



1985

Attrition in Applied Social Research: A Methodological Study

David William Rivers
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ATTRITION IN APPLIED SOCIAL RESEARCH:

A METHODOLOGICAL STUDY

by

David William Rivers

A Dissertation Submitted to the Faculty of the Graduate
School of Loyola University of Chicago in Partial
Fulfillment of the Requirements for the Degree of
DOCTOR OF PHILOSOPHY

MAY

1985

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ACKNOWLEDGEMENTS

I acknowledge, first of all, the thoughtful guidance of my committee, Drs. John Edwards, Emil Posavac, Frank Slaymaker, and Deborah Hotch. In particular, John Edwards and Deborah Hotch have made a special contribution to both my academic and professional activities.

I appreciate my entire family for their constant help. A special thanks is owed to my parents, William and Elizabeth Rivers, for their unending support and encouragement. The contribution of my wife, Carol, cannot be measured. She has endured a difficult process with invaluable patience and understanding.

VITA

The author, David William Rivers, is the son of William Clarence Rivers and Elizabeth (Fuchs) Rivers. He was born November 24, 1955, in Washington, D.C.

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CHAPTER I

INTRODUCTION AND REVIEW OF THE LITERATURE

A major threat to the validity of applied research studies is subject attrition (Cook & Campbell, 1979; Jurs & Glass, 1971; St. Pierre & Proper, 1978). Several approaches to dealing with attrition have been suggested and many have been implemented. Only in rare instances though, has the efficacy of these approaches been assessed (for example, Hagen, Foreyt, & Durham, 1976). A careful review of the methodological literature reveals that no systematic assessment of the general problem of attrition has been undertaken. Because attrition per se is a practical problem related to the execution and management of a study, it is typically not of interest in itself. Consequently, applied researchers have a limited understanding of the problem of attrition.

While our understanding of the attrition process is lacking, information that is pertinent is not. Evidence regarding causes, effects and solutions to the problem of attrition can be found in many research reports. The problem is that the typical objective of attrition analyses is to rule out threats to the validity of the particular study. Attention is not paid to the issue of delineating and specifying the underlying variables that determine attrition in general.

By utilizing these research reports as an abundant source of data regarding attrition, the present study was undertaken to consolidate systematically those data in a thorough analysis of attrition. All

aspects of attrition were considered, with emphasis on: the effects of attrition, hypotheses regarding its causes, and methodological and statistical strategies aimed at reducing its impact. The sections that follow present a review of the methodological literature pertaining to attrition. This review will provide a detailed illustration of the point made above -- that a great deal of information has been presented regarding attrition, but little has been done to coordinate and systematically evaluate that information.

Defining Attrition

Terms such as "subject mortality" (e.g., Campbell & Stanley, 1966) and "subject dropout" (e.g., Baekelund & Lundwall, 1975) are often used instead of "attrition." However, no matter what term is used, the operational definition remains the same: Attrition is the loss of units from a research study. In analytical contexts it is often expressed simply as a dummy variable. For example, an attrition variable might have the values 0 (for subjects in the analytic sample) and 1 (for subjects not in the analytic sample) (see St. Pierre & Proper, 1978).

Many authors have developed more comprehensive definitions of attrition. Very often the nature of these definitions is related to the context in which they are used. In the national evaluation of Project Follow Through (a large-scale, quasi-experimental program in compensatory education) St. Pierre and Proper (1978) conceptualized attrition in terms of three categories: Policy, Program, and Sample. Policy attrition refers to the loss of a unit (at any level) for administrative reasons. Program attrition describes the loss of subjects due to subject behaviors such as mobility, dislike of treatment, illness, or death.

These behaviors are what is generally thought of when attrition is mentioned. The third type of attrition occurs within the office of the evaluator and/or data collector and is called Sample attrition by St. Pierre and Proper. Attrition here is the result of subjects being omitted from analyses because of data deficiency reasons (e.g., incomplete posttest, missing data on critical variable, inadequate cell size). The usefulness of defining three types of attrition is derived from the context in which they are used. In the case of a large-scale program evaluation the three categories correspond to three functionally distinct sources of attrition.

One variable that is often used as a basis for defining and/or describing attrition is time. In psychiatric and psychotherapeutic research studies, attrition is sometimes defined as "time in treatment." A distinction has been made between "immediate (after one visit), rapid (by 1 month), and slow dropouts (between 2 and 6 months)" (Baekeland & Lundwall, 1975, p. 740). Hague, Donovan and O'Leary (1976) found it useful to distinguish between individuals who withdrew at the end of a 2-week evaluation period and those who remained for some but not all of the treatment period.

The temporal categorization of attrition is particularly prevalent in medical research. Techniques such as survival analysis (e.g., Friedman, Furberg, & DeMets, 1980) and the life-table method (e.g., Colton, 1974) use time as a primary variable when defining attrition (i.e., mortality). For example, if a five year study of surgical treatment versus a medical intervention was being conducted, the mortality rate at five years would not be an adequate description of attrition. It is neces-

sary to study the mortality rate at various points in time throughout the five year period. The reason for this is that the mortality rates may not be consistent over time. This is likely to be the case in surgical interventions which may carry a high initial operative mortality (Friedman et al., 1981).

There are many examples in which researchers use the reasons for attrition as a means for defining or categorizing attrition. Herceg-Baron et al. (1979) in a study of acutely depressed patients divided attrition into two classes. One group of patients who were withdrawn from the randomized treatment by the clinical evaluator were classified as symptomatic failures. Patients who were withdrawn from the treatment because of reasons such as deviations from the treatment protocol, failure to attend appointments, and refusal to continue with the prescribed treatment were labeled nonsymptomatic withdrawals. The distinction between these two categories of dropouts served as a basis for the authors' analysis of attrition.

When survey methods are employed, a comparison is often made between subjects who cannot be located versus those who refuse to participate (Alwin, 1978; Mash & Terdal, 1977; Wise, 1977). The importance of this distinction is that the nature of efforts to increase completion rates depends on which of the two categories is the reason for the attrition. There is a difficulty however with this method in that it may not be possible to determine whether a person was not located or declined to participate (for example, when using mailed surveys).

Summary

In sum, it has been shown that there are two basic ways to define attrition. The first way is the simple approach of operationally defining a subject in a study as in or out. Either the persons completed the requirements for participation or they did not. A more elaborate approach is to define attrition with respect to some other criterion. Examples of such criteria are 1) the components of a large-scale program evaluation, 2) time, and 3) the reasons for attrition. These comprehensive definitions do provide a more detailed and specific description of attrition. And as definitions should be, they are a statement of the precise meaning of attrition. However, as much as they are definitions of attrition, they are illustrations of a general methodological goal. That goal is the specification and confirmation of a causal model of attrition. Each of the criteria used in the definitions could be incorporated as components of a model that describes the causal processes that determine attrition. For example, temporal categorization might furnish data that show that time in treatment is an important variable that influences attrition. The point is that the best use of the comprehensive definitions that are often used for attrition is as a source of information regarding causal processes. For it is knowledge of these processes that will suggest appropriate techniques for dealing with attrition.

The Effects of Attrition

The effect of attrition is best illustrated in the case of a "true" experimental design (Campbell & Stanley, 1966). A true experiment is one that involves the random assignment of units to treatment and control groups. An unequivocal conclusion regarding treatment effects rests on the assumption that at the outset of the experiment, the treatment and control groups were essentially equivalent with regard to ability, motivation, experience, and other relevant variables. Any differences observed in their performance following treatment can safely be attributed to the treatment and not to other causes, provided the groups remain intact. However, when units are lost from the experiment, for whatever reason, there is a serious threat to the assumption of equivalent groups and errors may occur in conclusions regarding the effect of the treatment.

Attrition is also a threat to the validity of quasi-experiments (e.g., nonequivalent control group design). Regardless of the nature of any selection differences between groups in a quasi-experiment, subsequent attrition is likely to have a biasing effect of its own. Attrition may compound the problems caused by selection biases. The advantage of the true experiment over the quasi-experiment is that the overall bias may be reduced by eliminating initial selection differences through randomization (Cook & Campbell, 1979).

Conducting an experiment in a field setting is a complex endeavor. Control over confounding variables, the essence of the laboratory study, becomes a critical problem in a field experiment. The ability to make cause and effect inferences regarding treatment (i.e., program) and out-

come depends on the extent to which the researcher can rule out alternative explanations for the observed effect. Selection differences due to subject attrition is one alternative explanation that may often be a threat to the validity of causal inferences.

Analyzing for Bias

The loss of cases from an experiment does not necessarily invalidate the results of the study. In general, two pieces of information must be examined before doubt may be cast upon the validity of the results. (See Campbell and Stanley (1966) and Cook and Campbell (1979) for a thorough discussion of the role of validity in experimentation.) First, if the persons who drop out of the treatment and control groups are similar to each other but are not representative of those who remain, the external validity of the study is weakened. (External validity refers to the ability to generalize the results of a study to populations, settings, and treatment and measurement variables.) On the other hand, if the persons who drop out of the control group are different in a meaningful way from those who drop out of the treatment group, the internal validity of the study is weakened. (Internal validity refers to the ability to attribute observed differences in comparison groups to the treatment.) In analysis of variance terms: If attrition is non-random within groups, external validity is threatened; if attrition is non-random between groups, internal validity is threatened (Jurs & Glass, 1971). Such findings do not render other analyses of the data meaningless; rather, the results of such attrition findings must be used to understand and perhaps qualify the results of the evaluation/experiment (St. Pierre & Proper, 1978).

Effects Other Than Bias

The threat that attrition poses for the internal and external validity of an experiment is most often mentioned as the primary consequence of attrition (e.g., Cook & Campbell, 1979; Jurs & Glass, 1971; Riecken & Boruch, 1974). Other detrimental effects due to attrition are rarely discussed in the literature, but are significant nonetheless. For example, attrition may have a substantial impact on the statistical power of an experiment (St. Pierre, 1980). Power refers to the probability of rejecting the null hypothesis and is a function of the alpha level of the significance test, the magnitude of the treatment effect in the population, and the sample size. All else being equal then, as attrition decreases the sample size, the power to detect a difference between treatment groups also decreases. Even if attrition is entirely random, it can serve to reduce chances of detecting a real impact of treatment.

Some of the other effects of attrition are indirectly related to research issues. Lyall (1975) has pointed out that attrition may be a crucial factor in determining the costs of implementing a social experiment. When a high attrition rate is expected (for example, in longitudinal studies), some researchers oversample at the onset of the experiment (St. Pierre, 1980). Oversampling increases staff time, data collection, and computer costs. Procedures utilized to minimize attrition and to follow-up dropouts are also costly (Wise, 1977). Given the limited availability of funds for applied social research, it is evident that the threat of attrition should not be taken lightly when planning an experiment in a field setting.

Attrition can also be viewed as having an effect on the client, i.e., the receiver of the program or treatment. The objective of every social program is to educate, counsel, train, or in some way serve the participants. When subjects drop out of a research study, they are also dropping out of a program that is designed to help them. While there is some evidence that program dropouts are not failures (e.g., Silverman & Beech, 1979), Baekeland and Lundwall (1975) report that in studies involving schizophrenics, alcoholics, and drug addicts, the dropout generally fares less well than the person who stays in treatment. Of course not every individual who is recruited, volunteers, or is required to participate in a program is likely to be "helped," no matter what their level of participation. And moreover there are some programs that may not help anyone. But people do drop out of programs that otherwise might have been of benefit to them. Attrition, therefore, should not be thought of as only having an impact on research considerations. Attrition should be viewed in a larger context that includes its effects on the ultimate beneficiary of research, the program participant.

The Extent of Attrition in Applied Research

In order to gain a perspective on the problem of attrition, the following sections provide a review of studies that furnish data regarding the extent of attrition in applied research.

Reported Attrition Rates

In an extensive review of the literature, Baekeland and Lundwall (1975) report attrition figures for several types of treatment pro-

grams.¹ The range of attrition rates was 20% - 57% for patients in general psychiatric clinics, 33% - 50% for patients involved in group psychotherapy, and 32% - 79% for psychiatric inpatients on open wards. The authors cite seven studies concerning the outpatient treatment of alcoholism that had dropout rates of 52% - 75%. Patient losses in outpatient detoxification programs ranged from 26% to 69% and losses in inpatient detoxification ranged from 23% to 39%. Patients in hypertension programs are reported to drop out at the rate of 20% to 50% during the first year.

Attrition rates in educational studies are usually expressed in terms of the length of the study. Follow-up nonresponse rates for Project TALENT were 51% at year 1, 67% at year 5, and 77% at year 11. St. Pierre (1980) reports attrition rates for three large scale studies. Project Developmental Continuity had an estimated attrition rate of 60% over five years, the rate for Project Follow Through was 50% over a four-year span, and during an eight-month period the National Day Care Study encountered a 15% attrition rate. An attrition rate of 20% per year is proposed as a generally accepted figure for educational studies.

The dropout problem in obesity research is well recognized (Wilson, 1978). Harris and Bruner (1971) found that attrition rates may range as high as 83%. A similarly high dropout rate of 70% is reported by Heckerman, Brownell, and Westlake (1978). Other studies have furnished somewhat lower rates: 38% (Hagen, Foreyt, & Durham, 1976) and 36%

It is important to note that many of the programs were not part of research projects. How well attrition data from such programs can be generalized to programs within research studies is open to question.

(Franzini & Grimes, 1980).

Vannicelli, Pfau, and Ryback (1976) in a review of follow-up studies of alcoholics indicate that attrition rates got worse over a number of years of research. Prior to 1967 reported attrition rates were in the area of 25%. In later years, Vanicelli et al. (1976) found that most studies had attrition rates of 30% to 50%.

Reports of Attrition Bias

It is clear that the proportion of persons who drop out of research studies is often rather large. However, citing studies with high attrition rates does not directly address the question of whether attrition is a significant biasing factor in applied research. It is relatively easy to find data regarding rates of attrition in the literature. However, there are fewer examples of studies that include estimates of the biasing effects of attrition (St. Pierre & Proper, 1978). Cook and Campbell (1979) present several examples of attrition patterns that they presumed to be fairly widespread. If their presumption is correct the results of many studies are likely to be biased.

Of those studies that furnish actual analyses of attrition data, Boeckmann's (1981) reanalysis of the results from the New Jersey Negative Income Tax Experiment is especially thorough. She utilized an approach to attrition analyses suggested by Jurs and Glass (1971) and concluded that differential attrition compromised both the internal and external validity of the results of the experiment. Wise's (1977) analyses of Project TALENT reveal that follow-up data were biased because those persons who provided data had higher averages on both a general academic aptitude composite and a socioeconomic index than the first-

wave sample as a whole. Baltes, Reese, and Nesselroade (1977) have illustrated that differences between completers and dropouts in longitudinal studies of adolescent and adult personality and intelligence have compromised the results of those studies. Similarly, Riegel, Riegel, and Meyer (1967) have shown that studies of developmental trends in adults are based on increasingly biased samples. According to their study, dropouts are biologically and psychologically different from non-dropouts. Results from an epidemiological study by Cox, Rutter, Yule, and Quinton (1977) demonstrate a serious bias in their data. They also argue that attrition was a significant problem in several other psychiatric studies. In an evaluation of the Emergency School Aid Act, Coulson (1976) found that the group of individuals that dropped out between pretest and posttest included disproportionately high percentages of disadvantaged students, minority students, and students with low pretest scores. The detailed analyses showed however that the attrition was not differential and was only a threat to external validity.

These few examples show that there is reliable evidence that attrition may often be a threat to the validity of applied research findings. In fact, Riecken and Boruch (1974) claim that attrition is never random; that it always has a systematic component that may bias results. Inconsistency in attrition rates and patterns of bias is an indication of the highly particularistic nature of attrition processes (Cook & Campbell, 1979). Attempting to understand these processes, researchers have developed hypotheses and explanations about the causes of attrition. These are discussed in the following section.

Hypothesized Causal Factors

Hypotheses regarding the causes of attrition can be roughly divided into two categories. The first category consists of hypotheses that are specifically related to characteristics of the program or treatment and its participants. These are hypotheses related to the substantive characteristics of a research study. The second category of hypotheses are those related to the methodological characteristics of study. The usage of these two categories parallels that of Glass, McGaw, and Smith (1981) in their coding of studies for meta-analysis.

Substantive Characteristics

Demographic Characteristics

The variables most widely studied for their relationship to attrition are the demographic characteristics of research participants. In a review of 51 studies of psychological and medical treatments, Baekeland and Lundwall (1975) reported that for 16 of those studies age was found to be related to attrition. In general, younger patients were more likely to drop out of treatment. Turner, Gardner, and Higgins (1970) attribute this relationship in part to the greater geographical mobility of younger people. On the other hand, in longitudinal studies with adults, attrition is more likely for older subjects. This is due in part to an age-related increase in mortality (Schaie, Labouvie, & Barrett, 1973).

Baekeland and Lundwall (1975) report that sex was related to dropping out of treatment in 13 out of 29 investigations (44.8%). Women were more likely to drop out. However in three psychiatric epidemiolo-

gical studies, Cox et al. (1977) found that men were more difficult to follow-up than women. And in many studies no relationship between sex and attrition was found (Baekeland & Lundwall, 1975; Fiester, 1977).

There is little evidence pertaining to the relationship between participants' ethnicity and attrition. Where it has been studied it appears that minority participants are more likely to drop out of studies than are non-minority participants (e.g., Boeckmann, 1981; Coulson, 1976).

Educational level, intelligence, and measures of academic aptitude have been found to be correlated with attrition. In longitudinal studies of aging (Schaie et al., 1973) and in psychotherapy research (Baekeland & Lundwall, 1975; Weissman, Geanakoplos, & Prusoff, 1973) attrition was more likely for less educated subjects. A study of older adults by Siegler and Botwinick (1979) found that low intellectual ability as measured by the WAIS (Wechsler, 1955) was predictive of attrition. Similarly, subjects with low academic scores were found to drop out more often in two educational studies (Coulson, 1976; Wise, 1977).

Perhaps the most widely recognized demographic predictor of attrition is socioeconomic status (SES). Consistently, attrition rates are higher for low SES participants than for high SES participants. This finding has been reported for educational studies (Coulson, 1976; Wise, 1977), psychotherapy studies (Baekeland & Lundwall, 1975; Weissman et al., 1973) and studies of behavior therapy (Fleischman, 1973).

Personality Variables

Various personality variables have been found to be correlated with attrition. Generally the study of this relationship has been

restricted to psychotherapy and alcoholic treatment studies. Baekeland and Lundwall (1975) report that the following variables were examined in at least 10 studies and most often they were found to be positively associated with attrition: social isolation and unaffiliation, social instability, aggressive and passive-aggressive behavior, sociopathic feature, lack of motivation, lack of psychological mindedness, and behavioral and/or perceptual dependence. A "U-shaped" relationship was found between attrition and symptom levels and symptom relief. For example, attrition is high among less anxious and/or depressed patients. Attrition is lower among patients who are anxious and in need for relief. For patients with high symptom levels, who may be less tolerant of delay and frustration, attrition rates again tend to be high. In studies focusing on alcoholics, many personality variables have been found to be related to attrition but the findings are inconsistent (Hague, Donovan, & O'Leary, 1976; O'Leary, Rohsenow, & Chaney, 1979).

Social Psychological Variables

Several social psychological variables have been examined for their relationship to attrition. Perceptions of the attractiveness of a treatment are thought to be related to attrition (Cook, Cook, & Mark, 1977). Hagen et al. (1976) found that subjects in an obesity program who reported that they liked the treatment manual and the treatment itself were more likely to remain in treatment. Some studies report the importance of participants' expectations (Menapace, Anthony, Kaufman, Ross, & Gioe, 1974; Otto & Maas, 1974). Mobley, Hand, Baker, and Meglino (1979) found that expectations and a role choice model to be useful in predicting military attrition.

Another social psychological phenomenon that is of critical importance in studying attrition is mobility. It has been suggested that mobility is particularly relevant in studies involving young adults (Wise, 1977), migrant farm workers (St. Pierre, 1980), and urban populations (Cordray & Staneski, 1976). As Wise (1977) points out, attrition is almost always correlated with mobility.

Program/Treatment Characteristics

The discussion above has focused on those subject variables that may be predictive of attrition. Another set of substantive characteristics that have been studied for their relationship to attrition are features that are specific to the program or treatment being studied. For example, certain administrative factors may have an impact on attrition. These may include the source of referral to a clinic, waiting list procedures, and convenient location and hours (Weissman et al., 1973). Cook et al. (1977) have also noted the importance of making a program convenient.

The characteristics of the persons implementing a program may be relevant. Several authors have reported that the influence of therapist characteristics should be considered when examining attrition from psychotherapy (Baekeland & Lundwall, 1975; Feister, 1977; Weissman et al., 1973). Attrition rates can vary widely across therapists (Feister, 1977). Greater involvement in a program by participants may reduce attrition (Tracy, 1977). And the attractiveness of a given treatment has been indicated as a likely correlate of attrition (Cook & Campbell, 1979). In the New Jersey Negative Income Tax Experiment persons who were in groups receiving lesser amounts of guaranteed income were more

likely to drop out than persons in groups with higher guarantees (Watts & Rees, 1977).

Summary

The focus of much of the research on attrition has tended to be on the substantive characteristics of research studies, particularly the demographics of participants. This emphasis is not necessarily due to a belief that these features are the most important. Rather it may be due to the relative ease with which the study of such relationships may be undertaken. There are two factors involved. First, data are nearly always available on one or more subject characteristics (e.g., demographics, personality, aptitude) and often available for various aspects of the program or treatment. Second, there is usually sufficient variability in these characteristics to allow an analysis of their relationship to attrition. For example, to analyze the relationship between the sex of participants and attrition in a particular study, all that is required is data for sex and attrition status and a sufficient number of both males and females (i.e., some variability). On the other hand, the study of the effect of methodological characteristics on attrition is not as easy. There is generally one obstacle. The methodological features of research are usually designed so they do not vary. In fact, the primary objective of experimental design is to ensure that the only thing that does vary is the treatment that is applied to the various groups of subjects. Introducing additional variability in the methodology results in an additional factor that must be considered in the analysis of the data. While most researchers strive to avoid multi-factor research designs, occasionally they do occur. The next section

describes studies that have employed methodological factors in the study of attrition.

Methodological Characteristics

One of the standard methodological approaches to social research involves the comparison of two or more groups of subjects. The attrition rates of these individual groups may often be different. These differences are generally attributed to the differing desirability of the treatments or programs associated with each group. The simplest example of this is the basic treatment-control group study where persons in the treatment group are recipients of some desirable program or benefits while the persons in the control group are not. It is rather obvious that in general persons in desirable treatment groups are more likely to continue participating than persons in less desirable control groups.

Assignment Procedures

It has been suggested that in addition to the differential desirability of treatment groups, the manner in which subjects are assigned to those groups may also be a factor in attrition. The reactions that people have to the assignment process in social research was the subject of a series of studies by Camille Wortman and her colleagues (Wortman, Hendricks, & Hillis, 1976; Wortman & Rabinowitz, 1979). The first study examined the relationship between subjects' awareness of a random assignment process and their feelings about participating in the study (Wortman et al., 1976). There were no differences between subjects who were aware that they were randomly assigned and subjects who were una-

ware of the assignment process. However, subjects who became aware of the assignment process (via a confederate) were significantly more negative toward the project and its administrators than were the other subjects. Differences between treatment and control groups within each of the awareness conditions were also analyzed. For unaware subjects, there were no differences between treatment and control subjects. The results were ambiguous for aware subjects. The control subjects were just as positive toward the project as the treatment subjects but they were willing to return fewer questionnaires than treatment subjects and had greater feelings of envy toward the other participants. Within the group of becoming-aware subjects, control subjects were angrier, felt worse, and were less motivated than the treatment subjects. The implication of these results is that participants who have negative feelings about a research study may have the additional reaction of dropping out of the study. Thus the results from Wortman et al.'s (1976) study suggest that it is wise to inform persons that they are being randomly assigned to one group another. This guards against the risk that they will find out in some other way and be angry and resentful toward the study and perhaps drop out. Of course the decision whether to inform participants about the nature of the assignment process may be guided more by ethical than methodological considerations.

The second portion of the study by Wortman et al. (1976) was an extension of the first. Focusing on subjects who were aware of the assignment process, comparisons were made between those subjects who were explicitly told that they had a choice whether or not to participate versus those subjects who were not explicitly told that they had a

choice. There were no differences between treatment and control subjects in the explicit-choice condition. However in the no-choice condition, control subjects were significantly less favorable in their reactions to the study. These results imply that in studies where random assignment to treatment groups is employed, differential attrition may be less likely when research participants are explicitly told that they may choose not to participate in the study.

The focus of the study by Wortman and Rabinowitz (1979) was on the effect of different principles of assigning research subjects to groups. Participants in the study (college students) were informed that they would be assigned to an innovative educational program on the basis of one of four selection criteria: merit, need, first come - first served, or random assignment. Participants' self-interest was manipulated by providing them with false feedback regarding their standing on the assignment criterion. (High self-interest would imply an expectancy of being selected for the program.) The first objective of the study was to determine if there is an overall preference in the population for some selection procedures over others. The results indicated that the random assignment procedure was rated as being significantly more fair than any of the other three procedures. The second objective was to evaluate the effect of self-interest on ratings of selection criteria. In the merit, need, and first come - first served conditions subjects in the high self-interest conditions rated their assignment procedure as more fair than subjects in the low self-interest conditions. However, there were no differences in the perceived fairness of the selection criteria among random assignment subjects in the different self-interest

condition. The relevance of this study to the problem of attrition again pertains to the nature of the reactions that research participants have to the manner in which they are assigned to treatment groups. It would appear that attrition may be less of a problem when random assignment is used rather than assignment based on a merit, need, or first come - first served principle.

Differential Vigilance

Another methodological issue related to differences between treatment and control groups which may affect attrition is differential vigilance (Cook & Campbell, 1979). This may be a problem particularly with longitudinal studies and is the result of experimenters being more interested in the subsequent fate of treatment subjects than of control subjects and they may make a greater effort at following up treatment subjects than controls (Riecken & Boruch, 1974). Differences in attrition rates may also be due to the treatment group having more up-to-date addresses than the control group, this being a result of more frequent contact via the treatment (Riecken & Boruch, 1974).

Study Length

The overall length of a research study is a variable that is likely to have an effect on attrition. The problem of attrition in longitudinal research is well known (Wise, 1977). St. Pierre (1980) noted a positive linear relationship between attrition and the length of a number of educational studies. Simply stated, the longer a study continues, the more opportunity there is for participants to drop out.

Participant Burden

Several authors have indicated that the amount of burden that is placed on subjects who participate in research is a significant factor affecting attrition. The burden may be due to the treatment or to research and evaluation tasks. Jurs and Glass (1971) suggested that the length and pleasantness of treatment sessions and tasks are related to attrition. An overly long interview or survey may create similar problems (Riecken & Boruch, 1974). Moreover, surveys that include questions that are confusing, intrusive, or embarrassing can provoke dropping out. In a study that involved subjects continuously monitoring their smoking behavior, those persons that dropped out cited dissatisfaction with the demands of the recording procedure as their reason for quitting (Frederiksen, Epstein, & Kasevsky, 1975). Likewise, in a longitudinal study reported by Wise (1977), a low response rate to a follow-up survey was attributed to the large amount of information requested.

Summary

Certainly it is not an especially profound statement to say that the more difficult and time consuming a study is for subjects, the less likely it is that they will want to participate. It is clear that problems of external validity may arise when a portion of total sample drops out because of the burden of participating. But a more important issue to be confronted is: To what degree does the burden of participating differ across treatment groups? It may often be the case that the demands placed on treatment subjects are greater than those placed on control subjects. While this difference in experiences for the two groups of subjects may create interpretive problems in and of itself

(construct validity), it may exacerbate those problems by creating differential attrition between the groups. This set of circumstances is not limited to treatment/evaluation burden. Other methodological characteristics may vary across treatment groups and result in differential attrition. When this occurs and the variables are appropriately measured or controlled a better understanding of the attrition process is gained. But, as noted earlier, researchers generally aim at eliminating methodological variance. They are not looking to see how attrition may be related to methodological factors. There is a clear need for studies that incorporate systematic evaluations of the impact of various research methods on attrition. This is especially true because it is only through research methods that attrition rates can be reduced. While the characteristics of subjects may be related to the probability of their dropping out of research studies, it is not possible to manipulate those characteristics as a means of minimizing attrition. Rather, techniques for countering subject dropout are based on research methods and on the substantive characteristics of studies that can be controlled. Several approaches have been described in the literature and they are discussed in the following section.

Methodological Strategies for Reducing Attrition

A great many methodological strategies have been recommended for reducing attrition. The degree to which these strategies have been shown to be effective varies widely. There appear to be three levels of demonstrated effectiveness. At the lowest level are those techniques that have been suggested as being useful though there is no hard evidence to support that claim. Generally these approaches are based in

part on common sense. The second level of effectiveness corresponds to those strategies that have been employed by researchers with the feeling that they had a positive influence by reducing attrition. This opinion is based on the fact that a given method was used and a low attrition rate resulted. However, no comparison groups were used so it is not possible to know what the attrition rate would have been had the method not been used. The evidence is strictly conjectural. The highest level of demonstrated effectiveness is achieved by those strategies that have been evaluated in a comparative framework. Occasionally this is an experimental comparison, but more often the comparison is less rigorous such as a before-after design. Even at this level, there are few studies that have been implemented in a way that allows causal attributions to be made between a methodological technique and a reduction in attrition. Nonetheless, the evidence for effectiveness is superior to that for the methods at the other levels. These three levels of effectiveness will serve as a guide for reviewing the various strategies. Those techniques having the greatest degree of demonstrated effectiveness are discussed first.

Strategies with Demonstrated Effectiveness

The use of monetary deposits is often suggested as a means for reducing attrition (Jurs & Glass, 1971; Wilson, 1978). The usual approach requires that subjects leave a monetary deposit with the experimenter at the beginning of the study. The deposit is returned contingent upon the subject's compliance with study requirements (e.g., attendance, completing surveys). Hagen et al. (1976) found that the use of a monetary deposit in an obesity study significantly reduced attrition.

An alternative to holding a refundable deposit is to use cash payments as an incentive for continued participation. Fleischman (1979) used a "parenting salary" in a social learning-based program for problem children. There were fewer dropouts among the group of families who received the salary. This effect was most dramatic among low-income and/or single parent families. This latter finding points out the fact that the decision to use a cash deposit or a cash incentive depends on the situation. Perhaps the first consideration is whether or not the experimenters can afford to pay subjects for their participation. If the scope and/or the budget of a project does not allow this, then the use of a refundable deposit may be a viable alternative. The problem with deposits, however, is that a particular program must be attractive enough to make demanding a deposit feasible (Jurs & Glass, 1971). And what of the control group? Clearly a deposit cannot be requested of persons who are getting "nothing" in return. The possible applications appear to be broader for monetary incentives or salaries rather than refundable deposits.

Another use of money for minimizing attrition is described by Maslany and MacKay (1974). In a longitudinal study of achievement, 60 subjects out of the original 320 were located by the researchers at follow-up. These subjects were supplied a list of the names of the missing subjects and informed that they would receive a sum of money for each address they provided that resulted in a completed questionnaire. Two hundred additional addresses were obtained and 150 of these previously unlocated subjects returned completed questionnaires.

Problems in obtaining responses to follow-ups, particularly those

using mailed surveys, are well known (Wise, 1977). Vannicelli, Pfau, and Ryback (1976) increased the return rate of a mailed follow-up survey from 43% to 91% using a combination of four techniques. First, they emphasized to subjects that follow-up is an integral part of the treatment program. The notion that follow-up measurements should not have the appearance of being "tacked on" to the end of a study has been proposed by other authors as well (Mash & Terdal, 1977; Wilson, 1978). Second, Vannicelli et al. (1976) included a personalized letter in each survey packet. Reminder letters were sent to those persons not responding to the first mailing; and last, telephone reminder calls were made. The success of these methods in increasing the return rate indicates the possible value of a multifaceted approach. In an approach similar to Vannicelli et al's (1976), Sewell and Hauser (1975) obtained a large increase in the return rate of a follow-up survey by sending four separate waves of questionnaires. The final rate was 32% higher than would have been realized with only one mailing.

Success in reducing attrition can sometimes be achieved in simple ways. Panepinto and Higgins (1969) reduced dropout rates in an alcohol clinic simply by sending patients appointment letters whenever they missed a scheduled visit.

The effectiveness of methods used to influence reactions to random assignment was discussed above. To reiterate, Wortman et al. (1976) showed that two techniques may be useful in decreasing attrition in studies where random assignment is employed. First, the subjects should be made aware of the various assignment conditions prior to randomization. And second, research administrators should stress subjects' free

choice about participating.

Strategies with Reported Effectiveness

A good example of a study providing moderate evidence of the effectiveness of attrition counter-measures is given by Coulson (1976). Three procedures were thought to be helpful in reducing school-level attrition. First, incentive payments were given to school administrators and staff who participated by administering tests or completing questionnaires. Second, respondent burden was minimized by keeping questionnaires as brief as possible. It was felt that this was a key factor in the acceptance of the project by the school staffs. And third, a type of fringe benefit was given to key administrators in each sample district. They were invited to the researcher's offices in Southern California for an informal orientation at no cost to the school districts.

Student-level attrition in the Coulson study was handled in a number of ways. To increase completion of posttest data, make-up sessions were given. Also field representatives were sent into each sample school to encourage and aid the respondents in completing questionnaires. Data collectors were instructed to maintain careful logs on each sample student that would indicate when attrition was becoming excessive in any particular site. This allowed the researchers to take remedial action such as additional makeup sessions. And last, sample sizes for certain subgroups of students were increased to ensure sufficient numbers for analysis at the completion of the evaluation. The usefulness of this last method rests on the assumption that attrition was random (Jurs & Glass, 1971).

Methods that increase the attractiveness of a treatment program may serve to reduce attrition. For example, Wedel (1965) provided additional, ancillary services to the participants in a study of alcoholics. Sometimes the researchers can help alleviate minor obstacles or incidental costs of participating. Hudson (1969) furnished subjects with transportation to a remote location where an autotelic teaching device was being used for the treatment.

To increase the return rate of a mailed, follow-up survey in a study of former high school students, Wise (1977) utilized several methods. Four separate waves of questionnaires were mailed with success similar to that found by Sewell and Hauser (1975) noted above. Because the survey was an 11-year follow-up, address maintenance was very important. The chief mechanism used for maintaining addresses was an annual newsletter. This letter had a cutout form for participants to notify researchers of address changes. Additionally, the letters were marked "address correction requested" which indicates to the post office that the researchers are to be notified of any change of address. The annual newsletter also attempted to increase subjects' motivation to participate by giving them feedback from previous surveys and informing them of future plans.

Other address maintenance techniques included correspondence with high school class reunion coordinators. For a special nonrespondent survey, a retail credit company was used to track down a number of difficult-to-locate cases. But most of subjects were located by an in-house staff that utilized several sources of information for locating subjects. These included many directories and files from the study

itself, school directories, telephone books, area code books, zip code book, almanac, road atlas, maps, Haines directory (reverse street-telephone directories), reunion class lists, marriage bureau record file, and Department of Motor Vehicles file. Two pieces of information collected at the beginning of the study were found to be most helpful. These were the respondent's birthdate and parents' names. The birthdate was used in searching many information sources, such as the Department of Motor Vehicles records. Since parents are generally less mobile, contact with them was often easy and they were nearly always cooperative in providing information on the participant's current address.

The importance of having detailed information for follow-up has been noted by others (e.g., Chandler, 1974; Clarridge, Sheehy, & Hauser, 1978). Data found to be useful include social security number, driver's license numbers, names and addresses of people who would always know where to contact the participant, name and address of employer, names and addresses of colleges attended, and names of spouse and other family members.

Strategies with Suggested Effectiveness

The sections above have discussed those attrition counter-measures that have been found to be effective to some degree on at least one occasion. There are many additional methods that have been suggested in the literature with little or no supportive evidence regarding their effectiveness. As mentioned before, many of these methods are based to a degree on common sense. It is likely as well that the experiences of particular researchers have led them to believe that certain attrition counter-measures are effective, though this effectiveness was not demon-

strated through an empirical evaluation. Nonetheless these measures deserve mentioning because of their potential usefulness. They are reviewed briefly.

The measurement component of research studies is often cited as a source of attrition problems. A possible solution is to use deception or unobtrusive measures (Jurs & Glass, 1971). Creating a non-reactive experimental setting has been described by Webb, Campbell, Schwartz, and Sechrest (1966). Jurs and Glass (1971) also propose that measurements could be rescaled so they are more meaningful to the respondents. Furthermore, it may be helpful to have interviewers who are sensitive to respondents' attitudes and who can relate easily and comfortably to respondents (Kershaw, 1971).

There are procedures that can be undertaken before or at the onset of a research project that might be effective in preventing dropouts. Researchers should explain clearly to subjects the aims, scope, probable results, possible side effects, and duration of the treatment or program to which they are assigned (Baekeland & Lundwall, 1975). Mobley, Hand, Baker, and Meglino (1979) have proposed a strategy in the context of military attrition that would employ a pre-recruit training session that attempts to enhance expectancies of completing and to provide realistic expectations regarding the outcomes of military life. Such an approach might be useful in applied social research settings as well. Kershaw (1971) indicates that it may be wise to enlist the cooperation of community leaders who endorse participation in the study. At the initial stages of the project, potential participants would inquire with the leader and be encouraged to participate. The attractiveness of differ-

ent treatment groups may be related to attrition. Cook et al. (1977) suggest the possibility of using alternative treatments in place of no-treatment control groups. However this prohibits an assessment of the absolute effectiveness of the treatment of interest.

The influence of "significant others" may be important. Baekeland and Lundwall (1975) hypothesize that increasing the involvement of the significant other may reduce attrition in psychotherapy studies. Many treatment programs are implemented in group settings. In such instances the group may be used as a source of approval or disapproval with regard to attendance patterns (Hagen et al., 1976). The researchers themselves can have a direct influence by applying appropriate pressure to participate in the form of phone calls and follow-up letters (Hagen et al., 1976). Finally, it has been recommended that investigators maintain frequent contact with participants (Wilson, 1978). Regular phone calls or mail contact are suggested and may be particularly important in longitudinal studies.

Summary

Several methodological strategies for reducing attrition were described. These involved the use of incentives for the participants (e.g., cash payment), incentives for administration and other site-level personnel, various tracking and follow-up techniques, informed consent procedures, and methods for minimizing respondent burden. The importance of collecting appropriate background data on participants was also discussed.

The empirical evaluation of attrition counter measures has been limited to a very few techniques and to only a few research settings.

It is not known whether a particular counter measure will be more successful in one setting than in another. Furthermore, the relative effectiveness of various counter measures in any given setting is unknown. As a result, the use of particular counter measures by applied researchers is based primarily on conjecture.

Analytical Techniques for Dealing with Attrition

There are two components to the analysis of research data with respect to attrition. The first part consists of methods that are utilized to determine the extent to which data from a study may be biased because of attrition. The second part is closely related to the first and consists of the various statistical techniques that may be employed in an effort to reduce the biasing effects of attrition.

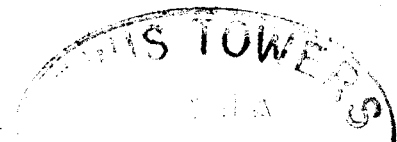
Techniques for Analyzing for Bias

Riecken and Boruch (1974) and Cook and Campbell (1979) have delineated a straightforward, yet thorough strategy for detecting attrition bias in a social experiment. The first analysis is to examine the rate of attrition in the various treatment groups. If the rate is different across the groups, possible bias is indicated. Similar attrition rates are not necessarily an indication of a lack of bias however. Bias is related to the underlying causal processes that influence dropping out. A similar attrition rate in two groups could be the result of two distinctly different processes. Therefore, further analyses are required to fully evaluate the possibility of bias. One of these is to examine the reasons that respondents give for dropping out. Bias would be indicated if these were different across the groups. A third set of analy-

ses involve comparing the dropouts from each group on blocking and background variables and covariates. In addition pretest measures should be compared. These analyses may reveal that the persons dropping out of one group are in some ways different from the persons dropping out of the other groups.

An alternative set of analyses that is suggested is the complement of those above. Instead of examining for differences between groups for the dropouts, a comparison of pretest and background data is made for those persons who remain in the study. Providing subjects were randomly assigned to groups, these analyses estimate the degree to which the randomization has remained intact.

Jurs and Glass (1971) propose a more detailed analysis that examines bias in terms of threats to both internal and external validity. A 2 X 2 analysis of variance framework is utilized. One factor represents treatment condition (e.g., treatment versus control) and the other factor represents attrition (i.e., dropped out of the experiment versus remained in). With pretest scores serving as the dependent variables, a main effect of the attrition factor signifies a threat to external validity. A threat to internal validity is indicated if there is a significant interaction effect. That is, the type of person who dropped out of the treatment group is different (as measured by the pretest) from the type of person who dropped out of the control group. A good illustration of the Jurs and Glass approach is furnished by Boeckmann (1981). Her reanalysis of data from the New Jersey Negative Income Tax Experiment indicates that attrition may have compromised both the internal and external validity of the results.



St. Pierre and Proper (1978) suggest a slightly different approach that utilizes multiple regression. The analysis addresses the question of whether the relationship between attrition rate and a relevant variable differs across various treatment groups. For example, if pretest score is the variable of interest, a hierarchical model provides a regression of attrition rate on pretest for each group. If the slopes of the regression lines are different, a selection bias due to differential attrition is indicated.

Another approach to detecting bias due to attrition is through a temporal analysis. In particular settings attrition may vary as a function of time. A typical pattern is one in which attrition is initially high and subsequently levels out, resembling a Poisson or exponential distribution (Baekeland & Lundwall, 1975). Such a pattern indicates that attrition is operating in a nonrandom fashion. An example of this can be found in medical research. In the evaluation of surgical versus medical interventions, surgery may carry a high initial operative mortality (Friedman et al., 1981). At a later point in time the pattern of attrition for the two groups may look substantially different.

The utility of a temporal analysis is not limited to any one setting. In any study, the pattern of attrition over time may vary across treatment groups. For example, control group participants may be likely to drop out early in a study, due to resentment. Attrition in the treatment group may occur later in time when participants become dissatisfied, bored, or overburdened by a program. An analysis that describes these different patterns would point toward possible bias. Perhaps the biggest obstacle to a temporal analysis of attrition is a lack of infor-

mation regarding the precise (or even approximate) time that a person drops out of a study. Unless data are available as a result of a repeated measurement framework or consistent monitoring, this type of analysis may be impossible.

Issues Regarding a Lack of Attrition Data

The problem of a lack of data hinders many reserchers when confronted with a high attrition rate. The difficulty is that in order to conduct an adequate analysis of attrition, one must know something about the people who have dropped out of the study. In most cases all data for dropouts come from pretest surveys and/or from archival records. These sources are often lacking in the detailed, descriptive information that can faciltate an analysis of attrition. There are at least three solutions to the problem. An obvious one is to collect more data as early as possible from all participants (Bernstein, 1976). The nature of these data would depend on one's theories or hypotheses about attrition processes. If one expects that attrition may be higher among low income participants, then a question about earnings would be indicated. In general, the better one can describe the participants, the easier it is to analyze for attrition bias. However it is often difficult to collect a great deal of personal and biographical data from respondents because such questions increase an already high respondent burden and they may appear intrusive or unnecessary.

A second alternative to the problem of a lack of data is to conduct a special follow-up survey of some or all of the dropouts (Wise, 1977). The major advantage of this approach is that data can be collected for variables and measures of primary interest to the research

study (e.g., posttest survey). While these data might be included with the data from the non-dropouts, they would be especially useful when compared to the data for the non-dropouts in an assessment of the extent of attrition bias. An additional benefit of a follow-up survey is that respondents can be asked why they dropped out of the study. Answers to this question may furnish the best data for estimating the likelihood of bias due to attrition. The disadvantages of this method are that it is costly and there are no guarantees that the follow-up won't suffer from attrition problems of its own.

The third approach to gathering more data is through the use of an exit interview (Cordray & Staneski, 1976). The content of such an interview might be like that of the follow-up survey or it may simply gather descriptive and biographical data. Certainly the person should be asked why they are dropping out. The feasibility of an exit interview depends upon the nature of the research setting and logistics. An appropriate setting would be one in which the researcher (or his/her allies) has both some control and an opportunity to make contact with participants near the time when they drop out of the study. Studies that take place in institutional settings (i.e., schools, hospitals, prisons) often meet these requirements.

Techniques for Adjusting for Attrition Bias

Given that attrition has occurred, various analytic approaches have been proposed that try to statistically correct or adjust for bias resulting from differential attrition.

Analysis of Covariance

A traditional and widely used approach is the analysis of covariance (ANCOVA) (Huitema, 1980). The application of ANCOVA in the case of attrition parallels that for the nonequivalent control group design. That is, an adjustment is made for differences between treatment and control groups with the use of one or more covariates. Covariates are chosen because of their correlation with treatment group membership. Assuming that random assignment has been employed, the covariates would be those variables related to attrition because it is attrition that determined the final group membership. While the use of ANCOVA to adjust for group differences due to attrition appears to be the proper approach, it is in fact inappropriate. A number of authors have pointed out the interpretive problems with ANCOVA when it is applied to nonequivalent group studies (Boruch & Rindskopf, 1977; Campbell & Boruch, 1975; Huitema, 1980; Magidson, 1977).

True-Score ANCOVA

A major cause of problems with ANCOVA is measurement error in the covariates. This error introduces bias into the estimates of the regression slopes and thus bias into the estimate of the treatment effect (Cook & Campbell, 1979). This problem has led to the development of techniques that attempt to correct for the effects of measurement error. A widely cited approach is that suggested by Porter (1967) and known as "true-score ANCOVA." A hypothetical true-score covariate replaces the observed covariate in the ANCOVA model. The true-score is estimated by regressing the observed score toward the respective group mean by a factor of the within-group reliability.

An important consideration when employing true-score ANCOVA is the selection of an appropriate reliability estimate. Among the choices are a parallel forms estimate, a test-retest estimate, the pooled within-group correlation between the pretest and posttest, and an estimate of internal consistency (e.g., Cronbach's alpha). There is disagreement as to which estimate should be utilized, though one solution is to use several estimates and examine the results for convergence (Huitema, 1980).

Developing a Model of Attrition

The appropriateness of any statistical technique for analyzing data with attrition problems depends upon the particular model on which the analysis is based. In the case of true-score ANCOVA, the analysis is based on a selection model that assumes that assignment to groups is a function of true covariate scores. This may be the case when subjects select themselves into programs (or selectively drop out) and this selection process is based on their true standing on one more variables, not their observed standing (i.e., measured with error) on the variables (Kenny, 1975).

An alternative analytical model is needed for the situation where selection results in groups of subjects from different populations. When these groups are observed at two points in time, it is possible that the change for one group will be quite different from the change for another group. Various growth models have been developed to describe the nature of this change. Two of these are a parallel mean growth model and a fan-spread model (Huitema, 1980; Kenny, 1975). In the case of the parallel growth model, gain score analysis is appropriate. The fan-spread model, on the other hand, characterizes a situation

where the variance increases from pretest to posttest and gain score analysis will have biased results because of the different variances. Kenny (1975) describes an alternative form of analysis that equalizes the variances through standardization. Standardized change score analysis is designed to separate differential growth from treatment effects.

There are many complex issues that have not be discussed here that must be dealt with when using any of the analyses described above. These issues are described in detail in Cook and Campbell (1979), Huitema (1980), and Kenny (1975). The most important consideration in the use of these techniques is an understanding of the selection process. Each analysis is based on a specific selection model and provides unbiased results only when the model is accurate. Misspecifying a selection model can lead to severely biased results. In order to adequately specify a selection model one must have an understanding of the selection process. In some cases this is not a problem, such as when subjects are selected from different census tracts, different organizations, or volunteer and nonvolunteer populations (Kenny, 1975). On the other hand when attrition occurs and subjects select themselves out of a program, the choice of a selection model, and thus a mode of analysis, is considerably more problematic. In this case, knowledge of attrition processes is essential for determining which analysis is most appropriate. When such knowledge is lacking, the best alternative is to conduct several analyses. Huitema (1980) suggests that the following analyses should be computed when selection factors are unknown: (1) ANCOVA, (2) true-score ANCOVA using both internal consistency and pretest-posttest reliability estimates, (3) gain-score ANOVA and/or standardized change-

score analysis and, (4) if additional data are available, multiple ANCOVA and multiple true-score ANCOVA and/or dry-run analyses. Each of these analyses are based on different selection models and the results they provide should span the results that would be obtained in the analysis of known selection model. Confidence in one's conclusions is greatest when the results of the various analysis are consistent.

Causal Modeling and Econometric Strategies

Generally, the statistical methods described above can be considered to be part of the applied social researcher's repertoire of skills. The analyses are based on the familiar techniques of analysis of variance and multiple regression. Recently, other statistical approaches that are less familiar and mathematically more complex have been applied in the area of social research. Two of these are prominent -- the structural equation (causal) modeling techniques of Joreskog (1977) and the econometric strategies described by Barnow, Cain, and Goldberger (1980), Hausman and Wise (1979), and Heckman (1979).

The causal modeling approach consists of two primary components: a structural equation model and a measurement model. The structural model specifies the relationship between the relevant variables (e.g., pretests, posttests, covariates, treatment group status) as though they are perfectly measured. The measurement model specifies how these latent, unobserved constructs are related to the measured variables. Two measures of each latent construct are required. Maximum likelihood estimates are obtained for the various parameters and the overall model is tested for goodness of fit via chi square. A major advantage of the causal modeling approach is that it corrects for measurement error.

This advantage over standard ANCOVA is illustrated by Magidson's (1977) analysis of a nonequivalent control group situation. He points out that the technique could be similarly applied in the context of differential attrition. A major problem with structural equation modeling is that an infinite number of causal models can be proposed in a given situation. Formulating the correct model is especially difficult in studies where causes are unknown and/or unobserved (Magidson, 1977), which is likely to be the case when differential attrition occurs. More detailed information regarding the use of structural equation models can be found in Bentler and Woodward (1979), Long (1976), Joreskog (1973, 1974, 1977), and Joreskog and Sorbom (1978).

The econometric strategy involves a simultaneous equation system. The basic framework uses one equation as a model of the outcome (i.e., posttest), a second equation is an attrition model. Heckman's (1979) approach estimates the attrition equation via probit analysis, and employs that information as a regressor in the outcome equation. Hausman and Wise (1979) describe a slightly different approach with three equations in an analysis of attrition bias in the Gary Income Maintenance Experiment.

The application of econometric methods in social research appears limited to studies such as the negative income tax experiments. Given their complexity and unfamiliar nature (to social scientists), use of these methods in the near future would seem to be limited to studies being conducted by or in consultation with econometricians.

Summary

It is evident from this review of the methodological literature that attrition poses a significant problem in applied social research. There are few conclusions that can be drawn from this review regarding the best ways for dealing with attrition. The evidence does point to two possible strategies. The first and most obvious is to prevent attrition from occurring at all in the first place. If subject dropout can be minimized, then the concerns over bias in analytical results are eliminated. The second strategy is to understand fully the attrition process so as to specify adequately the appropriate model for the analysis. The most likely solution is a combination of these two strategies. In any case the requirements necessary for following either approach are the same. However, in order to follow these strategies it is necessary to have theoretical knowledge and scientific hypotheses regarding the attrition process. For the most part, such knowledge and hypotheses have yet to be developed.

There are several theories in social psychology that might be drawn upon as a means of conceptualizing the bases of attrition. Social psychological theories are important because they suggest several independent variables that may play a role in the attrition process. For example, expectancy-value theory suggests that a person's behavior is based upon his or her expectations about the likelihood of various consequences of behavior and his or her evaluation of those consequences (Feather, 1982). With regard to attrition, if a person believes that participating will lead to more favorable outcomes than will dropping out, he or she will continue to participate. Thus, an independent vari-

able suggested by expectancy-value theory is the beliefs a person has about the consequences of participating in a research study. Using expectancy-value theory, an appropriate counter measure would be one that informs the participant of the positive consequences of participating and stresses the likelihood that they will occur.

Another relevant theory is that of cognitive dissonance (Festinger, 1957) which suggests several other variables that may be important: the justification a person receives for participating, the amount of effort that is expended, the perceived choice in participating, and the degree of commitment made by the participant. The conflict between dissonance theory and reinforcement theory indicates that the role that incentives (justification) play may depend upon the amount of choice a person has in participating. If there is low choice, a high incentive will increase participation (reinforcement explanation). If there is a high degree of choice, a low incentive (i.e., insufficient justification) will increase participation (dissonance explanation).

Still other relevant variables are suggested by role theory (e.g., Biddle, 1979) which concerns the strength and clarity of expectations conveyed to and experienced by a person when performing a social role. Theoretically, persons who have been given clear and repeated expectations about their role as a program participant and who regard this role as relatively central to their self image will be less likely to drop out.

Expectancy-value, cognitive dissonance, role, and reinforcement theory are only a sample of the many social psychological theories and approaches that could be applied to the study of attrition. It would be

possible to test predictions of one or more of the theories in the context of an applied research study. However, doing so would likely create additional factors requiring a larger sample and complicating the primary research design. More data would need to be collected from the participants, increasing burden and costs. Should such a study be successfully implemented, the results may be generalizable only to a particular set of circumstances. Several of these studies are needed before conclusive evidence would be available about the utility of a given social psychological approach to dealing with attrition. This is not to say that empirical evaluations of techniques for minimizing attrition are not worthwhile. On the contrary, they are the most definitive way to establish the efficacy of any approach to attrition.

Given the effort and expense required to undertake an experimental study of attrition, it would be valuable to scrutinize any available evidence pertaining to attrition. Such evidence may help to avoid unnecessary studies and to improve the design of those that are needed. Furthermore, examination of available evidence may reveal the importance of variables that are not necessarily suggested by current theories. The present study was conducted for the purpose of gathering data pertaining to attrition from existing research reports. This approach can be viewed as a preliminary step to more empirically-based studies of attrition. This study pooled information from a large number of studies. The rationale for this approach rests on the assumption that any applied research study that has already been completed can furnish some information regarding attrition. That information includes such data as the attrition rate, the correlation between attrition and certain vari-

ables, the counter measures that were employed, and the statistical analyses used. The next chapter describes the methods for gathering and analyzing the data that were collected from each study.

CHAPTER II

METHOD

Sample Selection

A purposeful sampling strategy was employed to obtain a relatively representative sample of applied social research studies. The objective was to obtain a heterogeneous group of studies in order to gather a wide range of information about attrition. Specifically, the studies were chosen to represent seven categories of research studies (see Table 1).

Six sources that contain abstracts of applied research studies were used as the basic pool of studies (see Table 2). Each of the abstracts in the designated areas was read and considered for selection according to six criteria, displayed in Table 3.

The criteria were used such that any study being included in the sample may have had a problem due to attrition. The most recently completed studies were considered first. For those studies that met the criteria, an effort was made to obtain the relevant research reports. Studies were selected until the target sample size for each study category was reached (see Table 1). When a study report was obtained, it was verified that the study met the selection criteria. If not, an additional study was chosen. Appendix A contains a list of the primary bibliographic references for each study.

Table 1
Study Categories and Sample N's

Program Type	N
Elementary Education	10
Secondary and Higher Education	10
Training Programs	10
Health Services and Medical Treatment	15
Mental Health	15
Welfare and Social Service	15
Criminal and Civil Justice	15

Table 2
Sources of Research Study Abstracts

Source	Time Frame	Sections/"Terms" Searched
Federal Evaluations 1980	1977-1980	All agencies except HEW
Compendium of HEW Evaluation Studies	1976-1981	All divisions
Psychological Abstracts	1978-1981	"Mental Health Program Evaluations"
ERIC	1978-1981	"Program Evaluations"
Abstracts of Health Care Management Studies	1978-1981	"Chronic Disease Facilities and Programs, Community Attitudes and Public Relations, Management Science and Operations Research, Nursing Service, Occupational Th., Outpatient Care, Patients, Psychiatric Facilities and Services, Physical Th., Rehabilitation, Social Services"
Evaluation Studies Review Annual	1979-1981	All studies

Table 3

Criteria Used in Selecting the Sample of Studies

- 1) The research was completed during the period from 1976 through 1981.
- 2) The research concerned a social problem.
- 3) Individual participants were identified as belonging to one or more treatment/ comparison groups beginning at a specific point in time and continuing over time.
- 4) Data were collected from the individual participants.
- 5) The study "N" was at least 25 per treatment/ comparison group.
- 6) The length of the study was at least 2 weeks.

Data Coding

The term "data" is used loosely here in that it refers to all of the information that is contained in the documentation available for each study. These data include both qualitative and quantitative information. There were actually three categories of data that were "collected" for each study: 1) The substantive characteristics of the study; 2) The methodological characteristics of the study; and 3) Specific information regarding attrition. The codebook found in Appendix B served as a guide for the collection of these data. The substantive and methodological characteristics indicated in the codebook were selected for study because of their possible relationship to one or more of the attrition variables. As noted in the Introduction, previous research has indicated that particular variables may play an important role in attrition. Variables such as the length of a study, the burden placed on participants, the demographic characteristics of the participants, the assignment method used, and the perceived choice of the participants have all been discussed in the research literature as possibly being a factor in determining attrition. Information pertaining to each of these variables, and many others, was coded for each study.

A major goal of the analysis was to determine how the substantive and methodological characteristics of each study were related to the various attrition variables. The substantive features of a study pertain for the most part to basic characteristics of the program or treatment being evaluated. For example, the setting, type, and length of the program are all substantive characteristics. The demographic characteristics of the study participants were also included as substantive char-

acteristics. The methodological characteristics consist of matters of research design, evaluation methods, and analytic techniques. The attrition variables include data and information concerning attrition rates, type of attrition, reasons for attrition, attrition counter measures, and analyses specific to attrition.

Each available study report was read thoroughly in order to accurately code the data. In addition, notations were made for each study with regard to the location and/or content of analyses and discussions of attrition issues. These notes facilitated a more detailed review of the study during the analysis phase of the project.

Missing Data

As expected, the availability of data for each of the variables in the codebook varied from study to study. For the most part, sufficient data were retrieved in order to conduct the major analyses of this study. However, there were a number of variables for which data were missing consistently across the studies. Data were missing for 40% or more of the studies on these variables: socioeconomic status, ethnicity, and geographic location of the participants; frequency and duration of program/treatment sessions; degree of awareness that participants had about the assignment process; degree of choice for participants; and the timing of the assignment process with respect to participants' consent.

There were two important sets of analyses that were limited by the missing data. The first pertains to the burden placed on research participants. A complete analysis of burden would include data on the frequency and duration of program/treatment sessions and the duration of data collection sessions. Since data were predominantly missing for

these variables, the analysis of participant burden was limited to the following variables: length of the program/treatment, length of the study, number of data collection methods used, and the participants' access to the study setting.

The second set of analyses limited by missing data were those concerning the effect of the selection and assignment process on attrition. As discussed in the Introduction, attrition may be affected by the manner in which people are assigned to groups in a research study. The perceived choice that people have about participating, their awareness of the process being used, and the timing of that process are all important variables that are likely to have a direct effect on the reactions that people have to participating in a research study. With data being missing on these variables for 40% or more of the studies, analyses in the area of subject selection and assignment were limited to the method of selection used and the method of assignment used.

Analysis

The analyses used in this study consisted essentially of the comparison of descriptive statistics and distributional characteristics. Inferential statistics were not used because of the multiple analyses being conducted, the limitations of the sample, and the exploratory purposes of the study. The analyses were specifically directed at answering a number of research questions, displayed in Table 4.

Table 4

List of Study Questions

What was the extent of attrition?

What was the effect of attrition with respect to external validity bias and internal validity bias?

How often was bias reported by authors and did reports correspond to objective evidence?

What were the determinants of attrition?

How were the participant characteristics, program/treatment characteristics, and methodological characteristics related to attrition?

What reasons were given for attrition?

What counter measures were used to minimize attrition?

What were the most effective counter measures?

What strategies were used to analyze for attrition bias and how were these related to reports of bias and to attrition rates?

What general data analytic strategies were used and how did these relate to reports of attrition bias and to attrition rates?

What analytic strategies were used to adjust or compensate for attrition bias and how did these relate to reports of attrition bias and to attrition rates?

CHAPTER III

RESULTS AND DISCUSSION

Sample Characteristics

The following sections describe some of the general characteristics of each study. These include various aspects of the program/treatment, the participants, and the research methods employed in the study. The tables in Appendix C display each characteristic broken down according to the study categories used in drawing the sample of studies.

Characteristics of the Program/Treatment

A total of 18 different settings served as sites for the studies. The three most frequently used settings were the community, an elementary school, and a medical or psychology office or clinic (see Appendix C, Table 1).

Because of the multifaceted nature of many of the studies, more than one program type may be reported for a particular study. More than one third of the ninety studies (38.9%) involved the use of an educational component (see Appendix C, Table 2). Other frequently used experimental treatments were non-psychological counseling/assistance programs (31.1%), training programs (26.7%), and psychotherapy/emotional counseling programs (22.2%). There were three program types that were represented in at least six of the seven study categories. These were

educational, non-psychological counseling/ assistance, and training.

Fifty-nine studies reported a definite length for the program/ treatment (see Appendix C, Tables 3 and 4). (Note that the program might be shorter or longer than the research study itself.) Twenty-five studies indicated that the program/ treatment was variable in length or indefinite. The length of the research studies is discussed below in the section Characteristics of the Research Methodology. The overall range in length of the program/treatment was from one week to nine years. Nearly two-thirds of the programs (66.1%) lasted one year or less, with the median length being 39 weeks. The study category with the longest programs was early and elementary education. The median length was 114.5 weeks. Welfare and social service programs were of the shortest duration, having a median length of 26 weeks. It is important to note that despite a median length of only 26 weeks, three of the nine welfare and social service programs were greater than two years in length. This points out the fact that there is a great deal of heterogeneity within each of the study categories. This is true not only for program length, but for many other variables as well. As noted in the Methods chapter, one goal of the sampling strategy was to obtain a group of studies with a broad range of characteristics.

Characteristics of the Study Participants

Data pertaining to the characteristics of the participants in each study were collected on five variables. For three of these variables, ethnicity, socioeconomic status, and geographic area, no information was available for 45% or more of the studies. The majority of the studies however did provide data regarding the age and sex of the participants.

(It is surprising that even for these two basic, descriptive variables, data were missing in 29% and 30% of the studies, respectively.)

On the average, 52.9% of the studies' participants were male. (See Appendix C, Tables 5 and 6.) There were ten studies in which all of the participants were male and six studies in which all were female. Criminal and civil justice studies generally had high proportions of male participants. Welfare and social service programs and health services and medical treatment programs tended to have low proportions of male participants.

The mean of the average age reported for each study is 29.3 years (see Appendix C, Table 7). The median is somewhat lower, 21.5 years. Participants tended to be older in studies of health services and medical treatment programs and welfare and social service programs (see Appendix C, Table 8). As might be expected, early, elementary, secondary, and higher education programs involved young participants.

Characteristics of the Research Methodology

This section describes some of the basic characteristics of the research methodology employed in each study as well as where the results of the study were published and who was the performing organization.

The authors of the studies' reports represented 15 different types of organizations. Twenty-four studies were performed by a private contractor/research organization, 13 performed by an unspecified academic department, and 12 by a hospital/ medical school (see Appendix C, Table 9). These three organizations account for 56.3% of the studies.

Slightly more than half of the study reports (52.2%) were published in a journal (see Appendix C, Table 10). Other frequently used

publication sources were ERIC (13.3%), organization publication (13.3%), and organization unpublished report (10%).

A total of 17 different research designs were employed in the studies (see Appendix C, Table 11). The most frequently used approach was the pre-post control group design (35.6%). Two other designs used often were the nonequivalent control group design (21.1%) and the one-group panel study (13.3%).

There were seven different methods of participant selection used in the studies (see Appendix C, Table 12). The most frequently used selection procedure was one that involved selecting participants that met specific criteria or when all of a group of target participants were chosen (i.e., exhaustive selection). An example of the use of selection criteria would be a study in a medical clinic setting where all adult patients having diabetes are enrolled in the study. An example of exhaustive selection would be a panel study of welfare recipients that includes all persons receiving benefits from a regional office. These two selection procedures accounted for 39.7% of the studies.

Institutional groups were used in 20.4% of the studies. Examples of these were participants selected from school classrooms, nursing homes, and prisons. Another procedure used frequently (18.2%) was the solicitation of participants by program or research personnel.

Seven different methods were used for assigning participants to treatment groups (see Appendix C, Table 13). The 19 cases reported as "missing" in the table represent studies in which only one group was used, obviating the need for assignment. For those studies having two or more groups, the great majority (73.2%) used either random assignment

or natural groups as an assignment method. Random assignment of individuals was the method most frequently employed (46.5%). Existing natural groups were used to designate the various treatment groups in 26.8% of the studies.

The length of the studies ranged from three weeks to nine years (see Appendix C, Tables 14 and 15). Note that this is the length of the research project, not necessarily the length of the program being evaluated. (See Characteristics of the Program/Treatment above for a discussion of program length.) The median length was exactly one year. This was also the modal category, with 19 studies being one year long.

Early and elementary education studies tended to be the longest, having a median length of 148 weeks. The welfare and social service studies were also rather long. Their median length was 2 years. Each of the other study categories had a median length of at least one year.

Extent of Attrition

Upcoming sections of this report present the results of a number of exploratory and descriptive analyses. The focus of the analyses and the major dependent variables are the observed attrition rates for each study. Specifically three different rates were examined. They are the overall rate (i.e., the dropout rate for all participants in a study), the treatment rate (i.e., the dropout rate for all participants in treatment groups of a study), and the comparison rate (i.e., the dropout rate for all participants in comparison groups of a study).

The purpose of this particular section is to furnish a perspective with which to view the results of this study. Information is presented regarding the distribution, average, and range of attrition rates found

in the 90 studies.

Table 5 provides descriptive statistics for the three attrition rates. The average overall rate for all of the studies was 30.1%. The median was virtually the same at 30. A large standard deviation (20.3) and a wide range (0-89) indicate a broad distribution of attrition rates. Reference to Table 6 also illustrates the broad distribution of overall rates, particularly in the range of 0-50.

Tables 5 and 6 also furnish attrition data for the treatment and comparison groups in each study. Note that for nine studies no data were available pertaining to the specific attrition rate for treatment and comparison groups. In addition there were 29 studies that did not incorporate a comparison group in the research design. For these 29 studies the overall attrition rate and the treatment group attrition rate are equal.

The average attrition rate for the treatment groups was 29.8%. This value and the median, standard deviation, range and distribution are all very similar to those for the overall attrition rates. These values are somewhat misleading because the treatment group average reported in Table 5 includes studies that did not have a comparison group. For the 52 studies that utilized comparison groups and had complete data, the average attrition rate for the treatment groups in those studies was 26.5%. For the 29 studies that did not use comparison groups, the average attrition rate for the treatment groups was 35.7%. An examination of the characteristics of studies using comparison groups versus those not using comparison groups revealed one relationship that may explain the difference in attrition rates. Among the studies that

Table 5
Descriptive Statistics for
Attrition Rates

	Study Groups		
	Overall	Treatment	Comparison
Mean	30.1	29.8	26.3
Median	30	29	25
Standard Deviation	20.3	20.8	20.1
Range	0 - 89	0 - 89	0 - 76
N	90	81	52
Missing	0	9	9
No Comparison Group	-	-	29

Table 6
 Frequency Distribution of
 Attrition Rates
 (Number of Studies)

Attrition Rate (%)	Study Groups		
	Overall	Treatment	Comparison
0 - 10	15 (16.7)	16 (19.8)	12 (23.1)
11 - 20	23 (25.6)	18 (22.2)	12 (23.1)
21 - 30	8 (8.9)	9 (11.1)	9 (17.3)
31 - 40	18 (20.0)	13 (16.0)	6 (11.5)
41 - 50	11 (12.2)	11 (13.6)	4 (7.7)
51 - 100	15 (16.7)	14 (17.3)	9 (17.3)
Total	90 (100.0)	81 (100.0)	52 (100.0)

Note. Numbers in parentheses indicate column percentages.

had comparison groups, 49% of the researchers reported using one or more attrition counter measures. Among the studies that did not have comparison groups, only 31% of the researchers reported using an attrition counter measure. This finding indicates that researchers employing comparison groups may be using a more rigorous methodology and may exert more control over attrition problems.

The average comparison group attrition rate (26.3%) was very close to the average treatment group rate (26.5%) for the 52 studies employing treatment and comparison groups. This does not mean that in each study the difference between treatment and comparison group rates was near zero. The averages for all studies are similar because in some studies the treatment group rate was higher than the comparison group rate (yielding a positive difference) and in other studies the pattern was reversed (yielding a negative difference). The positive and negative differences cancel each other and result in an average difference near zero. There were 19 studies in which the treatment group rate was greater than the comparison group rate (mean difference = 11.7%), 26 studies in which the comparison group rate was greater than the treatment group rate (mean difference = 8.2%), and 7 studies in which there was no difference.

The appropriate measure for assessing bias in average differential attrition rates is to compute the absolute value of the difference between the treatment and comparison group rate in each study and then compute the average of these values. The average difference for the 52 studies was 8.4%. Differential attrition rates are discussed in more detail in the section, Effect of Attrition, below.

While the mean and median for the comparison group rates are different from those for the treatment group rates, the standard deviation and range are similar. Table 6 illustrates the relative similarity in the distribution of attrition rates for treatment and comparison groups.

Another method for studying attrition rates is to examine the cumulative likelihood of particular levels of attrition. Table 7 provides the cumulative frequencies of various levels of attrition for the overall, treatment, and comparison groups. The table shows that for this sample of studies, the probability of achieving a tolerable rate of attrition of 20% or less is only 42%. In order to be 90% confident regarding an expected attrition rate, one would have to anticipate a rate of nearly 50%.

In summary, it has been shown that this sample of studies has a broad range of attrition rates. The average rate overall and for treatment groups was approximately 30%. The rate for comparison groups was lower at 26%. The extent of attrition in the studies is substantial, as illustrated in the table of cumulative likelihoods. The following section assesses what impact attrition may have had on the results on the studies.

Effect of Attrition

The purpose of this section is to present an analysis of the extent to which attrition was a biasing factor in each of the research studies. In the Introduction, an explanation was provided regarding the major effects that attrition may have on the validity of research results. To reiterate, external validity is threatened if attrition is non-random within groups, and internal validity is threatened if attri-

Table 7
 Cumulative Frequency Distribution
 of Attrition Rates
 (Proportion of Studies)

Attrition Rate	Study Groups		
	Overall (N=90)	Treatment (N=81)	Comparison (N=52)
0%	.056	.062	.173
10%	.167	.198	.231
20%	.422	.420	.462
30%	.511	.531	.635
40%	.711	.691	.750
50%	.833	.827	.827
60%	.933	.914	.942
70%	.956	.963	.962
80%	.989	.988	1.000
90%	1.000	1.000	

tion is non-random between groups.

In the present study, two approaches were taken to determine if attrition may have been a biasing factor in each of the studies. First, the authors' own reports of bias in their studies was noted. Two variables were analyzed; one for reports of external validity bias, and one for internal validity bias. For each variable, one of two alternatives were coded. If an explicit report of bias was made by the author, this was coded as a "claim of bias." If the authors explicitly reported that no bias was present or if no report of bias was given at all, these were both coded as "no claim of bias."

The second approach for assessing the possibility of bias was to utilize two relatively objective criteria collected from each study. The first criterion was the rate of attrition for the particular study. When analyzing for external validity bias, the overall attrition rate was used with rates greater than 20% indicating possible bias. The 20% criterion was chosen because rates greater than 20% are unusually high according to other authors. Cordray and Orwin (1983) reported attrition rates for a sample of 475 psychotherapy studies in several different settings. More than 80% of the studies had attrition rates of 20% or less. A generally accepted figure of 20% attrition per year in longitudinal studies was reported by St. Pierre (1980).

When analyzing for internal validity bias, the differential rate between treatment and control groups was used with a differential of greater than +/- 10% indicating possible bias. The choice of a 10% criterion for differential attrition rates was somewhat more arbitrary than the 20% criterion selected for overall rates. There are no reports in

the methodological literature of typical differential attrition rates. Moreover, Cook and Campbell (1979) have pointed out that an internal validity bias can occur even if the differential attrition rate is zero. Thus, a differential attrition rate is not a necessary condition for bias. However, if the differential attrition rate is high, bias is strongly indicated. It was decided to choose a conservative criterion, one that would very likely be indicative of bias. The 10% criterion is believed to be conservative and represents 25% of the studies in the sample.

The second criterion for both external and internal validity bias was whether or not a statistically significant correlation between attrition and a number of other variables was reported. Since this is the correlation as presented in the research report, this particular analysis is limited to those studies furnishing data regarding attrition correlates.

It is important to note that these analytic criteria are not the recommended means for analyzing bias due to attrition. There are more precise and complete procedures available as noted in the Introduction (e.g., Jurs & Glass, 1971). However those procedures are possible only when the original research data are available. In the absence of such data, the criteria used in this study serve the useful purpose of being proxy measures of attrition bias.

External Validity Bias

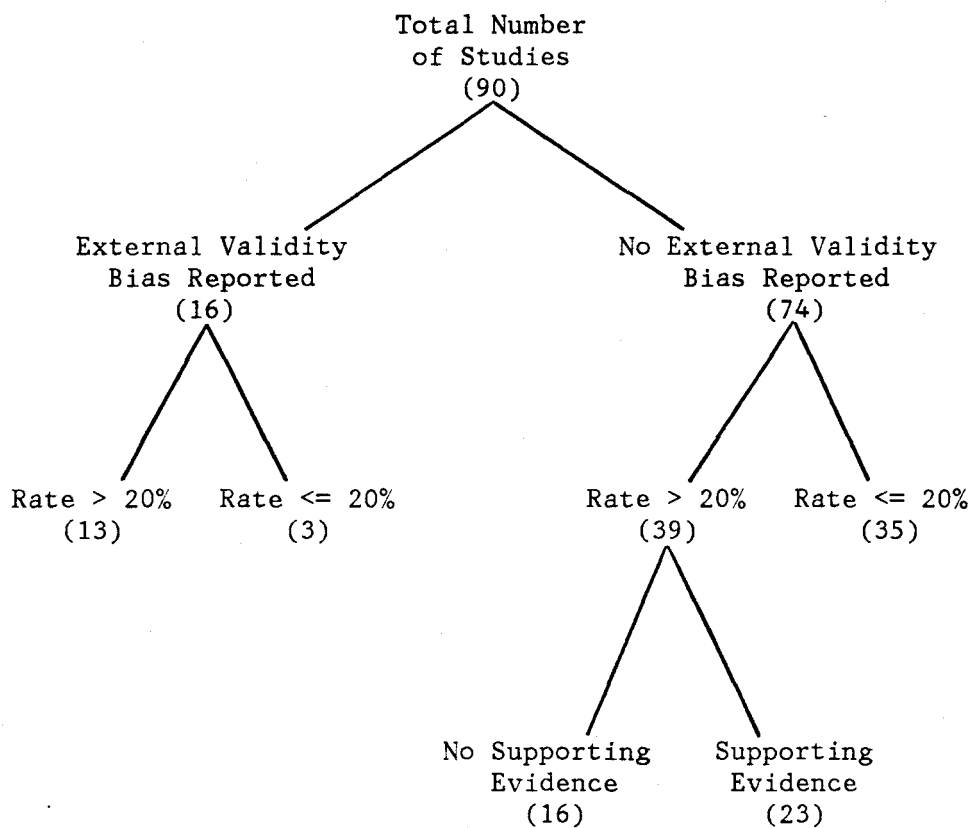
Reports by Authors

Out of the 90 studies, 16 (17.8%) were reported to have an external validity bias due to attrition (see Figure 1). The average overall attrition rate for the 16 studies was 33.1%. The average rate for the 74 studies not reporting an external validity bias was 29.5%. Of these 74 studies, 39 had overall attrition rates greater than 20%. This indicates that despite a high attrition rate, the authors must have believed attrition to be random. However 16 (out of the 39) studies provided no evidence (in the form of attrition analyses) to support this claim. This finding illustrates a deficiency in the quality of many research reports with respect to validity issues. For example, the study of foster care placements by Stein and Gambrill (1977) had an attrition rate of 32%. The entire discussion of attrition consisted of a footnote that listed a variety of reasons that attrition occurred. The nature of these reasons indicated that attrition was generally nonrandom. However, no analysis or discussion was provided concerning the impact that attrition may have had on the results.

Analytic Criteria

This section presents the results of an analysis using two criteria as indicators of possible external validity bias. The first is a cutoff point of an overall attrition rate of 20%. Table 8 indicates that 52 out of the 90 studies had rates greater than 20%. Attrition rates of this magnitude are not definitive indicators of bias. If attrition is a random process, little bias will result regardless of the

Figure 1

Tree Diagram for Authors' Reports
and Evidence Regarding External Validity Bias

rate. However a high rate is suggestive of a possible external validity problem.

To provide a more complete evaluation of the threat to external validity, a second criterion was used. As noted in Table 8, a statistically significant correlation between attrition and another variable was found in 31 studies. As with a high attrition rate, such a correlation is suggestive of an attrition problem. It is interesting to note that only 47 studies furnished an analysis of the relationship between attrition and other variables. This criterion might therefore be considered conservative and reinterpreted as showing that of the 47 studies that analyzed attrition problems, 66% (31) found significant correlations with attrition.

The best evidence of a possible external validity bias due to attrition is provided when there is a high attrition rate and a correlation between attrition and one or more other variables. This joint occurrence is represented in the lower, right cell of Table 8. Twenty-five studies were found to have an overall attrition rate greater than 20% and a significant correlation between attrition and one or more other variables. Based on these criteria, 28% of the studies were biased with regard to external validity.

Comparison of Author Reports and Analytic Criteria

Table 9 illustrates the relationship between the authors' reports of external validity bias and the analytic criteria indicators of bias. Of the 16 studies reported by authors to have an external validity bias, 13 were found to be biased by the analytic criteria. The three studies not found to be biased did have at least one significant attrition cor-

Table 8

Number of Studies by Correlational Criterion
and Overall Attrition Rate Criterion

(External Validity Analysis)

Number of Attrition Correlates	Overall Attrition Rate		Total
	0% - 20%	21% - 100%	
None	32	27	59
1 or More	6	25	31
Total	38	52	90

relate, but each had overall attrition rates of 20% or less. The results of the analytic criteria are in close agreement with the reports of the authors, when those reports were stating a bias.

When viewing Table 9 from the perspective of the analytic criteria there is less agreement. Twenty-five studies were found to have an external validity bias according to the analytic criteria. Of those 25 studies, only 13 were reported to be biased by the authors. This means that in spite of having an attrition rate in excess of 20% and finding attrition to be correlated with one or more variables, there were 12 authors who reported no evidence of external validity bias.

Internal Validity Bias

Reports by Authors

The following sections discuss only those studies that involved the use of comparison groups. The focus is on internal validity as it relates to attributions of treatment causality in studies that compare the treatment group to one or more comparison groups.

There were 52 studies that used comparison groups and had nonmissing data for treatment and comparison group attrition rates. Thirteen were reported by the authors to have an internal validity bias due to attrition (see Figure 2). A primary indicator of such bias is a differential attrition rate between treatment and comparison groups. Overall, the average differential attrition rate was $\pm 8.4\%$. For the 13 studies reporting a bias, the average rate differential was $\pm 11.9\%$. The average rate differential for the 39 studies not reporting a bias was $\pm 7.2\%$.

Table 9

Number of Studies by Authors' Report
of External Validity Bias and Analytic Criteria

Authors' Report	Analytic Criteria		Total
	Bias	No Bias	
Bias	13	3	16
No Bias	12	62	74
Total	25	65	90

Note: The analytic criteria indicating bias were an overall attrition rate greater than 20% and a significant correlation between attrition and one or more variables.

There were nine studies that did not report a bias but had a differential rate of greater than +/- 10%. Despite this strong indication of possible bias, four of the nine studies provided no evidence to support a claim of no bias. As noted above with respect to external validity, this finding illustrates that some authors fail to adequately analyze and present results concerning threats to internal validity.

Analytic Criteria

The first criterion used to analyze for possible internal validity bias was a differential attrition rate of +/- 10%. There were 13 studies that had differential rates greater than +/- 10% (see Table 10).

The second criterion is the same one that was used in the analysis of external validity bias, i.e., the significant correlation between attrition and one or more variables. Twenty of the 52 studies had such correlations. It is the joint occurrence of the two criteria that points particularly to a possible internal validity bias. Six studies had differential rates greater than +/- 10% and a significant correlation between attrition and one or more variables. One interpretation of this finding is that attrition is nonrandom within groups because of the correlation. Therefore, a threat to external validity is indicated. Attrition is nonrandom between groups because of the correlation and the large differential in attrition rates between the groups. Thus there is a threat to internal validity.

Comparison of Author Reports and Analytic Criteria

The corroboration of the authors' reports of internal validity bias and the analytic criteria indicators is reported in Table 11. Of

Figure 2

Tree Diagram for Authors' Reports
and Evidence Regarding Internal Validity Bias

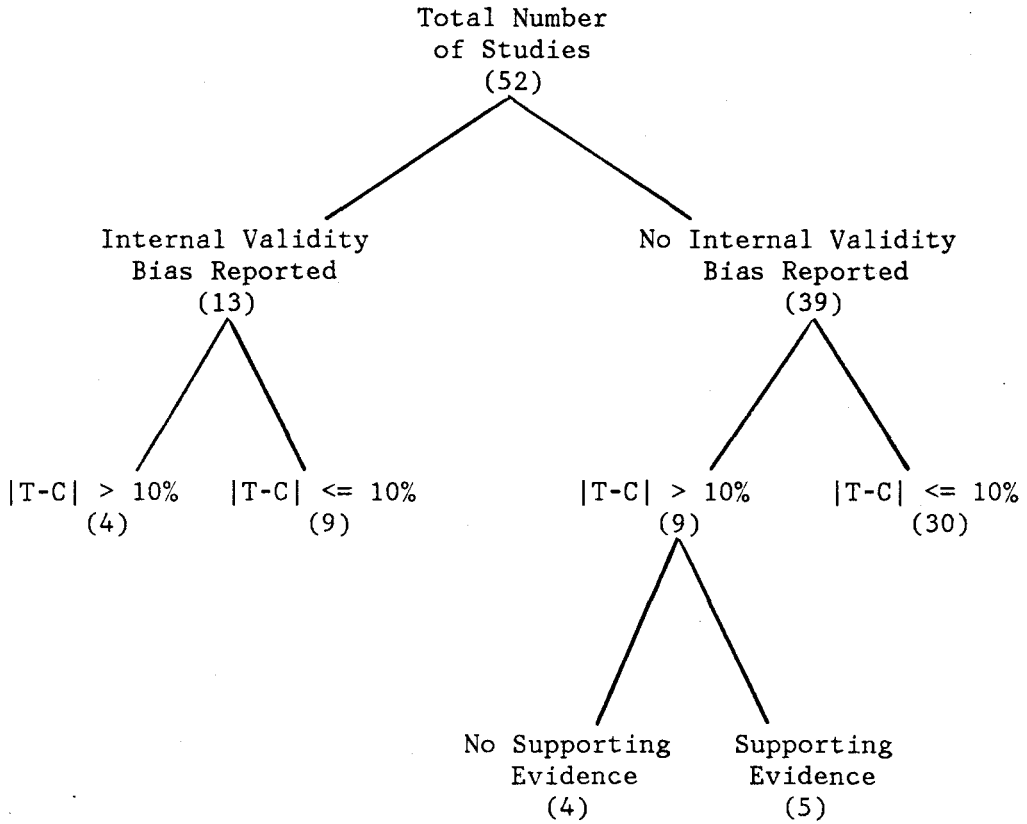


Table 10

Number of Studies by Correlational Criterion
and Differential Attrition Rate Criterion

(Internal Validity Analysis)

Number of Attrition Correlates	Differential Attrition Rate		Total
	\leq +/- 10%	$>$ +/- 10%	
None	25	7	32
1 or More	14	6	20
Total	39	13	52

the 13 studies reported to be biased by the authors, only four were indicated as possibly biased by the analytic criteria. The reason that there was a lack of agreement for nine studies was because the differential rates were less than or equal to $\pm 10\%$. This finding is interesting because it points out that a differential rate is not a necessary indicator of bias. That is, the attrition rates of treatment and comparison groups can be similar and there still may be a bias to internal validity. This could occur when the attriters from the treatment group are different from the attriters from the comparison group.

Of the six studies indicated as possibly biased by the analytic criteria, two were not reported to be biased according to the authors. In both cases the authors provided explanations as to why they felt attrition was not a damaging factor in their study.

Determinants of Attrition

As noted in the Introduction, hypotheses regarding the determinants of attrition can be divided into two categories. The first category, substantive characteristics, consists of hypotheses related to characteristics of the program or treatment and its participants. The second category of hypotheses are those related to the methodology and procedures employed in undertaking a research study. The sections that follow present the results of a variety of analyses designed to explore the relationship between attrition and these substantive and methodological characteristics.

Table 11

Number of Studies by Authors' Report
of Internal Validity Bias and Analytic Criteria

Authors' Report	Analytic Criteria		Total
	Bias	No Bias	
Bias	4	9	13
No Bias	2	37	39
Total	6	46	52

Note: The analytic criteria indicating bias were a differential attrition rate greater than 10% and a significant correlation between attrition and one or more variables.

Substantive Characteristics

Participant Demographic Characteristics

The results presented in this section rely on analyses that were conducted by the authors of the studies in the sample. In coding the data from the studies, it was noted if the relationship between attrition and a particular demographic characteristic was found to be significant, not significant, or not analyzed at all. Further investigation of the reports also noted the direction of the relationship.

Table 12 presents the results of the authors' analyses of five demographic characteristics. Of the 25 studies reporting an analysis of age, eight found that younger participants were more likely to drop out, four found that older participants were more likely to drop out, and 13 found no relationship. The finding that younger participants may be more likely to drop out parallels the findings of Baekeland and Lundwall (1975) (see the Introduction). The greater mobility of young people is thought to be the primary reason for this. On the other hand, Schaie, Labouvie, and Barrett (1973) reported that in longitudinal studies with adults, older participants are more likely to drop out. This was corroborated in a four year study of adults by Schulz and Hanusa (1978).

Little relationship was found between participants' gender and attrition. Out of four studies finding a significant relationship, two reported that females were more likely to drop out and two reported that males were more likely to drop out.

With respect to ethnicity, Table 12 reveals that most often whites were found to be more likely to drop out than non-white participants.

Table 12

Number of Studies Reporting a Relationship
Between Attrition and a Demographic Variable

Variable	(Positive Label)	Relationship			No Analysis Reported
		Positive	Negative	Not Significant	
Age	(Older)	4	8	13	65
Gender	(Male)	2	2	18	68
Ethnicity	(White)	12	3	7	68
SES	(High)	1	4	8	77
Education	(High)	0	2	14	74

This finding may be important, in that there is little evidence in the research literature regarding the relationship between attrition and ethnicity.

Five out of the 13 studies reporting an attrition analysis of socioeconomic status found a significant relationship. Consistent with previous research, four of the five significant relationships were negative, i.e., attrition was more likely for low SES participants.

The relationship between participants' education and attrition was examined in 16 studies. The majority (14) found no relationship. On two occasions, less educated participants were more likely to drop out. Though not demonstrative, these findings are consistent with those reported by Baekeland and Lundwall (1975) and Weissman, Geanakoplos, and Prusoff (1973).

In addition to the typical demographic characteristics described above, a host of other individual attributes were analyzed by authors for their relationship to attrition. In all, 54 significant findings were reported. The vast majority (42) of these findings indicated that the persons most likely to drop out were in some way worse off than those persons who stayed in. Table 13 provides a list of characteristics used by authors to describe those individuals who were significantly more likely to drop out. Only two significant relationships described better off individuals being more likely to drop out. These were both from a single study of a detoxification service and revealed that patients who were less addicted and/or more employed were more likely to drop out (Hamilton, 1979). This type of finding is often interpreted to mean that participants who are less in need of a program

Table 13

Characteristics Used by Authors
to Describe Attriters

Depressed
Isolated from family
Few friends
Perceive regimen to be difficult
Less knowledge of regimen
Low reading achievement
Non-nuclear family
Poor health status
Alcohol abuser
Serious delinquent behavior
Poor arm coordination
Reside in room or institution
Less zestful
Unemployed
Broken home
Less addicted
More employed

may be more likely to drop out.

The remaining 10 significant findings were for variables that had little evaluative nature to them. For example, location of hospital, location of prison, and religious affiliation were each found to be related to attrition in one study or another.

Summary. Five basic demographic characteristics were analyzed for their relationship to attrition in several studies. Generally, attriters tended to be young, white, or of low SES. There were, however, several exceptions to this generalization. As such, demographics are not very useful predictors of attrition on a general basis. The relationship varies too much from one situation to another. However, since demographics may be related to attrition in specific circumstances, they are very important in the analysis of data from individual studies. In order to appropriately analyze for attrition bias it is necessary to have information concerning the characteristics of the participants. This is particularly important when these characteristics are related to the dependent variables in the study. Furthermore, the more that one knows about the individuals who drop out of the study, the easier it is to understand the attrition process. And knowledge of this process (i.e., What were the motivating and facilitating factors in attrition?) allows a better analysis of the threats to validity caused by attrition.

Program/Treatment Characteristics

In an applied research study, there are many features of the program or treatment being evaluated that may have an effect on attrition. Generally these remain constant within a particular study and thus it is difficult to examine their impact at a single study level. By coding

the program characteristics of each study and analyzing across several studies, a better understanding of their relationship to attrition is gained.

The majority of the program/treatment characteristics are categorical variables. The specific analysis used for these variables was a comparison of the mean rates of attrition (overall, treatment, comparison) across each level of the particular program characteristic. The discussion below focuses on those variables that were found to have mean attrition rates for specific levels of the variable in excess of 5% greater or less than the grand mean. The grand means for overall, treatment, and comparison group attrition rates were 30, 30, and 26, respectively. The 5% criterion is designed to very liberal in order to uncover any potentially important relationships. Given the large variability of the attrition rates (the standard deviation exceeds 20), a traditional statistical test would be unlikely to reveal any "significant" relationships.

For the non-categorical variables, the basic analysis was an examination of the correlation between the variable and the various attrition rates. Further analyses were conducted, including a comparison of rates across different levels of the variable (e.g., above and below the median).

Nine program characteristics were examined for their relationship to attrition. As illustrated in Table 14, attrition was related to three of these characteristics. There was no apparent relationship with six of the characteristics. These are discussed first.

Characteristics Not Related to Attrition. Only one of the charac-

Table 14

Relationship Between Program Characteristics
and Attrition Rates

Program Characteristic	(N)	Attrition Rate		
		Overall	Treatment	Comparison
Program Length		-	-	-
Program Type		-	-	-
Program Modality		-	-	-
Program Status		-	-	-
Comp. Grp. Treatm.		-	-	-
Awareness of Treat.		-	-	-
Setting				
Elementary Sc.	(11)	H	H	H
Secondary Sc.	(11)	L	L	-
Community Ctr.	(11)	H	H	-
Access to Setting				
On Location	(19)	L	L	L
Involve. Signif. Others				
Treatment Grp.	(21)	H	H	H

Note. - = Rate for level(s) not 5% higher or lower than grand mean.
H = Rate 5% higher than grand mean.
L = Rate 5% lower than grand mean.

teristics, the length of the program being evaluated, is a non-categorical variable. The first analysis was to examine the correlation between the program length and attrition rates. The correlations with overall, treatment, and comparison group rates were all less than .07.

The median program length was 39 weeks. A comparison of attrition rates for studies lasting 39 or fewer weeks versus those lasting more than 39 weeks reveals that the attrition rates were slightly higher for studies with the greater program lengths. For the average overall attrition rates the difference was 1.9%, for the treatment group rates it was .5%, and for the comparison group rates it was 4.8%. The reason that there appears to be little relationship between program length and attrition may be due in part to the fact that many of the studies (46%) continued after the program itself ended. Generally, this portion of the study consisted of follow-up activities with much of the attrition occurring after the program had ended. The overall study length is more likely to be related to attrition and is discussed below (see the section, Methodological Characteristics).

Program type was not related to attrition, which is not surprising in that most studies involved a multifaceted program (e.g., training, job placement, and counseling) such that one program type was generally confounded with other program types.

Program modality refers to the level of delivery of a program, i.e., to an individual, a family, or a group. There was no strong relationship with attrition for this variable, although there was a tendency for studies using groups to have higher attrition rates than studies not

using groups. One explanation for this might be that when individuals are in a group situation they may receive less attention and may feel that a program is not meeting their particular needs.

With respect to program status it was expected that there may be a difference in attrition rates between studies of ongoing programs versus studies of one-time experimental or demonstration programs. There was virtually no difference in the attrition rates for the two types of studies, indicating that new, experimental programs may be no more prone to attrition than established, ongoing programs.

It was found that studies using a treated comparison group had comparison group attrition rates similar to studies using a no-treatment comparison group (i.e., a control group). This is an interesting finding because it might be expected that untreated control groups would have high rates of attrition due to resentment or demoralization. This would depend on their having knowledge about the treatment group. However, there was no relationship found between comparison group attrition and whether or not that group was aware of the existence of the treatment. On the other hand, it could be argued that untreated control groups would not be subject to the kind of program-related attrition that a treated comparison group might have. These competing forces cancel each other out and may explain the lack of difference in attrition rates for treated versus untreated comparison groups.

Characteristics Related to Attrition. An apparent relationship was found for the setting in which the study was conducted. The average attrition rate was higher for studies conducted in elementary schools. This was true for the overall, treatment group, and comparison group

rates. A close examination of the 11 studies conducted in elementary schools provides a explanation for this finding. Seven of the studies lasted for one year or longer. This means they spanned multiple academic years allowing natural educational turnover to become a factor. From year to year students are likely to be unlocatable due to transferring schools or moving entirely out of a district. This was a major reason for attrition reported by several authors (e.g., Eash, Haertel, Pascarella, Conrad, & Iverson, 1980; Landsberger, Kingsley, & Pratto, 1976; Rosario, Love, & Smith, 1980). Another reason for attrition problems in elementary schools is absenteeism on the day of testing (Hotch, Edwards, Bickman, Rivers, 1980; Landsberger et al., 1976).

The attrition rates for studies in secondary schools were lower overall, and among treatment groups. This might be explained by the fact that those studies with the lowest rates were for programs that were highly desirable to the participants. In a study by Eash, Sparkis, and Rasher (1975), there were more applicants than openings in the treatment group. Two other studies (Reilly & Mokros, 1981; Yongue, Todd, & Burton, 1981) involved short-term, innovative curriculum programs.

Of the five studies that were based at a community center, two were training programs, one was a health services program, and two were criminal justice programs. It appears that the high attrition rates (overall and treatment) were due more to miscellaneous factors rather than to the setting. For example, the study by Burch and Mohr (1980) used poor data collection methods that resulted in the loss of posttest data.

The degree of access that participants had to the research setting was related to attrition. Not surprisingly, when participants were already on the location of the research study, the attrition rate for such studies was lower. Examples of such settings are hospitals (inpatients), psychiatric institutions, workplaces, homes, and prisons.

Attrition rates were generally high for studies that had treatment programs that involved significant others (e.g., spouse, parent). For example, a study of adherence to medical regimens examined the impact of social support by a relative or friend. There were five studies that focused on the impact of welfare on families. Another four studies were in the area of early education and involved the cooperation of parents. The reason that these studies tended to have high rates of attrition may be due to the high degree of burden placed on the participants in many of the studies. This makes sense in that a program that includes the involvement of significant others is likely to be one that is somewhat complex and requires a strong commitment from the participants (e.g., Caplan, Robinson, French, Caldwell, & Shinn, 1976). However, in certain circumstances it would seem that the influence of a significant other might be such that continued participation in a program is more likely. For example, when the role of the significant other is to provide support or encouragement for the participant, attrition may be less likely. In fact Caplan, Van Harrison, Wellons and French (1980) reported that of three treatment groups, the one with the highest completion rate was the one that included the active support of a significant other as a part of the treatment.

Summary. Out of the nine characteristics of the program/treatment

that were analyzed, six were found to be unrelated to attrition. These included program length, type, modality, and status; and whether treated or untreated comparison groups were used.

Three of the program/treatment characteristics were found to be related to attrition. Attrition rates were higher for studies conducted in elementary schools. It was noted that a major factor in this attrition was the natural turnover that occurs in schools from year-to-year. For the 11 studies conducted in secondary schools, the overall and treatment group rates tended to be low. Among this group of studies, those with the lowest rates were of short duration and involved desirable programs. Studies conducted at a community center tended to have high rates overall and for treatment groups.

As expected, attrition rates were lower when participants had direct access to the study setting.

One somewhat peculiar finding was that studies that involved the participation of significant others had higher rates of attrition. It was suggested that this may have been due to the more complex and involved nature of these studies.

The importance of these findings is that they point out that some studies may be more or less prone to attrition because of the setting and circumstances in which they are undertaken. Few, if any, generalizations can be made about which variables are most important. It can only be suggested that researchers should carefully examine aspects of the program or treatment being evaluated for features that may influence an individual's desire or ability to participate.

Methodological Characteristics

This section discusses the relationship between the various aspects of the research methodology employed in each study and the attrition rates. The method of analysis used is the same as that for the program/treatment characteristics described above. Seven different methodological characteristics were analyzed (see Table 15). Four of these were found to be related to attrition. The three characteristics found to be unrelated to attrition are discussed first.

Characteristics Not Related to Attrition

The only non-categorical, methodological characteristic analyzed was study length. The correlations between study length and overall, treatment, and comparison group attrition rates were all less than .03. A comparison was made of the attrition rates of studies that were above and below the median study length of 52 weeks. The mean overall attrition rate for studies longer than 52 weeks was 2.2% higher than the rate for studies that lasted 52 weeks or less. For treatment group rates, the difference was 3.0% and for comparison group rates, the difference was 2.0%. A similar comparison of rates for several categories of study length reveals even smaller and, in some cases, reverse differences.

The lack of a relationship between study length and attrition is perplexing. Several authors (e.g., St. Pierre, 1980; Wise, 1977) have reported a relationship between the length of a study and attrition. It seems logical that the longer the study, the more likely it is that attrition will occur. That this was not true for this sample of studies may be due to the fact that there were many other differences among the studies, in addition to study length. If a sufficiently large, random

Table 15

Relationship Between Methodological
Characteristics and Attrition Rates

Methodological Characteristic	(N)	Attrition Rate		
		Overall	Treatment	Comparison
Study Length		-	-	-
Loc. of Researcher		-	-	-
Data Collector		-	-	-
Selection Method				
Referred	(8)	H	H	H
Research Design				
Pre-Post C. Grp.	(32)	-	L	-
Assignment Method				
Random	(33)	L	L	-
Self Selection	(6)	-	H	-
Data Collection Method				
Mailed Quest.	(10)	H	H	H

Note. - = Rate for level(s) not 5% higher or lower than grand mean.
 H = Rate 5% higher than grand mean.
 L = Rate 5% lower than grand mean.

sample of studies were chosen, a relationship between study length and attrition would likely be found.

Two other methodological characteristics, location of researcher and data collector, were found to be unrelated to attrition. It might be argued that studies conducted by internal evaluators would be less prone to attrition because of the close contact they have with the program and their knowledge of the setting and conditions in which it is performed. However, attrition rates were nearly equal for studies conducted by internal versus external evaluators or researchers. The above argument may in fact be valid, but only for internal evaluators who develop strategies for reducing attrition based on their knowledge of the study conditions.

Attrition rates might be expected to vary according to who is responsible for data collection. An argument similar to the one above for "location of researcher" could be made. For example, when data are collected by program personnel or internal evaluators, attrition rates may be lower because of the collectors' familiarity with the participants. This familiarity may make it easier to locate participants and/or gain their cooperation. However, such familiarity may influence participants to drop out because of their desire to remain anonymous. These opposing tendencies may account for the fact that there was little relationship with attrition found for data collector.

Characteristics Related to Attrition

The average attrition rates were high for studies that used a selection method of referral. Within this group the studies with the highest rates were delinquency treatment programs, a criminal offender

project, and a mental health treatment program. Since participants were being referred to these programs by another source, their motivation and interest in the new program may not have been high. This is illustrated in the study by Berger, Crowley, Gold, Gray and Arnold (1975). The participants were juvenile probationers referred to a service program by one of three sources: an intake worker, a judge, or a caseworker. They became involved in the program not because they necessarily wanted to be there, but because they were sent there by an authority source. With participation being voluntary, many subsequently dropped out.

The nature of the referring source may be an important factor in attrition. When the referring source is an authority, as in the example above, and the referral takes on the tone of a directive, attrition may be high due to resistance and disinterest by the participants. On the other hand, if the referral source is one of a service nature, such as a medical or mental health clinic, it is less likely that there will be attrition problems due to resistance or disinterest (e.g., Tyrer & Renington, 1979).

There were 32 studies that utilized a pre-post control group research design, the so called "true experimental design" by Campbell and Stanley (1966). For these studies, attrition rates for the treatment groups tended to be low. The rates for comparison groups and overall were also lower than average, but not more than 5% lower than the grand mean. Studies with this design may have had lower rates because of the greater control over the experiment that the researchers may have had. That is, for the researchers to implement a true experimental design they likely had some control over the study setting. This con-

trol may have led directly to lower attrition rates. Or, indirectly, a setting which facilitates an experimental design may be one that is not subject to high attrition levels.

As discussed in the Introduction, the method of assigning individuals to study groups may affect attrition because of the reactions that participants have to the assignment process. For the 33 studies that employed random assignment, the average overall and treatment group attrition rates were more than 5% lower than the group as a whole. The average comparison group rate was also lower though not as much. This finding supports the research of Wortman and Rabinowitz (1979) that found that research participants rated random assignment as being more fair than other assignment procedures.

An additional explanation for the lower attrition rates for studies employing random assignment is the same as that for pre-post control group research designs. Random assignment is the major component of that design and is the aspect of control that researchers must employ. Control over the research design may lead to lower attrition. Likewise a setting that allows a randomized experiment to be undertaken may be one that is less prone to attrition.

Table 15 also indicates that studies that utilized an assignment method of self-selection had higher treatment group attrition rates. There is little meaning to be drawn from this finding however. There were only five studies that used this technique, and of those five, three actually had attrition rates lower than the average for all studies.

The use of mailed questionnaires as a data collection method

resulted in higher attrition rates on the average for 10 studies. Six of the ten studies used mailed questionnaires as the sole method of collecting data at posttest. It is not surprising that these studies would tend to have high attrition rates, given the general problem of nonresponse to mailed surveys.

There are circumstances when the use of mailed surveys can reduce attrition. Caplan et al. (1980) used a mailed questionnaire as a supplemental followup for those subjects who missed the posttest. While attrition was still high in this study, it was lower than it might have been without the mailed questionnaire.

Summary

Seven methodological variables were analyzed for their relationship to attrition, with three having no apparent relationship. These included study length, location of researcher, and data collector.

Attrition rates tended to be relatively high for studies using a selection method of referral. It was suggested that the nature of the referring source (e.g., an authority) may be important. Among the other categories of selection method, the one with the lowest attrition rates was the method of selecting participants who met specific criteria (e.g., low-income, single parent). This is logical in that these individuals are often the ones most in need of a particular program. While researchers may have little influence over the method of selection, they should be prepared to address the problems it may cause. With respect to selection by referral, it might be possible to make alterations in the selection process in order to minimize participants' resistance. For example, they could be informed that they are being selected for a

program that is designed to meet their specific needs. Or, they could be allowed to provide input regarding the implementation of the program.

When a randomized pre-post control group research design was employed, the attrition rates were generally low. Similarly, rates were low in studies that employed random assignment, regardless of the specific design. It was discussed that the lower rates may be due to the nature of study settings that allow for a randomized research design. The use of other methods (the most frequent were nonequivalent control group, single group panel study, and single group pre-post) does not necessarily mean that attrition will be high. It is when these designs are used in situations where the researcher has little control and where the setting is complex that attrition is likely to be high.

Last, it was found that studies using mailed surveys for data collection had high attrition rates. This was especially due to six studies relying on mailed questionnaires as the sole source of posttest data. Attrition rates may have been lower in these studies if the researchers had used multiple mailings and follow-up techniques, such as reminder letters.

These findings illustrate the importance of methodological factors in attrition. In terms of preventing attrition, some factors are more manageable than others. For example, it may not be possible to alter the selection process when referrals are an established part of a program. On the other hand, the researcher may have control over procedures such as the method of assignment and data collection procedures.

Participant Burden

A number of variables that may be indicators of participant burden were examined for their relationship to attrition. The variables examined were the access that the participants had to the study setting, the length of the program/treatment, the length of the study overall, and the number of data collection methods used.

As reported above in the section Program/Treatment Characteristics, access to the study setting was found to be related to attrition. When the participants were already on the location of the study setting, attrition rates tended to be low.

Neither the length of the program nor the length of the study was related to attrition. For both variables the correlations with overall, treatment, and comparison group attrition rates were all less than .07.

The number of data collection methods used was found to have very little relationship to attrition. The correlation with overall attrition rate was .14. While the number of data collection methods used may be indicative of the burden that a study participant feels, having more methods may actually reduce measurement attrition. That is, researchers may use multiple methods in such a way that at least some data are available for each participant. There may be attrition from specific data collection methods but not from the study as a whole.

The lack of relationship that these variables had with attrition does not mean that burden is an irrelevant factor in an individual's decision to remain in a research study. These variables were only remote estimates of that burden. Other variables, that are better indicators of burden, may in fact be strongly related to attrition. An

attempt was made to collect data on variables such as the frequency and duration of program sessions and the duration of data collection sessions. Data were not available on these variables for more than 70% of the studies. Given the incomplete nature of the above analysis, it might be assumed that the more demands that are placed on research participants, the more likely it is that they will drop out. On the other hand, it could be stated that the more investment a participant has in a study (in terms of time and effort), the more likely it is that they will continue participating in order to justify that effort. It would be useful to know if there are tolerable limits to the burden placed on research participants. This would allow researchers to maximize the information they collect while at the same time minimizing attrition due to burden.

Studies with Extreme Attrition Rates

The sections that follow present a qualitative analysis of studies that had either an extremely high attrition rate or an extremely low rate. Twenty studies were chosen, 10 with the highest rates and 10 with the lowest rates. The rates in the high category ranged from 56% to 78%, those in the low category from 0% to 10%.

The reports of each study were examined thoroughly in an effort to discover any underlying or subtle factors that may have played a role in the extreme attrition rate. In particular, those features of the methodology that may have affected attrition are discussed. The comments that the authors made specifically about attrition were also noted.

Studies with Extremely High Rates

The study of the national Follow Through Program (Cline, 1974) was subject to attrition problems partly because of the many opportunities for data loss. Attrition occurred at three levels throughout the project. For cohort III, the overall attrition rate was 68%, with 66% dropping out of the treatment group and 71% dropping out of the comparison group. Administrative policies resulted in the loss of classrooms as well as students. There were the typical losses of students due to mobility and illness. These reasons were particularly prevalent because of the length of the study, more than 4 years. In addition, there was an elimination of participant data because of missing data items and inadequate cell size for analysis.

The major contributing factors to attrition in the Follow Through program would seem to be the length of the study and its large scale. Research studies in academic settings are prone to attrition when data collection spans two or more academic years. Natural educational turnover makes it difficult to locate research subjects, even if they remain in the same school district. And given the scale of the Follow Through study, the task of keeping track of participants and/or maintaining intact study groups was particularly difficult.

Eash et al. (1980) conducted a study of child parent centers. While the program itself lasted for a single academic year, the posttest data were not collected until the fall of the following year. The overall attrition rate was 60%. The authors attributed this to two factors. The first was the "natural attrition" that occurs in educational settings, (e.g., turnover from one year to the next). The second factor

was that the posttest data were not collected under the supervision of the researchers, and they believed that this resulted in data not being available for some study participants.

A study of the relationship between classroom behavior and test performance was conducted by Landsberger et al. (1976). The study lasted for two academic years and had an attrition rate of 64% for the one study group. Out of the initial sample of 317 students, 26% were not available at the end of the study due to transfer and absenteeism. What was more of a problem than this "educational turnover" was the number of subjects lost due to missing data. Thirty-eight percent of the initial sample was eliminated because of missing data items. Clearly these two classes of attrition are the result of different processes and each has different implications regarding bias. However the study report included no analysis of these attrition problems.

The study by Goldberg (1978) was one of many that have been conducted of the Job Corps program. This study focused on a single group of applicants with particular interest in the patterns of dropping out. Of 673 participants, only 178 (26%) completed the program and the posttest interview. With respect to program attrition, the researchers stated that they expected one third of the participants to never show up for a program session, and one third to drop out at some point during the program. As it turned out there was a lower "no-show" rate and a higher "dropout" rate. They attributed this to many ambivalent youngsters enrolling because of the economic recession at the time and then later dropping out. Despite dropping out of the program or never showing up, posttest data were available for many of these individuals.

However, out of the initial sample, posttest data were not available for 194 (29%) subjects. The reasons for the missing interviews included unavailable respondents or those not located, refusals, and interviewer fraud.

A study of primary care utilization and patient satisfaction over a three year period was conducted by Sussman, Rosen, Siegel, Witherspoon and Nesson (1979). A posttest mailed survey with one followup yielded a response rate of 43%. It is apparent from the report that the participants had no knowledge of the study until they received the survey. The utilization data were collected from medical records and thus were not prone to attrition. The component of the study that was concerned with patient satisfaction however, relied entirely on the mailed survey and suffered a high measurement attrition rate of 57%.

A study of behavioral weight control (Kolotkin & Moore, 1983) was subject to the same attrition problems that many obesity research studies have (Wilson, 1978). A major reason for high attrition in these studies is the strong commitment and changes in lifestyle that are required. A single group of 271 program participants was examined. Of these, 59% did not complete the 12 week program or take the posttest. This high rate occurred despite the use of counter measures such as a \$20 program fee and a \$25 deposit refundable upon completion.

In a study of a social service program for adolescents, Brame and White (1980) examined the various stages at which clients dropped out. Throughout the 2 1/2 year study, only 22% of the clients completed their designated treatment. The majority of attrition was due to individuals dropping out or never showing up for treatment. Forty-eight percent of

the original group dropped out in this way. Attrition was especially high in this study because of the additional ways that attrition could occur. Sixteen percent of the clients were found to be ineligible for treatment or were expelled by the staff. Eight percent were removed by parents or probation authorities. And an additional 6% were referred out by the staff to another agency.

This study is very useful because it documents the various ways that individuals can be "lost" from a research study. All of the individuals who failed to complete the program shouldn't be lumped together into one category of "attriters" when analyzing the data. Since the processes by which these individuals "dropped out" were very different, it is important to examine the differences among these groups because each may have a different biasing effect on the data. In fact, Brame and White (1980) found that the clients who dropped out and those who completed the program were both overrepresented among the low impairment group. However, the clients who were withdrawn from the program (referred out, removed, and expelled) were overrepresented among the high impairment group.

The study by Berger et al. (1975) was an evaluation of a volunteer program in a juvenile court. The participants were juveniles placed on probation and randomly assigned to an experimental service program or a comparison group receiving traditional services. The attrition rate over the 12 months of the study was 60% overall, 58% for the treatment group, and 64% for the comparison group. Participation in the program was voluntary and the authors attributed the high attrition rates to the "undependability" of probationers and their "propensity to fail to show

up for appointments and to disappear entirely."

Kloss (1979) conducted a study of a treatment program for complex criminal offenders. Their report provides few details regarding attrition. Sample data for the full 20 month study period were available for only 46% of the participants. It is unclear why data were missing for 54% of the initial sample.

The study of a child abuse intervention program (Burch and Mohr, 1980) suffered from high attrition because of poor methodology. The program was described as an ongoing, open-ended group meeting, with participants coming in and out as they wished. A pretest was administered to 65 treatment group participants and 41 comparison group participants. Four months later during a weekly meeting, the posttest was administered to those persons in attendance who also had taken a pretest. Only 21 of the original treatment group completed the posttest. For the comparison group, the posttest was given to those participants who were still under the jurisdiction of Child Protective Services. This was true for only 10 of the original group of 41. It is clear that a higher completion rate might have been achieved with a better effort at data collection.

Summary. A close examination of the 10 studies that had the highest attrition rates revealed a number of factors that may have contributed to high rates of attrition. Perhaps the most significant of these was educational turnover. Three of the studies were conducted in elementary schools over periods of time exceeding 1 year. In each case many participants were lost to the study between academic years.

There were several other factors that appeared to affect attrition in each of the studies. Of these, some might be considered to be more

likely to occur in other studies as well. For example, extensive missing data could be a problem in any study using questionnaires or other self-report measures. A strong commitment and change in lifestyle was a factor in a weight-control program and could play a role in many studies that evaluate programs that require participants to be highly involved. The use of a mailed survey as the exclusive method for collecting posttest data resulted in high attrition in one study. This type of attrition-prone methodology is often used (see the section Methodological Characteristics above).

There were other attrition related factors in these studies that may occur less frequently in other research studies. These include a lack of control over data collection by the researcher, a poor data collection method, and having undependable participants.

Studies with Extremely Low Rates

Reilly et al. (1981) evaluated a child development/parenting program in several high schools. A low overall attrition rate of 10% appears to be due in large part to a self-selection process. Intact classroom groups were used, with the treatment group having elected to take the parenting program. A short study length of one semester also contributed to a low attrition rate.

An attrition rate of 4% was reported by Furukawa, Cohen and Sumpster (1982) in a study of an innovative curriculum for a college psychology class. The students in the two treatment groups were selected because of their low SAT verbal scores. A baseline measure from students of like ability was used for comparison. The authors reported that the 4% attrition rate compared favorably to the 6-15% rates found

in similar studies. They implicitly attributed the low rate to the quality of the program. The study duration of a single semester for one treatment group and a 5-week summer session for the other also accounts for the low rate.

Gideon, Littell and Martin (1980) conducted an evaluation of a training program for certified alcoholism counselors. They reported that 91 subjects began the program and completed it. There was no mention of attrition in the report. Given the length and extensiveness of the program (59 weeks, 400 hours of training), it seems doubtful that not a single person dropped out. This study is an example of imprecise reporting of methodology that makes interpretation of research validity difficult.

A summer career training program for high school students was evaluated by Yongue et al. (1981). All students were participating in an Upward Bound program and were randomly assigned to a treatment group (field exposure) or a comparison group (didactic classroom). Both groups met for 6 hours per week for 6 weeks and no attrition was reported for the 23 students in each group. The lack of attrition may have been due to the students self-selecting themselves into the Upward Bound summer program. The short program length may have been a factor as well.

A very well-conducted randomized clinical trial was reported by Knatterud, Klimpt, Levin, Jacobson and Goldner (1978). Data were collected over a 9-year period from 619 diabetic patients. Very close monitoring of data collection and good tracking efforts allowed the researchers to maintain contact with all 619 patients over 9 years.

Haynes, Sackett, Taylor Gibson and Johnson (1978) conducted a study of hypertension in an industrial setting. Following a screening program, 230 workers who were diagnosed as hypertensive agreed to participate in the study. Over a 1-year period, 10% of the participants dropped out of the study. This low rate of attrition appears to be due primarily to the fact that the study itself required little effort on the part of the workers. The program was administered to them at work on company time and data collection was minimal, consisting of 6-month blood pressure measurements and the retrieval of archival data from company records.

A randomized clinical trial in a medical practice setting was reported by Spitzer, Sackett, Sibley, Roberts, Gent, Kergin, Hackett and Olynich (1974). After assignment to either conventional treatment or a nurse practitioner, the subjects were asked to volunteer for the study. There was a refusal rate of less than 1% and over the 1-year study period less than 1% of the participants dropped out. The apparent reason for this low rate is that the patients were selected because they had an ongoing relationship with the practice and their involvement in the study only required that they continue their normal participation in the practice.

A psychotherapy evaluation was conducted by Sloane, Staples, Cristol, Yorkston and Whipple (1975). Persons who had applied for treatment and met the study criteria were randomly assigned to two treatment groups or a 4-month waiting list. After a period of 1 year, interview data were collected from all but 2% of the patients. No attrition from the therapy itself was reported. The therapy under study lasted four

months, after which patients could receive more therapy if they wished. Also at four months the control group patients could begin therapy. Frequent contact had been maintained throughout the waiting period and this might account for the fact that no patients in the control group dropped out. As far as the other patients are concerned, there are no other details regarding the methodology or the therapy that would explain the unusually high continuance rate.

Weisbrod and Helming (1980) reported making a great effort to reduce attrition in their study of an alternative to in-patient care for the mentally ill. A total of 130 persons seeking in-patient admission were randomly assigned to a community care treatment group or an in-patient comparison group. Data were collected from every participant at 4-month intervals over a period of 1 year. Though few details were given about specific attrition counter measures, the authors gave one example of their concern for the problem. In the case of one patient who moved out of state, a staff person flew there in order to obtain data on the patient's activities.

Lewis (1981) report the results of an evaluation of a juvenile awareness program at San Quentin prison. All of the participants were youths having a long record of delinquency who were already participating in a probation camp. Random assignment was used, with 53 youths being assigned to the 3-week treatment program and 55 youths comprising a control group. Pretest and posttest were collected from all but one of the youths. No explanation was given for the nearly perfect completion rate. It was probably due to the control that the authorities and/or researchers had over the youths. Their participation was most

likely a required part of their probation.

Summary. There were essentially three sets of factors that played a clear role in the low attrition rates of the above studies. There were three educational studies that lasted one semester or less. This short duration seems to be very important in light of the finding that three of the studies that had extremely high attrition rates were educational studies lasting one year or more. An additional factor in two of the educational studies with low rates was the fact that the participants were self-selected into the programs.

Two of the studies with low rates required very little effort on the part of the participants. In one case the study took place on company time at work and in another study the participants merely had to continue their normal use of a medical office.

A good methodological effort contributed to a low attrition rate in two studies. In one case no details were given, just that a great effort was made to reduce attrition including the tracking down of all participants. In the other, the researchers used various monitoring techniques and frequent contact to keep track of a longitudinal sample.

Reasons for Attrition Given by Authors

Table 16 provides a list of the reasons for attrition as stated by the authors of the studies. These are not statements of the causes of attrition necessarily, but rather how attrition occurred. A total of 25 different reasons were given by the authors of 50 studies. More than one reason was specified for many of the studies.

For each reason listed in Table 16 a notation is also made for the type of attrition to which it pertains. The three types of attrition,

Table 16

Reasons for Attrition Given by Authors

Reason	Number of Studies	Type of Attrition
Unable to locate/unavailable	24	Program
Moved	21	Program
Refused to be interviewed	14	Program
Deceased	9	Program
Refuse to participate/uncooperative	8	Program
Morbidity/incapacitated	6	Program
Incomplete data	5	Sample
Ambivalent/lack of commitment	4	Program
Policy decision	3	Policy
Ineligible for program	3	Policy
Natural/turnover	3	Program
Scheduling difficulties	3	Program
Dropped out of school	2	Program
Institutionalized/incarcerated	2	Program
Missed program sessions	2	Program
Child runaway	2	Program
Improved status/discharged	1	Policy
No match found for analysis	1	Sample
Achieved program goal	1	Policy
Data collection fraud	1	Sample
Dissatisfied with program/treatment	1	Program
Site/school level dropout	1	Policy
Returned to school	1	Program
Improved academic performance	1	Policy
Fail to complete program requirements	1	Program

Note. Up to three reasons were coded for each study.

Policy, Program, and Sample, have been described by St. Pierre and Proper (1978) and are discussed in the Introduction.

Six reasons were given that could be considered a type of Policy attrition. The most frequent were a general policy decision (N=3) and participants being ruled ineligible for a program (N=3).

Attrition is most often considered to be Program attrition, which defines many types of individual behaviors that result in a loss of data. There were 16 different reasons given by authors in this category. The most frequently mentioned were: unable to locate/unavailable (N=24), moved (N=21), and refused to be interviewed (N=14). Note that the various reasons may not be mutually exclusive. For example, the researchers may only know that they were not able to locate certain participants, when in fact they had moved.

The third category of attrition is Sample attrition which is the result of decisions made by the researcher after data have been collected. The best example of this type of attrition and the most frequently occurring was the dropping of subjects due to incomplete data (N=5). Generally this would happen only when there are large amounts of data missing for an individual or when data on key variables are missing.

Relationship Between Reasons for Attrition and Bias

It is important to examine each of the reasons for attrition in terms of how they may be related to the validity of a study. If the nature of the reason indicates that attrition was not a random event, there may be a threat to external validity. Additionally, if the reason is associated with the treatment-comparison group assignments, internal

validity is threatened.

Few, if any of the reasons can be considered definitely indicative of a random process. Rather each one pertains to a situation or a behavior that describes an individual who is different from those persons who remained in a study. For example, participants who cannot be located are probably unlike those participants who were easily located. They may be less well-off (e.g., education, income) and thus having to move from place-to-place or be working a night-time job or simply out on the streets. The people easily located may be more likely to have a spouse and/or children (and thus be at home), to be more dependable, and to have a more stable lifestyle.

Assuming that each reason is at least potentially descriptive of a nonrandom process, then the relationship that each has with treatment group assignments should be assessed. In other words, are the reasons for attrition different for persons in the treatment group than they are for persons in the comparison group? Generally this question can only be assessed by gathering that information and analyzing it for an individual study. However, among the list of reasons in Table 16 there are some that are either possibly or definitely related to group assignment.

There are six reasons listed in Table 16 that pertain to treatment group participants only. They are: ineligible for program, missed program sessions, achieved program goal, dissatisfied with program/treatment, improved academic performance, and fail to complete program requirements. Persons who drop out of the treatment group for some of these reasons (e.g., missed program sessions) do so because they are less interested or less motivated than those people who remain. The

potential for internal validity bias lies in the fact that similarly disinterested or unmotivated persons do not drop out of the control group because they have no participation requirements. The result is a bias in favor of the treatment group because the most interested and motivated individuals are the ones evaluated. Conversely, some individuals may drop out of a program because they have achieved a desired goal. This would create a bias against the treatment group.

In summary, it has been shown that the reasons for attrition, even when broadly stated, can be useful indicators of potential threats to the validity of the results of a study. Most reasons appear to be descriptive of nonrandom events and thus indicate a threat to external validity. In addition, it was noted that when the reasons for attrition are different for the various treatment and comparison groups, there is a threat to the internal validity of the study.

Attrition Counter Measures

The use of specific measures to counter attrition is discussed in this section. Up to five measures were coded for each study, with 39 studies reporting the use of one or more measures. While all of the measures were explicitly mentioned by the study authors, it may not have been explicitly mentioned that the measure's primary objective was to reduce attrition.

Table 17 presents a variety of information for each counter measure. This includes the number of studies that used the measure, the average attrition rates for those studies, the number of authors who indicated that the measure was successful, and the number of authors who indicated that the measure was not successful.

Table 17

Information Concerning Each Attrition Counter Measure

Counter Measure	N	n	Authors' Reports of Success		Attrition Rate		
			Yes	No	Overall	Treat.	Comp.
Cash for eval. time (part.'s)	14	9	7	2	27.7	23.3	28.3
Coordinator resp. for data coll.	11	8	7	0	22.5	22.9	25.5
Extensive tracking technique	10	7	8	0	25.0	24.7	23.1
Close monitoring of particip.	9	7	5	0	29.1	27.6	28.1
Sensitive interviewers/personnel	9	7	6	0	36.3	38.5	40.0
Make-up eval. sessions/ mailing	6	4	1	1	44.7	37.2	52.8
Over-sampling	4	2	-	-	31.0	28.7	13.5
Frequent contact	4	3	0	1	19.5	6.3	8.3
Minimize evaluation burden	3	3	0	1	22.7	19.7	24.0
Cash for eval. time (others)	3	3	-	-	43.0	42.0	45.3
Explicit commit. from part.'s	3	1	1	1	55.7	55.0	64.0
Advance notice of data collect.	3	2	2	0	50.0	48.3	73.5
Non-cash incentive	2	1	-	-	49.0	51.5	27.0
Reminder letters	2	2	-	-	50.5	51.5	41.5
Cooperation of controlling ind.	2	2	-	-	59.0	60.5	49.0
Emphasize confidentiality	2	0	-	-	28.5	28.5	-
Info/education for motivation	2	1	1	0	24.0	24.0	0.0
Cash incentive	1	0	-	-	35.0	35.0	-
Cash deposit	1	0	0	1	59.0	59.0	-
Minimize program burden	1	1	-	-	18.0	10.0	19.0
Non-cash for eval. time (part.'s)	1	1	1	0	60.0	58.0	64.0
Multiple evaluation sessions	1	1	0	1	25.0	6.0	55.0
Field representatives	1	0	-	-	54.0	-	-
Explicit commit from admin.	1	1	-	-	10.0	12.0	9.0
Emphasize imp. of follow-up	1	0	1	0	11.0	11.0	-
Clear explanation of project aims	1	1	-	-	68.0	66.0	71.0
Supplemental sampling	1	1	0	1	20.0	16.0	25.0
Return of unused medication	1	0	-	-	41.0	-	-
Group discussion for motivation	1	0	-	-	48.0	-	-
Lengthy data collection period	1	1	1	0	15.0	15.0	17.0

(Table continues on next page.)

Table 17
(Continued)

Counter Measure	N	n	Authors' Reports of Success		Attrition Rate		
			Yes	No	Overall	Treat.	Comp.
Give impression-partic. expected	1	1	-	-	38.0	34.0	48.0
Allow S to choose interview loc.	1	0	1	0	11.0	11.0	-
Offer specific program group	1	1	-	-	37.0	45.0	18.0
Fee for participating	1	0	0	1	59.0	59.0	-
Timing of random assignment	1	1	-	-	30.0	33.0	28.0
Post study program for C. grp.	1	1	-	-	30.0	33.0	28.0
Replacement/crossovers	1	1	-	-	18.0	26.0	9.0
Number of Counter Measures Used							
0	51	26	-	-	29.0	29.9	22.5
1	9	6	2	0	28.8	25.2	31.0
2	9	6	2	0	28.7	26.2	16.3
3	8	5	0	2	31.5	32.6	35.0
4	7	4	3	1	39.7	36.8	34.5
5	6	5	5	0	30.8	29.5	36.8
One or More	39	26	12	3	31.6	29.6	30.0
TOTAL	90	52	12	3	30.1	29.8	26.3

Note. Up to five counter measures were coded for each study.

N = Number of studies.

n = Number of studies with comparison groups and complete attrition data.

A total of 37 different counter measures were used in the studies. Seventeen of these were used in more than one study. Interpretation of the average attrition rates for each counter measure is difficult for a number of reasons. First, there are a small number of studies involved. Second, most studies used more than one counter measure. This means they are confounded with one another and a direct interpretation of the effect of a single counter measure is not possible. Finally, the direction of any causality is questionable. For example, perhaps researchers planning a study anticipate that attrition rates will be very high and employ the use of a counter measure. In this case the anticipated attrition "caused" the use of the counter measure. And even if the counter measure is effective, the attrition rates may still be higher than average and the counter measure will actually appear ineffective.

Because of the difficulty in interpreting the average attrition rates for each counter measure, the reported opinion of the studies' authors regarding their success in minimizing attrition was coded. This information provides an additional indicator of the utility of the counter measures. An unambiguous judgement regarding attrition was stated in 15 of the study reports. Such an opinion was expressed in a variety of ways. For example, Polich, Armor and Braiker (1980) reported that their attempts to locate participants at follow-up were "quite successful," and that their sample completion rate was "very respectable." In another instance, Weisbrod and Helming (1980) stated that "great efforts were made to reduce attrition," and that "data on all patients were gathered." Note that this opinion was a reflection of all counter measures used in a given study.

Since each study may have used up to five different counter measures, it is useful to examine the attrition rates according to the number of counter measures used. As reported in Table 17, attrition rates were generally higher for studies that had three or more counter measures compared to studies that had two or fewer. This was particularly true for studies with four counter measures. It would seem that the more that is done to prevent attrition, the more likely it is that attrition will be high. However, as noted above, there is a basic problem of interpreting the direction of causality. It is very possible that some researchers anticipated that attrition rates would be high and decided to use a number of methods to counteract the problem. Even if the measures were moderately effective, the level of attrition may still have been high. As a matter of fact, several of the researchers who used multiple counter measures reported that they were successful in reducing attrition (see Table 17).

Analysis of the Frequently Used Counter Measures

Reference to Table 17 indicates that there were five counter measures used in nine or more studies. These were: cash for evaluation time (participants) (N=14); coordinator responsible for data collection (N=11); extensive tracking techniques (N=10); close monitoring of participation (N=9); and sensitive interviewers/personnel (N=9). For the vast majority of studies in which these counter measures were employed, the authors indicated that they were successful in minimizing attrition. The average overall attrition rates for studies using these measures were all below the grand mean of 30, except for studies using sensitive interviewers/personnel.

The following sections discuss in more detail these five counter measures. Information is provided regarding the setting in which the measures were employed, the organization that performed the study, the scale of the study, and other factors.

Cash for Evaluation Time (Participants)

In 14 studies a cash payment was made to participants for the time they spent being interviewed, filling out questionnaires, etc. These studies' were conducted in a variety of settings and in five different study categories. One relatively common element in these studies was the socioeconomic status of the participants. For 10 of the studies, the participants were reported to be in a low income group. This supports the finding of Fleischman (1979) that monetary incentives are most successful among low income participants.

Regardless of the effectiveness of paying research participants, the ability to do so depends on the resources of the performing organization. Ten of the studies were conducted by private contract/research organizations, two by a federal or state organization, and one by a university based institute. Each of these groups is likely to have greater resources, through large grants or contracts, than would a smaller organization such as a single academic department.

Reflecting the scope of these studies and the likelihood of a large grant is the number of participants involved in the study. The average sample size was slightly greater than 2,000, with 10 of the studies having sample sizes greater than 1,000.

Coordinator Responsible for Data Collection

One or more coordinators responsible for data collection were used in 11 studies. This method was used in several settings; six of the seven study categories were represented. A good example of the use of a coordinator is provided by Trismen, Waller and Wilder (1975). The study was a large scale evaluation of a compensatory reading program in 221 elementary schools. An individual from within each school, usually a teacher, was responsible for the administration of tests; the receipt, distribution, and return of all test materials and questionnaires; and to serve as a public relations representative for the study. The coordinator received an honorarium of \$100.

As in the above example, a local coordinator is especially useful in large scale studies. Eight of the 11 studies had sample sizes over 1,200. Reflecting both the scale of these studies and the potential costliness of the method, all 11 studies were performed by private contract/research organizations.

Extensive Tracking Technique

An extensive tracking technique was used in 10 studies. While the specific methods varied across each study, the major objective was the same: To make contact with former participants in order to obtain post-test or follow-up data. The average length of these studies was slightly more than 2 years.

The tracking strategy employed by Polich et al. (1980) illustrates several useful methods. A diverse group of locating tools were used, including the following: admission records from the initial treatment program; information obtained from mid-study interviews (e.g., "Who will

know where you are in the next few years?"); postcards with forwarding addresses; personal calls to the participant's address and to the addresses of relatives; police records; social welfare department records; and contacts with community agencies.

Clearly, an extensive tracking technique can be costly and requires a great deal of effort. This is reflected again in the performing organizations: eight were private contract/research groups and one was a federal organization.

Close Monitoring of Participation

Various techniques of monitoring participation were used in nine studies. Most often this involved the use of a log or file that kept information such as the current status of a participant, the dates and types of contact made with program personnel, and schedules for further contact.

Monitoring was most prevalent in relatively long term studies and ones with large sample sizes. The average study length was 2.7 years and the average sample size was 1,309. The setting for these studies varied. They included schools, work places, a medical office, and the general community. The predominant performing organization was a private contract/research group (N=8).

Sensitive Interviewers/Personnel

While having sensitive interviewers or study personnel is probably an objective of most research programs, it was explicitly reported for just nine studies. The settings for these studies included an elementary school, work places, a hospital, homes, and the general community.

The type of program or treatment being evaluated might play a role in deciding the importance of having sensitive study personnel. The programs in this group of studies were varied. They included educational programs, nursing care, job training, psychotherapy, and nonpsychological counseling or assistance.

Once again the type of performing organization that most often reported the use of this counter measure was a private contract/research organization (N=7). The two other studies were performed by a federal agency and a university-based institute.

Summary of Counter Measures by Categories

Attrition can occur in a research study for a variety of reasons (see the section Reasons for Attrition). If these reasons can be anticipated by the researcher, attrition counter measures can be employed to address them. That is, there are counter measures that have particular objectives and may be more or less successful in minimizing different types of attrition. If the reasons for attrition or the types of attrition cannot be anticipated, it is possible to use a multifaceted approach.

The following sections describe four groups of attrition counter measures that were used in the studies. Each group can be thought of as having a particular objective in mind with regard to reducing attrition. The first group of counter measures are those that offer a specific incentive to the participants of a study. The second group is a diverse set of counter measures that attempt to motivate, stimulate, or otherwise encourage the individuals in a study to maintain participation. The third group consists of techniques that attempt to reduce the over-

all burden that is placed on research participants. And last, there are a number of procedures that improve the management aspects of a research study.

Incentives for Participating

A very straightforward approach to minimizing attrition is to offer a direct incentive to the participants. One type of incentive is designed to increase program participation. This would include a non-cash payment, such as a token redeemable for products; a cash payment; and a cash deposit that would be refunded upon completion of the program.

A different type of incentive is designed to increase completion of evaluation requirements; such as the filling out of questionnaires and being interviewed. Both a cash payment for evaluation time and a non-cash payment for evaluation time were used in the studies.

Strategies to Motivate, Stimulate and Encourage Participation

Within this diverse category there are three types of counter measures. One type consists of basic techniques for providing participants with general information and reminders about the research study. These include maintaining frequent contact with the participants, providing advance notice of data collection, and sending reminder letters.

A second type of motivating strategy is one that utilizes various techniques of social influence. Two measures were used that are designed to appeal to the power of particular individuals. One technique was to gain the cooperation of controlling individuals and the other was to obtain an explicit commitment from administrators. By ensuring

the cooperation of these individuals it is hoped that they will exert the power they have over the participants as a means of minimizing attrition.

Another technique of social influence similar to the two above but bordering on the use of coercive power was to give an impression that participation is expected. This was used in a study of a preretirement program in an industrial setting (Glamser, 1981). The authors stated: "Although the programs were not mandatory, workers were given the impression that participation was expected" (p. 246). Clearly the ethics of this technique should be evaluated before it is applied in most social research settings.

A fourth social influence strategy was used in one study to utilize the social conformity influence of a group. Specifically, a group discussion for motivation technique was employed in a preventive health program to achieve greater participation through individual commitment to a group goal (Lund, Kegeles, & Weisenberg, 1977).

The last type of motivating strategy is one that uses a psychological appeal of one sort or another. For example, one technique was to obtain an explicit commitment from the participants. Another method was to require a fee for participating. Both of these techniques are based on cognitive dissonance theory (Wicklund & Brehm, 1976). By making an explicit commitment, it is hoped that the individual will be consistent with that behavior and maintain participation. The payment of a fee should motivate an individual to complete the program in order to justify having made the payment.

Two strategies were used that try to heighten interest and give a

sense of involvement in a program by increasing the knowledge of the participants. In one case the approach was described as information/education for motivation. Another study used the more specific technique of a clear explanation of project aims.

A standard method for encouraging participation is to emphasize confidentiality. This might be thought of as a method to reduce some fears that individuals have, particularly about the release of personal data.

Another psychological strategy is to emphasize the importance of follow up. In particular, this method stresses to individuals that their participation makes an important contribution to the research goals of the study.

Reducing the Burden Placed on Research Participants

Two of the counter measures used in the studies are general statements about reducing burden. They were approaches to minimize evaluation burden and to minimize program burden. There were three other more specific techniques mentioned. The use of sensitive interviewers and personnel may help to reduce stress and make participating in a study more comfortable. And the use of multiple evaluation sessions and allowing the subject to choose the interview location can make participation more convenient.

Strategies to Improve the Management of a Research Study

There are a number of procedures that researchers can use in the basic management of a research study that may be helpful in reducing attrition. Many of these techniques have to do with the data collection

process. Two of these were used frequently in the studies and are discussed above. They are the use of a coordinator responsible for data collection and an extensive tracking technique. Other strategies used less frequently were: make-up evaluation sessions or mailings, cash payment to individuals other than participants (e.g., teachers) for evaluation time, field representatives to encourage survey response, and a lengthy data collection period. Each of these procedures is designed to reduce attrition that occurs during data collection.

There are three procedures that are used occasionally that do not prevent attrition but are designed to compensate for it by manipulating the sample size. Over-sampling is done at the beginning of a study so that sample sizes will be large enough after attrition occurs. Similarly, after attrition has occurred, supplemental sampling may be used to increase the sample size. The use of replacements or crossovers is done when attrition is predominant among the treatment group. In this case comparison group members are re-assigned to the treatment group. Note that these three techniques do not adjust or compensate for any bias that attrition may have caused.

There are two other miscellaneous study management procedures that may reduce attrition. The first is the close monitoring of participation. This procedure can have many benefits. Participants that miss program sessions can be contacted and encouraged to remain in the study. The current addresses of participants can be maintained. And breakdowns in the implementation of a program can be detected.

Another miscellaneous procedure has to do with the timing of random assignment. The idea is to delay random assignment until after

the subjects have agreed to participate in either the treatment or the comparison group. This should reduce the treatment-correlated attrition that occurs because of negative reactions to group assignments.

Analysis Techniques

Analytic Strategies for Detecting Attrition Bias

Once the data for a study have been collected from the participants, it is necessary to determine if there may be some bias in the data due to attrition. There are several analytic strategies that can be employed to do this. A total of 15 different techniques were described in the study reports. Table 18 furnishes a list of these methods and information concerning the number of studies in which they were used, the reports of bias in those studies, and the average attrition rates for those studies.

One or more analyses for detecting attrition bias were conducted in 49 studies. Generally, the decision to undertake such analyses is based in part on the extent of attrition observed in the study. The average overall attrition rate for studies conducting analyses was 36.6%, and the average treatment-comparison group differential was 10.1%. As expected, these rates are considerably higher than those for studies that did not conduct a bias analysis. The average overall rate for studies not conducting analyses was 22.4% and the average differential was 5.8% (see Table 18).

In many cases, more than one type of analysis was performed. This was the case for 27 studies. There was no apparent relationship between the number of analyses performed and the attrition rates of those studies.

Table 18

Information Concerning the Use of
Attrition Bias Analysis Methods

Method of Analysis	N n		Bias Reported		Attrition Rate			T-C
			Ext.	Int.	Overall	Treat.	Comp.	
T vs C Rates	25	19	8	10	35.0	34.4	28.6	10.0
Attriters vs Completers	23	14	12	7	37.6	37.4	31.5	7.0
T vs C Completers	13	12	1	6	41.3	41.8	35.8	9.8
T vs C Reasons	11	8	6	6	30.7	28.9	23.0	6.4
Completers vs Entire Sample	9	2	3	2	39.4	38.0	23.0	4.5
Location Effort	6	5	5	5	32.3	32.2	29.5	3.0
Causal Mod./Simul. Eq.	6	6	5	4	23.5	22.7	25.8	3.2
Attriters vs Completers: Between Groups	5	4	1	2	33.6	38.4	20.5	19.2
Temporal	4	2	0	0	47.5	48.0	29.5	5.5
T vs C Attriters	2	2	1	1	17.5	15.5	21.0	5.5
Weighted vs Unweighted	2	1	1	1	38.5	41.0	16.0	21.0
Jurs & Glass 2x2	1	1	0	1	25.0	6.0	55.0	49.0
Hierarchical Regression	1	1	0	0	68.0	66.0	71.0	5.0

(Table continues on next page.)

Table 18
(Continued)

Method of Analysis	N	n	Bias Reported		Overall	Attrition Rate		T-C
			Ext.	Int.		Treat.	Comp.	
Completers vs Entire Samp: Between Groups	1	1	0	0	10.0	7.0	13.0	6.0
Regress Attrition on Variables	1	1	0	0	10.0	7.0	13.0	6.0
One or More Analyses	49	31	16	16	36.6	36.2	30.8	10.1
No Analysis	41	21	0	0	22.4	21.4	19.6	5.8
TOTAL	90	52	16	16	30.1	29.8	26.3	8.4

Note. Up to seven different analyses were reported for each study.
 N = Number of studies.
 n = Number of studies with comparison groups and complete
 attrition data.

Most of the frequently used methods were those that have been recommended in the methodological literature (e.g. Cook & Campbell, 1979; Riecken & Boruch, 1974) and were described in the Introduction. These include the comparison of the treatment and comparison groups with respect to: the rate of attrition (N=25); the characteristics of completers (N=13); and the reasons for dropping out (N=11). Each of these comparisons provides information concerning the threat to internal validity. Consequently, an internal validity bias was reported for several of these studies (see Table 18).

The two other methods used most frequently are designed to analyze for external validity bias. These were the comparison of the characteristics of attriters versus completers (N=23) and a similar comparison of completers versus the entire sample (N=9). An external validity bias was reported for many of the studies using these analyses.

General Review of the Strategies

Each of the analyses listed in Table 18 falls into one of three categories. Two of the categories consist of procedures that are statistically oriented and the other category consists of more qualitatively oriented procedures that are designed to assess the attrition process.

Basic Statistical Strategies. The majority of techniques for analyzing for attrition bias involve a statistical comparison of the characteristics of two groups. These characteristics may involve demographic variables, personality variables, pretest measures, etc. Among the groups that can be compared are: attriters versus completers; attriters versus completers - between groups; completers versus the entire

sample; and completers versus the entire sample - between groups. Each of these comparisons examines the representativeness (external validity) of a sample after attrition has occurred. The two between-group procedures make this assessment separately for treatment and comparison groups.

A similar comparison of groups can be made that examines the impact of differential attrition and the threat to internal validity. Initially, a simple comparison can be made of the rates of attrition for treatment versus comparison groups. Then, a comparison can be made of treatment versus comparison completers or treatment versus comparison attriters. An interpretation of either of these analyses depends somewhat on the initial equivalence of the treatment and comparison groups. For example, if a comparison of treatment and comparison groups reveals that these groups are similar, that does not mean that differential attrition did not occur. The groups may have been nonequivalent to begin with, and differential attrition may have resulted in having two groups of completers with equivalent characteristics.

Reliance on a single analysis can lead to a misinterpretation of attrition. The Jurs and Glass (1971) 2x2 analysis combines several comparisons and allows an examination of initial equivalence, external validity, and internal validity (see the Introduction). This analysis and each of the individual comparisons described above are very straightforward and easy to compute. The only requirement is that the appropriate data are available for each participant.

Complex Statistical Strategies. The second type of statistically oriented procedures are more sophisticated than the others and were used

less frequently in the studies. Two of the approaches are similar in that they both involve the comparison of two analytic procedures. In the case of the simultaneous equation technique, an analytic model that includes a correction for attrition is compared to the same model without the attrition correction. One basis for estimating attrition bias is the difference in parameter estimates of the two models (Farkas, Smith, Stormsdorfer, Bottom, & Olsen, 1980). The weighted versus unweighted approach is similar in that an analysis using a weighting procedure is compared to a similar analysis without weighting. One type of weighting procedure is to assign a large weight to the completers who are most similar to the attriters (Aldinger, Bale, & Magidson, 1977). Both the simultaneous equation procedure and the weighted versus unweighted procedure require an understanding of the attrition process in the particular study as well as having data on the relevant variables. In other words a model of attrition needs to be specified that includes all of the variables that differentiate attriters from completers (Barnow, Cain, & Goldberger, 1980; Hausman & Wise, 1979; Heckman, 1979).

There are two other miscellaneous statistical techniques that can be used to examine attrition bias. A hierarchical regression analysis has been described by St. Pierre and Proper (1978). The approach is to regress the attrition rate variable on pretest scores separately for each group. If the slopes of the regression lines are not parallel, a differential attrition bias is indicated. A similar procedure is to regress an attrition dummy variable on several other variables. This analysis indicates if attrition is related to the entire set of vari-

ables and is an estimate of external validity bias.

Attrition Process Analyses. A basic analysis of the attrition process is to examine the reasons that are given by people for dropping out. In particular, the comparison of the reasons given by treatment group members versus those given by comparison group members provides information concerning internal validity bias.

A unique analysis used in a few studies consisted of an analysis of the level of effort required to locate participants (e.g., Polich et al., 1980). Various effort variables were used, including elapsed time, number of persons/agencies contacted, and the number of hours spent in locating a case. The basic idea of the analysis is determine if hard-to-locate cases are different than easy-to-locate cases. If so, it may be extrapolated that cases not located are different as well. Thus, an external validity bias is indicated.

Last, a temporal analysis of attrition can furnish data about possible bias. For example, Caplan et al. (1976) found that most of the dropouts for one treatment group occurred after the first session whereas most of the dropouts of a different group dropped out later in the study. These differing patterns leads one to suspect that the individuals in the two groups were dropping out for different reasons and thus a differential attrition bias may have resulted. The analysis is by no means conclusive, but it provides valuable data concerning the attrition process.

General Data Analysis Methods

The major methods of analyses used in each study varied widely. With up to three types of analyses being coded for each study, a total of 25 different analyses were reported (see Table 19). Five analyses were used in 10 or more studies. These were multiple regression (N=23), chi square (N=21), analysis of variance (ANOVA) (N=18), ANCOVA (N=16), and between groups t-test (N=15).

It is interesting to examine the average differential attrition rate ($|T-C|$) for each of the analyses. Among those analyses used in at least five studies, the average differential rate was greater than 10% for ANCOVA, repeated measures ANOVA, multivariate analysis of variance (MANOVA), and descriptive analyses. Keeping in mind that more than one analysis may have been used in a given study, it can be conjectured that the differential attrition rate may have influenced the choice of analysis. For example, ANCOVA may have been used in some cases in an effort to adjust for treatment-comparison group differences due to attrition. As noted in the Introduction, however, the use of ANCOVA in this situation is likely to result in bias because of measurement error and selection differences (Cook & Campbell, 1979).

The average differential rate was rather low (i.e., less than +/- 6%) for studies employing multiple regression, the within group t-test, and causal modeling/simultaneous equation techniques.

In addition to examining the attrition rates, it is useful to assess the relationship between reports of bias by authors and their choice of analysis. Among studies reporting either an external or internal validity bias, the most frequently used analyses were multiple

Table 19

Information Concerning the Use of
the General Data Analysis Methods

Method of Analysis	N	n	Bias Reported		Overall	Attrition Rate		T-C
			Ext.	Int.		Treat.	Comp.	
Multiple Regression	23	9	8	6	32.4	33.6	29.1	4.2
Chi Square	21	13	1	1	31.7	33.7	36.5	9.7
ANOVA	18	11	6	6	34.3	34.6	24.4	7.8
ANCOVA	16	14	2	6	35.9	32.8	31.8	10.8
T-test (Btwn Groups)	15	9	1	0	24.3	22.4	31.1	7.3
T-test (Within Groups)	9	4	2	1	23.6	20.6	10.8	5.8
Raw Gain Score Analysis	8	6	2	2	28.5	25.3	16.8	6.3
Causal Mod./Simul Eq.	6	6	6	5	25.2	24.2	27.8	3.7
Repeated Meas. ANOVA	5	4	0	1	32.8	30.8	20.8	13.5
MANOVA	5	4	1	0	25.0	24.6	23.2	11.8
Descriptive	5	1	1	1	25.2	26.2	16.0	21.0
Nonparametric Test	4	3	0	2	24.2	26.0	24.3	8.3
Resid. Change Scores	4	3	1	1	45.4	48.2	45.3	9.0
Simple Correlation	4	1	1	0	30.0	31.0	26.0	10.0
Cross-Lagged Corr.	3	2	0	0	33.3	32.7	39.0	8.0
Stand. Change Scores	1	1	1	0	51.0	48.0	56.0	8.0
Treat. Effect Corr.	1	1	1	0	10.0	12.0	9.0	3.0
Life Table	1	0	0	0	41.0	-	-	-
Fisher Exact Test	1	1	0	0	0.0	0.0	0.0	0.0
Logistic Regression	1	0	0	0	45.0	45.0	-	-
Multiple Class. Anal.	1	1	0	0	38.0	35.0	41.0	6.0
Hotelling's T2	1	1	0	0	31.0	42.0	30.0	12.0
Time Trend ANOVA	1	0	0	1	54.0	-	-	-
Z Test	1	0	0	0	19.0	19.0	-	-
MANCOVA	1	1	0	1	51.0	51.0	52.0	1.0
Benefit-Cost Anal.	1	1	0	0	0.0	0.0	0.0	0.0
TOTAL	90	52	16	16	30.1	29.8	26.3	8.4

Note. Up to three different methods of analysis were coded for each study.

N = Number of studies.

n = Number of studies with comparison groups and complete attrition data.

regression, ANOVA, ANCOVA, and causal modeling/simultaneous equation techniques. At first glance it may appear that these researchers used more sophisticated analytic strategies when there was a validity bias due to attrition. A more likely explanation is that those researchers who used more sophisticated analyses of treatment effects (e.g., ANCOVA) also tended to conduct analyses to determine if there was bias due to attrition. The data reflect this possibility. Overall, 54% of the studies analyzed for attrition bias. For studies that utilized multiple regression, the percentage was 61%; for ANOVA it was 61%; for ANCOVA, 75%; and for causal modeling/simultaneous equation technique, 100%. Alternatively, researchers who did not analyze for attrition bias may have been more likely to use more basic statistical techniques (e.g., t-test). The data reflect this as well. For example, for studies using Chi square, the percentage that had analyzed for bias was 43%; for t-test between groups, it was 27%; and for t-test within groups, it was 33%.

Analytic Strategies to Adjust or Compensate for Attrition Bias

The primary analytical problem caused by attrition is due to the differences that are created among treatment and comparison groups. Several strategies have been proposed that attempt to adjust for these differences as well as differences due to selection processes. (See the Introduction.) One or more of these strategies were used in 24 studies (see Table 20). In these studies, the analysis was explicitly designed to address the problem of differences at pretest between treatment and comparison groups. Other studies may have used the same analysis (e.g., ANCOVA), but the intent was not to adjust for these differences.

Table 20

Information Concerning the Use of
Analyses to Adjust for Attrition Bias

Method of Analysis	N	n	Bias Reported		Overall	Attrition Rate		T-C
			Ext.	Int.		Treat.	Comp.	
ANCOVA	12	11	2	6	32.0	37.0	16.0	21.0
Conservative Anal.	6	4	1	1	29.0	30.4	16.0	20.0
Causal Mod./Simul Eq.	5	5	5	4	26.2	25.8	28.4	2.6
Resid. Change Scores	2	1	0	1	51.5	51.0	42.0	9.0
Weighting Cases	1	1	1	1	32.0	37.0	16.0	21.0
Stand. Change Scores	1	1	1	0	51.0	48.0	56.0	8.0
One or More Analyses	24	21	8	11	33.3	32.9	29.7	11.7
No Analyses	66	31	8	5	29.0	28.6	24.0	6.1
TOTAL	90	52	16	16	30.1	29.8	26.3	8.4

Note. Up to three different methods of analysis were coded for each study.

N = Number of studies.

n = Number of studies with comparison groups and complete attrition data.

One of the analyses listed in Table 20 is not strictly designed to adjust for differences. A conservative analysis can only be used when there is attrition from the program/treatment only. In this case, data for all study participants are analyzed, regardless if they dropped out of the program. Using this analysis decreases the chance of detecting a treatment effect, but avoids any bias due to differential attrition (Cook & Campbell, 1979).

As noted in Table 20, when one or more adjustment analyses were used, the average differential attrition rate was 11.7%. This was 3.3% higher than the average differential rate for all studies and 5.6% higher than the average rate for studies not conducting an adjustment analysis. Out of the 16 studies that reported an internal validity bias, 11 conducted one or more analyses to adjust for initial differences.

There is no way to know if the analytic adjustments were successful in minimizing the bias caused by attrition. The use of these techniques requires extensive knowledge of the attrition process. Since this knowledge is imperfect one "cannot have complete confidence that the analysis properly takes into account all potential biases." (Cook & Campbell, 1979, p. 197).

The best that can be done is for the researcher to explore the attrition process as carefully as possible. Various analytic models can then be formulated that are based on this information. And finally, the results of the analyses must be interpreted in light of the potential biases posed by attrition. The overall objective is to separate the effect of differences caused by attrition from differences caused by the

treatment. Cook and Campbell (1979) furnish a thorough discussion of this problem and guidelines for handling it.

CHAPTER IV

RECOMMENDATIONS

This chapter presents a series of guidelines and suggestions for handling attrition in applied social research studies. They are based on the findings of this study, previous research, and the methodological literature. Recommendations are made with an emphasis on pre-study planning.

Developing Hypotheses About Attrition

At the initial planning phase of a study it is useful to develop several hypotheses about attrition. These do not have to be formal statements, but they should indicate how, when, and why attrition is likely to occur. To do this the researcher should carefully examine the study setting and the potential participants. Questions should be asked, such as: What are the participation requirements? Will some people not meet those requirements? Why and when would someone drop out?

There are two purposes that are served by the development of the hypotheses about attrition. The first is that it facilitates the selection of counter measures that will most effectively minimize attrition in that particular study. Second, the hypotheses indicate which variables are likely to be needed for an appropriate analysis of attrition bias. For example, the researcher may feel that individuals who must use public transportation are more likely to miss program sessions and

thus drop out of the study. Based on this hypothesis a counter measure can be devised to address the problem. One possibility might be to provide a flexible schedule of sessions that meets the needs of those who use public transportation. With regard to data analysis: in order to analyze for bias due to the dropping out of people who use public transportation, it is necessary to have data on that variable for every individual at the beginning of the study. An analysis can then be conducted to determine if those persons who dropped out were more frequent users of public transportation than those persons who remained in the study. A potential bias would result if the persons who use public transportation are different from those who do not, in ways that are related to the outcomes of interest in the study (e.g., income).

The hypotheses about attrition should be directed in particular in two areas. The first is to assess in what ways attriters are different from completers. This addresses external validity. The second is to determine in what ways attriters from the treatment group are different from attriters from the comparison group. Or, alternatively, to determine in what ways completers in the treatment group are different from completers in the comparison group. This addresses internal validity.

Selecting Attrition Counter Measures

The first step in the process of selecting counter measures is to prepare a list of the ways that attrition might occur. For each item in the list, one or more techniques for minimizing that type of attrition can be proposed. The choice of techniques might be based on previous research, experience, or common sense. For example, if it is likely that persons in the control group will resent being randomly assigned,

it may be possible to offer them a post-study treatment. Or if it is thought that many people will miss program or evaluation sessions, it might help to send reminder notices, hold multiple and/or make-up sessions, or pay the participants. If it is known at the start of a long-term study that participants will be difficult to locate at follow-up, it would be useful to gather information at the start of the project that might be helpful in locating those individuals. Such information includes the names and addresses of their employer and any friends or relatives who would know where to reach them, the names of schools they attended, and identification numbers such as social security and driver's license.

Clearly it will not be possible to implement every attrition counter measure that is thought to be useful. Priorities must be considered. There are at least three ways to decide the relative merits of the counter measures. The first is to rank the list of ways that attrition is believed to occur in terms of the total proportion of attrition that each accounts for. If one type of attrition is expected to account for 75% of all attrition, one might decide to devote all resources to preventing that type of attrition. Or the top three or four types of attrition might be addressed.

A second alternative is to list the various counter measures in terms of their likely effectiveness. The three or four most effective might be chosen. Accurately predicting the effectiveness of counter measures is difficult, if not impossible. However the relative effectiveness of various techniques may be known and this may be enough in which to base a decision. (See the section above, Attrition Counter

Measures.)

The third alternative for selecting counter measures is to list them according to their cost and/or feasibility. This can be an enlightening task because there may be several productive counter measures that can be utilized with very little cost.

None of the three alternatives for selecting counter measures is ideal, and it is likely that a combination of the three will be used. Preferably a set of techniques can be chosen that will most effectively prevent the most frequent types of attrition at the least cost. It will be up to the researcher to decide upon priorities and how resources are to be allocated.

There is one additional consideration in the use of attrition counter measures. They may affect the construct validity of the treatment. When counter measures are applied to the treatment group only, the effects of those measures become part of the treatment. For example, if treatment group members are paid for the time they put in for program sessions, that payment becomes part of the treatment. Assuming this group is being compared to a no-treatment control group, any effects that are attributed to the treatment must include the payment as part of the treatment. Any counter measure that is applied differentially between groups can present this problem. The researcher must weigh the consequences of high attrition against a possible re-definition of the treatment construct.

Analyzing for Attrition Bias

Analyzing research data for attrition bias is a straightforward and relatively easy task. The first consideration is the selection of variables to use in the analysis. These should be determined according to original hypotheses about attrition and any that may have been developed through the course of the study. Essentially any variable that is thought to be related to attrition should be analyzed. These include demographics, background characteristics, and pretests. Ideally attrition has been anticipated and data on all relevant variables have been collected.

The analysis should be directed at two issues: the representativeness of the resultant sample (external validity) and the equivalence of the treatment and comparison groups (internal validity). The initial step in the process is to examine the overall rate of attrition. The higher the rate, the more likely it is that the data may be biased. Even with very low rates of attrition, it is still advisable to conduct analyses for bias. Little effort is involved and any doubts about possible bias due to attrition can be eliminated.

The basic analysis for assessing external validity is to compare the characteristics of the attriters to those of the completers. Finding significant differences between these groups indicates that the final sample is not representative of the initial sample and thus external validity is in doubt.

To assess the threat to internal validity, the treatment and comparison groups are compared in three ways. The first is to examine the rates of attrition across the groups. If the rates are very different

then a differential attrition bias is probable. Even if the rates are similar, a bias could exist if the reasons for dropping out and/or the characteristics of the persons who dropped out are different across the groups. Therefore, the second analysis is to examine the reasons for attrition. A likely pattern that would point to bias is to find that the persons in the treatment group dropped out for reasons related to participating in the treatment (e.g., dislike of treatment) and the persons in comparison group dropped out for other reasons (e.g., unable to locate).

Finally, the characteristics of the treatment and comparison group completers are compared. The pretest scores of these groups are compared as well. This analysis provides evidence regarding initial equivalence of the various groups, which is necessary for making accurate estimates of the effect of the treatment at posttest. If the groups are found to be different, there is a threat to internal validity.

If sample sizes are sufficient, the analysis suggested by Jurs and Glass (1971) is useful. Within a 2x2 analysis of variance framework, external validity and internal validity are examined simultaneously (see the Introduction).

General Data Analysis

When attrition bias is evident, the foremost consideration in the analysis process is to recognize the limitations of the data. The limitations do not necessarily affect the way in which the data are analyzed. But they definitely affect the interpretation of the results. To the extent that an external validity bias has been indicated, the authors must limit their generalizations of the study results with

respect to the characteristics of the sample completers. If an internal validity bias is indicated, the author must report that differences between groups at posttest may be due at least in part to initial differences between the groups. In other words, the effects of the treatment are confounded with the effects of differential attrition.

It is the purpose of the various analytic adjustment procedures to separate the effects due to the treatment from the effects due to attrition. The common aspect of these procedures is that they incorporate a model of attrition into the analysis. For an appropriate adjustment to be made, the variables that are included in the model must be accurate and complete descriptors of the attrition process. To the extent of that the researcher cannot completely specify a model of the attrition process, the analysis is likely to be biased.

Future Research

It was pointed out in the Introduction, that this study was designed as a preliminary investigation of attrition in applied social research. The analyses resulted in several interesting and important findings. It is clear that attrition may be affected by several factors, including the characteristics of the participants, features of the program or treatment, methodological characteristics, and attrition counter measures. The findings of this study are not conclusive. However, they serve the useful purpose of indicating where additional research is needed.

The primary area in need of study is the use of attrition counter measures. To evaluate adequately the effectiveness of a counter measure, it is necessary to vary systematically its application within the

context of an applied research study. Preferably, the counter measure would be randomly applied to portions of the treatment and/or comparison groups. The effectiveness of the counter measure could then be directly evaluated in this experimental framework. It is important to note that the counter measure would become a "factor" in the research design of the basic study. Therefore, sample sizes may need to be increased and the analytical model should include the counter measure.

Assuming that an experimental evaluation of a counter measure is feasible, a decision must be made as to which technique(s) to use. The section above, Selecting Attrition Counter Measures, detailed a strategy for choosing the most appropriate counter measure(s) for a given situation. Additionally it may be useful to include a social psychological perspective on both the selection of the counter measures and the evaluation of their effectiveness. This is particularly important for those counter measures that attempt to motivate, encourage, or influence individuals to continue participating.

As an example of the application of social psychological theory to the use of counter measures, a researcher may decide to use an expectancy-value approach (Feather, 1982; Rosenberg, 1956) in an informational context. One reason for selecting this method is that it is inexpensive. The counter measure could consist of presenting information to participants that indicates to them that the program is very likely to lead to several positive consequences. This information should be presented to a random sample of the participants. The expectancy-value theory predicts that the participants receiving the counter measure will be less likely to drop out because they will have high subjective expect-

tations of achieving desired outcomes.

The theory also allows for a more informative evaluation of the counter measure. The participants are told that the program is very likely to lead to various positive consequences. However, each participant will have a different opinion of how valuable each of the consequences is. A measure of the participants' evaluation of the consequences can be obtained. The theory makes the additional prediction that attrition will be lowest among those individuals who rated the consequences of the program as highly valuable. (A parallel analysis could be made of the degree to which the participants believe that the program will actually lead to the specified consequences.)

The incorporation of expectancy-value theory into the study of attrition counter measures is just one of many possible approaches to studying attrition. Similar studies are needed regarding the influence that different program and methodological characteristics have on attrition. An area of study that has been generally neglected is the follow-up of individuals who have dropped out of research studies. If these individuals can be located and interviewed, information may be obtained about the attrition process that is otherwise unavailable. A great deal could be learned by simply asking people why they dropped out.

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APPENDIX A

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APPENDIX B

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
1	1-3	SNUM	STUDY NUMBER	101-730	
1	5	CATEGORY	STUDY CATEGORY	1	EARLY & ELEMENTARY ED
				2	SECONDARY & HIGHER ED
				3	TRAINING PROGRAMS
				4	HEALTH SERVICES & MEDICAL TREATMENT
				5	MENTAL HEALTH
				6	WELFARE & SOCIAL SERVICE
				7	CRIMINAL & CIVIL JUSTICE
1	7-8	AUTHOR	PERFORMING ORGANIZATION	1	PSYCHOLOGY DEPT
				2	SOCIOLOGY DEPT
				3	ECONOMICS DEPT
				4	POLITICAL SCIENCE DEPT
				5	CRIMINOLOGY DEPT
				6	PUBLIC HEALTH DEPT
				7	EDUCATION DEPT
				8	OTHER/UNSPECIFIED ACAD DEPT
				9	HOSPITAL/MEDICAL SCHOOL
				10	FEDERAL ORG
				11	STATE ORG
				12	MUNICIPAL ORG
				13	PRIV CONTRACT/RESEARCH ORG
				14	UNIVERSITY BASED INSTITUTE
				15	DENTAL SCHOOL
				16	CMHC
1	10-11	SETTING	PROGRAM SETTING	1	PRE SCHOOL
				2	ELEM SCHOOL
				3	SECONDARY SCHOOL
				4	COLLEGE OR UNIV
				5	HOSPITAL
				6	CLINIC/MED/PSYC OFFICE
				7	CMHC
				8	COMMUNITY CENTER
				9	HOME, INCL FOSTER
				10	WORK
				11	PRISON/JUVENILE PLACE CTR
				12	CHURCH
				13	COMMUNITY/SOCIAL
				14	GOVERNMENT AGENCY
				15	SCHOOL & COMMUNITY
				16	NURSING HOME/HOME CARE
				17	RESIDENTIAL TREATMENT CTR
				18	NURSING HOME
				19	CRIMINAL/CIVIL COURT

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION

1	13	STATUS	PROGRAM STATUS	1 2 3 4	ONGOING PROGRAM BEFORE STUDY NEW & CONTINUING PROGRAM ONE TIME EXPERIMENT/ DEMONSTRATION PROGRAM/ CLINICAL TRIAL NONSPECIFIC PROGRAM
1	15-16	TYPE1	PROGRAM TYPE	1 2 3 4 5 6 7 8 9 10 11 12 13 14 15	EDUCATIONAL TRAINING JOB PLACEMENT PSYCHOTHERAPY/EMOTIONAL COUNSELING NON-PSYCHOLOGICAL COUNSELING/ASSISTANCE DIRECT BENEFITS (MONETARY, COMMODITY) INDIRECT BENEFITS (INTANGIBLE, E.G., RECR.) MEDICAL CARE NURSING CARE ANCILLARY SERVICES PHARMACEUTICALS DENTAL CARE BEHAVIORAL CONTINGENCIES METHODOLOGICAL STUDY CORRELATIONAL RESEARCH STUDY
1	18-19	TYPE2	PROGRAM TYPE	''	''
1	21-22	TYPE3	PROGRAM TYPE	''	''
1	24	MODAL1	PROGRAM MODALITY	1 2 3 4 5	INDIVIDUAL GROUP FAMILY AUTOMATED MOTHER-CHILD
1	26	MODAL2	PROGRAM MODALITY	''	''
1	28	MODAL3	PROGRAM MODALITY	''	''
1	30-32	PWEEKS	PROGRAM LENGTH	### 777	AVERAGE NUMBER OF WEEKS VARIABLE OR INDEFINITE

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
1	34-35	PTIMES	PROGRAM FREQUENCY	## 66 77 88	AVERAGE NUMBER OF TIMES PER WEEK VARIABLE LESS THAN 1/WEEK FULL TIME/CONTINUOUS CARE
1	37-38	PHOURS	PROGRAM DURATION	##	AVERAGE NUMBER OF HOURS PER WEEK
1	40	IMPLEM	CONSISTENCY OF IMPLEMENTATION ACROSS UNITS	1 2 3	IDENTICAL CONSISTENT VARIABLE
1	42-43	AGE	PARTICIPANTS' AGE	##	AVERAGE AGE
1	45-47	SEX	PARTICIPANTS' SEX	###	PERCENT MALE
1	49-51	ETHNIC	PARTICIPANTS' ETHNICITY	###	PERCENT MINORITY
1	53	SES	PARTICIPANTS' SOCIOECONOMIC STATUS	1 2 3 4	LOW MEDIUM HIGH VARIABLE
1	55	GEO	PARTICIPANTS' GEOGRAPHIC AREA	1 2 3 4	URBAN SUBURBAN RURAL VARIABLE
1	57	ACCESS	PARTICIPANTS' ACCESS TO SETTING	1 2 3	ON LOCATION OFF LOCATION VARIABLE (FOR SOME T GRPS)
1	59-60	DATE	PUBLICATION DATE	##	YEAR
1	62	SELECT	METHOD OF SELECTION	1 2 3 4 5 6 7	AUTONOMOUS PRESENTATION PRESENTATION IN RESPONSE TO ADVERTISEMENT OR PR SOLICITED BY PROGRAM OR RESEARCH PERSONNEL COMMITTED/REQUIRED REFERRED INSTITUTIONAL GRP MET CRITERIA/EXISTING PROG WITH EXHAUSTIVE SELECTION

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION

1	64-65	ASSIGN	METHOD OF ASSIGNMENT	1 2 3 4 5 6 7 8 9 10 11	RANDOM INDIVIDUALS/FAMILIES MERIT NEED FIRST COME - FIRST SERVED MATCHING NATURAL GROUPS CRITERIA/DECISION RULE NATURAL GROUP SELF-SELECTION RANDOM GROUPS TIME RULE/TRICKLE IN MIXED
1	66	AWAREPRO	AWARENESS OF PROGRAM/ TREATMENT	1 2 3 4	T GROUP ONLY C GROUP ONLY T & C GROUP NEITHER GRP AWARE
1	68	AWAREASG	AWARENESS OF ASSIGNMENT PROCESS	1 2 3 4	T GROUP ONLY C GROUP ONLY T & C GROUP NEITHER GRP AWARE
1	69	CHOICE	PARTICIPANTS' CHOICE FOR RESEARCH	1 2 3	EXPLICIT IMPLICIT NO CHOICE
1	70	TIMING	TIMING OF ASSIGNMENT	1 2 3 4	AFTER CONSENT FOR T OR C AFTER APPLY TO T, BEFORE CONSENT FOR T OR C BEFORE CONSENT FOR T OR C, WITH NO APPLY TO T AFTER CONSENT FOR STUDY, NO KNOWLEDGE OF T OR C
1	71-72	CARDNUM	CARD NUMBER	1	
2	1-3	SNUM	STUDY NUMBER	101-730	
2	5-7	LENGTH	STUDY LENGTH	#### 777	NUMBER OF WEEKS MORE THAN 776 WEEKS
2	9-10	DATAFREQ	FREQUENCY OF DATA COLLECTION	##	NUMBER OF TIMES
2	12-14	DATADUR1	DURATION OF DATA COLLECTION (MINIMUM)	####	NUMBER OF MINUTES

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
---	---	---	---	---	---
2	16-18	DATADUR2	DURATION OF DATA COLLECTION (MAXIMUM)	###	NUMBER OF MINUTES
2	20	LIKING	EVIDENCE FOR PARTICIPANTS' LIKING OF PROGRAM	1 2	YES NO
2	22	CONVEN	EVIDENCE FOR CONVENIENCE OF PROGRAM	1 2	YES NO
2	24	DATAQFAC	FACE TO FACE QUESTIONNAIRE DATA COLLECTED	1 2	YES NO
2	26	DATAQMAI	MAILED QUESTIONNAIRE DATA COLLECTED	1 2	YES NO
2	28	DATAIFAC	FACE TO FACE INTERVIEW DATA COLLECTED	1 2	YES NO
2	30	DATAIPHO	TELEPHONE INTERVIEW DATA COLLECTED	1 2	YES NO
2	32	DATAOBS	OBSERVATIONAL DATA COLLECTED	1 2	YES NO
2	34	DATAIND	INDIRECT METHOD DATA COLLECTED	1 2	YES NO
2	36	DATAARCH	ARCHIVAL DATA COLLECTED	1 2	YES NO
2	38	DATAOTH	DATA COLLECTED FROM OTHERS	1 2	YES NO
2	40	DATAAUTP	AUTOMATED DATA COLLECTED	1 2	YES NO
2	42	DATARATE	RATING OR EVALUATION OF PARTICIPANT	1 2	YES NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
2	42	DATAPHYS	PHYSIOLOGICAL DATA	1 2	YES NO
2	44	COLLECT1	DATA COLLECTOR	1 2 3 4 5	PROGRAM PERSONNEL INTERNAL EVALUATION PERSONNEL EXTERNAL EVALUATION PERSONNEL SUBCONTRACTOR (NON PROG. OR EVAL. PERSONNEL) ARCHIVAL SOURCES
2	46	COLLECT2	DATA COLLECTOR	' '	' '
2	48	COLLECT3	DATA COLLECTOR	' '	' '
2	50	EVALLOC	LOCATION OF EVALUATOR/ RESEARCHER	1 2	INTERNAL (AFFILIATED WITH PROGRAM) EXTERNAL (NOT AFFILIATED WITH PROGRAM)
2	53-54	DESIGN	RESEARCH DESIGN	1-16 17 18 19 20 21 22 23 24 25	CAMPBELL & STANLEY (1966) MATCH SAMPLES/NATURAL GRPS RANDOM ASSIGN-NATURAL GROUPS PRE-POST, POST ONLY NO C GRP 2 STAGE PRE-POST RANDOM ASGN PRE-POST T VS CNTL NORM GRP NAT VARIATION/SELF SELECTION QUASI-EXP SOLOMON 4-GROUP NONEQUIV GRP PANEL STUDY ONE GROUP PANEL STUDY
2	56-57	TGROUPS	NUMBER OF TREATMENT GROUPS	###	
2	59-60	CGROUPS	NUMBER OF CONTROL GROUPS	###	
2	62-65	NPRET1	PRETEST N FOR TREAT. GROUP 1	##### 9998	MORE THAN 9997
2	71-72	CARDNUM	CARD NUMBER	2	
3	1-3	SNUM	STUDY NUMBER	101-730	
3	5-8	NPRET2	PRETEST N FOR TREAT. GROUP 2	#####	

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	----	-----	-----	-----	-----
3	10-13	NPRET3	PRETEST N FOR TREAT. GROUP 3	####	
3	15-18	NPREC1	PRETEST N FOR CONTROL GROUP 1	####	
3	20-23	NPREC2	PRETEST N FOR CONTROL GROUP 2	####	
3	25-28	NPREC3	PRETEST N FOR CONTROL GROUP 3	####	
3	30-33	NPOSTT1	POSTTEST N FOR TREAT. GROUP 1	####	
3	35-38	NPOSTT2	POSTTEST N FOR TREAT. GROUP 2	####	
3	40-43	NPOSTT3	POSTTEST N FOR TREAT. GROUP 3	####	
3	45-48	NPOSTC1	POSTTEST N FOR CONTROL GROUP 1	####	
3	50-53	NPOSTC2	POSTTEST N FOR CONTROL GROUP 2	####	
3	55-58	NPOSTC3	POSTTEST N FOR CONTROL GROUP 3	####	
3	60	UNIT	PRIMARY UNIT OF ANALYSIS	1 2 3 4 5	INDIVIDUAL GROUP/CLASS SITE/SCHOOL FOSTER PLACEMENT VARIED-INDIV, FAM, EPISODE, ETC

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
---	---	---	---	---	---
3	62-63	ANALYS1	METHOD OF ANALYSIS	1	CHI SQUARE
				2	T-TEST (BETWEEN GROUPS)
				3	ANOVA
				4	ANCOVA
				5	TRUE-SCORE ANCOVA
				6	STANDARDIZED CHANGE SCORE
				7	CAUSAL MODELING
				8	ECONOMETRIC/MAX LIKLIHOOD
				9	MULTIPLE REGRESSION/ CORRELATION
				10	TIME SERIES
				11	ANALYSIS OF DIFF SCORES/ RAW GAIN SCORES
				12	TREAT-EFFECT CORRELATIONS (STANDARDIZED-PRE VS POST)
				13	LIFE TABLE
				14	NO ANALYSIS PRESENTED
				15	T-TEST (WITHIN GROUPS)
				16	REPEATED MEASURES ANOVA
				17	MANOVA
				18	FISHER EXACT TEST
				19	NON PARAMETRIC TEST
				20	RESIDUALIZED CHANGE SCORES
				21	CROSS-LAGGED CORRELATIONS
				22	SIMPLE CORRELATION
				23	LOGISTIC REGRESSION
				24	MULT CLASSIFICATION ANALYSIS
				25	HOTELLINGS T2
				26	TIME TREND ANOVA
				27	DESCRIPTIVE
				28	Z TEST
3	65-66	ANALYS2	METHOD OF ANALYSIS	' '	' '
3	68-69	ANALYS3	METHOD OF ANALYSIS	' '	' '
3	71-72	CARDNUM	CARD NUMBER	3	
4	1-3	SNUM	STUDY NUMBER	101-730	

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
4	5	EVIDENCE	ATTRITION EVIDENCE	1 2 3 4 5	NO EVIDENCE AVAILABLE (TO READER) EVIDENCE AVAILABLE BUT NOT MENTIONED ATTRITION MENTIONED BUT NOT ANALYZED ATTRITION ANALYZED BUT RESULTS NOT PRESENTED ATTRITION ANALYSES PRESENTED
4	7-8	RATET1	ATTRITION RATE FOR T. GROUP 1	###	PERCENT
4	10-11	RATET2	ATTRITION RATE FOR T. GROUP 2	###	PERCENT
4	13-14	RATET3	ATTRITION RATE FOR T. GROUP 3	###	PERCENT
4	16-17	RATEC1	ATTRITION RATE FOR C. GROUP 1	###	PERCENT
4	19-20	RATEC2	ATTRITION RATE FOR C. GROUP 2	###	PERCENT
4	22-23	RATEC3	ATTRITION RATE FOR C. GROUP 3	###	PERCENT
4	25-26	ATTRATET	ATTRITION RATE FOR TREATMENT GROUPS (ALL)	###	PERCENT
4	28-29	ATTRATEC	ATTRITION RATE FOR CONTROL GROUPS (ALL)	###	PERCENT
4	31-32	ATTRATEO	ATTRITION RATE OVERALL	###	PERCENT
4	34-36	ATTPOL	POLICY ATTRITION	####	PERCENT OF TOTAL ATTRITION
4	38-40	ATTPROG	PROGRAM ATTRITION	####	PERCENT OF TOTAL ATTRITION
4	42-44	ATTSAMP	SAMPLE ATTRITION	####	PERCENT OF TOTAL ATTRITION

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
4	46-48	PROGONLY	ATTRITION FROM PROGRAM ONLY	###	PERCENT OF TOTAL
4	50-52	MEASONLY	ATTRITION FROM MEASUREMENT ONLY	###	PERCENT OF TOTAL
4	54-56	PROGMEAS	ATTRITION FROM PROGRAM AND MEASUREMENT	###	PERCENT OF TOTAL
4	58-60	PATTERN	ATTRITION PATTERN OVER TIME	1 2 3 4 5 6 7 8	POSITIVE SKEW NEGATIVE SKEW UNIMODAL MULTIMODAL POSITIVE LINEAR NEGATIVE LINEAR RECTANGLUAR VARIABLE
4	62	TVSCNON	ANALYSIS-T VS C NONATTRITERS	1 2	YES NO
4	64	TVSCATT	ANALYSIS-T VS C ATTRITERS	1 2	YES NO
4	66	JG2X2	ANALYSIS-JURS & GLASS 2 X 2	1 2	YES NO
4	68	STPMR	ANALYSIS-ST PIERRE MULT REGRESSION	1 2	YES NO
4	71-72	CARDNUM	CARD NUMBER	4	
5	1-3	SNUM	STUDY NUMBER	101-730	
5	5	REASCOMP	ANALYSIS-REASONS COMPARED	1 2	YES NO
5	7	TEMPORAL	ANALYSIS-TEMPORAL	1 2	YES NO
5	9	ATTVSNON	ANALYSIS-ATTRITERS VS NONATTRITERS	1 2	YES NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
5	11	RATECOMP	ANALYSIS- ATTRITION RATES COMPARED	1 2	YES NO
5	13	NONVSSAM	ANALYSIS- NONATTRITERS VS ENTIRE SAMPLE	1 2	YES NO
5	15	EFFORT	ANALYSIS- EFFORT REQUIRED TO LOCATE CASE	1 2	YES NO
5	17	WEIGHT	ANALYSIS- WEIGHTED VS UNWEIGHTED	1 2	YES NO
5	19	REASONS	REASONS FOR ATTRITION	1 2 3 4	BASED UPON DATA BASED UPON SPECULATION NONE GIVEN 1 & 2
5	21	FOLLOW	ATTRITERS FOLLOWED-UP	1 2 3	ALL SOME NONE MENTIONED
5	23-24	FOLLOWN	NUMBER OF FOLLOW-UPS ATTEMPTED (ATTRITION- BASED)	##	
5	26	BIASINT	INTERNAL VALIDITY BIAS	1 2 3 8	BIAS REPORTED NONE REPORTED BUT POSSIBLE NO BIAS EVIDENT NOT RELEVANT FOR STUDY
5	28	BIASEXT	EXTERNAL VALIDITY BIAS	1 2 3	BIAS REPORTED NONE REPORTED BUT POSSIBLE NO BIAS EVIDENT
5	30	BIASADJ	ADJUSTMENT FOR BIAS	1 2 3	EXPLICIT IMPLICIT NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION

5	32	ADJMETH1	METHOD OF BIAS ADJUSTMENT	1 2 3 4 5 6	WEIGHTING CASES COVARIANCE CAUSAL MODEL CONSERVATIVE ANALYSIS STANDARDIZED CHANGE SCORE RESIDUALIZED CHANGE SCORES
5	34	ADJMETH2	METHOD OF BIAS ADJUSTMENT	''	''
5	36	ADJMETH3	METHOD OF BIAS ADJUSTMENT	''	''
5	38	ATTOUT	ATTRITION USED AS OUTCOME VAR	1 2	YES NO
5	40	CORRAGE	AGE CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	42	CORRSEX	SEX CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	44	CORRETHN	ETHNICITY CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	46	CORRSES	SES CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	48	CORREDUC	EDUCATION CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	50	CORRINT	INTELLIGENCE CORRELATION WITH ATTRITON	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	52	CORRAPT	APTITUDE CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	54	CORRPERS	PERSONALITY CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
5	56	CORRSOC	SOCIAL PSYCH CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	58	CORRPRE1	PRETEST 1 CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	60	CORRPRE2	PRETEST 2 CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	62	CORRPRE3	PRETEST 3 CORRELATION WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	64	INTERACT	INTERACTION RELATIONSHIP WITH ATTRITION	1 2 3	SIGNIFICANT NOT SIGNIFICANT NOT ANALYZED
5	66	ATTDATA	DATA AVAILABLE FOR ATTRITERS	1 2 3 4	BACKGROUND VARIABLES ONLY PRETEST MEASURES ONLY BOTH NO DATA AVAILABLE
5	68	EXIT	EXIT INTERVIEW/ EVALUATION CONDUCTED	1 2	YES NO
5	71-72	CARDNUM	CARD NUMBER	5	
6	1-3	SNUM	STUDY NUMBER	101-730	
6	5	SECOND	SECONDARY ANALYSES CONDUCTED	1 2 3	REGARDING ATTRITION REGARDING OTHER RESULTS NONE APPARENT
6	7	COST	COST INFO ABOUT ATTRITION COUNTER MEASURES	1 2	YES NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
6	9-10	PUBTYPE	PUBLICATION TYPE	1 2 3 4 5 6 7 8	JOURNAL BOOK ERIC NTIS NCJRS GOVERNMENT REPORT ORGANIZATION PUBLICATION ORGANIZATION UNPUBLISHED REPORT
6	12	NONRESP	ATTRITION DUE TO SURVEY NONRESPONSE	1 2 3	ALL SOME NONE
6	14	ACTPASS	ATTRITION- ACTIVE OR PASSIVE	1 2 3	MOSTLY ACTIVE 2 MOSTLY PASSIVE INDISTINGUISHABLE
6	16	OTHERS	INVOLVEMENT OF SIGNIF OTHERS	1 2 3 4	T GROUP C GROUP T & C GROUP NEITHER GROUP
6	18	PREATTR	ATTRITION FROM PRETEST	1 2	YES NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
6	20-21	REASON1	REASON FOR ATTRITION (AUTHOR)	1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24	NATURAL (TURNOVER) LACK OF CONTROL OVER DATA COLLECTION DROPPED OUT OF SCHOOL MOVED INCOMPLETE DATA DECEASED MORBIDITY/INCAPACITATED IMPROVED STATUS - DISCHARGED REFUSED TO BE INTERVIEWED NO MATCH FOUND FOR ANALYSIS ACHIEVED PROGRAM GOAL INELIGIBLE FOR PROGRAM REFUSE TO PARTICIPATE/ UNCOOPERATIVE INSTITUTIONALIZED/INCARCER UNABLE TO LOCATE/UNAVAILABLE MISSED PROGRAM SESSIONS CHILD RUNAWAY POLICY DECISION AMBIVALENT PARTICIPATING/ LACK OF COMMITMENT DATA COLLECTION FRAUD DISSATISFIED WITH PROG/TREAT SCHEDULING DIFFICULTIES SITE/SCHOOL LEVEL DROPOUT GENERAL DROPOUT VS OTHER REASONS
6	23-24	REASON2	REASON FOR ATTRITION (AUTHOR)	''	''
6	26-27	REASON3	REASON FOR ATTRITION (AUTHOR)	''	''
6	29	CNTWRKA	ATTRITION COUNTER MEASURE WORKED (AUTHOR)	1 2	YES NO
6	31	CNTWRKO	ATTRITION COUNTER MEASURE WORKED (OPINION)	1 2	YES NO
6	33	CNTDIR	ATTRITION COUNTER MEASURES DIRECTED ACROSS GROUPS	1 2	EQUIVALENTLY NON-EQUIVALENTLY

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
-	-	-	-	-	-
6	35-36	COUNTER1	ATTRITION	1	CASH INCENTIVE
			COUNTER	2	OTHER INCENTIVE
			MEASURE 1	3	CASH DEPOSIT
				4	MINIMIZE PROGRAM BURDEN
				5	MINIMIZE EVALUATION BURDEN
				6	MEANINGFUL MEASUREMENT SCALES
				7	CASH FOR EVALUATION TIME (PARTICIPANTS)
				8	OTHER BENEFIT FOR EVALUATION TIME (PARTICIPANTS)
				9	CASH FOR EVALUATION TIME (PERIPHERAL INDIVIDUALS)
				10	OTHER BENEFIT FOR EVALUATION TIME (PERIPHERAL INDIVIDUALS)
				11	MULTIPLE PROGRAM SESSIONS
				12	MULTIPLE EVALUATION SESSIONS
				13	FRINGE BENEFITS FOR ADMINISTRATORS/ KEY INDIVIDUALS
				14	FIELD REP'S TO ENCOURAGE MAIL/LOCAL SURVEY RESPONSE
				15	LOG TO MONITOR PARTICIPAT/ CLOSE MONITORING
				16	EXPLICIT COMMITMENT FROM PARTICIPANTS
				17	EXPLICIT COMMITMENT FROM ADMINISTRATORS
				18	FREQUENT CONTACT
				19	SENSITIVE INTERVIEWERS/ PROGRAM PERSONNEL
				20	COOPERATION OF COMMUNITY LEADERS
				21	EMPHASIZE IMPORTANCE OF FOLLOW-UP
				22	REMINDER LETTERS
				23	REMINDER CALLS
				24	APPOINTMENT LETTERS
				25	APPOINTMENT CALLS
				26	MAKE AWARE OF ASSIGNMENT CONDITIONS
				27	EMPHASIZE FREE CHOICE

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION

				28	ANCILLARY SERVICES
				29	NEWSLETTER
				30	RETAIL CREDIT COMPANY
				31	UNOBTRUSIVE MEASURES
				32	CLEAR EXPLANATION OF PROJECT AIMS
				33	COOPERATION OF SIGNIFICANT OTHERS
				34	MAKE-UP PROGRAM SESSIONS
				35	MAKE-UP EVALUATION SESSIONS/MAILINGS
				36	SPECIAL NONRESPONDENT SURVEY
				37	SUPPLEMENTAL SAMPLING
				38	LOCAL COORDINATOR RESPONS FOR DATA COLLECTION/PR
				39	RETURN OF UNUSED MEDICATION
				40	INFORM/EDUC FOR MOTIVATION
				41	GROUP DISCUSS FOR MOTIVATION
				42	EXTENSIVE TRACKING TECHNIQUE
				43	LENGTHY DATA COLLECT PERIOD
				44	ADVANCE NOTICE-DATA COLLECT
				45	OVER-SAMPLE AT BEGINNING
				46	GIVE IMPRESSION THAT PARTICIPATION IS EXPECTED
6	38-39	COUNTER2	ATTRITION COUNTER MEASURE 2	''	''
6	41-42	COUNTER3	ATTRITION COUNTER MEASURE 3	''	''
6	44-45	COUNTER4	ATTRITION COUNTER MEASURE 4	''	''
6	47-48	COUNTER5	ATTRITION COUNTER MEASURE 5	''	''
6	50	TREATCOM	TREATMENT FOR COMPARISON GRP	1 2	YES NO

C	COLS	VAR NAME	VAR DESCRIPTION	VALUES	VALUE DESCRIPTION
6	52	NREPORT	FAILURE TO REPORT PRE-ONLY N'S	1 2	YES NO
6	54	ECON1	ANALYSIS- ECONOMETRIC	1 2	YES NO
6	56	ECON2	ANALYSIS- ECON CORRECTED VS UNCORRECTED	1 2	YES NO
6	58	POWER	ANALYSIS- POWER	1 2	YES NO
6	60	BIVAR	ANALYSIS- KEY VAR-OUTCOME VAR RELATIONSHIPS	1 2	YES NO
6	62	NONVSBTW	ANALYSIS- NONATTRITERS VS ENTIRE SAMPLE - BETWEEN GROUPS	1 2	YES NO

APPENDIX C

Table 1

Number of Studies by
Program/Treatment Setting and Study Category

Setting	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	0	0	1	0	0	0	0	1
Pre School	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.1)
Elementary School	8 (80.0)	0 (0.0)	0 (0.0)	2 (13.3)	1 (6.7)	0 (0.0)	0 (0.0)	11 (12.4)
Secondary School	0 (0.0)	6 (60.0)	1 (11.1)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	7 (7.9)
College or University	0 (0.0)	3 (30.0)	1 (11.1)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	5 (5.6)
Hospital	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	4 (26.7)	0 (0.0)	0 (0.0)	4 (4.5)
Medical/Psych Office	0 (0.0)	0 (0.0)	0 (0.0)	7 (46.7)	2 (13.3)	1 (6.7)	0 (0.0)	10 (11.2)
Comm. Mental Health Center	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	3 (20.0)	0 (0.0)	0 (0.0)	3 (3.4)
Community Center	0 (0.0)	0 (0.0)	2 (22.2)	1 (6.7)	0 (0.0)	0 (0.0)	2 (13.3)	5 (5.6)
Home	1 (10.0)	0 (0.0)	0 (0.0)	2 (13.3)	0 (0.0)	5 (33.3)	1 (6.7)	9 (10.1)
Work	0 (0.0)	0 (0.0)	4 (44.4)	1 (6.7)	0 (0.0)	1 (6.7)	0 (0.0)	6 (6.7)
Prison/Juven. Placement Ctr.	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	4 (26.7)	4 (4.5)

Table continues on next page.

Table 1
(Continued)

Setting	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
General Community	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	7 (46.7)	6 (40.0)	14 (15.8)
Government Agency	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	1 (1.1)
School & Community	0 (0.0)	1 (10.0)	1 (11.1)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	3 (3.4)
Home Care/ Nursing	0 (0.0)	0 (0.0)	0 (0.0)	2 (13.3)	1 (6.7)	0 (0.0)	0 (0.0)	3 (3.4)
Residential Treat. Center	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	1 (1.1)
Nursing Home	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	1 (1.1)
Criminal/Civil Court	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (1.1)
Total	10 (100)	10 (100)	9 (100)	15 (100)	15 (100)	15 (100)	15 (100)	89 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 2
Number of Studies by
Type of Program/Treatment and Study Category

Type of Prog./Treat.	Study Category							Total N=90
	Elem Educ N=10	Sec/Hi Educ N=10	Train- ing N=10	Health N=15	Mental Health N=15	Social Serv. N=15	Crim Just. N=15	
Educational	10	8	3	6	1	1	6	35
Training	2	4	9	3	0	3	3	24
Job Placement	0	0	6	0	0	2	3	11
Psychotherapy/ Emot. Counsel.	0	0	0	1	12	3	4	20
Non Psych. Counseling	0	2	2	5	4	4	11	28
Direct Benefits	0	0	0	0	0	7	0	7
Indirect Benefits	0	0	0	0	0	1	0	1
Medical Care	0	0	0	8	3	1	0	12
Nursing Care	0	0	0	3	0	0	0	3
Ancillary Services	1	0	0	2	2	2	0	7
Pharmaceuticals	0	0	0	2	2	0	0	4

Table continues on next page.

Table 2
(Continued)

Type of Prog./Treat.	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Dental Care	0	0	0	1	0	0	0	1
Behavioral Contingencies	0	0	0	0	1	1	0	2
Methodological Study	0	0	0	1	2	0	0	3
Correlational Study	0	3	0	0	0	1	2	6

Note. Up to 3 types of program/treatment were coded for each study.

Table 3
 Number of Studies by
 Length of Program/Treatment and Study Category

Study Category

Program Length	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	Total
Missing	4	4	1	3	8	6	5	31
<= 6 Weeks	0 (0.0)	1 (16.7)	1 (11.1)	1 (8.3)	0 (0.0)	1 (11.1)	2 (20.0)	6 (10.2)
7 Weeks to 6 Months	0 (0.0)	1 (16.7)	3 (33.3)	3 (25.0)	3 (42.9)	4 (44.4)	3 (30.0)	17 (28.8)
> 6 Months to 1 Year	2 (33.3)	3 (50.0)	0 (0.0)	6 (50.0)	3 (42.9)	1 (11.1)	1 (10.0)	16 (27.1)
> 1 Year to 2 Years	1 (16.7)	1 (16.7)	5 (55.6)	1 (8.3)	1 (14.3)	0 (0.0)	3 (30.0)	12 (20.3)
> 2 Years to 4 Years	3 (50.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	3 (33.3)	0 (0.0)	6 (10.2)
> 4 Years	0 (0.0)	0 (0.0)	0 (0.0)	1 (8.3)	0 (0.0)	0 (0.0)	1 (10.0)	2 (3.4)
Total	6 (100)	6 (100)	9 (100)	12 (100)	7 (100)	9 (100)	10 (100)	59 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 4

Descriptive Statistics for
Length of Program/Treatment by Study Category

Program Length	Study Category							Total (N=59)
	Elem Educ (N=6)	Sec/Hi Educ (N=6)	Train- ing (N=9)	Health Health (N=12)	Mental Health (N=7)	Social Serv. (N=9)	Crim Just. (N=10)	
Mean	117.3	36.5	40.1	75.4	33.6	77.0	63.3	63.6
Median	114.5	36	59	50	36	26	34.5	39
Standard Deviation	76.3	26.6	28.2	126.2	21.8	87.1	78.5	79.8
Range	36 - 208	6 - 84	6 - 65	5 - 468	8 - 60	4 - 208	1 - 260	1 - 468

Note. Numbers in parentheses represent the number of studies with nonmissing data.

Table 5

Number of Studies by
Participants' Gender and Study Category

Gender (% Male)	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	5	3	2	4	2	8	3	27
0 - 20	1 (20.0)	0 (0.0)	1 (12.5)	3 (27.3)	1 (7.7)	4 (57.1)	0 (0.0)	10 (15.9)
21 - 40	0 (0.0)	2 (28.6)	1 (12.5)	5 (45.4)	4 (30.8)	2 (28.6)	1 (8.3)	15 (23.8)
41 - 60	4 (80.0)	5 (71.4)	3 (37.5)	0 (0.0)	3 (23.1)	0 (0.0)	1 (8.3)	16 (25.4)
61 - 80	0 (0.0)	0 (0.0)	0 (0.0)	1 (9.1)	2 (15.4)	0 (0.0)	2 (16.7)	5 (7.9)
81 - 100	0 (0.0)	0 (0.0)	3 (37.5)	2 (18.2)	3 (23.1)	1 (14.3)	8 (66.7)	17 (27.0)
Total	5 (100)	7 (100)	8 (100)	11 (100)	13 (100)	7 (100)	12 (100)	63 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 6

Descriptive Statistics for
Participants' Gender (% Male) by Study Category

Gender (% Male)	Study Category							Total (N=63)
	Elem Educ (N=5)	Sec/Hi Educ (N=7)	Train- ing (N=8)	Health Health (N=11)	Mental Health (N=13)	Social Serv. (N=7)	Crim Just. (N=12)	
Mean	40.4	45.1	55.2	41.3	57.2	24.1	83.8	52.9
Median	51	49	52.5	32	49	16	96.5	49
Standard Deviation	22.7	9.8	32.6	34.0	28.7	35.6	22.2	32.3
Range	0 - 53	25 - 53	0 - 94	0 - 100	15 - 100	0 - 100	38 - 100	0 - 100

Note. Numbers in parentheses represent the number of studies with nonmissing data.

Table 7
 Number of Studies by
 Participants' Average Age and Study Category

Participant's Average Age	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	0	6	1	5	5	6	3	26
4 - 12	9 (90.0)	0 (0.0)	0 (0.0)	2 (20.0)	1 (10.0)	1 (11.1)	1 (8.3)	14 (21.9)
13 - 20	0 (0.0)	4 (100)	4 (44.4)	0 (0.0)	1 (10.0)	1 (11.1)	7 (58.3)	17 (26.6)
21 - 40	1 (10.0)	0 (0.0)	4 (44.4)	1 (10.0)	4 (40.0)	2 (22.2)	4 (33.3)	16 (25.0)
41 - 60	0 (0.0)	0 (0.0)	0 (0.0)	3 (30.0)	3 (30.0)	2 (22.2)	0 (0.0)	8 (12.5)
61 - 82	0 (0.0)	0 (0.0)	1 (11.1)	4 (40.0)	1 (10.0)	3 (33.3)	0 (0.0)	9 (14.1)
Total	10 (100)	4 (100)	9 (100)	10 (100)	10 (100)	9 (100)	12 (100)	64 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 8
 Descriptive Statistics for
 Participants' Average Age by Study Category

Average Age	Study Category							Total (N=64)
	Elem Educ (N=10)	Sec/Hi Educ (N=4)	Train- ing (N=9)	Health Health (N=10)	Mental Health (N=10)	Social Serv. (N=9)	Crim Just. (N=12)	
Mean	8.0	16.7	28.6	49.0	35.6	43.4	19.5	29.3
Median	6	17	25	54	33.5	50	16.5	21.5
Standard Deviation	6.2	.5	15.5	22.4	20.8	23.9	7.5	21.2
Range	4 - 25	17 - 17	16 - 64	11 - 75	7 - 82	6 - 75	10 - 36	4 - 82

Note. Numbers in parentheses represent the number of studies with nonmissing data.

Table 9
Number of Studies by
Performing Organization and Study Category

Performing Organization	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	0	1	0	0	1	0	1	3
Psychology Department	0 (0.0)	1 (11.1)	0 (0.0)	1 (6.7)	4 (28.6)	0 (0.0)	1 (7.1)	7 (8.0)
Sociology Department	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (7.1)	2 (2.3)
Economics Department	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (7.1)	0 (0.0)	0 (0.0)	1 (1.2)
Criminology Department	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	1 (1.2)
Public Health Department	0 (0.0)	0 (0.0)	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.2)
Education Department	4 (40.0)	1 (11.1)	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	6 (6.9)
Unspecified Academic Dept.	1 (10.0)	2 (22.2)	0 (0.0)	0 (0.0)	0 (0.0)	8 (53.3)	2 (14.3)	13 (14.9)
Hospital/ Medical School	0 (0.0)	0 (0.0)	0 (0.0)	7 (46.7)	5 (35.7)	0 (0.0)	0 (0.0)	12 (13.8)
Federal Organization	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (7.1)	0 (0.0)	1 (7.1)	3 (3.4)
State Organization	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (7.1)	1 (6.7)	4 (28.6)	6 (6.9)
Priv. Contract Research Org.	5 (50.0)	2 (22.2)	8 (80.0)	3 (20.0)	1 (7.1)	3 (20.0)	2 (14.3)	24 (27.6)

Table continues on next page.

Table 9
(Continued)

Performing Organization	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
University Institute	0 (0.0)	2 (22.2)	0 (0.0)	2 (13.3)	0 (0.0)	1 (6.7)	3 (21.4)	8 (9.2)
Dental School	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.2)
Comm. Mental Health Center	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (7.1)	0 (0.0)	0 (0.0)	1 (1.2)
High School	0 (0.0)	1 (11.1)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.2)
Total	10 (100)	9 (100)	10 (100)	15 (100)	14 (100)	15 (100)	14 (100)	87 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 10
 Number of Studies by
 Publication Source and Study Category

Publication Source	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Journal	0 (0.0)	4 (40.0)	3 (30.0)	9 (60.0)	12 (80.0)	10 (66.7)	9 (60.0)	47 (52.2)
Book	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	3 (20.0)	0 (0.0)	3 (3.3)
ERIC	8 (80.0)	2 (20.0)	1 (10.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	12 (13.3)
NCJRS	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (1.1)
Government Report	0 (0.0)	0 (0.0)	1 (10.0)	1 (6.7)	1 (6.7)	1 (6.7)	2 (13.3)	6 (6.7)
Organization Pub. Report	1 (10.0)	1 (10.0)	5 (50.0)	3 (20.0)	1 (6.7)	0 (0.0)	1 (6.7)	12 (13.3)
Organization Unpub. Report	1 (10.0)	3 (30.0)	0 (0.0)	2 (13.3)	0 (0.0)	1 (6.7)	2 (13.3)	9 (10.0)
Total	10 (100)	10 (100)	10 (100)	15 (100)	15 (100)	15 (100)	15 (100)	90 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 11
 Number of Studies by
 Research Design and Study Category

Research Design	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health Health	Mental Health Serv.	Social Serv.	Crim Just.	
One-Shot Case Study	0 (0.0)	0 (0.0)	1 (10.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	2 (2.2)
One-Group Pre-Post	0 (0.0)	1 (10.0)	0 (0.0)	2 (13.3)	2 (13.3)	1 (6.7)	0 (0.0)	6 (6.7)
Static Group Comparison	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.6)	1 (1.1)
Pre-Post Control Group	0 (0.0)	3 (30.0)	6 (60.0)	8 (53.3)	5 (33.3)	3 (20.0)	7 (46.7)	32 (35.6)
Post-Only Control Group	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (6.7)	0 (0.0)	0 (0.0)	2 (2.2)
Nonequivalent Control Group	4 (40.0)	2 (20.0)	3 (30.0)	2 (13.3)	3 (20.0)	3 (20.0)	2 (13.3)	19 (21.1)
Institutional Cycle	0 (0.0)	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (1.1)
Matched Samp./ Natural Grps.	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	2 (13.3)	0 (0.0)	0 (0.0)	3 (3.3)
Random Assign. Natural Grps.	2 (20.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	0 (0.0)	3 (3.3)
Pre-Post, Post Only/No C Grp.	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	1 (1.1)
2 Stage Pre-Post Rnd. Asg.	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	1 (1.1)

Table continues on next page.

Table 11
(Continued)

Research Design	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Pre-Post/ Norm C. Grp.	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	0 (0.0)	1 (1.1)
Natural Var. Self-Select.	0 (0.0)	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	2 (2.2)
Quasi-Exp Solomon 4	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	1 (1.1)
Nonequivalent Panel Study	2 (20.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	2 (2.2)
One Group Panel Study	2 (20.0)	2 (20.0)	0 (0.0)	0 (0.0)	0 (0.0)	5 (33.3)	3 (20.0)	12 (13.3)
Pre-Post Crossovers	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	1 (1.1)
Total	10 (100)	10 (100)	10 (100)	15 (100)	15 (100)	15 (100)	15 (100)	90 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 12

Number of Studies by
Method of Selection and Study Category

Method of Selection	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	0	0	1	0	0	1	0	2
Autonomous Presentation	0 (0.0)	0 (0.0)	1 (11.1)	1 (6.7)	3 (20.0)	0 (0.0)	1 (6.7)	6 (6.8)
Response to Advertisement	0 (0.0)	2 (20.0)	1 (11.1)	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	3 (3.3)
Solicited by Researchers	1 (10.0)	1 (10.0)	0 (0.0)	7 (46.7)	4 (26.7)	3 (21.4)	0 (0.0)	16 (18.2)
Committed/ Required	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (6.7)	0 (0.0)	1 (6.7)	2 (2.3)
Referred	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	2 (13.3)	2 (14.3)	4 (26.7)	8 (9.1)
Institutional Group	7 (70.0)	4 (40.0)	2 (22.2)	1 (6.7)	2 (13.3)	0 (0.0)	2 (13.3)	18 (20.4)
Met Criteria	2 (20.0)	3 (30.0)	5 (55.6)	6 (40.0)	3 (20.0)	9 (64.3)	7 (46.7)	35 (39.8)
Total	10 (100)	10 (100)	9 (100)	15 (100)	15 (100)	14 (100)	15 (100)	88 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 13
 Number of Studies by
 Method of Assignment and Study Category

Method of Assignment	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health Health	Mental Health	Social Serv.	Crim Just.	
Missing	1	3	1	2	2	7	3	19
Random Individuals	0 (0.0)	3 (42.9)	6 (66.7)	6 (46.2)	6 (46.2)	4 (50.0)	8 (66.7)	33 (46.5)
Matching Nat. Groups	1 (11.1)	0 (0.0)	0 (0.0)	1 (7.7)	2 (15.4)	0 (0.0)	0 (0.0)	4 (5.6)
Natural Groups	5 (55.6)	2 (28.6)	2 (22.2)	1 (7.7)	4 (30.8)	2 (25.0)	3 (25.0)	19 (26.8)
Self-Selection	1 (11.1)	2 (28.6)	1 (11.1)	0 (0.0)	0 (0.0)	1 (12.5)	1 (8.3)	6 (8.4)
Random Groups	2 (22.2)	0 (0.0)	0 (0.0)	3 (23.1)	0 (0.0)	0 (0.0)	0 (0.0)	5 (7.0)
Time Rule/ Trickle	0 (0.0)	0 (0.0)	0 (0.0)	0 (0.0)	1 (7.7)	0 (0.0)	0 (0.0)	1 (1.4)
Mixed	0 (0.0)	0 (0.0)	0 (0.0)	2 (15.4)	0 (0.0)	1 (12.5)	0 (0.0)	3 (4.2)
Total	9 (100)	7 (100)	9 (100)	13 (100)	13 (100)	8 (100)	12 (100)	71 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 14

Number of Studies by
Length of Study and Study Category

Study Length	Study Category							Total
	Elem Educ	Sec/Hi Educ	Train- ing	Health	Mental Health	Social Serv.	Crim Just.	
Missing	0	1	0	1	2	3	1	8
<= 6 Weeks	0 (0.0)	1 (11.1)	1 (10.0)	0 (0.0)	0 (0.0)	0 (0.0)	2 (14.3)	4 (4.9)
7 Weeks to 6 Months	0 (0.0)	1 (11.1)	1 (10.0)	5 (35.7)	2 (15.4)	4 (33.3)	2 (14.3)	15 (18.3)
> 6 Months to 1 Year	2 (20.0)	4 (44.4)	1 (10.0)	6 (42.9)	7 (53.8)	1 (8.3)	5 (35.7)	26 (31.7)
> 1 Year to 2 Years	2 (20.0)	1 (11.1)	7 (70.0)	0 (0.0)	2 (15.4)	2 (16.7)	5 (35.7)	19 (23.2)
> 2 Years to 4 Years	4 (40.0)	2 (22.2)	0 (0.0)	2 (14.3)	2 (15.4)	3 (25.0)	0 (0.0)	13 (15.8)
> 4 Years	2 (20.0)	0 (0.0)	0 (0.0)	1 (7.1)	0 (0.0)	2 (16.7)	0 (0.0)	5 (6.1)
Total	10 (100)	9 (100)	10 (100)	14 (100)	13 (100)	12 (100)	14 (100)	82 (100)

Note. Numbers in parentheses indicate the percent of studies with nonmissing data.

Table 15

Descriptive Statistics for
Length of Study by Study Category

Study Length	Study Category							Total (N=82)
	Elem Educ (N=10)	Sec/Hi Educ (N=9)	Train- ing (N=10)	Health (N=14)	Mental Health (N=13)	Social Serv. (N=12)	Crim Just. (N=14)	
Mean	147.9	76	61.4	79.1	73.8	125.3	53	86.4
Median	148	52	78	52	52	104	52	52
Standard Deviation	89.3	75.6	30.9	119.1	59.2	103.5	34.2	83.7
Range	31 - 312	6 - 208	6 - 104	11 - 468	13 - 208	13 - 312	3 - 104	3 - 468

Note. Numbers in parentheses represent the number of studies with nonmissing data.

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The dissertation is therefore accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy.

April 15, 1985
Date

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