DOCUMENT RESUME

ED 141 730 CG 011 577

AUTHOR Sparta, Steven
TITLE The Monitoring and Follow-up of Counseling and Psychotherapeutic Treatment: A Step Toward Accountable Methods.
PUB DATE Apr 76
NOTE 24p.; Paper presented at the Annual Meeting of the Western Psychological Association (Los Angeles, California, April 8-11, 1976) ; Some pages may be marginally legible due to print quality of the original document

EDRS PRICE MF-$0.83 HC-$1.67 Plus Postage.
DESCRIPTORS *Accountability; *Counseling; *Evaluation Methods; *Followup Studies; Higher Education; Interpersonal Relationship; Interviews; Literature Reviews; Longitudinal Studies; Problem Solving; *Program Effectiveness; *Psychotherapy; Self Concept

ABSTRACT

Counselors and psychotherapists have a moral obligation and some practical reasons to make demonstrable the effects of their treatments. The followup study is a commonly used method whose use is nevertheless unsystematized. This paper describes a followup study in which the author contacted by telephone and interviewed 11 former clients. He analyzed the results and used them in combination with a review of the literature, to generate ideas about the utility and accuracy of followup studies in general. He stresses the need for repeated, accurate measurements. He warns against the use of generalist treatments on uniform clients, and proposes instead a more basic approach that illumines the treatment process more clearly. (Author/BP)

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ED141730

The Monitoring and Follow-up of Counseling and Psychotherapeutic Treatment: A Step Toward Accountable Methods:

Steven Sparta

University of California, Los Angeles
Veterans Administration Outpatient Clinic

Presented at the annual
meeting of the Western Psychological
Association, Los Angeles Cal.
April, 1976

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ABSTRACT

Counselors and psychotherapists have a moral obligation and some practical reasons to make demonstrable the effectiveness of their treatments. Out of these concerns, psychologists are giving more attention to explicating and evaluating their treatments. Among some of the older methods recommended in the literature that have accountability relevance for treatment, the follow-up study has long been mentioned. However, information concerning the use of follow-up studies in counseling has remained unsystematized. This study reports the results of an exploratory follow-up study intended to generate ideas about its utility in serving accountability concerns. The results of the study plus a search of the relevant literature suggested ways for the conducting, monitoring and follow-up of treatment. The described procedures were then examined for their evaluative significance in counseling.

Counselors and psychotherapists face their most serious challenge in demonstrating the effectiveness of their treatments. The subject of treatment efficacy began in earnest with the publication of Eysenck's 1952 paper, and the controversy concerning efficacy (or lack of it) has not yet abated. This question alone is serious enough to warrant ethical concerns among psychologists, but even more disturbing possibilities exist. In an exhaustive review of psychotherapy effectiveness, Bergin (1971) concluded that there are probably good results from some treatments, bad results from others, and overall, a cancelling effect which averages out to a misleading total figure. The possibility of deleterious effects underlines the importance of using only tested and demonstrably effective treatments. Until the goal of using only tested treatment interventions is accomplished, it can be argued that counseling or psychotherapy is an abstruse lore where one man's opinion - whether trained or untrained - is as good as the next (Sorenson, 1967).

What should have been a moral imperative on the part of practicing psychologists has now become a very practical state of affairs. A portion of the zeitgeist of the American public is reflected in the rising consumer movement, and as a consequence words like "accountability" have become increasingly common. Psychologists cannot afford to ignore this situation.

Out of the above concerns, psychologists are giving more attention to explicating and evaluating the treatments they use. Among some of the older methods recommended in this area, the use of follow-up studies has long been mentioned in the literature.

Its supporters have included Eysenck (1966), Bergin and Strupp (1970), Williamson (1952) and Fiske (1974), but information has been unsystematized. In the direction of attaining accountable methods, the present study reports results of a follow-up study. The study was intended to generate ideas concerning the usefulness of the follow-up as a step toward accountable methods. The results of the study plus a search of relevant literature suggest ways of conducting, monitoring and follow-up of treatment and how such methods may be useful in an evaluative context.

Literature Review of Follow-up Studies

In connection with follow-up procedures in therapy outcome studies, Bergin (1971) reported that there have been consistent deficiences. For one, follow-up does not usually include intensive observation procedures such as those that occur during the therapy period. If the period of time between treatment termination and follow-up goes unobserved, one never knows what are the long range effects of treatment. This occurs because there is no precise determination of what the intervening influences may have been that affected the client's status. Periodic fluctuations in posttreatment functioning could be plotted, and environmental events and interpersonal encounters could be correlated with these fluctuations. The contribution of such inquiry would be to cast more light on the natural history of the condition treated, and to reveal more accurately which changes can be validly attributed to the influence of the treatment.

Follow-up study is well suited for investigating long-term goals of treatment. This may be of some significance if the desired behavior is not expected to manifest itself until some point in the future. Since the comparison of behavior from one time period to another can be methodologically cumbersome, as it usually is when investigators attempt to make post-hoc evaluations of treatment, it is important that the investigator collect measures of pre-treatment and during-treatment functioning. Without a baseline from which follow-up data can be compared, there is no reasonable way to make inferences concerning client behavior change over time. This point will be elaborated upon in the discussion of the model.

The usefulness of follow-up has been mentioned numerous times in the literature, but serious investigation has been almost nonexistant. Eysenck (1966) stated that follow-up research has been neglected by the researcher and therapist alike, and advocates the systematic investigation of therapy outcome. Bergin and Strupp (1970) argued for research that would specify "...mechanisms of change, their experimental refinement, and their practical elaboration." Eysenck further noted that the follow-up was important for study of long term effects that would allow for more precise investigation of post-therapeutic improvement or regression.

The usual follow-up study examines a program or treatment at some future point in time in order to determine if the clients have acquired some set of specified skills, changes in beliefs, or changes in affective states. In addition, the follow-up may

investigate whether some attained change at the conclusion of treatment will remain stable over time. This type of follow-up essentially follows an ex-post-facto design, whereby the experimentor attempts to relate, after the fact, the treatment to an outcome recorded at some time after the administration of the treatment.

Noting the lack of using follow-up studies, Kremer (1970) maintains that it is probably the single most neglected guidance function, and mentions three possibilities for this: a lack of time on the part of counselors to devote to this kind of work; a lack of money budgeted for this kind of work; and a lack of secretarial work to accomplish the paper work involved. Hutson (1968) holds that follow-up studies are made only sporadically, that they are poorly done, and that few institutions or people conduct them on a regular basis.

Concerning the methodological side of conducting follow-up studies, much of the literature reveals that the prevalent measuring device is the questionnaire, even though this type of measurement device is prone to many hazards in the kind of data it produces. Accounting for non-responders is another problem, since incomplete samples most likely yield biased results (Rothney and Mooney, 1952).

Little (1970) further criticized the use of follow-up studies for their failure to use recommended statistical analyses. Also, there is said to be a failure to design studies in which sophisticated statistical analyses would either be appropriate

or helpful. Stemming from inadequate design and statistical analyses, there occurs major errors of inference in establishing causal relationships among variables.

In summary, in regard to a short review of a sparse literature, it was noted that:

1. Follow-up studies usually receive less than 100% response return, with evidence that incomplete samples yield biased results.

2. The instruments that have been used have usually been either the questionnaire or interview, with resulting information being only as extensive as the nature of the measuring device.

3. Follow-up studies are usually planned at some point after the conclusion of treatment, making the conclusions from make-shift designs limited.

4. The statistical analyses employed have not always been appropriate to the design used, yielding non-useful information and conclusions.

5. Such studies have been characterized as being weak in their design and inadequate in their statistical treatment, partly due to the fact that the studies were not conceived as research from the beginning stages.

6. Between the time of treatment termination and follow-up there has been no control against the influence of experimental history in accounting for any observed change.

Exploratory Follow-Up Study

For the purpose of generating ideas about the strength and weakness of follow-up study, as it relates to accountable methods

in counseling and psychotherapy, the author has conducted a follow-up study of college students. All students attended Rio Hondo college in Whittier, California, where the students had taken part in a group counseling experience in 1973. At the time the follow-up was conducted it was of interest as to how information collected after the completion of treatment could be used to improve that treatment in subsequent use.

Subjects. Eleven out of the possible 22 former clients were contacted by telephone and interviewed one year after counseling. All subjects had been in counseling for career counseling, and all took part in a replicable group counseling procedure described by Healy (1974).

Measure. A thirteen item strustured interview guide was constructed. All items were open-ended to allow subjects the freedom of reporting their most salient memories. This freedom of responding was felt to be important in order to avoid the inaccurate classification of responses according to a priori chosen categories. This measurement bias is discussed in a later portion of the paper dealing with actor-observer interactions in recording data. While the construction of the guide allowed for some freedom of response, the nature of the guide's structure insures data that is related to specific areas, allowing for comparison among individuals. (See appendix A)

Procedure. Eleven former client were contacted by telephone and interviewed by the author. In addition to adhering to the questions and order of the interview guide, respondants were also encouraged to discuss any other facet of the counseling experience. After the completion of telephone calls, and respondants replies recorded,

the results were analyzed according to their content.

Results. Responses were clustered into categories including areas of Perceived Help; Change of Plans and Activities, Recommendations for Improvement, Most Memorable Component of Counseling, and Needed Assistance after one year from the termination of counseling.

In the cluster of Perceived Help, three identified areas were in problem solving ability, knowledge of self, and interpersonal communication. Changes of Plans that were reported could only be interpreted as positive or negative depending upon the individual context in which they occurred. Changes in plans could alternately be interpreted as changes indicative of further refinement of decisions, or of further confusion. Recommendations for Improvement included the desire for better leadership and coordination by group leaders; more specific information after the conclusion of group counseling (like job entry level requirements, salaries, prospects for employment.etc.); and a strong desire for consultation follow-up on an individual basis. Most respondants spontaneously reported positive appraisal of the experience, and recognized such opportunities as the ability to gain newer perspectives on thinking by hearing other group members, and in gaining a mor knowledge of self. Percentages of responses to specific categories are as follows: Stimulated to seek further career information (63%), examine fit of occupation (45%), ability to see peers plans (100%), opportunity to systematically examine self (72%), no change of plans or incidental changes (81%), and those having a positive evaluation of the experience (63%).

Again, the purpose of the follow-up was not to systematically evaluate the efficacy of treatment, but to generate ideas concerning methodology for improving treatment. Of particular importance was the observation that grouped data processing tends to obscure some very relevant clinical information. An individual's relative gain toward some desired objective should be an important criterion for efficacy, but it becomes clouded in typical pre and post difference comparisons. For example, let us assume that 5 people only have a theoretical value of 2 in problem solving ability, while another 5 people have a theoretical value of 10 in problem solving ability. All ten of these people begin with a counseling treatment which includes as one of its goals the increase of problem solving ability. After these ten people finish with the counseling treatment, let assume that the counselor again measures problem solving ability to assess any change from pre to post treatment. If we assume that all 10 people terminate with a problem solving value of 10, than traditional nomothetic analysis yields the following results and interpretation:

Pretreatment $\bar{X}$ (N=10)	Posttreatment $\bar{X}$
6	10

Problem solving ability almost doubles for the clients of the counseling treatment! This analysis obscures the fact that in actuality 5 people make non-significant gain while the remaining 5 make spectacular gain in problem solving ability. Merging the two groups cancels out the effects that differentiate one group from another, producing a kind of cancellation that Bergin (1971) discussed in his review on treatment efficacy.

Another limitation of the type of follow up used in this paper (which is typical of most follow ups) is the repeated incomplete sample return rate that questionnaires are notorious for. There is reasonable suspicion that a 100% return would produce biased results in unknown ways.

It was also apparent that unless some type of baseline is provided before and during the treatment, systematic evaluation of treatment effects is near impossible.

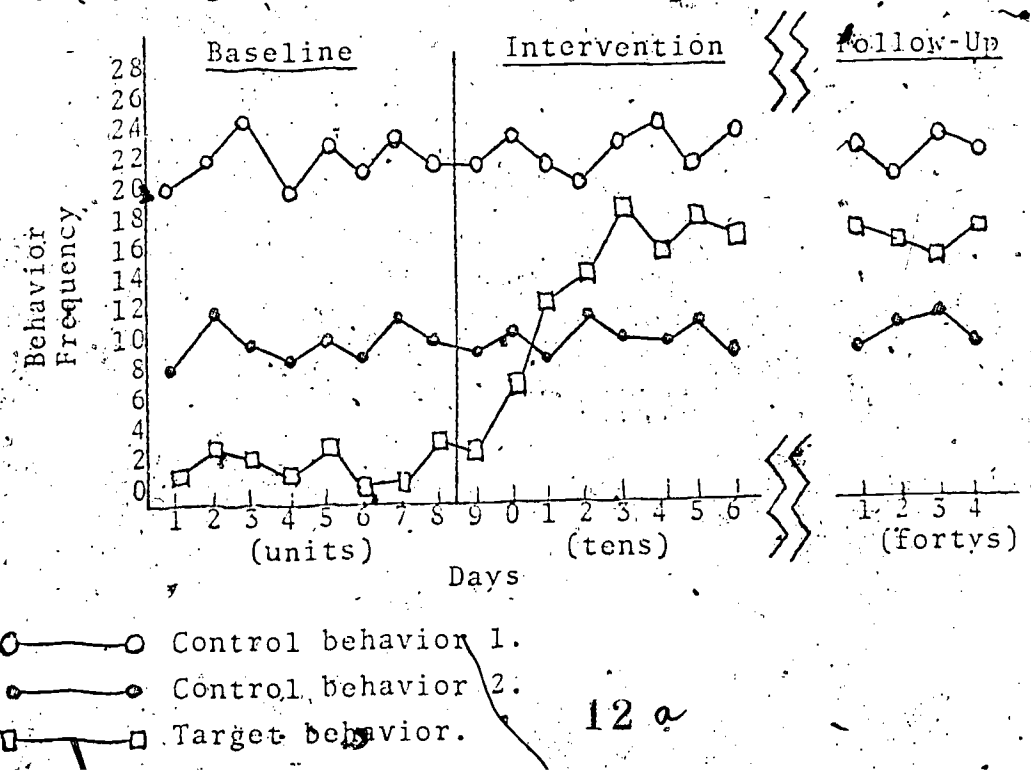
Procedures for Monitoring and Follow-up

In reviewing the types of experimental and quasi-experimental designs that allow an ongoing and future evaluation of a treatment or a program, many of the designs were some variation of time series analysis (See Campbell and Stanley, 1963). So called AB, ABA, and ABAB designs are basically designs which call for the recording of baseline along with the withholding or application of treatment. These appeared to have little utility in a daily schedule of a busy psychologist, where the therapist cannot easily withhold or apply treatment strategies in order to determine changes. This appears unpractical as a tool that can guide treatment.

Yet another variation of the time-series design exists, one that addresses issues raised in the results of the exploratory follow-up, called the intensive research design. Various writers have discussed this design within the context of psychological treatment (Thoresen and Anton, 1974; Gottman and Leiblum, 1974; and Chassan and Bellak, 1966), and there would appear to be much relevance to the present discussion.

The essential components of such a design is that it is

idiographic, provides information about change on an ongoing basis, and provides the necessary design and statistical sophistication for making reasonable inferences about client behavior change. In one method discussed by Thoresen (1972), behavior is observed and plotted before and during treatment. "Target" behaviors are those which are consistent with behaviorally stated goals of the treatment intervention. Since there is only one subject in each cell of the design (N=1), the client serves as his or her own control in a repeated measures design. Alternate behaviors selected according to their functional autonomy of the target behaviors are also observed and recorded. In this way concurrent observation of control and target behavior can be evaluated in terms of differential stability or gain. The behaviors are observed both before and during treatment. The same behavior can be repeatedly monitored at any point in the future, which would then constitute follow-up. The amount of control variables can easily invlve more than one behavior, as is true for target behaviors (See Figure 1).



Gottman and Leiblum (1974) in discussing the interrupted time series suggested that behavior be plotted before and during treatment, and instead of plotting multiple behaviors of the same client, behavior is plotted for more than one individual. In this case one individual's behavior (no treatment) serves as a control for the clients behavior (See figure 2.).

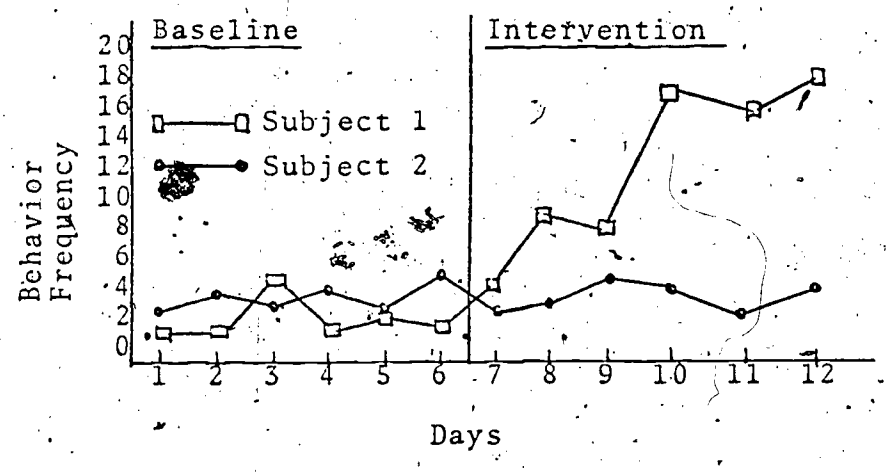


Figure 2. Example of the Interrupted Time Series Design.

These designs are open to the same criticisms as most time-series analyses, especially the lack of control for experimental history in accounting for the observed behavior change. To counter this weakness of the rival hypothesis, Gottman, McFall and Barnett (1969) proposed a design called the "time lagged control" design. While in the previous design there was one treatment, the time lagged control design incorporates a second intervention to another individual at a different time. The figure below illustrates not only a control for the target behavior, but also an independent

replication of the effect.

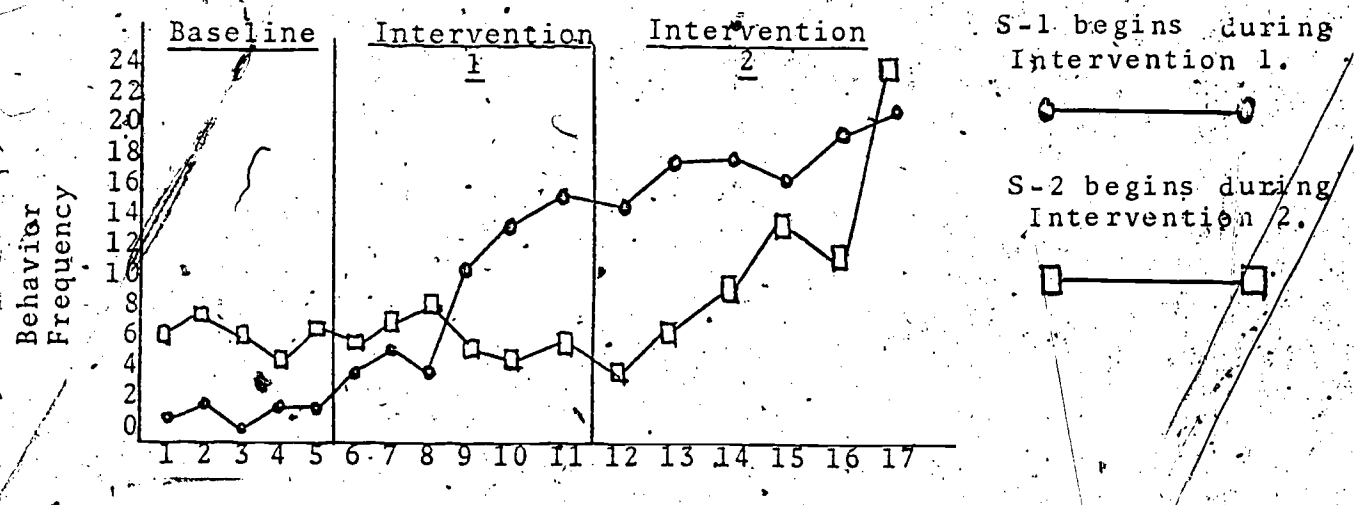


Figure 3. Example of the Time Lagged Control Design.

From the previous discussion it is recommended that follow-up be a component that is decided upon before treatment so that adequate steps can be taken to insure its design and statistical adequacy. As pointed out previously, without a baseline for comparison purposes, no reasonable inferences are possible concerning pre and post treatment behavior change. The percentages reported in the results section simply reflect the time period from which they were sampled and indicate nothing about change from pre-treatment functioning.

After this is taken note of, relevant client behaviors have to be chosen along with the selection or construction of appropriate measuring devices. The type of measurement should be tailored to the type of desired outcome, but Thoresen (1972) recommends that counseling goals be stated in behavioral terms. In this behavioral context, ratings of behavior (such as daily logs of target behaviors) are useful. Other goals might be more amenable to self-report data concerning affective states. Archival data can be very

useful within a time-series design and institutions that keep regular records, such as schools and hospitals, are likely sources. Archival records provide excellent unobtrusive measures of behavior, such as attendance records would provide a school psychologist in a referral for chronic truancy. But whether the data are derived from records, observations, self-report inventories, or other measurement means, they play an important role in the intensive design.

In regard to the time interval between observations or measurements, there are no rules and are idiosyncratically determined. The only generalization that can be made is that too many measurements are better than too few.

With the type of single case design being discussed, it should be noted that its usefulness is more in the area of generating hypotheses than it is in confirming them (Kieslar,1971). The comprehensive character of the data on one individual makes the change process more explicit and less reliant upon inference for understanding.

As Thoresen points out, intensive designs minimize the use of statistics to control for individual variability. Instead, behavior variability is not seen as accidental but as a function of the circumstances operating in the current environment of the subject. Thus, client behavior change that varies contiguously with treatment is thought to be a function of the treatment.

Concerning the statistical analysis of time-series data, it is first noted that time series is the repitition of two or more measurements of the same variable at different times. This provides a basis for infering whether the intended change has or

hasnot taken place. In such a design time is the master variable against which all else is ordered. The main purpose is to gain enough insight into the internal structure of the total time-series to permit valid generalizations about the system's behavior. Three cautions that should be mentioned are: 1) Time as the major ordering variable can bring with it countless other variables, only some of which can be adequately controlled by the investigator; 2) The time interval between observations is arbitrary and for continous variables can be made infinitely small; and 3) Repeated observations through time are often sequentially dependent, rendering invalid many of the usual statistical models which require independence of observations (Holtzman,1963).

For a more detailed discussion the reader is referred to Rao (1959,1965),Pothoff and Roy (1964), Khatri (1966),Grizzle and Allen (1969), and Algina and Swaminathan (1975). A brief description is provided below.

Data must first be identified as either independent or dependent, with different procedures for analysis. When observations are said to be independent, they are said to vary randomly around some constant value. When there is sufficient drift away from the constant value, then change can be assumed to have occurred. Practically speaking, this would simply involve computations for the mean and standard deviation of observed behavior, and determining whether two successive observation occur outside of the two standard deviation band (Gottman and Leiblum,1974).

For dependent observations, data are examined to determine if the data are autocorrelated. If data are autocorrelated, then prediction at some time in the future is possible by knowing the

trends of past events. Autocorrelation is determined by first calculating the correlation coefficient for sequential time-unit differences, that is by pairing observations as in the first order autocorrelation of observation 1 with 2, 2 with 3, 3 with 4 etc.. This is noted as the autocorrelation of lag 1 and subsequent autocorrelations of increasing lag are calculated. The correlations are then plotted as a function of lag, yielding a correlogram of obtained autocorrelations. The general correlation coefficient for lag k is obtained, using the following equation (Holtzman, 1963):

$$r_k = \frac{\sum_{t=1}^{N-k} (Z-\bar{Z})(Z_{t+k}-\bar{Z})}{\sum_{t=1}^{N} (Z_t-\bar{Z})^2}$$

N= number of observations
k= lag
Z= measurement

r_k is tested for significance using Bartlett's test. If r_k is greater than $2/\sqrt{N}$, where N equals the number of observations, then data is said to be dependent and r_k is significantly different from zero. If r_k is less than $2/\sqrt{N}$, or not significantly different from zero at $\alpha = .05$, then the data is considered independent and the first described analysis is used. If dependent data exist, then either a moving average or a first-differences transformation can be performed, elinating the linear trend, and enabling the use of analysis of independent observations.

In summary of the methods described, it has been said that effective follow-up also means effective treatment monitoring. By employing a series of ongoing measures that provide feedback to

the therapist about the effectiveness of his or her treatment, a baseline is also provided that can be used for comparing differences in future behavior. Three variations of intensive design were illustrated, as well as methodological cautions and procedures. Statistical treatment was also described, in order to make more valid appraisals of the obtained slope representing behavior change.

Discussion

In the variations of intensive study described in the preceding portion, repeated and accurate measurement is crucial. Within the context of Thoresen's discussion, counseling goals are behavioral ones, and measurement would be likely to entail behavioral observations or ratings of behavior.

Measurement of this type appears to be prone to a particular form of bias that should be noted. The bias has to do with actor-observer differences in the way in which events are perceived and analyzed by individuals, both counselor and client alike (Jones and Nisbett, 1972). Actors of behavior are said to differ from observers of behavior not only in respect to what data each has available for interpretation, but also in the way in data is processed. Individuals are thought to perceive their own behavior as determined by stimulus factors, or valences in the enviroment, while the behavior of others is perceived as due to personal traits. Observers, or experimentors, see behavior as "figure" against a "ground" of the enviromental context, leading to a bias in viewing the way in which an individual acts. In an intensive study where behavioral observations and ratings are being employed, there exists at least the possibility that counselors/researchers might see

within behavior qualities more in the perception of the counselor than the motivations of the client. This caution was noted in Allport's (1937) comment about a study concerning traits, but has not been much heeded. The study was concerned with the assigment of children's behavior into one of various a priori chosen categories. Allport noted that one child may lie because he is afraid of hurting the feelings of his teacher, while another may steal pennies in order to buy acceptance from his peers. For neither of these two children do the behaviors of lying or cheating constitute items on a scale called "honesty" which was the case in the study critiqued by Allport.[1] Similarly, within this paper's context, counselors must exercise caution when evaluating behavior against pre selected behavioral goals of "success". There exists the possibility that categorized behavior may exist more in the perception of the experimentor than the behavior of the subject.

The approach described is in some ways very differnet from methodological approaches normally taken in efficacy research of psychotherapeutic treatment. The first difference concerns the length of the time spand sampled in the subject's life. By the very nature of the intensive design, intimacy and comprehensiveness is explicitly sought. A second emphasis is in the amount of depth taken in investigating client change. Multiple measures of different characters, such as behavior observations coupled with self-reports of beliefs, are encouraged. In Sum, more data of breadth and depth is collected and analyzed in order to avoid fragmentary aspects of behavior evaluation in artificial contexts. A like criticism was

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1. Hartshorne and May (1928)

made by Carlson (1971) in the area of personality research. Carlson recommended more naturalistic, longitudinal, and single case study alternatives to the predominate experimental use of undergraduate psychology students as subjects for short amounts of time. The intensive design seems to incorporate these recommendations.

The idea of using comprehensive and relevant data is not a new idea, and it has been a hallmark of a type of counseling expounded by Williamson. Knowing as many of the relevant "facts" was important in a Williamsonian context because proper psychodiagnosis was directly based upon it. Treatment was seen as being only as adequate as an accurate formulation of the problem. The effectiveness of a treatment depended upon the proper collection and analysis of relevant information. It would seem that for research purposes, insuring the adequacy of the data is no less of a concern. The breadth and depth of the intensive design is consistent with these concerns.

Concerning this last point about "research" and "practice" in psychotherapy, a dichotomy is assumed which may not be productive. Implicit in this authors discussion of intensive designs in monitoring and follow-up of treatment is the notion that good research and therapy practice are not seperate. The use of comprehensive data reflecting client change should guide treatment in a formative evaluation manner. Each new piece of information should be applied to the previous situation, creating in turn new situations to be refined and modified. The added control and reliability provided by an intensive design is believed to make demonstrable the effects of treatment and increase its potential for success.

by the increased information available to the therapist, less reliance is made on inferences about client change. The public character of client change would probably have a reinforcing effect on the client as well.

The preceding discussion on treatment monitoring and follow-up is thought to point to differential emphases in attitudes toward accountability issues in therapy, and uses of measures as progress indicators of treatment. We seem to know too little of treatment process and outcome to foster on the public techniques with effects that we can only give educated guesses. We seem to know too little about specific problem populations to allow the use of "generalist" treatments on "uniform" clients. A more basic approach that illumintes the treatment process more clearly would be the appropriate first step, and this presentation was intended to stimulate thought in that direction.

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