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IMPLICATIONS FOR SOCIAL WORK

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ABSTRACT

Various issues in evaluative research are reviewed according to their relevance for the evaluation of social work practice. Specific items discussed are: plausible studies, what should be changed and why, the change agent, criteria for positive assessment, traditional research designs, time-series designs, organizational aspects of research, researchers vs clinicians, researcher's distance from populations served, incentives for research, and the dissemination of information and application of relevant knowledge. Where relevant, aspects of certain evaluative studies are discussed to illustrate the items reviewed.

Introduction

Few would argue that within the last decade the social work profession has been criticized from both within and without the ranks for its apparent inability to adequately meet client needs (Crompton, 1974; Feldstein, 1971; Hurst, 1971; Marler, 1972; Panitch, 1974; Piven, 1969; Riley and Fellin, 1972; Schwartz, 1973; Shonick, 1972; Specht, 1972; Turner, 1968; Wodarski, 1974; Younghusband, 1973). In the majority of cases most people would agree that these charges have produced positive results, such as the greater use of paraprofessionals, a greater awareness of the profession's role in sexist and racist practices, a greater number of women and minority group members teaching in schools of social work, greater use of technical aids such as videotapes in the preparation of social workers for practitioner roles, greater incorporation of new knowledge bases, such as socio-behavioral, systems theory, and so forth in the curricula of schools of social work, development of new educational programs, greater participation by students in planning their educational programs, development of new services to meet emerging client needs, and so forth. Another outcome stemming from these allegations has been the profession's recent emphasis on research, i.e., to use evaluative research with the supposition that the evaluation of services would provide a rational basis for the delivery of services (Briar, 1968; Briar and Miller, 1971; Fanshel, 1966; Maas, 1966 and 1971; Mullen and Dumpson, 1972; Newman and Turen, 1974; Stuart, 1971).

For the purposes of this paper evaluative research is defined as the systematic application of research methods to social work practice with the purpose of specifying those events which cause change in clients. This is an all-encompassing definition which includes interventions applied to individuals, groups, communities, and/or organizations. Even though few would argue the worthiness of this goal the functions that evaluative research could provide social work practice need to be put into proper perspective. The global assumption is that evaluative research will be the salvation of social work practice if we only do more of it and do it well. It is the thesis of this paper that certain ideas pertaining to evaluative research are dysfunctional and go beyond the scope of what it can do for the profession and that a very different perspective is needed before evaluative research can yield the profession a dividend worthy of the investment. For example, the question as to whether or not casework is effective, which has occupied the time of many researchers and practitioners for the last decade, may not be determined through evaluative research (Alexander and Siman, 1973; Briar, 1973; Fisher, 1973a, b; Gyarfas and Nee, 1973; Hartman, 1971; Herstein, 1969; Marks, 1972; Middleman, 1973; Moore, Hallowitz, Martorana and Geisman, 1973; Perlman, 1967). The question is too general; that is, not formulated in a specific manner that meets the basic requisites of evaluative research and, thus, as inappropriate as asking whether or not the social work profession is relevant to the society in which we live. It is understandable that social workers want to provide a rationale for service that has been questioned, thus the focus on applying the technology of

evaluative research to the evaluation of casework services. However, in order for evaluative research to assess casework services and help provide them on a more rational basis the global question of whether or not casework is effective has to be restated in terms of is casework effective, utilizing X, Y and Z techniques in X, Y and Z contexts with X, Y, Z therapists and X, Y, Z clients. For example, evaluative research could yield the following proposition, that a middle aged, middle class depressive male, who has a good work history, a satisfactory marriage, a college education and two children, is most effectively treated through brief therapy, consisting of structure, ventilation, clarification, and so forth provided by a middle aged male therapist, who has had his M.S.W. for at least five years and whose practice orientation has centered around the theoretical framework of brief therapy provided in a community mental health center.

Thus, several crucial considerations must be dealt with before evaluative research can be expected to reasonably aid the planning and decision making functions of practitioners, agencies, and the institutions involved in the delivery of social services. The purpose of this paper is to discuss these major considerations and to offer a guideline for the formulation and conduct of evaluative studies. We believe that if these items are given proper attention the use of research technology can function as an important tool in both the application and assessment of treatment intervention and the development of more efficacious social services. Hence, this paper addresses itself to the following issues: 1) Plausible studies are considered in terms of the types of questions that can be posed and answered regarding the phenomena social workers deal with; 2) What should be changed and why? This discussion will center on variables that influence the goals for measuring change that are chosen by evaluators; 3) The change agent is discussed in terms of specifying the operations involved in providing a service to clients in such a manner that enables evaluation; 4) Criteria for positive assessment are reviewed in terms of statistical and other criteria that are used for the evaluation of studies; 5) Next, the manuscript centers on traditional and time-series research designs in terms of their limitations and strengths for evaluating social work practice; 6) Organizational aspects of research are reviewed in terms of the impact of specific bureaucratic variables and their impact on the research investigation; 7) Dysfunctional aspects of separating researchers and clinicians for purposes of training, practice, and research are reviewed; 8) Researcher's distance from populations served is discussed in terms of how this affects the research process and accumulation of knowledge in social work; 9) Incentives for research are reviewed in terms of the effects of reward structures provided by funding agencies, universities, and social service agencies; and 10) The dissemination of information and application of relevant knowledge is reviewed in terms of variables that effect the incorporation of new knowledge into social work practice. Where relevant the examples from the authors' research experiences will be used to elaborate upon the issues.

Plausible Studies

Well designed studies in social work practice should specify concretely what the unit of change is, i.e., a client, a group, a community, or an organization. Also specified in empirically grounded terms would be to what this unit would be changed. For example, the goal of increasing a client's social functioning would be defined in terms of measurable behavioral objectives. Likewise, the investigator would also specify how this unit will be changed, i.e. the composite interventive behaviors to be exhibited by a worker. At the same time a study should specify in what context this interventive attempt will occur and the organizational characteristics of the context, such as agency size, number and variety of services provided, fluidity of the agency's internal structure, its immediate social environment, administrative style of supervisors, and so forth. Moreover, the characteristics of the person who will intervene should be clearly stated. For example, the unit of change may be ten anti-social children between the ages of 11 and 12 who exhibit the following behaviors; hitting each other, damaging physical property, running away, climbing out of windows, making loud noises, using aggressive verbal statements, throwing objects such as paper, candy, erasers, chairs, and so forth in a recreational group at a community center. The interventive approach is the group worker's choice of using positive reinforcement to increase pro-social behavior and punishment and extinction to decrease anti-social behavior and the use of other behavior modification techniques such as time-out, shaping, group contingencies, and so forth as needed. The context is an open agency, a community center that primarily offers recreational, leisure time, and educational services for 16,000 enrolled members. Each year the professional group work staff of the agency organizes approximately 200 clubs and classes for children and youth ranging in age from 6-18 years. The change agent is a young male between the ages of 18-24 who is in college, is highly motivated to work with children, has good verbal ability, is well adjusted, and so forth.

A well designed study would measure client behavior and worker interventions throughout the duration of the study. This process would enable one to get a better estimate of how the worker interventions affect the client's behavior. A significant error in social work practice is to focus solely on what is to be changed in the client and proceed only to measure it. Very seldom do we measure the interventions employed by a group worker providing services to children in order to change the client, such as the amount of praise, directions, positive attention, criticism, positive physical contacts, time-out, holding, threats, negative attention, and so forth. The assumption is that the treatment being applied, whether it be casework or group work, is a homogeneous coherent system of operations exhibited by each worker and is not influenced by the agency in which the treatment takes place, other factors such as client's sex, race, social class, and so forth, and/or the worker's experience, training, sex, race, social class, and so forth (Hollingshead and Redlich, 1958; Luborsky, Chandler, Auerbach, Cohen and Bachrach, 1971). This deficiency in evaluative research

hampers the researcher's ability to assess or control the quality of the interventive attempt. Through repeated measures of client and worker behavior the researcher can monitor change daily, thus acquiring a more accurate estimate of the effects of worker interventions upon client behavior.

What Should Be Changed and Why

It is essential that one understands that determination of what should be changed involves a value judgment, or a series of them. This is obviously a complex and frequently difficult issue to deal with, but one that must be considered if services are to be provided on a rational basis. One immediately confronts the profession's code of ethics as a major determinant in what should be changed, but a code of ethics can only serve as a guide. Frequently this is resolved by a formal or informal contract with the client, but contracts are a recent innovation in social work practice and probably represent the exception rather than the rule. The answer to what should be changed is not found in quantitative methods of research design technology but must be dealt with in terms of a complex set of values and norms held or adhered to by both the worker and the client. If this issue is not adequately dealt with the probability runs high that the remainder of the research will either be fruitless or yield a good answer to an inappropriate or trivial question.

As we look at most theoretical frameworks in social work we find that change is usually defined by the normative structure of the society, whether the changes are the client, a group of clients, a community, or an organization such as a social agency. For example, role theory constructs are used to explain why clients are not performing well in roles as defined by society, and techniques are derived from role theory to modify the client's behavior in order that he may assume his proper societal role. For instance, many young people do not adhere to the traditional values of the society (Erlich, 1971; Wheeler, 1971). Their behaviors are evaluated in terms of traditional values and labeled as inadequate role performances and specific socialization mechanisms are posited to correct these performances. Although societal norms cannot be flagrantly violated, the deficiency of this approach lies in the profession's tendency to seldom question the roles formulated by society and, therefore, accepting them as given.

As with role theory, many psychodynamic theories define what should be changed in terms of very high order constructs created to explain human behavior. Ego, defenses, motivation, traits, personality configuration, conscience, character, etc. are more examples of terms linked together and formulated into a system that purports to explain why some of us succeed and others fail in the achievement of an endless array of goals and tasks. However, the very notions of success and failure are heavily laden with valuative connotations and the techniques derived from such theories are again very often based upon implicit societal norms that are accepted as given and seldom questioned. When

a woman says to a worker that she wants to divorce her husband the worker may translate this as a treatable psychological problem rather than a plea for guidance in securing financial and legal aid. This may be entirely proper and correct, but if the worker's diagnosis is guided too much by the implicit norm that regards divorce as bad he may be doing a grave disservice to both the client, her children, the community, and the agency that hires him.

If one examines the goals of programs for the poor, delinquent, mentally ill, retarded, and so forth it becomes evident that they are focused toward the attainment of middle class values and are evaluated in terms of middle class criteria, such as having a "good" job, being married, having a "good" income, attainment of more education, and so forth, which are derived from the Protestant ethic of hard work, self control, good moral character, and so forth. The objective of this paper is to question the suitability of these goals in evaluative research. The majority of research executed in social work and related disciplines has defined the outcome criteria in such a way that the research starts with the assumption that it is an individual who must be changed and not his social system (Braginsky and Braginsky, 1973; Carroll, 1975; Haleck, 1971; Mandell, 1974; Ryan, 1971; Szasz, 1961, 1970; Trecker, 1973). What is needed are evaluative research studies which not only have well grounded empirically defined criteria when the service being evaluated is focused on the individual but studies which focus on well defined social system variables which may need to be changed in order to achieve the objectives of a program. For example, evaluative research may isolate behaviors that are involved in perpetuation of institutional racism and then determine whether interventions on the individual, system, and/or a combination of both are necessary for effective change. Likewise, the rehabilitation of correctional offenders may not only involve programs to change their behaviors but programs which change society's attitudes, provide decent jobs and housing, and so forth. Research which focuses on the reciprocal quality of individual and environmental variables would begin to capture the interdependent aspects of behavior and may lead to the development of more adequate theories of human behavior.

Thus, it is the contention of the authors that the single most important consideration in the planning and design of any evaluative research is the development of a clear and unambiguous statement or decision concerning what should be changed. If this issue is not adequately dealt with a number of undesirable consequences such as the abuse of the client, ineffectual intervention efforts, misguided use of personnel and facilities, failure to acquire needed information for planning, inappropriate change in theory and practice, and so forth may result. Yet in many research investigations this issue is often either wholly ignored or given only cursory attention. For example, it is extraordinarily common for a public aid recipient, delinquent, or mental patient to confront a worker with three primary needs; a useful and minimally gratifying job, an adequate income, and through these an opportunity to acquire the minimal amenities compatible with dignity and health in modern society. Yet,

it is decided that the client is deficient, requires internal change, and is given psychotherapy instead of training in specific employment and social skills, a job, and a set of opportunities that would come easily to many others. In short, the worker in such cases has decided that what should be changed is the client when a more appropriate option in many cases is to change the structure of job opportunities.

The Change Agent

Once a practice problem has been defined and a decision made concerning what should be changed the question arises as to what will bring about the desired change. Reid and Shyne (1969) executed a worthy study of brief or extended casework, and their findings suggest that brief casework is "better" than extended casework. However, examination of brief casework, as they defined it, shows it to be based on nine separate operations, and the question remains as to what accounted for the observed changes? Hudson (1971) reports an experiment with pre-school children and their parents which shows intensive casework was effective in changing certain performance abilities, but intensive casework was merely defined in terms of more frequent contacts and smaller caseloads and not in terms of techniques that would differentiate intensive from other forms of casework. Meyer, Borgatta, and Jones (1965) defined treatment methods in an analogous manner with the additional stipulation that intensive casework was also "professional" casework.

This approach is no longer adequate—indeed it never has been. The question remains as to what specific operations accounted for the change in these and other evaluative studies. Even though we know that certain globally defined services were better than others we do not know the exact nature or processes responsible for it. One might claim that casework is casework—be it intensive or regular, brief or extensive, professional or non-professional, etc. But we know better than that. We are not concerned with the simple truism that workers are different, quality of training differs, etc. We know, too, that in some instances we would do well to take into account such differences. One could argue that it really does not matter. However, in this age of increasing costs in the delivery of social service it seems to be an ethical obligation to find the most effective components of any seemingly efficacious method of change (Wodarski and Buckholdt, 1975). Likewise, a profession committed to helping people achieve more adequate levels of living has no right to continue aspects of service that are not directly related to client change (Feldman, 1970, 1971; Hayes and Varley, 1965; NASW, 1967). Thus, research investigations should help us isolate those programs that will help the client increase his level of functioning and answer such critical questions as: What is adequate treatment? Where should it be provided? What qualities should the change agent possess? How long should treatment be provided? What happens if there is no change in the client? (Wodarski, 1976).

Thus, most of the independent variables (social work services provided by workers) of concern in social work are far too globally conceptualized to be of much relevance to our clients. In most evaluative studies services are broadly defined as operations of specific departments in specified agencies, casework offered by MSW's and so forth. One of the authors recently completed a five year research investigation of three different types of group work (behavior modification, traditional, and group centered) with anti-social boys ages 8 to 18. If one type of group work used in the study, such as behavior modification, were shown to be "better" than the others we still would not have known what, within that method, was responsible for the change, i.e., structuring group contingencies, use of material reinforcers, using praise, punishment, extinction, time-out, shaping, and so forth (Wodarski, Feldman, and Pedi, 1976).

One could argue that all this really is not pertinent and that it is sufficient to show only differential results. However, this attitude is problematic. First, evaluative research methods are used in this context in a purely combative or political context (my dad's stronger than your dad, continue or expand our program because we've shown that casework works) rather than isolating the important components of the change process. A second problem concerns replication. It is difficult to replicate an experiment if we cannot precisely specify the nature and magnitude of the change agent—and that no doubt is a principal reason for ambiguous outcomes which occur upon replication of an experiment. A third reason, closely allied with the second, is that if we cannot specify the precise nature and magnitude of the change agent we contribute nothing to building a practice science—even though a positive outcome may be achieved. In other words, if a researcher demonstrates that a treatment intervention effort is successful but cannot point to the elements of treatment known to be responsible for the positive outcome he is in no way able to teach others how to improve their treatment skills on the basis of his research findings.

Likewise all of the foregoing points to a major weakness frequently observed in evaluative research, and that is a general failure to conduct adequate definition and measurement of the independent variable or the change agent. Many experimental social scientists make careful plans and heavy investments in defining and measuring one or more dependent variables and, by comparison, ignore the independent variable altogether. This problem deserves some elaboration.

For example, a worker conducts an experiment to test the hypothesis that professional casework services will effectively reduce the number of illegitimate births among a group of adolescent girls in a vocational high school. In order to test this hypothesis girls are randomly assigned to a control and an experimental group because random assignment is one of the best known means for holding all other extraneous effects constant. The worker may then proceed

under the belief that it is not necessary to define or measure further the proposed change agent--professional casework services--because all other extraneous effects are held constant or averaged out. In this example we want to point out that professional casework is not an appropriately specified change agent or independent variable. Rather, it is merely a vehicle through which the proper change agent will be administered. We would substitute for professional casework the terms professional psychological treatment, psychiatric treatment, ministerial counseling, behavioral modification, or medical treatment and the same assertion would hold. Each of them are only treatment modalities and not proper independent variables. Each of these treatment modalities makes available to its practitioners a "bag of tricks", tools, techniques, ideas, behavior, etc. that could be used as a change agent.

Actually, this example represents a case that cannot be adequately dealt with yet by specifying the proper change agent or independent variable that will reduce illegitimate births. We see that when we recognize that even the dependent variable has not been adequately specified. Given the general problem of reducing illegitimate births we must recognize that it too contains several important features. Obviously, we cannot have illegitimate births unless people are fornicating out of wedlock. Is the proper thing to be changed the act of sexual intercourse or the out of wedlock status? In order to reduce illegitimate births should the professional casework services be directed to reducing sexual contact or should it aim at effecting a marriage between the sexual partners. Either choice would likely imply a different set of treatments--change agents. Another option would be to theorize (diagnose) that the real source of the problem is the moral degeneracy of the female partner (this would respect the dual standard in this country which appears to be an accepted norm) and subject her to socialization training. This of course would imply a still different change agent. Another theorist might say that all the foregoing are incorrect, that what is needed is the simple recognition that the sexual partners have the right to govern their own bodily functions and the proper method of dealing with the problem of illegitimate births is to take care of the "birth" part of the problem through instruction in birth control techniques. A radical solution, so repugnant that it must be rejected, would consist of a program of involuntary sterilization. Abortion will also put an end to illegitimate births.

In short, the events which cause illegitimate births are the same as those which cause legitimate births and there are two means of reducing births; reduce or eliminate sexual activity and interrupt the normal birth process. There are many ways of accomplishing either of these, but from an evaluative research point of view it is important to recognize there is nothing inherent in professional casework, qua casework, that will apparently stop illegitimate births. This does not mean that casework is not a proper modality for dealing with some aspects of this problem. The point that must be stressed is that regardless of the professional treatment modality we must be completely

specific as to the change agent to be used. In this example, some of the change agents that could be considered are: training in birth control techniques, abortion, psychotherapy to modify a deficient superego, a reinforcement schedule designed to extinguish sexual impulses and/or behavior, separation or isolation of the sexual partners, counseling aimed at effecting a marriage, sterilization, etc.

Much of the foregoing discussion has really involved issues relating to proper specification of the dependent variable and value judgments concerning what should be changed. Once these have been dealt with we are in a much better position to select a change agent, but the choice is not thereby guaranteed. Indeed, we now have reached the point at which research technology can be of considerable use to professional social work practice. From a purely blind experimental point of view we could willy nilly select any proposed change agent and then test it to see if it produced the desired change. That would surely result in much wastage. An obviously better procedure is to rely upon both available theory and prior knowledge to identify those change agents which presumably offer some promise for effecting the desired change. Once these have been identified research technology can then be used to test each of them and compare the outcome in terms of which had any or the greater effect on the dependent variable.

The crucial issue at this point is to decide, using theory and prior knowledge, what change agents can reasonably be expected to bring about the desired change. Using, for example, the problem of illegitimate births, what is there about "casework" that we can reasonably expect to reduce birth? Stated in these terms, the proper answer to such a question must be "We don't know." On the other hand, if we have adopted the stance that sexual partners have the right to regulate their own bodily functions we might reasonably claim on the basis of sound prior knowledge that training in contraception will effect considerable reduction in the frequency of pregnancy. No doubt, qualified caseworkers may be selected to provide this training and assist couples or single individuals to get the medical help needed for examination, treatment, prescription and medical supervision. No doubt, all caseworkers are not qualified to give such training and they should be measured or tested in one way or another to determine that they have the knowledge base necessary for successful administration of the change agent--training in use of contraceptives.

An effective change agent cannot be specified in global, general terms. It must be specific and directly related to the problem. However, the necessary specificity of the change agent cannot be achieved unless and until one has dealt explicitly with the valuative issue of what should be changed, and on that basis has specified the proper dependent variable in measurable terms. After all that is done one must be certain that the change agent has been so defined and/or measured as to provide reasonable assurance that the

intended treatment will be successfully administered. If we decide that contraceptive training is the appropriate treatment we shall suffer the chance of failure in treatment and an erroneous research conclusion if we fail to determine that the workers themselves have an adequate knowledge base to conduct the training.

Criteria for Positive Assessment: The Amount of Change

In the majority of evaluative research studies the traditional means of judging the adequacy of social work treatment has involved comparing an experimental with a control or no treatment group. A treatment is considered successful if the amount of change is statistically significant. Indeed, we too claim this difference from the control or no treatment group to be a necessary outcome before we can conclude that treatment has produced a "better" or positive outcome. However, the criterion by itself is neither sufficient nor adequate. Many clients can be statistically different after treatment on a measure used to compare clients in control and experimental groups. However, they judge the significant change as having a trivial impact on their lives that was wholly unworthy of the money, time, and effort they and others invested.

Thus the important question is, does a statistical difference on the measure employed really mean something to the client, i.e., how relevant, important, meaningful, and so forth are the criteria for change for the client. For example, many evaluative research endeavors have used self-inventories as a basis for evaluating client change. Self inventories by themselves may be an inadequate criterion. For instance, in a well designed program to change the attitudes of welfare clients toward their work, their attitudes may change but their work habits may remain the same. Likewise, children who are anti-social may perceive significant amounts of change after being involved in treatment even though their behaviors remain relatively the same. Additionally, using traditional statistical techniques for evaluation of change does not enable one to assess which clients have statistically changed since traditional designs are based on the evaluation of group data and do not reflect individual change. Our objective in social work practice is not to change group scores but rather the behavior of individuals. Here again, we pose the question, what amount of change is necessary to be really relevant to meeting client needs? In many instances a highly significant finding may not lead to the improvement of the client's life (Plutchik, 1974).

Statistical significance is a very important criteria for it is used to rub out the hypothesis that observed change could be attributed to chance. However, it tells us virtually nothing about whether the observed change is important. Moreover, one can nearly always insure statistically significant outcomes merely by increasing sufficiently the size of one's sample. Thus, we claim that statistical significance is not a proper criterion for assessing a positive outcome in evaluative research. It is necessary but inadequate.

One must achieve statistically significant results, but it is only _after_ such results have been obtained that one can properly ask, "Was the treatment effective?" When a researcher demonstrates statistically significant results he has effectively ruled out chance (within certain error limits) as one hypothesis to account for the observed outcome, but he has by no means shown the treatment was effective.

What then do we mean by "effective" in the context of evaluative research? That is precisely the issue that we must decide in advance of conducting the study or at least before the results are in. An experiment, for example, might be conducted to determine whether supportive therapy, positive reinforcement, punishment and deprivation, or intensive psychoanalysis is the preferred modality for improving the performance of underachieving children in a school system. How are we to judge positive outcome in these cases? As we said before, it is not sufficient to show that a statistically significant result was obtained. Suppose, for the sake of argument, that two of the treatments were statistically significant when compared with a no treatment control group. That finding, as we said earlier, merely shows that chance is unlikely to account for the observed gains. But how large are the gains? One of the significant treatments may have produced only a two percent gain while the other produced a four percent gain in performance on relevant criterion variables. In these terms one treatment can be seen as twice as "effective" as the other. But how _important_ is a four percent gain? Unfortunately, that is the kind of question that simply cannot be answered by statistical and scientific methods—it involves a value judgment (this does not mean that value judgments cannot be treated scientifically; they can). The researchers, those who sponsored the research, those who will use it, and the subjects themselves may all have to participate in deciding how large a significant (real) observed gain must be before a treatment can be regarded as effective. Should we demand an overall gain of a specified number of score points, should we demand that the mean score of the target group exceed a specified cutting point, or should we require that every member of the target group obtain a score that exceeds a specified cutting point? The major point here is that a treatment or treatments in evaluative research must be judged as effective, or not, in terms of a well defined and specific criterion or set of criteria that should be set forth at the beginning rather than the end of the research. This is not an easy task for we must determine how much of an effect must be achieved in terms of a set of explicit values. Sometimes it is extremely difficult to enunciate those values and sometimes we do not even want to disclose the underlying real values that motivate an evaluative research study.

A solution to the myth of statistical significance is not to rely on this criterion alone when evaluating the impact of a social treatment. The treatment effect should be interpreted in terms of how the client perceives the change and various other criteria. Thus, use of multiple criteria for evaluation of change may alleviate many of the dysfunctional aspects of the sole use of

statistical significance. Multiple criteria evaluation allows for the measure-
ment of behavior which is multi-dimensional. For example, in evaluating a
treatment program for anti-social children the following criteria could be
employed: various inventories designed to measure anti-social behavior completed
by the children, parents, group therapist, and significant other adults such
as teachers, ministers, and so forth. Additionally, the attainment of behavioral
observational data enables comparisons between perceived behavioral change and
actual behavior. Likewise, the subjective evaluation of the interventions by
clients, practitioners, and significant others through interviews should be
used to assess the practical importance of the interventive attempts. By employ-
ment of multiple criteria for evaluation the investigator increases the
probability that strong aspects of each assessment procedure are included. Thus,
by securing data from various sources a more accurate evaluation of the study
can take place.

Another issue concerning the assessment of positive outcome centers around
whose criterion will be utilized for evaluation of treatment effects. At the
end of treatment the client may be very dissatisfied with the outcome but the
worker may feel that considerable and important changes have been made. How are
such potential conflicts to be managed or dealt with? More often than not
these conflicts arise when the experimenter's goals, or those of his sponsors,
are being served rather than the goals of the client. For example, legal
assistance for the poor may be judged as an ineffectual program by the clients
because the program disallows payment for a divorce in order that two people
living together can thereby establish themselves as a legally married couple.

Obviously the cost of the various treatments must also be weighed in
deciding which ones are effective. Rarely do we find that the treatment that
meets or exceeds the change criteria we establish is also the least expensive
in terms of dollar costs or duration. Neither is it a simple matter of claim-
ing that we want to use that treatment which produces the largest gain at the
cheapest cost. Often times we face the problem of competing objectives--save
money and help the client. In many cases we can achieve one only at the expense
of the other. We might be able to help the client considerably but find that
the cost of doing so would bankrupt the agency. On the other hand, the agency
might survive indefinitely if treatment expenditures are not allowed to rise
above a specified level but the treatment that can be given at such costs
is ineffectual in terms of the established change criteria. We are implicitly
referring here to the minimax principle--minimize losses and maximize gains.
However, this is only a principle, and even if it is achievable it may not be
adequate. Thus, we may find that five different treatments produce statistically
significant results and that they vary in cost and duration. We may even find
that one of those treatments produces the largest gain at the lowest unit cost
over the shortest duration (a rare outcome), and still we might judge it as
ineffectual in terms of any or all of the a priori criteria we established for
the experiment.

It is apparent from the above examples that selection of the criteria to be used in assessing the outcomes of evaluative research cannot be isolated from the issues previously discussed; what should be changed, what is the properly defined and measured dependent variable(s), and what is the properly defined and measured change agent? However, we have now come to recognize that the criteria against which an evaluative study are to be assessed are more often than not multiple in nature and require value judgments that rarely, if ever, can be dealt with by using the tools of science. If we fail to deal with the practice values concerning what should be changed and/or the values under-girding the criteria for determining which, if any, treatments are effective, it is unlikely that research technology will be of any significant benefit in developing a practice science.

Traditional Research Designs

For many years schools of social work have taught their students the "value" of research; that is to develop a practice science, to assess inter-ventions and treatment, and to understand causal influences and non-causal relationships among important variables relating to individual behavior and the actions of groups, organizations, communities, and larger social systems. One emphasis of such teaching has been (stressed in various degrees from time-to-time and place-to-place) the traditional experimental designs that grew largely out of the field of agronomy and was influenced significantly by the monumental contributions of Sir Ronald Fisher. Such designs have been elabo-rated by many scholars from psychology, education, and biology. No doubt, this approach to studying design in evaluative research has been and will continue to be an extraordinarily important tool. However, there are certain com-plexities in evaluative research that such designs cannot adequately cope with, and it is our conviction that social work research scholars are presently so committed to such strategems that significant progress in the use of research technology is likely to be impeded unless the wedlock between social work research and Fisherian design is critically examined and modified.

It is often decided or advocated that large scale designs, such as traditional experimental designs where subjects are placed in a pool and then randomly assigned to either experimental or control groups, are necessary in evaluative research. They are not always necessary, and we know very well that in some cases they will yield incorrect, costly and extremely misleading findings. The result can easily be one of continuing a treatment or intervention that has no effect or discarding one that is quite effective on the basis of the data from one large evaluative study if no replications of the study are executed. One problem stems from the nature and use of controls and control groups in traditional designs. The sole purpose of controls and control groups is to provide us with a basis for developing formal comparisons, and without some formal comparison we do not and cannot have science (Kerlinger, 1973). On the other hand, it is often very difficult to create or obtain adequate

controls for evaluative research. Usually we want to control for everything
except the experimental treatment. As we examine much of the evaluative
research in social work (Fisher, 1973; Maas, 1966, 1971; Mullen and Dumpson,
1972) we must ask what the control groups controlled for. In far too many
cases the control group is labeled as such without specifying or clarifying
what is being controlled and how. In many cases the most important source of
variation that should be controlled is ignored because an inappropriate
or inadequate experimental procedure was used. When using group data and group
research procedures control groups serve their purpose best when they provide
clients as similar to the experimental subjects as possible and when they
experience the same conditions as the experimental groups (except, of course,
for the experimental condition (Wodarski and Buckholdt, 1975). Adequate
controls are essential since a great deal of clinical research indicates that
nearly all persons tend to show improvement through time regardless of whether
they are exposed to social treatment (Bergin, 1971; Eysenck, 1965, and 1966).
Thus, control groups should be composed in a manner that enables them to provide
a baseline against which problematic behaviors can be evaluated during and
following treatment or intervention.

One of the best known and most effective devices for insuring that,
on the average, the subjects in control and experimental groups will be
alike in all respects except the experimental condition is to randomly select
a group of subjects from a well defined population and then randomly assign
them individually to the control and experimental groups. In spite of the
tremendous power of random selection and assignment for creating highly similar
groups these procedures do not adequately safe-guard against two very important
design problems; controlling what happens to the subjects after they are
assigned to the control and experimental groups, and accounting for the pattern
of response to treatment i.e., how the social work services affect the behaviors
chosen for change over the course of treatment.

Ideally, when we conduct an evaluative experiment we try to insure that
the control or comparison groups are identical to the experimental groups except
that the latter are exposed to some treatment that the former are denied. If
these conditions are met we can reasonably conclude that observed differences
between the groups are due to or caused by the specific treatment that was
administered. Unfortunately, it is often difficult to insure that control and
experimental groups experience identical conditions except for the experimental
treatment. When observed outcomes are in the predicted direction (say, the
experimental group did better on a task, as predicted, than did the control
group) there is a good likelihood that the experimental treatment was re-
sponsible. On the other hand, there is always the possibility that some other
unknown influence was operating on either the experimental or control group that
was responsible for the change rather than the experimental treatment. Of
course, the same possibility exists to account for failures in prediction
concerning both the direction and the magnitude of the outcome. The in-
creased possibility of extraneous influences occurring when treatment is

administered over a long period of time is a major characteristic of evaluative studies carried out to assess social work practice. Frequently, it is extremely difficult and in many cases impossible for the researcher to monitor and assess rival hypotheses when the experimental treatment is defined qualitatively rather than quantitatively thus hampering the measurement of the quality of social work services provided through time. One very important means of partially monitoring such disturbing influences is through simultaneously studying concomitant variation of several potentially biasing influences or variables. The most typical means of attempting to control disturbing influences is to hold them constant, e.g., making certain that experimental and control groups are exposed to the same light intensity, background noise, expectations of change, diet, etc. Nonetheless, such strategies do not eliminate or prevent the occurrence of accidental or surreptitious influences upon treatment and control groups.

Another means of controlling, assessing, and eliminating disturbing influences that may operate after treatment begins is to simultaneously track both dependent and independent variables over time. The effects of worker interventions (independent variables) on client behaviors (dependent variables), for example, are monitored through graphing the concomitant variation in worker and client behavior over time (Gottman and Leiblum, 1974). The time series experiment wherein simultaneous measurement is used is a classic in the physical sciences and is being increasingly used in the social and behavioral sciences. Its merits are enormous but it does not eliminate entirely the potential for extraneous variables to produce (or depress) an effect that otherwise would be attributed to the experimental variable(s) being examined. The time-series experiment does provide, however, a number of ways for testing rival hypotheses that are difficult or impossible to deal with in traditional before-after designs. The traditional before-after design more often than not is based on a cross-sectional behavioral sample whereas the time-series experiment includes much of the power of a longitudinal study.

An additional advantage of the time-series design is found in its ability to reflect the pattern of individual and group responses to treatment. This is rarely possible with the traditional Fisherian design. Gottman (1973) points out that the latter designs were developed largely from the field of agriculture when a typical problem was to examine the magnitude of a crop yield at a single point in time--harvest--or a function of many determining influences, e.g., rainfall, fertilizer, cultivation, soil composition, etc. Human subjects, individually or in social systems, rarely if ever, are assessed, evaluated or studied at points analogous to a harvest time. In those cases where such a point can be defined (marriage, graduation, death, promotion, etc.) interest in the subject's attributes are generally transitory and passing because of the dynamic character of human behavioral systems. Real interest focuses more often on the pattern of response to a pattern of treatment. The

time-series experiment enables us to study such patterns of treatment and response and to assess the impact of one on the other whereas this is difficult or impossible with the before-after or pre-post design.

Thus far the time-series experiment has found its highest culmination in the behavioral sciences in the learning laboratory of experimental psychology and the application of learning theory as expressed in behavior modification therapy. Its potential for use in the policy sciences is enormous as seen by its use in economics. An excellent example and discussion of its use in policy administration and evaluation is presented by Campbell (1967).

Through the use of repeated measures of independent and dependent variables time-series designs enable the researcher to monitor changes daily if necessary. Since these designs permit repeated measures of independent and dependent variables they provide a more accurate estimate of the magnitude and form of experimental effects than do the pre-and post-test designs of traditional experimental procedures. These designs, wherein the subject serves as his own control, are inexpensive in terms of money and energy required to implement them and administrative execution is easy. One significant asset is that they provide the necessary data to analyze which person's behavior has changed in comparison to group designs where such changes are difficult to assess.

Time-series designs

Because a great deal can be, and has been gained from use of time-series designs we describe several of them very briefly. The classical design in behavior modification is the A, B, A, B design which consists of four basic phases. It is interesting that social work's emphasis has been on the use of traditional experimental designs in the evaluation of services which involve grouping clients into experimental and control groups. This research philosophy is diametrically opposed to a basic practice principle that every individual is unique and needs to be considered in his own gestalt. The single case study, which has been championed in recent behavior modification research may alleviate many of the issues discussed if incorporated into research on social work services. In this approach the client serves as his own control and a client's change is evaluated against data provided by the client during a baseline period which precedes the application of treatment. This type of methodology also alleviates the moral and legal aspects of placing a client in a control group. It is too early to predict the effects of various legal decisions on the use of traditional control groups in evaluative research. The use of these may be challenged in the future on two legal points: (1) denial

of the right to treatment and (2) denial of equal protection.[2] The data presented
in Figure 1 provide an example of the typical ABAB design used to evaluate group
work service provided to ten 5-and 6-year-old anti-social children. In this
figure percentage frequencies of pro-social, non-social, and anti-social behavior

Insert Figure 1

are graphed for a group of ten children who met for two-hour sessions over a
period of fourteen weeks at a community center. This design consists of four
basic phases. In the first phase the children are exposed to a baseline period.
During this period the group workers do not rationally plan interventions that
are likely to influence the pro-social, non-social, and anti-social behavior
within the group. This is analogous to a traditional diagnostic technique
postulated by Sally Churchill (1965) wherein the group worker refrains from
intervention so that he can more accurately determine the treatment needs of
the group. After the children's observed incidences of anti-social behavior have
stabilized treatment is begun (phase two). Members' behaviors are monitored
until they once again stabilize, whereupon a baseline condition is reintroduced
(phase three or the reversal period). The procedure enables the therapist and
others evaluating the treatment program to determine whether the treatment itself

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For a detailed discussion of these issues see Birnbaum, M. The rights to
treatment, 46 American Bar Association Journal, 449 (1960); Harris, R.W., Note:
Implementing the right to treatment for involuntarily confined mental patients:
Wyatt V. Stickney, 3 New Mexico Law Review 338 (1973). Note: A right to treat-
ment for juveniles?, 1973 Washington University Law Quarterly 157 (1976).
Practicing Law Institute, the Mental Health Law Project, legal rights of the
mentally handicapped, Vols. I and II, Practicing Law Institute, New York, 1974,
Rastatter, Patrick C., Note: The rights of the mentally ill during incarceration.
The Developing Law, 25 University of Florida Law Review 494 (1973); Wodarski,
J.S. Recent Supreme Court Legal Decisions: Implications for Social Work Practice.
Paper presented at 103th Annual Forum National Conference on Social Welfare,
Washington, D.C., June 1976.

accounted for the various changes in behavior. Immediately after it becomes
evident that the treatment has been effective in reducing anti-social behavior
the treatment procedures are applied once again.

In some situations the A,B,A,B, design may not be feasible due to the
types of behaviors being modified and/or for various ethical reasons. The
primary reason for utilizing an alternate design is that in the A,B,A,B
design the modified behavior usually will not reverse itself and, in many in-
stances, reversals would be too damaging to the client or significant others
in his life. For example, when fighting is brought under control in a home it
would not be feasible to do a reversal on this behavior since in the past physical
harm has been inflicted on others. A design that may be utilized in lieu of
the A,B,A,B design is the multiple baseline design wherein a series of behaviors
for modification are operationalized. Predictions are made on how the various
techniques will affect different behaviors. Each behavior is then modified
according to a time schedule. Usually one or two behaviors are modified at a
time. For example, the worker might want to decrease such behaviors as yelling,
fighting, throwing objects, straying from the group; and increase pro-social
behaviors, such as task participation, appropriate verbal comments, and so forth.
The worker in this instance might choose first to ignore the yelling and use
positive reinforcement to increase appropriate verbal comments. Once the
yelling decreases and the appropriate verbal comments increase he would
sequentially modify the second, third, and fourth behaviors. In Table 1 an
outline is provided on how such a process operates. The technique being employed

Insert Table 1 about here

becomes more efficacious every time the behaviors change in the direction pre-
dicted for each child. This replication of results increases the practitioner's
confidence in his techniques and is necessary in evaluative research since the
conclusions gained from any one study or interventive attempt are always
considered tentative.

Another design which can be used in the A,B design. In actuality it is
the first half of the A,B,A,B design. It involves securing a baseline and
introducing treatment after the behavior to be altered is stabilized. This is a
minimum prerequisite for evaluating the effectiveness of interventive attempts.

In summary, all of these designs can be easily implemented in social work;
that is, they are economical in terms of money, energy required to implement
them, administrative execution, and so forth. Above all, they provide data which
will enable a worker to determine if his interventions have had an effect on
client behaviors. It is not practicable to indicate what design should be used
for this depends on the context of the social work practice situation in
terms of behaviors to be modified, time considerations, administrative concerns,

and so forth.[3]

The Organizational Aspects of Research

Evaluative research is based on the assumption that it takes place in a static agency, i.e., an agency that does not change. Even if evaluative research in social work meets all of the requirements of a well designed study one faces the problem of executing the study in social work environments characterized as being fluid or turbulent and having changeover staff, client drop-out, strikes, and so forth. These organizational characteristics and others, such as different administrative styles, level of worker training, number of years of experience, and so forth definitely affect evaluative research. Procedures must be developed to evaluate the confounding effect of the organizational aspects of the agency wherein the research took place. A minimal requisite of any evaluative research study is the adequate specification of the characteristics of the organization in which the study was conducted in order to permit consumers of the research to determine the applicability of findings to their particular organization (Feldman and Wodarski, 1974; Wodarski and Feldman, 1974).

Researchers vs Clinicians

The division of research and practice has proven detrimental to evaluative research in that one cannot effectively operate without feedback from the other. Such detachment of those professionals engaged only in research and those engaged only in clinical practice must be eliminated if evaluative research is to serve the profession simply because isolation of the two precludes the possibility for the exchange of necessary information for evaluative research to address itself to relevant clinical problems. However, this structural problem in social work appears to be one which is not easily overcome. For instance, in the education of social workers research and practice instruction is seldom intermixed. This dysfunctional aspect of professional training would be alleviated by integrating aspects of research that are relevant to clinical practice at all levels of

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For a detailed description of the various designs that might be used to evaluate social work practice interventions see Gottman, J.M., "N-of-One and N-of-Two research in psychotherapy," Psychological Bulletin 80, (1973), pp. 93-105; Browning, R.M., and Stover, D.P., Behavior modification in child treatment, Chicago: Aldine-Atherton, 1971, pp. 75-110; Barlow, D.H., and Herse, M. "Single case experimental designs: uses in applied clinical research," Archives of General Psychiatry, 29, (1973), pp. 319-325.

social work education. In fact, if the profession is to improve services
offered to clients through use of evaluative research the future may see the
development of a research practitioner who will be able to operationalize
social work practice goals and assess, through collected data, whether or not
such goals have been obtained (Broskowski and Schulberg, 1975; Perloff, Perloff,
and Sussna, 1976; Sechrest, 1975; Wodarski and Feldman, 1973).

The Researcher's Distance from Clients

The distance of the investigators from the population to be served must be
kept minimal if research is to prove relevant to client needs. This can be
accomplished by (1) placing clients on the review boards of granting agencies,
(2) hiring clients to conduct various aspects of the research, or (3) having
the investigator spend time with the clients in their actual physical surroundings.
Presently, large sums of money are being spent on research to improve the
criminal justice system. However, unless researchers gain relevant inputs con-
cerning needs of clients of this system their research endeavors may prove
non-productive. Surely one cannot expect relevant studies to be designed in the
ivory towers of university settings without the proper field work (Argyris, 1975;
Elmo, 1975; Gadlin and Grant, 1975; Bard, 1975; Nietzel and Moss, 1972;
Smith, 1973).

Incentives for Research

The means by which various federal agencies presently provide grants leads
to dysfunctional social work research. For example, many agencies and
universities are interested in conducting evaluative research. Unfortunately,
however, their purpose oftentimes is not to evaluate service but to secure
overhead which helps in these troubled times. Moreover, many researchers
are motivated to secure funds to increase professional stature through publica-
tions and financial position and are not genuinely interested in developing
services to meet client needs. These dysfunctional aspects of evaluative
research would be minimized if the granting agency monitoring the research more
closely scrutinized project goals and followed up on attainment, or lack
thereof, of those goals. Likewise, universities should change their incentive
structure for promotion. Rather than providing the incentives for undertaking
those projects which will make relevant contributions to meeting client needs,
but are costly in terms of time, energy, money, and so forth, the present
incentive structure reinforces researchers for executing projects that will bring
them publications most expeditiously, thus benefitting the university and the

researcher in terms of career goals (Ben-David and Sullivan, 1975; Coelho, Rubinstein, Bauer, Snow and Hilgard, 1971; Hall, 1972; Patterson, 1972).[4]

The Dissemination of Information and Application of Relevant Knowledge

Even if we address ourselves to all issues discussed in this paper, evaluative research will face a problem in the dissemination of information as it begins to isolate the crucial variables in social work practice. This problem is intensified when those who practice social work do <u>not</u> spend adequate time reading journals to obtain the necessary information to apply new knowledge (Rosenblatt, 1968; Weed and Greenwald, 1973). Likewise, if practitioners are to participate in evaluative research, which is a necessary condition in developing the treatment technology needed for the field schools of social work must begin to teach the requisite skills for this process such as the ability to formulate questions, decide on various data options needed to answer the questions, and so forth. Furthermore, skills must be developed to compose requisite questions for the development of a practice science, to make rational decisions on the basis of data and not on the vague criteria presently employed, and so forth.

Another problem will be in the implementation of various efficacious technologies as they are developed. A primary function of evaluative research in social work should be to provide a rational method for choosing change strategies to meet client needs. In an area where tradition, authority, and "common-sense" practice have ruled for years, a data-based set of alternatives should facilitate the delivery of more effective services. It would be a mistake, however, to assume that the availability of good evaluative research information will automatically lead to improved services. Histories of knowledge utilization in other human services fields show clearly that the change process is much more complicated than is implied by a simple linear model leading from knowledge production to knowledge utilization. In fact, in most cases, the effective dissemination and use of new knowledge is as problematic as its production.

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This process is especially characteristic of the discipline of social psychology (Jung, 1969 and 1971; Kelman, 1967; Ring, 1967; Schultz, 1969) which may be described as a psychology of college sophomores (Jung, 1971). This population is a captive one and thus can lead to large subject pools which are needed to conduct expeditious studies. Additionally, the costs and energy involved in carrying out such studies are minimal as is its relevance except for its use as a test of preliminary theoretical hypotheses.

For purposes of analyses, the problem of knowledge utilization can be seen as an adoption problem, that is, how to train social work students and practitioners to search for and evaluate research data when choosing intervention alternatives. In some cases, the practitioner may be capable of conducting his own evaluation, but in most cases, we suspect he must rely on information and conclusions reported by more qualified professional researchers and/or evaluators. Certainly, the social worker should be trained to personally monitor the effects of his intervention and to gather feedback on the direction and magnitude of change, but he will rarely possess the resources and skills of a professional evaluator which are required to perform a summative evaluation of the reliable outcomes of a program or a change strategy.

There are data to indicate that even after practitioners are exposed to new techniques and indicate that they are employing aspects of the newer treatment, it is discovered through evaluations that they are practicing in the same manner as in the past and have merely labelled it differently. Thus, to assure that professionals will keep abreast of current treatment developments and, moreover, will implement them, the incentive structure of social work practice must be changed. In order for practitioners to exhibit these behaviors they will have to be rewarded for them.

The problem of the adoption of change strategies which have been scientifically verified then becomes one of effective communication between the practitioner and the evaluator (Bernstein and Freeman, 1975; McNaul, 1972). We need much more information about the informational mechanisms used by social workers before we can make much progress here. While researchers communicate via professional journals and meetings, it is not likely that practitioners rely heavily on these mechanisms. In order to reach the practitioner, advocates of evaluative research need to include knowledge of the informational mechanisms and informal networks of the practitioner in their studies and then make use of them to disseminate knowledge. For example, the role of the change agent in education usually resides in the superintendent of schools or in other administrative positions. The research-evaluation work on innovative procedures and products usually is done in a university or a research and development setting and reported in journal articles or technical reports which are read by only a small minority of the school administrators. The administrator typically relies on reports from colleges and on his own professional organizations and publications for his information on new procedures and products. Researchers and evaluators in education are beginning to realize that they must be able to supply their information to the channels used by the administrators if they want their stories to be told. The same procedure will likely also hold for the dissemination of evaluative research in social work.

Summary

The intent of this paper has not been to play the game of who can best criticize social work practice nor to suggest that evaluative research will not help provide social work services on a more rational basis. Nor has the purpose been to degrade researchers and practitioners in the area. Instead, the goal has been to encourage a more extensive and better conceptualization of possibilities and limits of the utilization of evaluative research to improve practice endeavors.

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Figure 1: Average percentage of pro-social, non-social, and anti-social behaviors exhibited by ten children, according to number of group sessions.
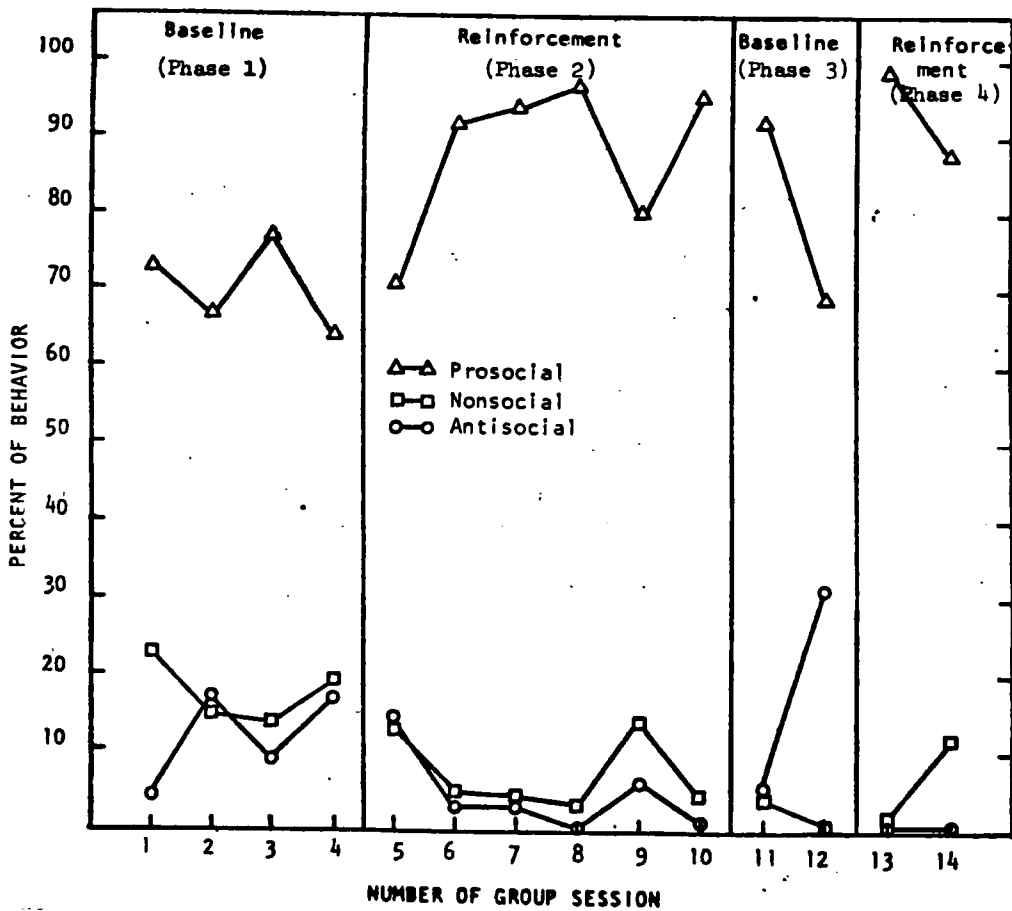
Table 1

Example of Procedure for Multiple Baseline Design, Using Extinction to Decrease
Anti-Social Behavior and Positive Reinforcement to Increase Pro-Social Behavior

Behavior to be Modified	I	II	III	IV
1. Not staying with group – task participation	Modification plan instituted			
2. Yelling – making appropriate verbal comments		Modification plan instituted		
3. Fighting – increasing helping behaviors, such as sharing and working together on a task			Modification plan instituted	
4. Throwing objects – cleaning up after group meeting				Modification plan instituted

NOTE: —— Lengths of time periods are not specified; these will depend on the rigor desired for showing
the effects of the modification plan. A typical period lasts until the behavior stabilizes
at a variance of less than 10% for three to five days.

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