

THE EXPERIMENTAL TESTING OF THE MINORITY FEMALE SINGLE PARENT PROGRAM

Phoebe H. Cottingham and Aida Rodriguez, The Rockefeller Foundation
Phoebe H. Cottingham, The Rockefeller Foundation, 1133 Avenue of the Americas, New York, NY 10036

INTRODUCTION

The purpose of this paper is to offer some lessons about conducting randomized experiments in collaboration with community-based social programs. We draw on operational experience in a six-year action-research setting, the Minority Female Single Parent Program (MFSP).¹ The MFSP asked community-based organizations to create and provide intensive job training, education, child care and counseling to poor minority single mothers. The program has supported services at four community-based organizations (CBOs) since 1982 in four cities.²

The original impact evaluation design for the MFSP program called for a comparison of participant's outcomes with those found for similar groups of women identified through local household surveys. After two years of work under this design, it was decided to change to randomized allocation of applicants to obtain the control group. Thus, we were able to observe a "natural" experiment in that the demonstration projects were asked to comply with introduction of random assignment midstream.

The private, voluntary agencies in the MFSP program are typical of agencies that are oriented toward reaching and helping people from a community focus. Because of their independence, these organizations are potentially useful for testing new innovative approaches before full-blown policy strategies are adopted. Yet, reports of how one implements experimental evaluations of social programs in these organizational settings are rare.³

The first section of this paper describes the research design procedures and rationale set forth by the researchers and the Rockefeller Foundation.

The second section of the paper lays out the lessons learned during the process of implementing random assignment. This includes the adjustments and innovations that developed in the field, as well as some observations on the likely impact of the research requirements on the program.

A brief concluding section summarizes what has been learned.

II. INITIAL PROCEDURES AND RATIONALE

The MFSP Program - The Early History. In 1981, in response to the problems of minority female single parents faced with poverty (increasing "feminization of poverty"), the Rockefeller Foundation decided to launch a demonstration-research program called the Minority Female Single Parent Program. Twelve national and regional community organizations were invited to submit proposals to be demonstration sites for up to five years. In the fall of 1982, six community-based organizations initiated projects offering comprehensive services to low-income minority single mothers. The services included remediation, skill training, child care, counseling and job placement, targeted to help the single mothers obtain economic independence. Each organization

was asked to serve 200 - 250 women annually.

From the beginning the Foundation had multiple objectives. Initially, the primary goal was to spotlight the problems of minority single mothers and the role of CBOs in possible solutions. Added to this objective was learning whether CBO programs could make a difference in the economic position of these mothers and children. Thus a research firm was asked to carry out an independent evaluation and to monitor the program.

The resources devoted initially to the evaluation of the program were modest relative to previous social experiments focused on poverty, such as the National Supported Work Demonstration and Project Redirection conducted by the Manpower Demonstration Research Corporation.⁴ The evaluation, it was thought, would not require the research standards of a randomized experiment, and therefore would be accomplished by allocating approximately one-fifth of the resources to evaluation versus program operations. At the time the program was initiated, large scale evaluations of government funded employment training programs were relying mainly on non-randomized comparison groups that were not constructed through random allocation of applicants for estimating program impacts. Instead, researchers attempted to construct control groups from national longitudinal surveys, based on characteristics of the program participants. Such methods were popular because they avoided the need to draw a control group from program applicants.

Therefore, the research plan adopted in 1982 called for a quasi-experimental impact study design. The outcomes for MFSP program participants in each site would be compared with those observed for matched comparison group samples of women not receiving MFSP services. The comparison group was generated by identifying the catchment area for each project⁵, screening households door-to-door for eligibility, and interviewing eligible women who had not applied to the program.⁶ Baseline and follow-up structured interviews were to be conducted with both program participants and comparison group members.

Additional data for the evaluation was gathered by way of a client-tracking Management Information System (MIS) that records the types and duration of activities and services provided to participants and qualitative observations by on-site researchers.

Change to an Experimental Research Design. In the middle of the second year of program operations, the Foundation reassessed the research design and program progress. This reassessment urged a significant shift in the scale and focus of the endeavor. It concluded that since evaluations based on non-experimental findings were being questioned on a number of grounds, shifting to a randomized design to study impacts should be considered.⁷

Strengthening the impact analysis coincided

with a new consensus that the central objective of the overall program should be to provide findings useful for public policy. This objective became stronger because of the growing uneasiness regarding low-income single mothers and children and evidence that public policy options would fast move to the national and local agendas. We needed to be sure that an evaluation of one of the few active demonstrations targeted at this population would not be flawed by methodological weaknesses.

It was decided to adopt a random assignment design for the impact evaluation at those sites who would agree to this change.⁸ Five months later (in November 1984) random assignment procedures began in four of the original six sites.⁹ The new design required random assignment of approximately 1200 women in each site and a 2:1 treatment-control group sample allocation.¹⁰ This target of 1,200 observations per site would provide an 80 percent probability of detecting an impact on employment and earnings of participants that was the size of the impact observed for AFDC recipients in the National Supported Work Demonstration.

The switch to random assignment did result in no observable differences between our experimental and control groups. What impact random assignment has on the natural flow of applicants to a program, and on experimental and control group behavior after randomization is less certain. We now look at these questions and others by analyzing how implementation of random assignment procedures unfolded in the four community organizations.

III. DOING RANDOM ASSIGNMENT IN CBOs

As soon as we made the decision to change the evaluation design, negotiations began with the CBOs to gain their cooperation. First, the agency directors were told of the Foundation's concern that without the randomization of applicants to gain a control group the findings would be flawed. It would be a waste of resources to let pass an opportunity to really learn if these programs worked. The basic conditions set forth for the new experimental design were as follows:

- a) the research team would be responsible for the actual allocation of applicants to either the experimental or control groups;
- b) women assigned to the control group could not be permitted to enter the program until after the follow-up period (which initially was 24 months and later became 30 months); and
- c) sites would be expected to increase the flow of applicants to provide enough eligible single mothers to fill both the experimental and control groups. To do this, the Foundation would increase its support for the program services by as much as 40 percent.¹¹

The directors received the news of new directions with mixed reactions. Several of the agency directors were already familiar with random assignment procedures, through prior agency experience, or academic training. They all wanted their organizations to continue in the demonstration, and were stimulated by the

challenge of contributing to the knowledge goals. They also recognized the staff might resist intake procedures that "arbitrarily" selected who would get into the program. In differing fashion, each director took the proposition back to their staff, and in some cases, agency boards, for reaction and internal decision. In some cases, additional meetings were held with Foundation officers and researchers. A workshop brought together staff from all four sites with the research team and Foundation staff.

Lesson One: Train and Retrain. Most program staff members are not familiar with the concept nor process of randomization. Obtaining and maintaining the cooperation of the staff, at all levels, requires careful explanation of the reasons for using randomization, as well as ongoing and regular interaction between the program staff, researchers and the funders. Constant flux in staff--as staff responsibilities change and new staff come on board--requires that opportunities be provided for the researchers to directly explain the reason for conducting a randomized experiment. Opportunities could include meetings and/or staff retreats.

All four sites agreed to conduct their programs with the randomized procedures during intake. More intensive discussions and training then followed with CBO staff to work out the details. Random assignment was in operation two months after training began. This was possible in large part because the research team could apply prior knowledge about random assignment procedures.

We learned, however, that interaction between the program staff and the researchers had to be maintained to successfully conduct random assignment. New questions arose as the CBO staff gained experience. By raising questions, the CBO staff contributed to the development of the research method. Many of these questions concerned the placement of the randomization during intake of program applicants.

Lesson Two: Integrate random assignment into the intake process. Deciding when during the intake process randomization is scheduled has to be discussed and negotiated with program staff members. Randomization schemes may have to vary by site to accommodate project needs. For impact evaluations, timing random assignment to exclude pre-program attrition (the self-selection by applicants) is furthered when staff accept the research goals.

The primary operational decision from the research perspective was selecting the optimal time in each agency's intake process when applicants would be separated into controls and experimentals. Since the objective was to evaluate the impact of the program on those participating in it, we needed to exclude applicants who would drop-out and not engage in program activities. Operationally this is nearly impossible. One would have to draw the control group on the first day of actual program activity. Therefore, we sought to identify a random assignment cut point that was one step back, when applicants were deemed eligible and interested but not yet active. These random assignment "cut" points imply that some appli-

cants assigned to the program will be "no-shows". If the "no-show" rates becomes excessive, it damages the ability to detect significant impacts unless the sample size is increased.

Attrition or dropping out of a program is a natural phenomenon, however, as individuals sort out their attachments to the program and other options. How much dropping out is "average" depends on the target population's fit with the program's focus. CBO staff understood that reducing attrition after random assignment would improve the impact findings. And they increasingly understood that pushing as much of the "natural" attrition before random assignment would improve impact findings. This understanding motivated staff to search for ways to stave off random assignment to a relatively late stage in the application process.

Sometimes this was achieved by requiring applicants to show up a second or third time before they are declared eligible for the program. This also gives time for the program staff to assess applicants' needs for education, training, child care, and other supports. Significant numbers of applicants do not return to the program after the initial orientation session. Estimates of the attrition rate before random assignment range from 50-70 percent of applicants. For example, a site may have 200 women attend an initial orientation session, but only 50-75 will return later to complete the application procedures.¹²

There is another factor in the motivational structure of line staff that works in favor of fitting random assignment comfortably to a program. Their profession is a management function (of "managing people") and this means control. Thus although staff may philosophically disagree with the idea of random assignment, they will, when it is mandated to do so for research purposes, be creative in finding ways to bring the random process into their management process, that is, under their control.

Creative merger of random assignment with internal management can be illustrated. Staff quickly learned that because random assignment required that they take everybody who was randomly assigned to the program, it would be better to have that assignment take place at a time when they could better match applicant interests with available training opportunities. The training slots in specific occupations were in high demand and some were more attractive than others to the clients. If a pool of applicants were put through random assignment without any matching by the person's interest and availability of openings, the training desired by an applicant might not be available, if and when, an applicant is selected as an experimental. In one site, staff resolved this problem by requesting that applicants be paired for randomization according to their interests and abilities for every opening. Random assignment by the research firm selected which of the two suitable applicants would be assigned to that opening.

Lesson Three: Inform applicants about randomization as part of program orientation. There are various options for how to explain random assignment to program applicants. Staff needs time to determine what approach works best for

their environment/staff--group explanation versus one-to-one; full description of "why random assignment" versus "research requirement". How applicants should be told about random assignment is a management issue.

The timing of random assignment relatively late in the application process forces significant contact between all applicants and the program staff before either knows who will be assigned to the program participation group. Therefore, opportunities to explain random assignment to applicants are multiple.

Initially some sites asked that the research contractor do the face-to-face explanation with applicants. Early experience changed the site perspective. CBOs elected to integrate their own explanation about the random assignment procedures into their orientation activities. Some preferred to downplay random assignment, and simply make it part of their "eligibility determination" process. Others wanted to have the chance to explain random assignment in a manner they thought would help the client understand the procedure. Either way, CBO staff retain "control" over the intake process by integrating random assignment into their operations.

All the sites agreed, however, that those applicants assigned to the control group would be promptly notified off-site by the research firm (those who were assigned to the control group were sent a \$10 dollar money order). Those assigned to the program were called by the CBO staff so that they could personally encourage them to join the program.

Despite early agreement on respective roles for the CBO staff and the researchers in implementing random assignment, how to explain random assignment was a continuing issue. The researchers argue, quite rationally, that since there are so few program opportunities relative to the population in need, the program operators should explain that the "lottery", needed for research purposes, was also a fair procedure for allocating limited and valuable program opportunities among the numerous women who are equally eligible for the program. Senior managers, CBO directors, and board members, readily acknowledged that the universe of need far exceeds program services available. We found that line staff, however, had much more difficulty acknowledging that resources are limited or utilizing this fact in their approach with clients. Several factors may explain this resistance.

Staff, first of all, are in direct contact with needy clients. They believe their job is to try to help or offer something positive to everyone. They see a challenge before them, of breaking alienation, resistance, lack of hope. Part of the problem is that the target population is not easily attracted to services, even though the services are "good". Therefore, the staff's job is to give encouragement. This conflicts with their having to tell the newly motivated applicant that a "computer" will randomly choose who gets into the program.

In addition, CBO staff members are confident that the program works or should work. No damage is possible, or resources wasted, in their minds. Therefore, the services ought to be provided to everyone. CBO staff are part of

the treatment. Their confidence determines the quality of the treatment.

A controlled experiment seeks to produce reliable evidence on whether, indeed, the program works. The ethic of producing evidence to protect individuals from worthless programs is inevitably pitted against an important factor in a worthwhile program, confidence and belief in what one is doing. To acknowledge that a program may not be successful interferes with established orientation and recruitment practices.

Lesson Four: Monitoring to reduce "violation" of random assignment and to maintain quality control. It is important to anticipate ways that program staff may --even unconsciously-- subvert random assignment. This entails 1) carefully monitoring of randomization to insure that those applicants previously assigned to the control group are not randomized a second time, possibly to the experimental group; 2) frequent communication with intake staff to try to insure that applicants "favored" by staff are not routed away from randomization and enrolled in a similar program at the same agency; 3) careful documentation of all possible entry points into experimental program and other programs in the agency; and 4) regular checking of those being served in the program to insure that controls are not receiving program services.

To protect the integrity of the random assignment process the research staff responsible for randomization must monitor carefully the process of randomization and have frequent contact with program staff. It is necessary to monitor who is being randomized and who is being served by the program to avoid repeat randomization of applicants and to avoid serving controls.

In addition, random assignment for entry into a special program in an agency that offers similar services in other units presents special problems. In all of the MFSP sites, there were other services being provided by the CBO with other funds. None were truly comparable with the MFSP program, which offered a fuller range of services to the MFSP clients in child care, counseling, special support groups, etc. But in some respects, especially in training opportunities, there were parallel program services. The random assignment program then has to compete with other agency programs. Staff may subvert the random experiment by referring selected individuals to services that have no randomized allocation for entry because they want to help them get some assistance.

While it is almost impossible for researchers to track all applicants and enrollees to other agency programs, regular contact with intake staff will allow early identification of "detouring" of applicants away from the randomized program. When the program under random assignment is the "most desirable" program, the random assignment gate is more protected from such "raids". Further, as long as the CBO has a surplus of applicants for other program services as well as the experimental program, competitive tensions can be minimized, and randomization seen as a justifiable condition.

Lesson Five: Staff expectations of client and referral agency reactions to random assignment may not reflect reality. Client reaction to

random assignment was not uniform nor negative as anticipated by some program staff members. Clients focused more on "how can I get in" than on the fairness of entry rules. In addition, there was no evidence that once referral agencies understood the "why and how" of randomization they reduced their referrals to the program.

A major concern expressed by program operators when the Foundation proposed using random assignment was possible negative impacts on the flow of applicants to the MFSP program. The program staff feared that with the introduction of random assignment, potential applicants would be scared away from even applying to the program. Some thought that women knowing that their chances for getting admitted to the program were limited would rather not apply than "face another failure [rejection]".

It is important to try to know the impact of random assignment on not only the staff of the program, but on the clients who are attracted to the population. Indeed, staff attitudes reflect their assumptions about applicant responses to random assignment.

We have some evidence regarding client reaction, mainly from a telephone attitude survey conducted by independent researchers with approximately 50 women who had just gone through random assignment and a limited number of videotaped reactions, both during random assignment and ex-post.¹³

The survey revealed that less than half of the women recall that random assignment was part of the intake process and even fewer can remember or were aware of details, such as the odds. In the telephone survey, respondents were also asked to rank various selection criteria for "fairness". The respondents thought testing was most fair, followed by the random assignment lottery, and last by "first come, first served".

Similar conclusions emerge from the videotapes. Applicants confuse random assignment with testing which most view as legitimate and fair. Random assignment was perceived as a benign computerized system for determining eligibility. Accustomed to bureaucratic procedures, random assignment just added another layer to the mix of AFDC, Food Stamp, Child Care, and other entitlement systems that have confusing regulations about who is eligible and who is not.

A few women had more focused reactions. "An individual's level of effort should count," said one applicant, noting correctly that random assignment "doesn't take account of the amount of effort [it took for me to get here]". Overall, applicants' concerns were much more focused on "how can I get in?" than the fairness of rules.

Program staff on the other hand, had concerns that referral agencies would cut back on sending women to the MFSP program. Evidence that these agencies truly play a strong role in the applicant flow or that this was a major factor did not emerge. With or without random assignment, direct recruitment activities were the dominant source of new applicants rather than agency referrals. Thus, staff responsible for direct outreach must be able to interpret the random assignment procedure as part of their explanations of program eligibility deter-

mination.

Lesson Six: The overall Impact of random assignment on applicant flow is not drastic. Preliminary evidence indicates that random assignment did not have a consistent negative impact on the number of applicants to the program as some expected. There was some stabilization (less variation) in the monthly applicant flow after random assignment, possibly as a result of more careful selection of applicants for the program after the implementation of random assignment. In addition, we found that some control can be exerted over the flow of applicants by using active recruitment techniques.

The implementation of random assignment could have an impact on the flow of applicants to a program for several reasons. As already noted, applicants may be reluctant to risk the "lottery", especially if they are uncertain about the "payoff" of the program for them. Program operators may also become more selective in who they recruit because there is more careful monitoring of program accomplishments and attention to attrition by both the funding agency and the research team. Our sites were urged, of course, to expand recruitment to increase the flow of applicants to accommodate the control group targets. Understanding the applicant flow for a program, the determining factors that are under the control of program operators and those that are in the external environment, remains one of the less researched areas of program evaluation.

We have attempted to track changes in the flow of applicants to the program after random assignment using MIS program data.¹⁴ Of course, we had hoped that the sites could increase the flow of applicants to compensate for the number of potential program participants lost to the control group. That is, even if the applicant flow was the same as before random assignment, a doubling of the flow was needed under random assignment to end up with a the equivalent number of program participants. In only one site was this objective met. In the Washington DC site (WOW), there was a doubling of applicants after the implementation of random assignment. Application flows are summarized in Table 1 and graphed in Exhibit 1. The fact that only WOW was able to double the applicant flow after random assignment, in spite of the fact that additional resources were given to all four sites to help the program in the random assignment, is instructive. WOW used the funds for a stronger recruitment strategy which resulted in substantially higher numbers of applicants than was the case prior to random assignment. It seems that some CBOS can, in spite of periodic staff changes, make a significant impact on the stream of applicants by dedicating staff time and energy to recruitment.

At the other sites, transitory declines after random assignment seem to be the norm. At OIC, for example, there was a steady decline in the number of applicants starting before random assignment. In this case, it is impossible to tell whether, in fact, random assignment facilitated the decline in applicant flow or whether the decline was due to a process that was already in place. And in more recent experience (not in table), this site has

rebounded with an increase in applications.

At the two other sites, AUL and CET, there was a marked decrease in applicant flow right after implementation of random assignment but one year later applicant levels were on the way up reaching near pre-random assignment levels. The difficulty in doubling the applicant flow after random assignment at three of the four CBOs did necessitate extending the period of sample accumulation by an additional year. As it turned out, the site that did manage to double its applicant flow, WOW, had difficulty absorbing the increase, and suffered higher than anticipated attrition rates in the experimental group requiring extended sample intake to redress the loss.

In the three sites that showed more modest fluctuations in their applicant flow, with initially some declines followed by periods of increase, the general picture is one of overall stabilization after random assignment. There was reduction of the variance of monthly applicant flows after random assignment. This decrease may be partially the result of more careful selection of applicants after random assignment because of greater concern with and monitoring of attrition rates. Certainly, greater stability emerges as the programs matured, but it is unrealistic to ever expect a "steady state" in social programs.

CONCLUDING COMMENTS

We have suggested a number of factors that impinge, both negatively and positively, on doing random assignment procedures in voluntary, community-based organizations. Overall, it is entirely possible to carry out these procedures without serious distortion of the program.

The greatest difficulties are the human/management factors, especially staff adjustments to a change in how they do part of their job. Helping program staff understand the larger societal objectives that may be served by procedures that they initially see as foreign to their objectives is important, as well as giving them a role in fine-tuning the procedures.

Given conditions urging a social experiment¹⁵, implementing random assignment in suitable experimental settings should focus on these priorities:

- 1) Spend as much time as possible on training/explaining as on "doing".
- 2) Work with those doing the treatment to select and adapt the random assignment procedures to their client flow.
- 3) Encourage program staff to integrate explanations of the research goals and procedures into their outreach and orientation activities. Expect local variation in how this gets done.
- 4) Monitor the experimental program within the total agency or organization context.
- 5) There are payoffs from using research methods to pinpoint reactions to and understanding of random assignment, especially to take corrective actions with staff or reduce anxiety about external attitudes.
- 6) Expect instability in applicants flows, as well as in program operations, and encourage aggressive recruitment to counter the inherent instability.

TABLE 1

Average Number of Applicants Per Month to the Minority Female Single Parent Program during the Quasi-experimental and Experimental Phase, All Sites*

Site	<u>Quasi-experimental Phase</u>		<u>Experimental Phase</u>		<u>Total</u>
	Oct. 1982- Aug. 1983 (11 months)	Sept. 1983- Oct. 1984 (13 months)	Nov. 1984- Aug. 1985 (10 months)	Sept. 1985- Aug. 1986 (12 months)	
Atlanta, GA.	33.7 (371)	51.2 (665)	28.2 (282)	34.1 (409)	1727
San Jose, CA.	37.6 (414)	34.8 (452)	25.6 (256)	26.5 (318)	1440
Providence, RI.	38.8 (427)	30.7 (399)	25.2 (252)	19.4 (233)	1311
Washington, DC.	33.7 (370)	24.0 (313)	58.7 (587)	64.3 (771)	2041
TOTAL	1582	1829	1377	1731	6519

* Information in this table from the Management Information System applicant files. Sample sizes in parentheses.

1. The Minority Female Single Parent Program (MFSP) is funded and coordinated by the Rockefeller Foundation. The authors have been the Foundation staff directly responsible for the coordination of the program. Impact and cost studies are being conducted by Mathematica Policy Research, Inc. (MPR). See John Burghardt, Stuart Kerachsky, Craig Thornton, and Alan Hershey, "Report on the Status of the MFSP Evaluation" forthcoming, for description of the research. The final report will not be available until 1991. Process analyses are also forthcoming by researchers at the Social Process Research Institute at the University of California, Santa Barbara, and the Center for Social and Behavioral Statistics at Northwestern University.

ENDNOTES

1. The Minority Female Single Parent Program (MFSP) is funded and coordinated by the Rockefeller Foundation. The authors have been the Foundation staff directly responsible for the coordination of the program. Impact and cost studies are being conducted by Mathematica Policy Research, Inc. (MPR). See John Burghardt, Stuart Kerachsky, Craig Thornton, and Alan Hershey, "Report on the Status of the MFSP Evaluation" forthcoming, for description of the research. The final report will not be available until 1991. Process analyses are also forthcoming by researchers at the Social Process Research Institute at the University of California, Santa Barbara, and the Center for Social and Behavioral Statistics at Northwestern University.
2. The CBOs participating in the full scale evaluation of the program are Opportunities Industrialization Center (OIC), Providence, Rhode Island; Wider Opportunities for Women (WOW), Washington, DC; Atlanta Urban League (AUL), Atlanta, Georgia; Center for Employment Training (CET), San Jose, California.
3. See H.W. Riecken, et.al., Social Experimentation: A Method for Planning and Evaluating Social Programs, Academic, New York, 1974, in which the lack of documentation is noted.
4. See Judith M. Gueron, "The Demonstration of State Work-Welfare Initiatives," in New Directions for Program Evaluation, vol. 28, San Francisco: Jossey-Bass, 1985, pp. 5-14. _
5. To generate the comparison group, zip code information from program applicants and enrollees was translated into Census tracts. Racial and ethnic data was used to determine the ethnic breakdown of applicants and enrollees and the information used to select sample blocks within census tracts that would result in high proportions of eligible non-applicants. From the pool of eligible blocks a random sample was drawn and a baseline survey administered to all women within the sample blocks who were screened as eligible.
6. For the site in Providence, Rhode Island, the local universe was too small to support a household survey inside the catchment area. New Haven, Connecticut, was used for the selection of the comparison group.
7. The National Academy of Sciences had just completed a review of the research sponsored by the federal government on youth employment programs that pointed up the uncertainty of research findings as a result of the failure of much of this research to draw control groups for impact analysis (Betsey, Hollister, and Papageorgiou, 1985). Conflicting estimates of impacts in studies using comparison group methods were also emerging, making it impossible to draw conclusions (now published in an article by Barnow, 1987). Fresh acceptance of the need to obtain true control groups was stimulated by studies demonstrating the inability to mimic controlled experimental results with non-experimental data (Fraker, Maynard and Nelson, 1984).
8. This decision required deciding a number of other issues, such as whether to invest in any follow-up data collection for the quasi-experimental sample, and whether to pool such findings with the experimental sample. It was decided to exclude the quasi-experimental impact data base.
9. By the time of the change in research design, one of the six sites had been dropped from the program and a second site was going through major program reorganization and thus was not chosen for the impact evaluation.
10. By September 1985, a 1:1 ratio had been adopted in three of the four program sites, reducing the overall sample target by about 10 percent.
11. Throughout the six years of the program, the sites have been required to match the basic program grant, mainly by tapping training and child care funds. The Women's Bureau, U.S. Department of Labor, has been a major funder of child care services at the MFSP sites.
12. Far more research is needed that helps policy and program planners know why the target population varies in its interest in a program designed to help them.
13. The telephone attitude survey was part of an independent study of the impact of the random assignment procedures. See Robert Boruch, Michael Dennis, and Kim Greer, "Improving Randomized Field Experiments: A Summary of Lessons from the Rockefeller Foundation's Minority Female Single Parent Experiment," May 1987, report prepared at the Center for Probability and Statistics, College of Arts and Sciences, Northwestern University.
14. The applicants filled out "application forms" at some point in the recruitment /intake process before random assignment and the program staff submitted these forms on a monthly basis to the research firm.
15. Some conditions that need to exist before evaluating a policy or program by experimental procedures are: (1) a problem exists that has significant human costs to the individuals involved and to society; (2) some evidence exists that there are alternative approaches that may more successfully address the problem than existing regimens; (3) knowledge from the experiment can be applied to change programs or policy. Robert Boruch has discussed similar conditions in a recent paper (Boruch, 1987).

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