his own complaints *in comparison with all the other complaint statements included in the sample.*)

Using such representative complaints in a Q-sample allows one to deal, statistically, with a population of complaints rather than one of people. While there may be considerable differences among individual patients with respect to their specific complaints, one may still propose with some confidence that these differences will be on dimensions which are to be found in patients in general, since the complaint sample is appropriately selected for just this purpose. Instead of collecting large amounts of raw data and correlating measures for individual patients on certain arbitrary or *a priori* dimensions, one instead collects statements based on empirically derived but hypothesized dimensions and asks individual subjects or raters to rank these statements. The analysis of variance is then used to determine whether such distinct dimensions do in fact exist, and a balanced design is used to test for the effect of certain characteristics of different subjects or raters. When a subject or rater repeats his ratings at different periods of therapy or with different instructions (for example, in the case of the rater, assessing different patients), it is possible to compute correlations among such ratings which may be factorized to see whether a stable structure emerges which is characteristic for the hypothesized dimensions.

The promise of this method lies in the nature of the data language utilized. The complaints involved are the ones which bring the patient to professional attention and as such are readily understood both by the patient, in self-description, and by the professional worker receiving such information. The model of a basic data language which can be used by different raters merely by a manipulation of instructions, already demonstrated to be productive in the area of self-concept study, can then be extended into the area of psychiatric (or behavioral) description.

The Psychiatric Evaluation Index

Our first step was to examine 603 consecutive "closed" case records of outpatients seen at a psychiatric clinic. All presenting complaints were noted and these complaints were then sorted into different categories until a meaningful system of classification emerged. In contrast to studies such as those by Lorr and Rubinstein (33) or Tatom (34), we were not concerned with classification into a given nosologic system of psychiatric diagnosis, but rather with meaningful assignment of verbalized complaints to a system on which agreement by both patients and professional raters would be possible.

As a consequence, an original two-dimensional system of complaint classification emerged. These dimensions were called the "determinants" and the "referents" of each given complaint. The determinant identifies the dominant or major characteristic of the complaint (that is,

its specific nature or type). The referent identifies any situational factor which the complainer relates in any way to the determinant (as the attributed cause or result of the determinant, or as co-existing with it). The seven determinants and five referents found relevant for classification are given below:

DETERMINANTS

1. *Affective:* Expressing mood disturbance (as, for example, elation, depression, discouragement, irritability).

2. *Anxietal:* Expressing anxiety consciously perceived and directly felt (as "nervousness," "uneasiness," "fearfulness," "worry") or indirectly expressed in terms of *thoughts* which are obsessive or phobic.

3. *Behavioral:* Expressing disturbance manifested by overt action, or by changes in overt action patterns.

4. *Mentational:* Expressing disturbance pertaining to intellectual functions, memory, orientation, or judgment.

5. *Reality Distortional:* Expressing gross failure in evaluating external reality, as evidenced by hallucinations, delusions, or autistic or paranoid thinking.

6. *Social Welfare:* Expressing only a desire for aid in changing a specific situation which is not primarily medical or psychiatric.

7. *Somatic:* Expressing a disturbance which the patient typically considers to be physical in origin, and for which he would be likely to seek medical rather than psychologic or psychotherapeutic help.

REFERENTS

a. *Physical Health:* Referring to bodily health or illness.

b. *Mental Health:* Referring to psychologic health or disturbance.

c. *Economic-Occupational:* Referring to financial or occupational situations.

d. *Interpersonal:* Referring to situations primarily in terms of relationships with other persons.

e. *Nonsituational:* No related situation specified.

An example of an item which fits category 1a is: "I am discouraged because I have headaches." The determinant in this item is of an affective nature (that is, "I am discouraged"), and "because I have headaches" refers to "physical health."

The combinations of the determinants and the referents provide 35 possible classification categories. All complaints were sorted into these categories and a balanced Q-sort sample was composed by selecting two representative complaints from each category. This gave a total of 70 complaints which could now be used as a common data language for the systematic assessment of individual complaint behavior. (See reference 35.) This representative complaint sample was named "The Psychiatric Evaluation Index" (PEI).

The PEI provides the patient with a structured group of complaints, many of which he finds relevant to his own behavior but which he might forget or ignore if they were not presented to him. The patient is asked to rank-order the sample of complaint statements, by means of a Q-sort method, in the order of applicability of these complaints to his own behavior. The PEI may be and has been used to permit professional persons evaluating the patient to rank the patient's complaint behavior as it is observed (36), and in studies concerning the relative severity of complaints. The PEI thus provides a relatively simple and clear-cut common data language for the different professional groups involved in dealing with psychiatric patients, and for the patients themselves. It should be stressed that this data language is not composed of an artificially constructed set of terms, but is one empirically derived from the actual complaint behavior of psychiatric patients.

Completed studies and investigations in progress indicate that the PEI has a high degree of internal consistency and is both meaningful and useful in differentiating different kinds and degrees of psychiatric problems in older persons, in relating such problems to concurrent variables, and in formulating new theoretical constructs relative to the assessment of behavioral dysfunction. While our findings were limited by the nature and size of our samples, they did suggest that the PEI can be useful also as a predictor of future social functioning and as a relatively simple technique for the longitudinal study of complaint behavior in psychiatric patients at all adult age levels (37), indicating the applicability of this new instrument in recording a systematic approach to the process of evaluating concurrent psychotherapy. The particular design of the PEI lends itself nicely to hypothesis testing studies with small samples (38).

DISCUSSION

It must be understood that we are suggesting an approach, not reporting a *fait accompli*. We have not yet utilized the PEI as a measure for the evaluation of changes associated with psychotherapy. Prior to such a study, it seemed to us necessary to gather further data as to the reliability and validity of this technique when applied to larger and more heterogeneous samples, with a view to refinement and possible reduction and simplification of the present classifications. When these studies, now in progress, are completed, we plan to utilize the revised PEI as follows.

A patient beginning therapy—and it can be any individual patient—would be provided with the PEI for sorting prior to beginning therapy and at arbitrarily selected intervals during the course of therapy. Using the same Q-sample, the therapist also would describe the patient at the beginning, during the course, and at the end of therapy. At that time

we would then have two sets each of a longitudinal series of Q-sorts. By analysis of variance and factor analysis, it could then be determined which factors were specific to the patient, which were specific to the therapist, and which were specific to the process.

Even this first study, of one patient interacting with one therapist, would provide important information about what occurred during the therapeutic process. Repeating the technique, however, would demonstrate, in time, factors that are common to patients in general, factors that are common to therapists in general (or to a particular therapist as he treats different patients), and factors that are common to the process and the outcome in general. One could begin to define changes in complaint patterns at various crucial stages of therapy and perhaps relate such shifts to events in the therapeutic process. One could begin to answer questions such as: Does the pattern of the patient's self-sorts (that is, the way the patient sees his problems) become increasingly congruent with the pattern of the therapist's sorts (that is, the way the therapist sees the patient's problems)? Will certain events in therapy affect certain complaint category scores (for example, raise or lower the measured level of anxiety), and if so, which events? Does "movement" in therapy, defined by external criteria, relate to changes in the patient's estimation of his own problems? And, finally, does the PEI pattern of a patient who has "improved" during therapy move in the direction of the type of sorts made by "normal" persons? (Normative data are being collected currently.)

Data arising from the PEI appear to be *psychometric* according to Meehl's use of that term (12, p. 15): The PEI provides a systematic behavior sample having four cardinal properties, namely, standardized conditions of administration, immediate recording of behavior or behavior products, objective classification of responses ("scoring"), and (in the future) norms. And the resulting data can be treated statistically (in Meehl's sense). The whole operation, except for the sorting itself, can be handled by clerical workers and digital computers. As Stephenson has said, "The scoring is not only objective, but remarkably so—all scores turn out to be pure numbers in *standard terms,* free from all other conceivable units. . . . The immediate record of the subjectivity is reduced, by the elegant devices of statistical method, to standard terms, to pure numbers as we have said, which remove the bewildering units of measurement that plague the actuarialist" (15, pp. 102–103).

That errors might arise from unconsidered intervening variables, and even from the introduction of the technique itself, is certainly possible. That this technique will not tap the "deeper personality changes" or "levels of adjustment" which might be associated with psychotherapy is

also possible. The technique we have described, however, is one of the few which can satisfy both the clinician with his idiographic orientation and the research scientist with his emphasis on nomothetic data. In addition, the method is relatively simple and efficient. Whether it is productive remains to be demonstrated.

SUMMARY

A review of the literature suggests that current studies are contradictory as to whether or not definite changes in personality or behavior are clearly associated with psychotherapy, and as to the best methods of evaluating those changes. Since the difficulty may lie, at least in part, in the clinician's experience of psychotherapy as essentially idiographic, and the scientist's study of the process or outcome in nomothetic terms, the relevance of intraindividual measures of the dependent variable is emphasized. Q-technique utilizing a sample of items derived empirically from the verbalized problems presented by actual psychiatric patients provides a common data language especially applicable to this purpose. The potential value of such a research technique, as exemplified in a new test instrument called "The Psychiatric Evaluation Index," for objective assessment of behavioral dysfunction related to the concurrent variable of the psychotherapeutic process, is outlined.

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DISCUSSION

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I have been impressed not only by the enthusiasm but by the evidence that we have heard this morning—evidence that change during psycho-

therapy apparently takes place using a great variety of kinds of psychotherapy. We have heard that rogerian, adlerian, group, dynamically oriented, minimal contact, and reciprocal inhibition therapy all cause change. I think that such change does indeed occur, and that this effect has to do with the importance of skilled communication. After all, there are only three substantive techniques that a therapist, a *physician*, can use to treat any kind of illness. He can apply physical force to body tissues, as in most surgical procedures and also in electroshock therapy. He can administer drugs, chemical substances, internally or externally. Or he can communicate with patients. And those of us here this morning are especially concerned with the importance of communication.

Evidence has been presented that much of the change associated with psychotherapy takes place in the first 20 to 30 interviews, which suggests that, at least in many cases (I agree with Dr. Wolberg that it is not true in *all* cases), short-term therapy, of the nature that psychiatrists like Jules Coleman and Jerome Frank have been emphasizing for a long time, can be very effective.

Many of the speakers and discussants commented that we were concerned to a large degree this morning with symptomatic change as a measure of the effectiveness of psychotherapy. I certainly agree with Dr. Zubin that symptomatic change as such a measure may well be superficial. John Whitehorn, whom I consider to be one of the wiser of the elder statesmen in psychiatry, has pointed out this difference in patients' and therapists' and communities' expectations. The patient wants his symptoms to be alleviated; the doctor wants to "cure" the patient's illness, which implies deeper change; the community simply wants the patient to be rehabilitated, so that he can get along with his neighbors, make a living, and pay taxes.

Symptoms, however, are the most obvious phenomena we can deal with. They provide a start for measurement. I don't believe they'll prove to be, in the long run, the important bases for measurement, but at least they provide useful information in the beginning. Now Nash's group does cast some doubt on the efficacy of using symptoms as a basis for measurement. However, there was some suggestion in their study that the placebo effect with medication seems to be rather similar to the placebo effect with psychotherapy, and that the placebo effect in *either* case may not hold well. I think there may be some important implications toward this point of discussion in that finding.

In this respect, in regard to Dr. Wolpe's paper, I'm wondering if perhaps it is the communication, the enthusiasm, the authority imparted by the therapist which is the important factor, rather than the specific technique. It is interesting that Dr. Wolpe placed the median number of

interviews in most of his therapeutic histories at 23 and that Dr. Shlien showed that even when *different kinds* of therapy were used, the greatest amount of improvement in self-esteem took place in the first 20 to 30 interviews. I wonder if reciprocal inhibition as a special technique may be most effective in dealing with what may at least appear to be comparatively simple conditioned reflexes, such as those concerned with penile erection.

Communication may be the important common factor in Dr. Nash's study as well, since the therapists involved were psychiatrists who *talked* with the patients. Ideally, I suspect, in such a project the patients should just pick up their pills at a desk. In one study we did several years ago, evaluating the use of a new tranquilizer, dimethylane (which proved to be of little therapeutic value), the therapist was limited to saying to the patient, "How are you," asking several questions from an inventory, and saying "Goodbye, I'll see you next week" without making any further comment. And we found that if the therapist did just that and then handed the patients either a placebo or a medication, the patient got a little better, but if he didn't give them the pill along with this kind of essentially *un*skilled communication, the patients didn't get better.

Finally, I would like to answer the several specific questions which were addressed to me. Dr. Wilder's question: From what kind of records were the symptoms in the Psychiatric Evaluation Index derived? They were derived from the patients' presenting complaints as listed in the standard case records made by residents and staff psychiatrists. In other words, the symptom items we used were simply those with which the patient answered the question, "What brings you to the clinic?"

Secondly, did the individual subjects receive help in Q-sorting? No. They were just told how to sort mechanically, in terms of putting one card on this pile and three cards on the next pile, and so on.

Thirdly, Dr. Garr's question: Can one do statistical studies of failures in psychotherapy? As the discussant from Detroit indicated, the answer is yes, one can. My associates and I have done such a study and turned up interesting if not clinically significant data, such as the fact that women drop out of therapy more frequently than men. What the implications of such a fact might be we don't know.

Dr. Zubin raised one criticism of our work which, interestingly enough, is exactly the same criticism raised by William Stephenson—that is, that our Index doesn't include the assets as well as the problems of the patient. We quite agree with this point and we are working on the solution, but it's much harder to find out what the assets of a patient who comes to a psychiatrist are, than it is to find out what the patient's complaints are.

In conclusion, I would like to make a plea on behalf of statistical re-

search. There has been some suggestion that it may not be the most effective kind of research, but it can certainly be useful and informative. After all, this is what scientific research is: Finding the answers to questions by techniques which can be replicated by other investigators. And I think that in statistical research we can at least find our beginning.