Tutorial

Problems in the experimental evaluation of social action programs

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INTERNATIONAL ASSOCIATION of OROFACIAL MYOLOGY

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The social action program setting both exacerbates conventional methodological problems and creates entirely new problems alien to the laboratory and indigenous to social program settings. An example of a standard methodological issue which is complicated by the social action setting is that of randomly assigning subjects to treatment conditions. The concept of achieving pre-experimental equation of groups through random assignment is one of the most fundamental concepts of experimental methodology. (Campbell & Stanley, 1966). Even with the possibility of utilizing the multiple time-series design and other quasi-experimental designs which do not require pre-treatment equation through randomized assignment, the researcher in social action programs at one time or another likely will have to come to grips with the difficulties of randomized assignment (Houston, 1972).

Administrators will frequently object to the implementation of an experimental design in a social program which requires the denial of services to some individuals otherwise qualified to receive those services (Aronson and Sherwood, 1967; Freeman and Sherwood, 1967). Campbell (Note 1) noted that program participants assigned to control groups will often themselves react negatively to deprivation of services.

This difficulty of randomly assigning subjects to control and treatment groups has been cited as the paramount, and insuperable, obstacle to implementing experimental assessments of social action programs (Caro, 1971a, 1971b). Since social action programs are usually concerned with providing services to needy target groups, withholding these services from randomly selected individuals in order to experimentally assess the program's impact on those receiving the services has been viewed by many as morally indefensible.

Stanley (1972) suggested that this objection may be circumvented by promising to remediate control group members if experimental participants are found to be appreciably improved at the conclusion of the experiment. Rossi (1972b) took a somewhat different tact. In citing the ethical problems which attend the deliberate withholding of services from eligible applicants, Rossi stated that the requisites of experimental design do not necessarily require that services be withheld from anyone. The primary requirement, according to Rossi, is that the experimental treatment be distinguishably different from any services provided the control group. Rossi (1967) recommended use of the term comparison group, in lieu of control group, to emphasize this point.

Rossi's (1972b) point is compatible with Campbell and Stanley's (1966) recognition that: "comparsion of X with no X is an oversimplification. The comparison is actually with the specific activities of the control group which have filled the time period corresponding to that in which the experimental group receives the X" (p. 13).

Hyman and Wright (1971) emphasized that whereas in the laboratory experiment control subjects may be induced to engage in some meaningless activity for an hour or two hours, this is not often possible in social action program research. Treatments in social programs are usually extended in time. This means that control group subjects may engage in any number of activities, many of them meaningful, and all of which may have an effect on the variables of interest to the investigator. A more reasonable approach, Hyman and Wright felt, is the comparative design where the investigator compares groups that have been exposed to different kinds of programs, or alternatively, to different treatments within the same program. This approach resolves the moral dilemma of witholding treatment from needy eligible applicants.

Program resources, however, are not always sufficient to permit their equitable allocation to everybody wanting them. In settings where the experimental variable represents a scarce commodity, the majority opinion seems to be that eligible applicants should be assigned randomly to treatment and control groups. Campbell (1967, Note 1, Note 2) and Porter (Note 3) expressed belief that random assignment in these situations is eminently fair. Campbell (1967) suggested that volunteers be recruited in excess of the number which can actually be given the treatment. Excess volunteers would be assigned to control groups.

In situations where demand outstrips supply, random assignment to treatment and control groups may be a fair procedure for determining which applicants will receive program treatment. It does have the potential, however, of arousing resentment among those denied services. They may become hostile and refuse to contribute subsequent data measures. They may also react to the denial of services by making a concerted effort to obtain them elsewhere. If this proves to be the case, the act of assignment to a non-treatment control group might prove to have as powerful an effect as treatment group assignment.

Wholey, Scanlan, Duffy, Fukomoto, and Vogt (1970) made a most important distinction regarding the random assignment of participants to treatment and control groups. They pointed out that experimental assessment of on-going service delivery programs is not usually undertaken. Random assignment is a serious issue only in planned variations of service delivery. Since these innovations are experiments, or tests, no one can say whether they will be beneficial until they are field tested. The measurable impact of social programs is usually quite small anyway. "Therefore, the expected value of the information increment by using control groups could presumably be higher than the expected value of the loss of benefits" (Wholey, et. al., 1970, p. 91). Wholey, et al., felt that control groups are a good investment until such time that social programs begin to have a higher and more frequent papoff. If the program proves to be a good one, chances of its being accepted on a large scale and being implemented over the long haul is enhanced if evidence of efficacy is empirical and experimentally reliable. Effective programs would be applied to far more people in the long run, in exchange for depriving a small control group of services for the short run.

Cook and Campbell (1975) noted that random assignment poses no problem in situations where several treatment variations are simultaneously offered and every eligible participant is assigned to some treatment condition. The control group participants may also be given irrelevant treatments not expected to change the dependent variable(s) of interest.

In situations where treatment recources are scarce and all desirous of the treatment cannot be served, program administrators may insist that services be extended to those most critically in need. This would have the effect of confounding selection with treatment. Cook and Campbell (1975) suggested a number of arguments that might dissuade the administrator from his position. First, it is not known whether the innovative treatment is effective. If it were known that the innovative treatment was effective, it would not be necessary to conduct an experiment. The only way to determine the efficacy of the proposed innovation is to conduct a "true" experiment. Offers can be made to provide remedial treatment to control group subjects if the treatment proves effective. Moreover, the offer to remediate control group participants provides replication opportunities or opportunities to test further planned treatment variations.

Campbell (Note 1) suggested that in situations where excess eligible participants exist and administrators insist that the most needy applicants receive the program services, it still may be possible to assess the effect of program treatment through use of the regression-discontinuity analysis (Campbell, 1975; Campbell & Stanley, 1966; Thistlewaite & Campbell, 1960). Utilization of the regression-discontinuity analysis would necessitate quantification of eligibility requirements and establishment of a sharp cutting point.

Randomized invitations

Campbell (1975; Note 2) suggested another procedure that might be adopted in situations where there is a new social program with limited resources. The target (eligible) population would be identified and then a random sample of this population would be invited to participate in the program. In this schema, those eligible but not invited would constitute the control group.

In addition to the potential resentment or hostility engendered among non-invited eligibles, randomized invitations present another serious drawback. In actuality, instead of the intended treatment and control groups, random invitations produce three groups: invited-treated, invited-untreated, and uninvited. If 100 random invitations are extended, for example, and 75 individuals accept the proffered services, this 75% of those originally invited constitutes a biased, self-selected group. What is the appropriate comparison group? If 75 eligibles are randomly selected to provide control data, it may be assumed that 19 of these individuals (25% of 75) would not have accepted the program services had they been proffered. Hence the treatment group, purged of its 25% non-cooperatives, and the control group, logically expected to have 25% non-cooperatives are not comparable.

Campbell (1975) recognized this drawback and suggested that the measures might be pooled for all those invited, regardless of whether or not they accepted. While this has the potential of attenuating treatment effects, it does rule out the plausible alternative hypothesis of confounded selection and treatment. The feasibility of pooling data in this manner would depend a great deal on the acceptance rate of those invited. If 90% or 95% of those invited actually accepted, the procedure might be feasible, whereas if the rate of acceptance was around 50% the pooling procedure would be suspect unless the treatment was immensely powerful.

Campbell (1975) suggested that it might be possible to estimate how much of the difference between treated and uninvited groups could be attributed to selection rather than treatment. Campbell also proposed that it might be possible to divide the uninvited group into those who would have and those who would not have accepted a proffered invitation. The comparison would then be between the treated group and members of the uninvited group classified as those who would have accepted a proffered invitation.

Differential attrition between treatment and control group participants

In laboratory experiments no more difficulty is encountered in obtaining data from control group subjects than from treatment subjects. Both groups are equally, and easily, accessible to the investigator. Due to the fact, however, that treat-

ments in social action programs are usually extended in time (Lerman, 1968), it is often not the case that control subjects are equally accessible. When data is collected from participants receiving different variations in treatment, all participants are equally measurable. But when a true control group is utilized, these members are likely to be less interested in contributing data and more difficult to keep tabs on. In other words, differential attrition can be expected between treatment and control group members.

Campbell (Note 1) suggested that post-experimental equation of control and experimental groups might be accomplished by degrading the percentage of respondents for the experimental groups by using a list of addresses as out of date as that for the controls, even though subsequent address up-datings are available for the experimental subjects. Campbell also suggested that post measures might be collected by an independent agent or research center to whom the experimental subjects owed no special fealty.

Research and service component conflicts

While random assignment of subjects to control and experimental groups and the subsequent problem of differential attrition are standard methodological issues which are exacerbated by the social action setting, the seemingly inevitable conflict between research personnel and service staff is unique to the social service setting. In traditional laboratory work one is not ordinarily required to coordinate one's research efforts with service delivery staff, simply because there is no such staff. Even in a social action program embodying planned, experimental treatment variation, however, where the experimenter has considerable administrative influence, it is often service-oriented, non-research staff who are required to collect data. If data collection requirements are too stringent, the service workers may become resentful of the imposition and even sabotage the research effort (Caro, 1971a, 1971b). This problem has been recognized at least since French (1953). Selltiz, Jahoda, Deutsch, & Cook (1963) recommended that all service personnel be as fully involved as possible in the experiment in order to reduce resistance, and that the researchers should emphasize how the research findings may be of help in improving service delivery.

Mann (1971) suggested that the researcher in a social action program may choose between two tactics for interacting with non-research personnel. The investigator might choose to assiduously cultivate personal relationships with non-research personnel in the hope that even if they resent the research they will cooperate because they like the researcher. This tactic, however, requires a great deal of time and may prevent the investigator from devoting adequate time to his research considerations. Alternatively, the researcher might like to remain as much as possible in the background on the theory that is is difficult to resent a shadowy form. This tactic permits the investigator to devote more time to his research activities.

Campbell (Note 1), Caro (1971a, 1971b), Weiss (1970) and Weiss and Rein (1969, 1970) also addressed the issue of researcher/practitioner conflicts. Caro (1971a, 1971b) gave relatively extended attention to the problem.

Dissemination channels

Once an experiment in a social action program has been conducted, where is it to be reported? Until quite recently there were precious few formal outlets for such studies (Brooks, 1965; Caro, 1971c; Cherns,, 1971), and, when they did appear, they were scattered across journals representing diverse substantive topical interests (Caro, 1971a). This dismal scene is considerably improved today, with appearance of the magnum opus *Handbook* (Struening & Guttentag, 1975), the long-awaited Social Science Research Council Committee edited report (Riecken & Boruch, 1974), and the first volume of the *Evaluation Studies Review Annual* (Glass, 1976). Perhaps of most significance is the recent appearance of the first scholarly periodical devoted to evaluation concerns, *Evaluation Quarterly*.

Glaser and Taylor (1973) suggested that the social action program researcher consider from the very beginning the potential ways that his findings might be subsequently implemented. Glaser and Taylor (1973) suggested that if the research director does not feel that these activities are compatible with his professional, scientific orientation, that he should consider adding to the research team a member who could specialize in dissemination and subsequent implementation of research findings. Glaser and Taylor (1973) insisted that researchers in action settings are obligated to educate themselves regarding the workings of a bureaucracy.

Caro (1971b), Ferman (1969), Mann and Likert (1971), Rossi (1972), and Sadofsky (1966) urged that evaluation reports be tailored to the audience. Selltiz and Cook (1948) urged that research on practical social problems be carried out in a manner conducive to implementing the findings. Selltiz, et al., (1963) warned that delays between data collection and dissemination of the findings will vitiate the perceived relevancy of the report. Selltiz, et al., (1963) also suggested that the audience which utilizes reports on social program research may not consist of scientists. They suggested that thought should be given to the proper format for presenting research results to a non-professional audience.

Weiss (1972) observed that evaluation reports seldom address the information needs of the audience which receives them. Weiss suggested that multiple reports be written, with each report tailored for a more or less specific audience. Weiss (1971) predicted that researchers could take several definitive steps toward increasing the probability that their findings will be implemented subsequently. Reports should: 1) explicate the theoretical premises underlying the program, 2) speci-

fy the "process model" of the program, and 3) concentrate on program components which are associated with success. Additionally, Weiss encouraged the early identification of potential users of the research findings, the involvement of administrators and practitioners in the research effort, preparation of non-technical reports for non-research audiences, and the specification of implications for future action.

To reiterate, there are few recognized dissemination media in the form of journal archives for research conducted in social action programs. Researchers in this area would likely benefit from the preparation of multiple reports of their findings, each tailored to a specific audience. Continuity in research is facilitated by implementation and subsequent testing of research findings and implications.

Other hindrances to conducting experiments in social programs

Caro (1971b) enumerated a number of problems which are likely to be encountered by researchers in social action programs. There may be disagreements about whether the researcher is free to publish and otherwise disseminate his research findings, especially in instances where negative findings are involved. Administrators may insist on interfering with what the social scientist considers critical issues in the design and execution of his research project. The social scientist's objectivity and neutrality may be resented as a "dampening" effect.

Kershaw (1972) identified several "operational" (as opposed to methodological or concentual) problems (p. 221). Among other issues, he addressed the problems of generating and maintaining local support, insuring organizational flexibility, techniques for enrolling participants, and counter-attritional methods that might prove useful.

Cain and Hollister (1972) emphasized that the success of experiments conducted in social action programs is highly dependent on the manner in which a number of organizational problems are handled. Cain and Hollister warned that administrators often resent researchers due to their statistical orientation. Kerlinger (1964) underscored the importance of resolving organizational problems: "A field investigator has to be a socially skilled operator. He should be able to work with people, talk to them, and convince them of the importance and necessity of his work" (p. 385).

Carter (1971) and Evans (1969) admonished social scientists who are invited to conduct research in social programs to scrutinize carefully the motivation of the program directors. Carter suggested that a frequently encountered ulterior motive is the desire to empirically confirm the administrators' preconceptions. Downs (1965) similarly warned that program developers often have highly rigid pre-conceptions about what should be researched and how it should be researched. Downs pointed out that program evaluation or program research could, in addition to its ostensible legitimate function, serve to: 1) settle internal disputes, 2) provide justification for decisions already made, 3) verify findings previously arrived at by an interested party, or 4) defer immediate action.

Argyris (1958), Riecken and Boruch (1975), and Rossi (1972b) warned that research in social action programs is not likely to result in prestige and recognition to the extent that is possible in academic social science. They, along with Freeman (1971), also warned that dependably continuous employment may be a rarity. Rodman and Kolodny (1964) and Williams and Evans (1972) posited an inherent status inconsistency between researchers and practitioners. Rodman and Kolodny, noting that the researcher is neither practitioner, administrator, nor affiliated with any usual program component, observed that his unique marginal position is likely to be misconceived by program personnel, who may see his role as that of a "spy" for higher-echelon administrators. Chein (1966) and Rossi (1972b) observed that social scientists conducting research in social action programs are likely to miss the "peer support" which would otherwise be afforded by colleagues in the academic setting.

Longood and Simmel (1972) and Rein and Miller (1967) observed that one of the most insuperable problems the social action program researcher will encounter is simple institutional inertia, or what Bateman (nd) referred to as the fear of the unknown. Longood and Simmel (1972) asserted that resistance to social innovations is pervasive, powerful and complex, and will be encountered at the cultural, institutional and individual level.

Summary

The researcher who conducts experiments in social action programs can expect to encounter most of the formidable array of problems which burden the laboratory investigator. Several standard methodological concerns, such as pretreatment equation of control and experimental groups through randomized assignment, are exacerbated by the exigencies of the social action program setting. Potential differential attrition of control group versus experimental condition participants is another problem that in one way or another must be resolved.

In addition to standard methodological issues, the field setting of a social action program can be expected to generate an entirely new subset of problems. The literature indicates that one of the most salient of these problems is the seemingly ubiquitous conflict which arises between those conducting the research component of an action program and the service personnel of the agency. Unless careful precautionary measures are taken the service personnel may develop a strong resentment to the detachment and objectivity of the research team. Advice on how to resolve research/service component conflicts may be found in the literature at least since French (1953), who advised the researcher to involve non-research

personnel as closely as possible in all phases of the research endeavor. Caro (1971a, 1971b) was near- Machiavellian in detailing how the researcher might attain power and influence in the social action program in order to assure that his research design is not invalidated by subsequent organizational changes.

The research scientist engaged in research in a social action program also will have to face a new set of priorities regarding publication and dissemination channels. He may have to adopt the strategy of preparing multiple reports on every phase of his research efforts. In order to insure the continuity of his research, he may have to proselytize for subsequent implementation of his findings in other social programs.

At the same time that the researcher in a social action program discovers that the service personnel resent him for what they perceive as objective detachment, he may well discover that his academic colleagues resent him for lack of objectivity. In short, he may find himself a "person in the middle" without substantial peer support of any kind. A short but cogent observation on the meta-methodological status of evaluation work was contributed recently by Sommer (1977), who urged the establishment of programs specifically to train social action program evaluators.

Reference Notes

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