Recent authors have encouraged the use of single-case experimental design in family therapy research. However, several issues related to the application of these designs have not been addressed. The present article suggests that the applicability of single-case methods to family therapy research may be limited. The first issue raised is the general lack of adequate dependent measures of family interaction that fulfill the requirements of the single-case experimental design. Suggestions for development of appropriate measures are given. Second, the necessary reliance on relatively weak single-case designs often allows only relatively weak conclusions. Finally, the strategy of beginning the single-case study of family functioning and family therapy in laboratory settings is suggested.

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In the past few years, articles have appeared advocating the use of single-case experimental designs in clinical practice in general (8) and family therapy research in particular (20). However, several issues related to the usefulness of the method have not been addressed. The purpose of this article is to suggest that, at present, the applicability of single-case design to family therapy research may be limited.

Brief Overview of Single-Case Design

The essential feature of single-case research is its use of repeated measurement of a single organism or organisms under controlled conditions to establish cause-and-effect relationships between independent and dependent variables (13). The basic procedures in the use of this type of design are as follows: First, select a usable dependent variable (DV), one that is operationally defined, objectively and reliably measured, easily emitted without fatigue by the subjects, and sensitive to changes that may occur in the experimental situation (22 and 24). Second, establish a repeated measurement period without intervention (baseline) and with minimal variability in the DV. Third, introduce the independent variable (IV) and note the effect on the DV. Finally, the process is repeated several times by reversing conditions or replicating the result across different behaviors, settings, or subjects.

The ability to detect cause-and-effect relationships between introduction of the IV and changes in the DV depends primarily on the processes of prediction and replication. The prediction process refers to the experimenters' ability to predict what the subjects' behavior would be like in the future if no IV were introduced (12). The comparison of the level of behavior after the IV is introduced with the level of

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behavior before the IV was introduced is the criterion for establishing the effect of the IV. If the level of behavior changes when, and only when, the IV is introduced, and this change is demonstrated several times (the replication process), the IV is judged to be responsible for the change in the IV, and causality is inferred.

Common Designs

Simply put, the two principle types of design for use with this method are reversal (ABAB) and nonreversal (multiple baseline) designs (1). Other designs demonstrate change in somewhat different ways, but the logical analysis is quite similar to the somewhat simple designs to be presented here. (For a more thorough explanation, see 9, 14, and 23.)

The most inferentially powerful type of reversal design in experimental psychology is the ABAB design. In this design, condition A represents the measurements of behavior before the IV is introduced. Introduction of the IV then follows in condition B. The effect of the IV is then replicated by removing the IV as noted by the second A condition. When the level of behavior returns to pretreatment level or reaches stability, the treatment is reintroduced (second B condition). The reintroduction of the IV is done to provide long-term treatment effects.

Analysis of the ABAB design is done by a logical comparison of the alternating A and B phases. Stable data in phase A allow one to project the likely level of behavior in the future if no intervention were to take place. Organismic changes in the DV at the introduction of the IV and the subsequent repeated replication of this effect allow us to conclude that the IV was responsible for the changes. If the DV changes in the predicted direction when the IV is introduced and also returns to baseline levels when the IV is withdrawn, then a cause-and-effect relationship between the IV and DV can be demonstrated. The logic is analogical to turning a faucet on and off. If the experimenter can repeatedly demonstrate control (with a given IV) over the DV, then causality can logically be inferred; if precise control cannot be demonstrated, the internal validity of the study is questionable. The principle requirement of the reversal designs is, of course, that the IV be reversible, that is, that the effect of the IV on the subject can be removed and that the subject's behavior will return to baseline rates.

The second principle design in experimental psychology and most commonly used in therapy research is the AB multiple-baseline design. In this design conditions A and B are the same as before, but no additional A or B conditions are imposed. Rather, a series of A and B conditions are replicated across at least three or four different situations. The most common replications are across different DVs in the same subject (within subject replication), the same DV in different settings (same subject, across-settings replication), or the same DV with other subjects (across-subject replication). Also the length of the A baseline phase typically varies within each experiment to control for maturation effects.

Analysis of the multiple-baseline design is essentially the same as for the ABAB design. The importance difference is that the replication of IV effect is done in different situations. The logic of analysis requires that the experiment demonstrate changes in the DVs when and only when the IV is applied to each DV. If such change is apparent, causality can be inferred. In other words, the effect of the IV is inferred from the untreated DVs (9).

A principle requirement of the multiple-baseline design is that the effect of the IV does not generalize across all the DVs under study. That is, that the DV's should not be highly correlated. If this requirement is not met, several DV measures are likely to change at the same time rather
than in a one-after-another manner as the IV is applied to each. Simultaneous change threatens the internal validity of the study, since it is impossible to show replications of treatment effect. If a second DV changes along with the first DV, some factor other than the IV may be responsible for the change in both DVs.

Other Single-Case Experimental Design Requirements

In addition to the basic design procedures described earlier, a powerful effect is necessary to produce rapid and dramatic changes in the DV when the IV is introduced (19). Relatively weak IV effects would not be detected by single-case designs, since the analysis of the data is done by visual inspection. Obviously, treatments that rely on cumulative effects or gradual change would be difficult to study using single-case designs.

Another requirement for successful single-case research is the establishment of nonreactive dependent measures. Nonreactivity is essential in establishing stable baseline phases prior to treatment introduction. The DVs selected must have a proven record of nonreactivity to repeated measurement during baseline and experimental conditions.

In addition to the design requirements unique to single-case research that need to be met, general psychotherapy research considerations (2) and those unique to family therapy research (6, 7, and 17) also need to be considered. The basic issues for family therapy research include the following:

First, assessment of multiple levels of the family system would include at least the individual identified patient, the marital dyad, and the total nuclear family. Second, the assessment should occur from multiple view points. That is, measurements taken from several sources of data (e.g., clients, therapists, significant others, etc.) would provide optimum judgments of treatment outcome. Third, assessment should allow for deterioration to be assessed if it occurs. Finally, assessment should continue periodically after treatment ends for at least three months to determine durability of treatment effects.

Basic Questions in the Application of Single-Case Designs

In considering the application of single-case designs to family therapy research, several basic questions need to be answered. First, can the dependent variable requirements be met? As mentioned earlier, appropriate dependent variables for single-case experiments must be operationally defined, objectively and reliably measured, easily emitted without fatigue by the subjects, and sensitive to changes that occur in the experimental situation (22 and 24). The dependent variables currently available for family therapy research, however, are primarily unvalidated (7). Self-report questionnaires of concepts believed to be related to family functioning or complex coding systems of family interaction (e.g., 5, 16, and 18). Both types of measures have limitations for single-case design application, as will be described later.

The problem of dependent variables selection is compounded when experiments require long-term repeated measurement. For short-term experiments, coding systems that provide many data points within a limited length of time can satisfy the dependent variable requirements. When experiments last several weeks or months, however, dependent variables are more difficult to identify. Keeping in mind that the minimum requirement is three data points (with minimum variability) in each A and B phase (9), for research in outpatient clinical settings there appears to be three main options for obtaining long-term measurement. The first option is repeated measurement in the office/lab. In this case the DV could be measurable several times during any one client contact. Probably the

Fam. Proc., Vol. 24, March 1985
A second option is naturalistic assessment in the home. This can be done by trained observers (e.g., 21 and 28) who make frequent home visits or by mechanical devices such as a tape recorder that turn off and on at certain intervals (e.g., 11). The principle advantage of this type of measurement is that dependent variables are easily obtained without fatigue by the subjects. The disadvantages, as previously mentioned, is the reality cost of training and maintaining coding staffs.

Finally, a third option is client reports about at-home behavior. This procedure has the advantage of being easily obtained and frequently measured, but sole use of self-report is not appropriate (7). It would be necessary therefore to obtain ratings from judges or other sources to validate client self-report data. The principle limitation of this type of DV is the rather heavy data collection assignment for the client and judges. Clients and judges would need to be well motivated and highly cooperative to provide quality data in an ongoing manner. "Forgotten" data recording or inaccurate reporting can be fatal to the experiment. For this reason, many studies using single-case designs have been done in institutional settings such as schools and hospitals. Such settings allow the experimenter to be less dependent on cooperation from subjects or other independent judges and yield greater experimental control.

Other settings for single-case design studies have been in studies of child-oriented treatments where parents provide some what objective ongoing data about their children's behavior. The central question in such studies has been one of gathering ongoing data on the parents behavior. In family therapy research, the problem is more pronounced, since all family members are considered to be in treatment. The question then becomes, "Who will be able to report ongoing data for all family members?" In other words, who is sufficiently outside the system to provide ongoing data collection? The answer to that question is usually a trained observer of some kind as previously discussed.

An additional problem with using self-report measures is that successful collection of repeated measurement often requires subject awareness of treatment procedures. In fact, one of the principle rationales given to clients to obtain their cooperation in ongoing measurement is that providing data will help them (clients) evaluate treatment effectiveness (e.g., 10 and 20). Usually this means that clients are informed about treatment goals and procedures. Consequently their feedback to the therapist by way of self-report DVs provides information about treatment effectiveness and suggests changes that may need to be made in treatment. The assumption of client awareness can be best met by treatment approaches that emphasize clear and direct treatment goals and procedures (e.g., behavioral, structural). When goals are clearly articulated, clients may be willing to provide repeated measurement, since the relationship between measurement and treatment is clear. In other types of treatment (e.g., experiential, strategic), where therapy goals and procedures are less clear and direct, clients may be less willing to provide repeated assessment, since the relationship of measurement and treatment is not apparent. For example, clients given paradoxical assignments are
usually not aware of the therapist's intent (25). Consequently, they may be less willing to provide repeated measurement, particularly when the treatment itself is confounding.

What is needed for the application of single-case experimental designs to family therapy research are dependent variables that are operationally defined, objectively and reliably measured, easily emitted without fatigue by the subjects, and sensitive to change. In addition, these measures should provide assessment across several system levels and from several vantage points. At present, other than some laboratory measures, very few DV's fulfilling these requirements seem to be available.

The second basic question in applying single-case experimental designs to family therapy research is, "Can the replication requirement be met?" The first way to meet this requirement is the reversal technique shown in the ABAB design. The reversal procedure (described earlier) is most appropriate when the experimental treatment can be removed and no carry-over from one phase to another is expected. In those cases where carryover effects are likely or where ethical considerations preclude reversal of treatment conditions, the nonreversal multiple baseline type designs are used. In general, carryover is expected whenever learning of some kind is involved. For example, one would not expect to be able to remove newly acquired communicative skills from a family. In such instances, treatment conditions could not be reversed, and the experimenter would need to use a multiple-baseline design. The major advantage of the multiple-baseline design is its ability to evaluate nonreversible conditions. The major disadvantage is that these types of designs are not considered as inferentially powerful as the reversal in demonstrating causal relationships (23). For example, Hersen and Barlow (9) note that "the multiple baseline design is considerably weaker than the withdrawal (reversal) design as the controlling effects on the treatment on each of the target behaviors is not directly demonstrated (e.g., as in the A-B-A design) (p. 227)." Also the requirement of establishing truly independent DV's for sequential IV application is often difficult to satisfy (15), as will be discussed. Additionally, many experiments the issue of independence of DV's cannot be assumed a priori. Rather it must be tested. If the several DV's do not change when the IV is applied to one, independence is assumed. Then the multiple-baseline experiment can proceed to examine IV effect on each independent DV.

In summary, the principle types of designs available for family therapy research appear to be the nonreversal designs. For practical applications, this requires that several noncorrelated DV's be measured in the same family (multiple baseline across behaviors) or several settings in which family interaction can be measured (multiple baseline across settings) or several families that are fairly homogeneous in dynamics and presenting problem (multiple baseline across subjects).

For multiple-baseline across-behaviors designs, the selection of several independent DV's is somewhat difficult since family therapy often has an impact on many areas of family functioning simultaneously. Thus intervention in the area of relationship rules, for example, may well have an impact on communication and structural patterns as well. The difficulty in using the multiple-baseline design, then, is first selecting multiple independent DV's that will not react to intervention in any other area, and second, limiting intervention to one aspect of family functioning at a time.

For multiple-baseline-across-settings designs, the task would be to have at least one DV measure that was obtainable in several settings (e.g., in the office, home, or business). The design requirement would then be to effect change in the DV measure.
in one setting at a time. The requirement, of course, would be that intervention in one setting would change the DV to that setting alone.

Multiple-baseline across-subjects designs involve establishing at least one DV measure that would be useful across several families that would have the same presenting problem. In other words, the DV to be measured would be approximately the same for each family, and treatment would be introduced to one family at a time. Multiple-baseline across-subjects designs are probably the most practical form of the single-case designs for family therapy research.

Finally, another question to be answered is, “Can single-case designs provide adequate experimental control?” Although an important consideration in all naturalistic research, for the single-subject researcher this issue has even greater significance than for the group-comparison researcher. The group-comparison researcher normally uses the techniques of making a secondary variable an IV, randomization, statistical control, elimination, and constancy to provide for control of variance in the experiment. Single-case researchers primarily use making a secondary variable an IV, elimination, and constancy to achieve the same control (22). Since relatively fewer methods are available to control excess variance, the ability to apply the available methods assumes even greater importance.

In single-case research, the control methods allow for “tuning-up” the DV to increase its stability before introducing the IV (24). This “tuning-up” process allows the experimenter to monitor closely the incoming data and to identify sources of variability and eliminate their influence from the experiment. The group-comparison research will usually not have an extensive “tuning-up” period. Rather, the experimenter by use of randomization and statistical control will equalize the effects of excessive variance equally between all groups (9). Consequently, the effect of excessive variance can be minimized by what is basically averaging a process between groups. The single-case experimenter, however, cannot average the effect of excessive variance. Either, he or she must identify and control the variance so that stable data in each phase of the experiment are obtained (24). This process of eliminating (or holding constant) sources of excess variability allows the experimenter to develop a stable DV before introduction of the IV. In order to establish causality between IV introduction and DV change, the variability of the data in each phase should be minimal. For example, wide variability during baseline makes interpretation of intervention effects difficult. That is because the change in the DV will not be apparent, since its effect will be masked by excessive variability.

Recommendations for Future Single-Case Research

The ability to apply the methodology of single-case experimental designs to family therapy research seems to depend upon progress in several areas.

First, additional dependent variables for assessing family functioning from multiple vantage points need to be developed. One possibility might be the development of mechanical devices. For example, video cameras that turn on at predetermined intervals may provide relatively low-cost (to subjects) observation of family interaction. Another possibility might be the development of physiological measurement devices that could be worn by subjects to collect heart and respiration rate data. Such physiological measures may give clues about such things as changes in family stress patterns, for example.

Other possibilities for useful DVs might be low-cost objective data of family func-
tioning, such as school attendance, number of arguments in family problem-solving sessions, talk time or other simple verbal coding procedures. Another idea might be the number of prescriptions filled in the study of psychosomatic families (4) or physical distance spacings in the study of marital dysfunctions (5). The underlying idea in all of these possibilities is the continued development of objective and reliable DVs related to family functioning.

Considerable attention should be given to the development of useful and low cost DVs for family therapy research. Such progress will take some time to accomplish but seems to be a necessary first step in the use of single-case designs. It should be noted at this point that development of this kind of "backing" of information related to family processes strikes a close parallel to other areas in which single-case designs have been applied. For example, Sidman (24), in referring to animal studies, points to the need for understanding the common behavioral pattern of particular organisms as a necessary first step in the identification and elimination of sources of variance before other studies are undertaken. For example, in the study of animal learning, it was first necessary to develop a means for reliable and objective data recording (the Skinner box) before the process of learning could be studied directly. Such a process seems necessary for family therapy. The first questions to be answered seem to be how to measure a reliable and objective manner the various behaviors typical in important family processes—a necessary first step in the application of single-case design.

Second, the most immediately promising area for applications of single-case designs seems to be in the laboratory setting. Research in such settings provides the best opportunity currently available for the identification and control of influential variables. The in-lab researcher generally has more direct control of environmental variables that can be identified, eliminated, held constant or made into a second IV, whereas naturalistic research often faces influential environmental variables that are difficult to identify and control; that is one reason why naturalistic, single-case designs over the long term (several weeks or months) are more problematic than short-term, in-lab research. Consequently, one would expect that initial single-case family therapy research would more realistically be done in the lab than in a naturalistic setting.

Conclusion

Presently there appears to be several practical limitations regarding the application of single-case designs to family therapy research. First, except for some laboratory experiments, the DV requirements of the design require measures that are currently unavailable in the family therapy field. New dependent measures related to family functioning need to be developed. The new dependent measures should be objective and reliable as well as provide assessment of several levels of the family and from several vantage points. Second, the most inferentially powerful single-case designs are not appropriate for much of the therapy research, since reversal of therapy effect is usually not feasible. Finally, the experimental control necessary for successful single-case research is seldom available to the researcher in the naturalistic settings. A practical solution to this problem is the implementation of studies based in the laboratory setting before initiating naturalistic, long-term therapy outcome studies. Such a strategy would provide the "backing" of information related to family functioning that is necessary to understand such family processes so that sources of excess variability can be identified and minimized. Although single-case designs hold considerable promise for research in

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Summer Practicum in Family Therapy
Rome, June 3-26, 1985

A six-week training program for family therapists will be offered for the 1985 session at the Istituto Familiare in Rome. The training begins June 3 and ends July 12, 1985.

The program is open to qualified therapists from all over the world, provided they are interested in family therapy. Participants come from ten countries: Argentina, Australia, Brazil, Canada, France, Germany, Italy, Japan, Sweden, and the United States.

The training offers therapists a structured environment designed to provide opportunities for learning, practice, and consultation. It includes seminars and workshops on specific topics in family therapy, as well as supervised group therapy with a focus on the development of effective therapeutic interventions.

Eligibility: Therapists who have completed a Master’s degree or equivalent in family therapy or related fields are eligible to apply. Participants must demonstrate a commitment to the principles and practices of family therapy.

Cost: The program fee is $3,500. A non-refundable deposit of $500 is due upon acceptance. The remaining $3,000 is due at the beginning of the program.

Application: Full applications, including a letter of interest and a curriculum vitae, should be submitted to Maurizio Andolfi, Istituto Familiare, via Reni 30, 00194 Rome, Italy. The deadline for applications is April 1, 1985. Accepted candidates will be notified by April 15, 1985.


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