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DIGEST OF THE SOCIAL
EXPERIMENTS

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IRP Special Report

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INTRODUCTION

Since the late 1960s, a large number of randomized social experiments have been conducted in the United States in order to evaluate proposed changes in various social programs. Some of these social experiments have been large and highly publicized--among them the Seattle-Denver Income Maintenance Experiment and the National Supported Work Demonstration--while others have been small and obscure. Some have "pilot tested" major innovations in social policy, others have evaluated incremental changes in existing programs. Most have evaluated policies targeted at population groups that are disadvantaged, notably the poor, the unemployed, and the handicapped.

This Special Report contains brief summaries of the 63 known social experiments that have been conducted in the United States. Each summary, typically three to four pages long and presented in a standardized format, provides information on the policies tested, outcomes of interest, evaluation design, major findings, costs, sample sizes, important methodological limitations, and problems encountered, as well as on other pertinent topics.

In preparing these summaries, we defined "social experiment" relatively narrowly, focusing on field studies of social programs in the United States in which there was a random assignment of individuals or households to alternative treatments and an emphasis on the measurement of impacts on either market behavior, the receipt of earnings, or transfer payments. Therefore, we have not prepared summaries of quasi-experimental field studies, social experiments conducted in other countries, or the large number of randomized field experiments that have focused on outcomes other than those listed above--for example, recidivism among recovering drug addicts and ex-convicts, changes in the health status of persons receiving alternative medical treatments, and improvements in standardized test scores among students. Moreover, the summaries cover only those social experiments that were completed by August 1990. (A table containing information on 28 experiments that were still in progress at that time, but which otherwise met our criteria, is provided in an appendix to this report.)

To develop a list of social experiments meeting our definition, we began with an existing list of 37 compiled by Greenberg and Robins.[1] We learned of a few others from a lengthy bibliography of randomized field experiments compiled by Boruch, McSweeny, and Soderstrom.[2] Names of a number of additional experiments were obtained in responses to letters we mailed to academics and employees of social science research firms who have been prominently involved in evaluations of social programs. Finally, we employed "snowballing" techniques: when interviewing persons who were associated with experiments with which we were already familiar, we asked if they knew of any others. We also learned of new experiments while reading reports on those already familiar to us. Although we have probably overlooked a few of the smaller social experiments that meet our criteria, we are confident most will find our list useful.

The information contained in the summaries of the individual experiments was obtained from two major sources. First, we reviewed at least one research report (and sometimes several) on each experiment. Second, information not available from the research reports was obtained from telephone interviews with staff members of the organizations that conducted the evaluations and the government agencies that sponsored them. Often one interview was sufficient, but sometimes several telephone calls were necessary.

Each summary contained in this Special Report has been assigned to one of seven categories. Within each category, the summaries appear in chronological order as determined by the date at which the treatments tested were first administered. The seven categories we use are based on the populations at which the experimental treatments were targeted:

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[1]David H. Greenberg and Philip K. Robins, "The Changing Role of Social Experiments in Policy Analysis," Journal of Policy Analysis and Management, Vol. 5, Winter 1986, pp. 340-362.

[2]Robert F. Boruch, A. John McSweeny, and E. Jon Soderstrom, "Randomized Field Experiments for Program Planning, Development, and Evaluation: An Illustrative Bibliography," Evaluation Quarterly, Vol. 2, November 1978, pp. 655-695.

-     The Low-Income Population

-     The Unemployed

-     Disadvantaged Youth

-     The Mentally Impaired

-     Persons Charged with or Convicted of Crimes

-     Substance Abusers

-     Multiple Target Groups

There are obvious overlaps among the categories. For example, some unemployed persons receive welfare benefits and some disadvantaged youths are unemployed. However, since most of the social experiments are clearly targeted at a distinct population, the categories to which they should be assigned were usually obvious. A few, relatively large, social experiments have been directed toward multiple groups. For example, the AFDC Homemaker-Home Health Aide Demonstrations was targeted at both the welfare population (more specifically, AFDC recipients) and the elderly and disabled. Such experiments were assigned to the Multiple Target Groups category.

Almost half the summaries presented in this report describe social experiments dealing with the welfare population, especially recipients of AFDC. The policies tested for this population were quite diverse, ranging from alternative schemes for providing transfers to various types of welfare-to-work programs. The other two major target populations in terms of numbers of experiments are the unemployed and disadvantaged youths. Interestingly, all the experiments directed at disadvantaged youths were initiated between the summer of 1978 and January 1981, less than a three-year span.

Most of the informational items appearing in the summaries are self-explanatory, but a few require a brief comment.

Information source(s) on experiment. Each summary indicates at least one source that provides more detailed, written information on the experiment being described, usually a final report. In some cases, the report has been published in a journal or book, but more typically it can only be obtained from the research firm that conducted the evaluation or the government agency that sponsored it.

Enabling legislation. Federal and sometimes state legislation has often mandated that social programs, especially those being tried on a pilot basis, be evaluated. Although such legislation has rarely specified that the evaluations be based on randomized experiments, some of the experiments that we reviewed were initiated as a result of legislative mandates for evaluations. Almost all social experiments targeted at disadvantaged youths, for example, resulted from the Youth Employment and Demonstration Projects Act of 1977.

Cost of experiment. Of all the information about social experiments that we attempted to collect, cost data were the most difficult for a number of reasons. Sometimes the information once existed, but the necessary records could no longer be located. When an experiment was administered by an existing government agency (which has increasingly become the case), it was often difficult to separate the incremental cost of administering the experimental treatment from other costs incurred by the agency. Sometimes the total cost was available, but administrative costs could not be separated from the cost of the evaluation.

Policy deliberations pertaining to treatment. The information under this heading is typically based on the opinions of those interviewed on whether the results of the experiments had any influence on policy. Since the utilization of research findings in formulating policy, if it occurs at all, is typically quite subtle and difficult to detect, opinions on this topic should be treated with some skepticism.

Number of treatment groups. The number of treatment groups always includes groups used for control purposes.

Major findings. The information on findings reported in the summaries was typically obtained from the final reports we reviewed. For a few experiments, alternative sets of findings have been produced by a different methodological approach than that used in the final report. We have ignored such findings. In addition, there are some experiments for which a large volume of results exists. To keep the summaries brief and the amount of reported numbers manageable, we have concentrated on those findings that pertain as directly as possible to the major experimental outcomes of interest.

DIGEST OF THE SOCIAL EXPERIMENTS:

I. The Low-Income Population

NEW JERSEY INCOME MAINTENANCE EXPERIMENT

Information sources: David N. Kershaw (deceased) and Jerilyn Fair, The New Jersey Income-Maintenance Experiment, Volume 1: Operations, Surveys and Administration; Harold Watts and Albert W. Rees, editors, Volume 2: Labor-Supply Responses, Volume 3: Expenditures, Health, and Social Behavior, and The Quality of the Evidence. Academic Press, 1976, 1977.

Funding source: Office of Economic Opportunity. Key personnel: James Lyday and Robert Levine.

Treatment administrator: Mathematica Policy Research. Key personnel: David N. Kershaw (deceased).

Evaluator: Institute for Research on Poverty, University of Wisconsin-Madison, and Mathematica Policy Research. Key personnel: Harold Watts.

Enabling legislation: None. Did require an Internal Revenue Service ruling that program payments were not taxable income.

Total cost: $7.8 million (1971); research and administrative costs only: $5.4 million.

Dates: August 1968-September 1972; data collected over same period; final report December 1973 (to Dept. of Health, Education, and Welfare).

Dissemination effort:

Briefings: Continuous briefings of OEO officials apparent from Appendix A, volume 1.

Testimony: By Watts and Kershaw to the House Ways and Means Committee, Dec. 1969, and by Kershaw to the Senate Finance Committee, August 1970.

Distribution of executive summaries: To Dept. of Health, Education, and Welfare.

Other: Controversial early report on preliminary data issued February 1970.

Policy deliberations pertaining to treatment: Nixon administration's proposed Family Assistance Plan was related in concept to this experiment. Preliminary data report noted above was prepared for hearings on this proposal. This early report showed the negative income tax increasing work effort. Dennis Coyle and Aaron Wildavsky, "Social Experimentation in the Face of Formidable Fables," in Lessons from the Income Maintenance Experiments, edited by Alicia H. Munnell, Federal Reserve Bank of Boston and Brookings Institution, 1987, state that this finding was cited by NIT supporters such as Senator Fred Harris and Ways and Means Chairman Wilbur Mills.

Location of treatment sites: Trenton, Jersey City, and Paterson, New Jersey, and Scranton, Pennsylvania.

Number of treatment groups: Nine (with one control group).

Treatment tested: The negative income tax consists of an income guarantee accompanied by a tax rate on other income. Eight combinations of guarantees and tax rates (partial reductions in payments as other income rises) were tested: (1) 50% (of poverty line) guarantee, 30% tax rate (on earnings); (2) 50% guarantee, 50% tax rate; (3) 75% guarantee, 30% tax rate; (4) 75% guarantee, 50% tax rate; (5) 75% guarantee, 70% tax rate; (6) 100% guarantee, 50% tax rate; (7) 100% guarantee, 70% tax rate; and (8) 125% guarantee, 50% tax rate. All families were paid for participating in interviews, and controls received a small monthly fee for mailing in a postcard with current address.

Sample size: By payment group: (1) 46; (2) 76; (3) 100; (4) 117; (5) 85; (6) 77; (7) 86; and (8) 138.   Total experimentals: 725; controls: 632.

Target population: Households having one nondisabled male between 18 and 59 years old, at least one other member, and a total family income not exceeding 150 percent of the poverty line.

Outcomes of interest: (1) Reduction in work effort. (2) Lifestyle changes.

Research components:

Process analysis: Two important questions considered (volume 2, chapter 11 and volume 3, chapter 12) were the extent to which experimentals understood program parameters and the extent to which state welfare changes contaminated the results. In January 1969, New Jersey instituted an AFDC plan allowing benefits to two-parent families (AFDC-UP), and until July 1971 these benefits were among the highest in the country. Thus differences between controls and experimentals did not have the same meaning that they were expected to have; low-guarantee experimentals found AFDC-UP offered higher payments.

Impact analysis: Conducted by regression.

Benefit-cost analysis: Not conducted.

Major findings:

1. Average nominal payments rose 6.4% over three years, but real payments went down because the cost of living rose between 11 and 17%. Unemployment also rose during this period, from 4.4% to 7.1% (weighted average).

2. Hours of employment reduction for male family heads was not statistically significant. However, a significant experimental elasticity was calculated; the experimental variable is defined as the ratio of the guarantee to the net wage. At the experimental mean, the regression results imply a reduction of 1 to 1.6% in hours worked between experimentals and controls.

3. Experimental wives worked 23% fewer hours per week than controls; differences in labor-force participation were highly significant. Differences are concentrated in white families. However, the reduction is from a fairly small base, as large families with nonworking wives dominated the sample.

4. Teenagers enrolled in the treatment were (for the medium-generosity plans) 25 to 50% more likely to complete high school than controls, other things being equal. Specifically, the

higher the tax rate, the more likely was high-school completion; but the higher the guarantee rate, the less likely was high-school completion. Experiment participation was associated with lower teen earnings.

5. Observed lifestyle changes were mostly negligible. Some increase in the ownership of both homes and major appliances by experimentals over controls was noted.

Time trends in findings: Findings reported are typically for the middle period of the experiment, because of the learning curve expected in the early quarters and the possibility of gaming behavior in the later quarters.

Problems and issues:

1. Although assignments to treatment were random, given pretreatment income, they were not independent of income. Sample designs resulted in experimentals with very low incomes being directed mainly into low-guarantee programs or into the control group. Families at 100% of the poverty line or less are overrepresented in the control group.

2. All authors admit that the long-term labor-supply effects of a permanent national program might differ from the effects of a three-year experiment. The biases are believed to be the following: effects on adult males are underestimated, effects on adult females and young people, overestimated.

3. No attempt was made to verify the income reports, so a misreporting of earnings might have occurred. Simultaneous, inappropriate receipts of both experimental transfers and AFDC payments are known to have occurred in several cases.

4. Experimentals also reported income more frequently than controls, and are believed to have learned to report gross rather than net income more quickly; as a consequence, early months of data from all sites is contaminated for purposes of comparison.

5. The sample is truncated by total family income, rather than by the income of husbands, leading to a very substantial underrepresentation of working wives.

6. A set of anomalous results for black households (essentially, male hours of labor) appears to be a product of unexpected labor-supply reductions in the black control group. Experimental labor hours changed very little among experimentals, but fell sharply among controls.

7. Reported results are for intact families. Experimental families were slightly more likely to break up.

Replicability: Designed for replicability through the Internal Revenue Service.

Generalizability:

1. Blacks were deliberately overrepresented in this study, to test the culture-of-poverty hypothesis.

2. Large numbers of Puerto Ricans in the sample do not correspond to their numbers in the

United States as a whole.

3. The sample was drawn from areas of concentrated poverty; poor people (principally white) living in nonpoor areas were not represented.

4. The concentration of findings on two-parent families limits current applicability.

5. The more generous treatment plans would have potentially applied to large numbers of nonpoor families, especially two-earner families, who were excluded from the sample design. Thus, potential NIT effects on the behavior of married women who work full-time cannot be estimated from this experiment.

RURAL INCOME MAINTENANCE EXPERIMENT

Information sources: U.S. Department of Health, Education, and Welfare, Summary Report: Rural Income Maintenance Experiment, November 1976. John L. Palmer and Joseph A. Pechman, editors, Welfare in Rural Areas: the North Carolina-Iowa Income Maintenance Experiment, Brookings Institution, 1978. A six-volume, unpublished final report is on file at the Institute for Research on Poverty, University of Wisconsin-Madison.

Funding sources: The Ford Foundation and the Office of Economic Opportunity; subsequently, Assistant Secretary for Planning and Evaluation, Dept. of Health, Education, and Welfare. Key personnel: Larry L. Orr, Dept. of Health, Education, and Welfare.

Treatment administrator: Institute for Research on Poverty, University of Wisconsin-Madison. Key personnel: D. Lee Bawden, Philip Salisbury, and William S. Harrar.

Evaluator: Institute for Research on Poverty. Key Personnel: D. Lee Bawden.

Enabling legislation: None.

Total cost: $6.1 million (1971); research and administrative costs only: $3.7 million.

Dates: 1970-1972; data collected 1969-1973; final report 1976.

Dissemination effort:

Briefings: Assistant Secretary for Planning and Evaluation.

Testimony: House Ways and Means Committee

Distribution of executive summaries: Dissemination of executive summaries was performed by the Dept. of Health, Education, and Welfare.

Other: Brookings volume cited above.

Policy deliberations pertaining to treatment: Although Bawden is not aware of any (but see comments on other income maintenance experiments), he notes that the New Jersey and Rural experiments led to the monthly reporting experiment, which itself affected policy.

Location of treatment sites: Duplin County, North Carolina, and Pocahontas and Calhoun Counties, Iowa.

Number of treatment groups: Six (with one control group).

Treatment tested: Five negative income tax plans were tested: (1) A 50% of poverty line income guarantee with a 50% tax rate; (2) 75% guarantee with 30% tax rate; (3) 75% guarantee with 50% tax rate; (4) 75% guarantee with a 70% tax rate; and (5) a 100% guarantee with a 50% tax rate.

Sample size: Although formally there were 809 families in the experiment, only 587 were families headed by working-age males whose behavior was of primary interest. (The others were female-headed families or those with an aged male head.) Of the 587, 318 were controls. Of the 269 experimentals, the allocation among the NIT plans listed above was (1) 37, (2) 67, (3) 75, (4) 30, and (5) 60.

Target population: Rural, low-income families, in which the male head was 18-58 and not disabled.

Outcomes of interest: (1) Work behavior. (2) Health, school, and other effects on poor children. (3) Savings and consumption behavior.

Research components:

Process analysis: Conducted. 54% did not know their guarantee within 20%; 23% did not know earnings were taxed; 8% thought earnings tax was 100%. Attrition bias was studied and found not likely to affect results.

Impact analysis: Conducted with sophisticated regression techniques.

Benefit-cost analysis: Not conducted.

Major findings:

For a weighted average of the sample, family income of experimentals compared to controls fell by 13%, family wage income by 13%, family wage hours by 13%. The employment rate of wives fell 28%, dependents, 46%; changes in male work efforts were small (a 1% reduction in hours).

"An income maintenance scheme which must administer a program to the self-employed will entail more cost from income reporting problems than cost from disincentives in labor supply" (Wendell Primus).

Among renters, the probability of buying a home was .06% higher among experimentals than among controls.

Time trends in findings: Sharp drop in farm incomes in last year of experiment, possibly deferral of sales of storable commodities to an untaxed year.

Problems and issues:

1. "... the sample size was probably too small to provide definitive answers to some of the relevant policy questions" (Bawden and Harrar).

2. More than one-fourth of the households in the sample had no relevance to the question under investigation, because they were female-headed or aged. This is one of the reasons that many effects of a large magnitude but no statistical significance were found.

3. The heterogeneity of responses was much larger than the planners expected. Investigators found it necessary to disaggregate the sample of 587 by state, race, farm, and nonfarm. After they had performed this disaggregation, most effects were statistically insignificant, though

some were large in size. Effects that were significant were just as often anomalous (coefficient had the wrong sign) as not.

4. As a group, farmers substantially underreported their incomes, and probably manipulated loopholes in the payment rules.

Replicability: The application of the NIT to the self-employed would require some carryover-income concept such as that used in this experiment.

Generalizability: Intended for generalization to rural poverty populations in the South and Midwest. Generalizations based on this experiment would be suspect, however, because of its small sample size and the internal heterogeneity of the sample groups.

SEATTLE-DENVER INCOME MAINTENANCE EXPERIMENT

Information sources:

1. SRI International, Final Report of the Seattle-Denver Income Experiment, Volume 1: Design and Results; Gary Christopherson, Volume 2: Administration; May 1983.

2. U.S. Dept. of Health and Human Services, Assistant Secretary for Planning and Evaluation, Office of Income Security Policy, Summary Report: Overview of the Seattle-Denver Income Maintenance Experiment Final Report, May 1983.

3. Robert G. Spiegelman, K. E. Yaeger, Michael C. Keeley, Philip K. Robins, Richard W. West, Nancy Brandon Tuma, Arden R. Hall, Yoram Weiss, Fred Dong, and Lyle P. Groeneveld: articles in The Journal of Human Resources, Fall 1980.

Funding source: Dept. of Health, Education, and Welfare, the DHHS, Assistant Secretary for Planning and Evaluation. Key personnel: Joseph Corbett.

Treatment administrators: Mathematica Policy Research (for payments and data collection); Seattle Central Community College and Community College of Denver (for vocational counseling). Key personnel: MPR: David N. Kershaw (deceased) and Gary Christopherson; Seattle CCC: N. John Andersen.

Evaluator: SRI International. Key personnel: Robert G. Spiegelman.

Enabling legislation: None.

Total cost: $77.5 million (1975); research and administrative costs only: $57.1 million.

Dates: October 1970-August 1977 (93 families were assigned to a 20-year Negative Income Tax plan, which is still ongoing, but data collection has stopped); data collected October 1970-December 1978; final report May 1983.

Dissemination effort:

Briefings: A series of seminars for HEW staff.

Testimony: Senator Moynihan chaired a committee hearing on the results of the income maintenance experiments, with a particular emphasis on the SIME/DIME results.

Distribution of executive summaries: Congressional staff, Congressional Budget Office staff, and the public.

Policy deliberations pertaining to treatment: David Greenberg, who worked for the DHHS at this time, states that the experiment data were used to estimate labor-supply parameters that, in turn, were incorporated into microsimulation models. These models were used to cost out and predict the future effects of various welfare-reform proposals under consideration by policymakers. In addition, the marital-stability findings were widely circulated at a time when welfare reform was under serious consideration by policymakers.

Location of treatment sites: Seattle, Washington, and Denver, Colorado.

Number of treatment groups: 48 (with one pure control group).

Treatments tested: There were two types of treatment. One consisted of a Negative Income Tax plan. In this plan, some treatments had a declining rate of benefit reductions (tax rate) as nonprogram income rose. The other treatment was a subsidy to vocational counseling and training.

Financial treatments (income guarantee as a percentage of poverty line, tax rate, and change in tax rate for each $1,000 of nonprogram income): (1) 95%, 50%, 0%; (2) 95%, 70%, 0%; (3) 95%, 70%, -2.5%; (4) 95%, 80%, -2.5%; (5) 120%, 50%, 0%; (6) 120%, 70%, 0%; (7) 120%, 70%, -2.5%; (8) 120%, 80%, -2.5%; (9) 140%, 50%, 0%; (10) 140%, 70%, 0%; and (11) 140%, 80%, -2.5%. Experimental subjects were randomly assigned to programs that were either three or five years in duration in order to test for effects owing solely to the temporary nature of the experiment. As noted above, 93 Denver families were switched without warning into a 20-year plan in the third year of their NIT participation for the same reason.

Counseling/training treatments: (1) Control, no treatment. (2) Free, nondirective, vocational counseling of a standardized form provided by staffs of the community colleges. (3) Free counseling plus a 50% tuition subsidy for either career-related training or enrollment at any institution the student wished to attend. (4) Free counseling plus a 100% tuition subsidy.

Sample size: Two presentations of sample sizes are relevant. Numbers are households.

(A) Financial/Counseling

Control/Control: 1,041.
Control/Experimental: 1,012.
Experimental/Control: 946.
Experimental/Experimental: 1,801.

(B) Second-year (after attrition) distribution of experimentals by the financial treatments listed above:

(1) 346, (2) 184, (3) 204, (4) 163, (5) 237, (6) 278, (7) 241, (8) 224, (9) 93, (10) 193, and (11) 251. There were 1,715 controls in the second year.

Target population: Families who met all of the following requirements: (1) Either married couples (with or without children) or single heads of households with at least one dependent child younger than 18; (2) either earning less than $9,000 per year (if just one worker in the family) or less than $11,000 per year (if two workers), in 1971 dollars; and (3) either includes a nondisabled husband, 18 to 58 years old, or a single, nondisabled, female head of household, 18 to 58.

Outcomes of interest: (1) Effects on labor supply. (2) Marital stability. (3) Other lifestyle changes.

Research components:

Process analysis: Investigators extensively tested the verbally articulated degree of comprehension of the program among experimentals, but did not find that the degree of comprehension was associated with any labor-supply effects. They also tested for Hawthorne effects by paying half of the Denver controls to report income on a monthly basis, as all experimentals had to do, and did not find any significant effects in interview data between reporting and nonreporting controls. The investigators did not, however, expect the surprising results for the counseling program, which are discussed below, and no interview data with experimentals or counselors are available to explain them.

Impact analysis: Conducted with sophisticated regression techniques.

Benefit-cost analysis: Not conducted.

Major findings:

1. "... a universal NIT program without any work requirements in which the mean guarantee level [is] about 110 percent of the poverty level and the mean tax rate is about 50 percent would lead to significant reductions in virtually every major dimension of labor supply."

Mean Second-Year Experimental Response Across 11 NIT Treatments
Compared With the Control Mean

Variable	Husbands	Wives	Female Heads
Annual hours of work	-9%	-21%	-14%
Annual weeks worked	-7	-19	-14
Probability of working at all during the year	-7	-19	-11
Earnings	-8	-20	-16

The major effect of the experiment is not a marginal reduction in hours worked per week but a lengthening of unemployment spells.

Among youths, the experimental effect was a major reduction in hours worked. "There is no evidence that the work effort reduction is accompanied by any increase in school attendance."

2. The counseling-only program did not significantly increase years of schooling. The 50% subsidy only had significant effects on schooling among female heads. The 100% subsidy significantly affected the schooling of husbands, wives, and female heads: the average increase in schooling was .11 to .27 years.

3. The counseling/training programs had a negative impact, as anticipated, on hours and earnings in the first year. The unexpected finding is that the impact on wages and earnings of the counseling/training programs, where significant, was negative in the subsequent years of the program and in the postexperimental data, for both husbands and wives. Single-female heads consistently show a positive earnings and wage effect from the counseling-only program (not necessarily significant), but the same generally consistent (not necessarily significant) negative impact from the subsidy programs. The effects appear to be independent of participation in the NIT experiment.

Disaggregated regressions suggest that the negative effect of counseling was most serious for the following groups:

> a. Husbands who were, before the experiment, unemployed or members of families with normal incomes below $5,000, and who were eligible for counseling only--they earned $1,600 to $1,700 less than their control counterparts in the year following the five-year experiment.

> b. Wives who, before the experiment, were employed; they earned $500 to $650 less than their control counterparts under all three counseling/training plans in the year following the five-year experiment.

The investigators believe that nondirective counseling led some husbands and some wives to enter into unduly ambitious academic programs, which they either did not finish or could not use to good effect in the labor market.

4. "(T)he negative income tax ... plans tested in SIME/DIME dramatically increased the rates at which marriages dissolved among white and black couples, and decreased the rate at which Chicano women entered marriages..." 28% of marriages among black experimentals broke up in the first three years, compared with 21% among black controls; 20% of marriages among white experimentals broke up, compared with 15% of marriages among white controls.

5. "SIME/DIME probably did not affect the health of participants...," "no experimental effect on psychological distress," "SIME/DIME ... does not appear to have had an effect on infant health status." For married women, the experiment seems to have raised fertility rates among Chicanos, to have had no effect on blacks, and to have had inconsistent effects on whites. Effects on the fertility rate of single women were insignificant.

"SIME/DIME resulted in increased debt." "(W)hite migrants receiving experimental treatments were more likely than controls to move to destinations with a better climate..." "SIME/DIME had little effect on intracity residential mobility and no effect on integration."

Time trends in findings:

1. Labor-supply effects tend to grow after the first year, diminish as the program nears an end, and vanish in the year after NIT program-eligibility expires. An exception is single-female heads, who continue to work fewer hours after the program ends.

2. The marital-stability findings are controversial, and one reason is that the experimental/control differential at an early point in the treatment was greater than the subsequent differential.

Problems and issues:

1. A portion of the labor-supply difference between experimentals and controls comes from a systematic underreporting of earnings and hours by experimentals. This problem was known at the time of the final report and does not appear to substantively change the conclusion.

2. Marital-dissolution effects are strongest in the first two years of the experiment, and seem to be sensitive to the presence of the counseling experiment, the preexperimental income, and the

guarantee level. The higher the guarantee level, the lower the experimental effect. Since assignment to guarantee levels was not independent of preexperimental income (low-income families were more likely to be assigned to low-guarantee plans), the marital-dissolution findings may be in part an artifact of the assignment system. The marital-dissolution findings remain controversial, partly because of the time trend noted above, because dissolution is a relatively rare event, and because attrition bias in the controls is difficult to evaluate. There is no evidence that the NIT increased the stability of marriages, which was the expected outcome.

3. Much of the benefit to be derived from the extremely large sample is dissipated by the excessive number of treatments. Cell sizes are then reduced further by race, marital status, previous employment history, etc.

Replicability: NIT plans were designed for national replicability. Methodology of the counseling program is summarized in the final report, and does not appear very different from vocational-counseling programs in common use.

Generalizability:

1. Findings may have been affected by the serious recession in Seattle in the early 1970s. The experiment was extended to Denver, a city with low unemployment rates, for this reason.

2. All participants were, at least initially, residents of low-income communities. Like the other experiments, SIME/DIME does not address the effects of an NIT on dispersed poverty, as opposed to concentrated poverty.

3. Other studies in the 1970s reported very low rates of return to schooling in the labor market. The counseling/training findings may be specific to that era.

4. SIME/DIME is the best available source for income- and leisure-substitution parameters that can be applied to project the effects of national policy proposals affecting low-income people.

GARY INCOME MAINTENANCE EXPERIMENT

Information sources: Kenneth C. Kehrer, Barbara H. Kehrer, Charles M. Wolin, Rebecca A. Maynard, Richard J. Murnane, Robert A. Moffitt, John F. McDonald, Stanley P. Stephenson, Jr., and Richard L. Kaluzny: Five articles in the Journal of Human Resources, volume 14, number 4, Fall 1979, 431-506. Also: Kenneth C. Kehrer, John F. McDonald, and Robert A. Moffitt, "Final Report of the Gary Income Maintenance Experiment: Labor Supply," Mathematica Policy Research, 1980.

Funding source: HEW, Assistant Secretary for Planning and Evaluation. Key personnel: Joseph Corbett.

Treatment administrator: Indiana University (technically a subcontractor through the Indiana Department of Public Welfare). Key personnel: Kenneth C. Kehrer and John Maiolo.

Evaluator: Indiana University, Mathematica Policy Research. Key personnel: Kenneth C. Kehrer and Andy Anderson.

Enabling legislation: None.

Total cost: $20.3 million (1973); research and administrative costs only: $14.8 million.

Dates: 1971-1974; data collected 1971 through 1974; final report 1980

Dissemination effort:

Briefings: Assistant Secretary for Planning and Evaluation.

Testimony: Kehrer testified to a House joint committee (joint between Ways and Means and another House committee) on welfare reform, and submitted a written statement to the Senate Banking Committee. He believes the context was the Carter welfare-reform proposal.

Distribution of executive summaries: Distributed extensively to government agencies, public-policy foundations, and the press.

Policy deliberations pertaining to treatment: Kehrer recalls the results of all four income maintenance experiments being discussed in the context of the Carter reform proposal.

Location of treatment site: Gary, Indiana.

Number of treatment groups: Five (with one control group).

Treatments tested: Four combinations of guarantee and tax rate were tested: (1) 75% of poverty line guarantee, 40% tax rate; (2) 75% and 60%; (3) 100% and 40%; and (4) 100% and 60%.

Sample size: Controls, 771; experimentals by plans listed above: (1) 313, (2) 314, (3) 203, and (4) 198; total experimentals, 1,028.

Target population: Black families with at least one child under the age of 18.

Outcomes of interest: (1) Employment. (2) Schooling. (3) Infant mortality and morbidity.
     (4) Educational achievement. (5) Housing consumption.

Research components:

Process analysis: Conducted. Experimental treatments with social service access and day care
terminated.

Impact analysis: Conducted through sophisticated regression and other analytical models.

Benefit-cost analysis: Not conducted.

Major findings:

(1) Statistically significant reductions in the employment rate of experimental husbands (2.7 to
4.9%) and female heads of households (25.8 to 26.8%), compared with controls. No
significant effects on married women's labor-market participation.

(2) No significant effects of the experimental tax rate were found.

(3) Some experimental teenagers were significantly more likely to continue schooling and less
likely to enter the labor market than controls. The effect was concentrated in lower-income
experimentals.

(4) Significantly fewer low-birth-weight infants were born to high-risk experimental mothers
than to high-risk control mothers, in which the high-risk group consists of women who smoke
and had previously given birth within 16 months or less. The experimental effect is
consistently greater the higher the risk.

(5) Experimental children in grades four through six had a significantly better reading
achievement than the controls. The effect is limited to the third or fourth year after enrollment
in the experiment. No effects were found for students grades seven through ten.

(6) Experimentals increased their rent payments about 4.3% above the rent levels paid by
controls. 6% of the net increase in income was spent on rent; an elasticity of rent payment
with respect to income of about .3 was calculated. There was a small, statistically significant
increase in the probability of an experimental buying a home compared with a control.

Time trends in findings: As noted above.

Problems and issues:

(1) The Gary experiment also intended to test two other treatments. One of these was a social
service access worker (a personal ombudsman); the other was the expansion of day care
services in one neighborhood. Both services were undersubscribed, and subsequently
discontinued.

(2) Gary findings are probably highly conditional on the specific Gary labor market. See
comment on generalizability.

(3) The Gary sample was not selected on the basis of total family income, and therefore does not have the truncation bias found in the New Jersey and Rural experiments against two-earner families.

Replicability: Treatment is intended for replication through the Internal Revenue Service.

Generalizability: The Gary labor market at the time of the experiment was heavily dominated by the steel industry, which offered almost exclusively full-time jobs. Opportunities for part-time work and for other marginal adjustments in hours like overtime and moonlighting appear to have been rare. This probably explains (a) the absence of responses to the experimental tax rate, (b) the absence of experimental responses among wives, and (c) the relatively high experimental responses among married men and female heads of households. Instead of a marginal choice about how many hours to work, many in the sample probably faced a discrete choice about whether to be employed full-time or not to be employed at all.

HOUSING-ALLOWANCE DEMAND EXPERIMENT

Information sources: A large number of reports are available from Abt Associates, but none have been published. The following are published sources: Raymond J. Struyk and Mark Bendick, Jr., editors, Housing Vouchers for the Poor: Lessons from a National Experiment, Urban Institute, 1981; Katharine L. Bradbury and Anthony Downs, editors, Do Housing Allowances Work?, Brookings Institution, 1981; U.S. Department of Housing and Urban Development, Experimental Housing Allowance Program: A 1979 Report of Findings, and The Experimental Housing Allowance Program: Conclusions, the 1980 report.

Funding source: HUD, Assistant Secretary for Policy Development and Research. Key personnel: Jerry Fitts and Terrence Connell.

Treatment administrator: Abt Associates. Key personnel: Ellen Bakeman.

Evaluator: Abt Associates. Key personnel: Stephen D. Kennedy and James Wallace.

Enabling legislation: Housing and Urban Development Act of 1970, title V; amended in 1974 for additional funding. The experiment was one component of an Experimental Housing Allowance Program, which included a Supply Experiment and an Administrative Agency Experiment; these other components were not random-assignment treatment evaluations.

Total cost: $31.2 million (1976); payments, $3.6 million; administration, $2 million; research and monitoring, $25.6 million.

Dates: April 1973-February 1977; data collected April 1973-February 1976; final report June 1980.

Dissemination effort:

   Briefings: To HUD officials.

   Testimony: None.

   Distribution of executive summaries: To HUD.

Policy deliberations pertaining to treatment: Kennedy is not aware of any.

Location of treatment sites: Pittsburgh, Pennsylvania, and Phoenix, Arizona.

Number of treatment groups: 19 (with two control groups).

Treatments tested: The principal treatments tested were payments to households based on a "Housing Gap" and payments based on a percentage of the rent.

   For the Housing Gap treatment, a panel of experts at each site estimated the cost of housing meeting certain standards in modest neighborhoods in that city. This number was $\underline{C}^*$. Payment ($\underline{P}$) was based on the formula $\underline{P=dC^* - bY}$, where $\underline{Y}$ was disposable income less $300 per year for each working member of the family, and $\underline{d}$ and $\underline{b}$ were experimental parameters

(higher $\underline{d}$ and lower $\underline{b}$ imply a greater generosity). Treatments also varied in housing requirements: a minimum rent requirement, set at .7 or .9 of $\underline{C^*}$, or a minimum standards requirement, under which occupied units would be inspected for conformity with standards for health and safety. Households living in units that did not meet the standard specified for the treatment to which they were assigned could not receive payments.

Treatment	d	b	Housing Requirement
1	1	.15	Minimum Standards
2	1.2	.25	Minimum Standards
3	1	.25	Minimum Standards
4	0.8	.25	Minimum Standards
5	1	.35	Minimum Standards
6	1.2	.25	Min. rent (.7)
7	1	.25	Min. rent (.7)
8	0.8	.25	Min. rent (.7)
9	1.2	.25	Min. rent (.9)
10	1	.25	Min. rent (.9)
11	0.8	.25	Min. rent (.9)
12	1	.25	No requirements

In the percentage-of-rent treatments, payment $(\underline{P})$ was determined by the formula $\underline{P=aR}$, where $\underline{R}$ is rent and $\underline{a}$ is a program parameter. There were no housing requirements.

Treatment	$\underline{a}$
13	.6
14	.5
15	.4
16	.3
17	.2

Controls were paid $10 a month for filling out a monthly form and $25 for periodic interviews. Experimentals were not paid for interviews.

The payment system for experimentals lasted three years. Information was collected for two years.

Sample size: The number of households invited to enroll either as experimentals or as controls were 3,600, 1,800 in each city. The numbers actually enrolling (not necessarily receiving payments) were recorded for the initial enrollment offer and two years later. The difference between the second and third columns is attrition, which in this case was of independent interest.

Treatment	Initial	Two Years
1	212	181
2	91	63
3	133	77
4	128	82
5	137	75
6	85	58
7	132	89
8	124	79
9	88	60
10	145	88
11	137	78
12	145	103
13	66	49
14	235	190
15	265	179
16	258	176
17	176	111
Controls	950	603

A different way of presenting the second-year numbers is as follows:

Treatment Type	Phoenix	Pittsburgh	Total
Housing Gap (min. standards)	174	204	378
Housing Gap (min. rent)	207	245	452
Housing Gap (no requirements)	40	63	103
Percentage of Rent	298	407	705
Controls	282	321	603
Total	1,001	1,240	2,241

Target population: Renter households, residing in the counties of the experiment, and meeting the following tests: (1) Disposable income (less than $300 per worker annually) less than one-quarter of the C^* figure for households of that size in that city; (2) assets of under $5,000 (under $10,000 if 62 or older); (3) either two or more related persons of any age or with household head who was handicapped, disabled, 62 or older, or displaced by an urban renewal project; (4) resident in unsubsidized housing--public housing tenants were only eligible if they moved.

Outcomes of interest: (1) Enrollment. (2) Rate of participation (actual receipt of payment). (3) Effects on housing expenditures, quality, and residential segregation.

Research components:

Process analysis: Obtained reasons for refusal to enroll, choice not to participate, and condition of initial housing units.

Impact analysis: By comparison of means and various response-surface estimation techniques.

Benefit-cost analysis: An relative cost-effectiveness study was conducted comparing costs of housing allowances and public subsidies to housing construction.

Major findings:

(1) Many families refused to enroll, a finding that is important in estimating the costs of a national program; refusal to enroll did not seem to be related to the variables in the experiment, but to a disinterest in receiving public assistance. Many of those who did enroll did not participate (receive a payment) and this nonparticipation was substantially affected by the stringency of the housing requirements, the household's race, and the relative availability of housing meeting the minimum requirements (which varied between the sites). Participation is stated as the percentage of those enrolling (all of whom were eligible for immediate payments on income grounds) who received one or more payments.

Treatment/Site	Enrollment	Participation
No housing requirements		
Percent of rent		
Pittsburgh	82%	100%
Phoenix	87	100
Housing gap		
Pittsburgh	78	100
Phoenix	90	100
Minimum standards		
Pittsburgh	75	40
Phoenix	84	54
Minimum rent (.7)		
Pittsburgh	74	81
Phoenix	82	74
Minimum rent (.9)		
Pittsburgh	73	58
Phoenix	81	54

Higher payments increased participation. At an average monthly payment level of $43, one-fourth of all renters who had to meet housing requirements participated; at twice that level, twice as many participated.

(2) Estimated Experimental Effect on Housing Expenditures and Services among Households Meeting Requirements Two Years After Enrollment (services are measured with a "hedonic index" based on characteristics of the housing unit; an asterisk indicates a difference that is statistically different from zero):

Site/Treatment	Change in Expenditures	Change in Expen. As Share of Payment	Change in Services
Pittsburgh			
No restriction	2.6%	5.7%	3.4%
Pct. of rent	8.0	14.0	3.0
Min. rent (.7)	-3.6	-7.8	0
Min. rent (.9)	8.5*	23.3	0.9
Min. standards	4.3	8.6	3.1
Phoenix			
No restriction	16.0*	19.0	12.6*
Pct. of rent	8.0	23.7	-1.0
Min. rent (.7)	15.7	25.5	11.0*
Min. rent (.9)	28.4*	41.3	18.9*
Min. standards	16.2*	27.4	10.2*

"To date no satisfactory explanation for this divergence (in sites) has been found."

(3) The impact on housing expenditures in the Housing Gap treatments differs according to whether the household initially occupied a unit satisfying the minimum standards at enrollment. Of those whose units did not, most who ended up participating satisfied the requirements by moving. The numbers of those who moved and the distances they moved were such that the impact on residential segregation would have been negligible.

Site/Treatment	Estimated Experimental Effects Change in Expenditures	As Share of Payment	Change in Services
Pittsburgh			
Satisfactory			
Min. rent (.7)	2.4%	(5.7%)	0.5%
Min. rent (.9)	4.6	(13.7)	-0.7
Min. standards	1.1	(2.3)	0.8
Unsatisfactory			
Min. rent (.7)	8.7	(15.4)	-0.9
Min. rent (.9)	15.8*	(38.8)	3.1
Min. standards	7.5*	(14.2)	5.6
Phoenix			
Satisfactory			
Min. rent (.7)	-1.2	(-2.7)	2.5
Min. rent (.9)	7.4	(15.4)	4.2
Min. standards	-0.7	(-2.1)	8.2*
Unsatisfactory			
Min. rent (.7)	42.0*	(41.7)	20.2*
Min. rent (.9)	42.6*	(50.0)	26.0*
Min. standards	23.6*	(32.8)	10.5*

(Asterisks denote statistically significant difference from zero.)

(4) Estimates of income elasticity range from .29 to .34 in Pittsburgh and from .26 to .44 in Phoenix. Estimates of price elasticity range from -.11 to -.18 in Pittsburgh, -.23 to -.24 in Phoenix. Differences come in part from econometric specification, in part from differences in the definition of income.

Time trends in findings: Only two years of data were collected.

Problems and issues:

1. Payments guaranteed over three years may not induce the same behavioral changes as a permanent program. For instance, a family that would need to move to receive payments might also realize that a second move would be necessary at the close of the experiment, because they could not afford the unit the experiment had subsidized.

2. The measure of housing services, a hedonic quality index, was developed for this experiment and is not completely satisfactory.

3. The least generous plans were assigned only to very low-income members of the sample, because otherwise many enrolled persons would have been eligible only for very small payments or none at all, limiting their benefits from participating in the experiment. However, this tends to confound the treatment effect with the characteristics of households assigned to the treatment.

4. "... the price elasticity per se is unlikely to be of much use in designing a housing-allowance program. A percent-of-rent formula offers such attractive opportunities for mutually beneficial fraud on the part of landlords and renters that (it) is hard to imagine it ever being implemented" (Harvey Rosen).

5. Most households change their housing units infrequently, and the effect of the experiment will occur with some lag; however, the timing of the lag is not known with certainty and requires modeling assumptions.

Replicability: Replicable. Administrative policy manuals developed, etc.

Generalizability:

1. The single most important finding of this experiment is the extremely low income-elasticity of housing demand among low-income people. This result was confirmed in the "Supply" portion of the Experimental Housing Allowance Program. One implication of this finding is that housing allowances would not result in large inflation of rents. Another implication is that in the objectives of a housing-allowance program, there is a trade-off between assisting large numbers of people and improving the quality of the existing housing stock.

2. Results of this experiment and the other components of the EHAP were used by the Urban Institute to project total costs of housing-allowance programs using microsimulation techniques.

COLORADO MONTHLY REPORTING EXPERIMENT AND PRETEST

Information sources: Robert G. Williams, David L. Horner, Alan M. Hershey, and Nancy L. Graham, First Year Research Results, Colorado Monthly Reporting Experiment and Pretest, Mathematica Policy Research, Inc. (1979); and John A. Burghardt, Impact of a Monthly Retrospective Reporting Requirement on AFDC-Benefit Payments: Evidence from the Second Year of the Colorado Monthly Reporting Experiment, MPR (1982).

Funding source: U.S. Dept. of Health and Human Services, Assistant Secretary for Planning and Evaluation. Key personnel: John Bayne.

Treatment administrator: Colorado Department of Social Services. Key personnel: Jacob Shockley and Joseph Thompson.

Evaluator: Mathematica Policy Research, Inc. Key personnel: Robert G. Williams, Alan M. Hershey, and John A. Burghardt.

Enabling legislation: Waivers of Colorado State Plan.

Total cost: $3.5 million (1977); $1.5 million for research.

Dates: Treatment administered 1976-1979; data collected 1976-1979; final report 1980.

Dissemination effort:
Briefings: DHHS staff; seminar for state; and administrators in management of monthly reporting systems.

Testimony: October 1977--Subcommittee of the House Government Operations Committee.

Distribution of executive summaries: All copies went to DHHS.

Policy deliberations pertaining to treatment: Hershey states that monthly reporting was incorporated into all AFDC and food stamp programs in the country, based on the early findings.

Location of treatment site: Denver, Colorado.

Treatments tested: (1) For controls subject to the existing AFDC system, there was a six-month eligibility determination and the expectation that they would inform caseworkers of changes in their circumstances, but there was no systematic device to report and monitor such changes. (2) Experimentals required to submit monthly reports on income and household composition and monthly grants were adjusted on the basis of the report for the prior month. Failure to meet reporting deadlines or incomplete/inconsistent reporting would lead to delays in payment or, ultimately, discontinuation.

Number of treatment groups: Two.

Sample size: 1,841 controls, 1,825 experimentals.

Target population: AFDC-Regular- and AFDC-Unemployed-Parent recipients.

<u>Outcomes of interest</u>: (1) Reductions in overall payments. (2) Impact of reporting on recipients. (3) Increases in administrative costs. (4) Responsiveness of grants to changes in family circumstances.

<u>Research components</u>:

Process analysis: Conducted. Concerns centered on the ability of recipients to complete forms and about the effects on caseworkers. The difference between the findings of the early and the later reports is partly attributable to a change in the treatment of controls by the agency, which moved to regular in-person redeterminations for the whole nonexperimental caseload, four months after the experiment began. The policy change coincides with an increase in discontinuance rates among controls and a decline in savings from monthly reporting. Other changes at this time (increasing automation of paperwork for the nonexperimental caseload, reductions in caseload per worker for controls but not experimentals) probably had a similar direction of impact on the experiment.

Impact analysis: Conducted. Comparison of means. Subsequent regression study of characteristics of discontinued households.

Benefit-cost analysis: Budgetary analysis only.

<u>Major findings</u>:

(1) Early findings were of a 4.3% reduction in AFDC-R payments and a 5.6% reduction in AFDC-U payments. The primary cause was higher rate of discontinuation, somewhat offset by higher rates of reapproval. 96% of report forms were returned but 44% of all treatment recipients had to wait at least an additional seven days for payment at least once during the first year owing to reporting errors, mainly of income. Grant changes other than discontinuations happened much more frequently in the treatment groups: increases five times more frequently, decreases 2.2 times more frequently. An increase in administrative costs was found of about 4% (from a nonexperimental demonstration in Boulder), or about one-twelfth the calculated savings in payments. Eligibility staff believed the experimental process was fairer than the prior one, and, overall, improved their work and working conditions.

(2) Later findings contradicted the earlier ones. Payment reductions over two years were about 1.8%, and in the second year were -0.7%. Investigators attributed the difference between the first-year and second-year results to improved administration of the control group by the Denver office during the second period, using regular in-person reviews of case circumstances. Monthly savings of 15% were found for the one-quarter of AFDC-R households who ever reported earnings and zero for the three-quarters of the caseload who never did. Changes in the administration of controls made administrative costs difficult to estimate.

<u>Time trends in findings</u>: In the early report, the effects of the system are markedly higher for new AFDC applicants than for prior recipients. This is confirmed in the later report: two-year estimates overstate the long-run impact on payments because the major impact was on new recipients, not on longer-run cases.

<u>Problems and issues</u>: Problems in the administration of the experiment occurred: ·

(1) Some households received payments as experimentals in the first assistance spell, as

controls in a subsequent spell, and vice versa.

(2) Several assigned experimental cases were channeled into the control group because agency workers believed they were short-term cases.

(3) Payment dates differed between the two systems, so that a "paydate" difference could account for some of any observed impact.

(4) Experimental cases were supposed to receive transition payments to cover the three weeks between their initial prospective payment and their first retrospective payment, but these checks were not always issued. An evident potential problem in the experiment is the self-selection of the Denver social services agency.

Replicability: Dependent on sophisticated computer hardware and software.

Generalizability: Impact would probably vary inversely with the degree of aggressiveness already present in caseload management techniques used in the different states and counties.

WIN JOB FINDING CLUBS

Information sources: Nathan H. Azrin, Robert A. Philip, P. Thienes-Hontos, and V. A. Besalel, "Comparative Evaluation of the Job Club Program with Welfare Recipients," Journal of Vocational Behavior, 1980, 133-145. Same authors, "Follow-Up on Welfare Benefits Received by Job Club Clients," Journal of Vocational Behavior, 1981, 253-254.

Funding source: U.S. Dept. of Labor, Employment and Training Administration. Key personnel: Azrin doesn't recall.

Treatment administrator: WIN agencies of five states, all of whom are receiving training from the Anna Mental Health Center. Key personnel: Personnel at different agencies.

Evaluator: Anna Mental Health Center. Key personnel: Nathan H. Azrin

Enabling legislation: None.

Total cost: Administrative costs of the program at the three sites where a counselor was assigned full-time to the Job Club treatment were $167 per placement, excluding office rent and furniture. Multiplying by the number of placements (300) at all five sites yields $50,100 (1977), excluding counselor-training and research costs. However, the participants would have received some treatment in any case, and Elise Bruml, in an internal DOL memorandum, estimates $87 per experimental, which would mean $42,369 (1977). The cost of research only was roughly $300,000 (1977).

Dates: September 1976-March 1978; data collected: essentially the same; final report 1978.

Dissemination effort:

Briefings: None.

Testimony: None.

Distribution of executive summaries: U. S. Dept. of Labor.

Policy deliberations pertaining to treatment: Azrin states that the Job Club became a requirement of the WIN program.

Location of treatment sites: New York City (Harlem); New Brunswick, New Jersey; Milwaukee, Wisconsin; Wichita, Kansas; and Tacoma, Washington.

Number of treatment groups: Six (with five control groups); this is not for the experimental design, but is the safest basis for evaluation. The experimental design had two, with one control.

Treatments tested:

1. Controls received whatever regular WIN (Work Incentive) program Intensive Manpower Service regime was in use at that site, such as counseling, training, subsidized job placement, and referrals to agency listings. The Wichita regime included group counseling and

role-playing. Because the control regime was not standardized across sites, it is safest to assume five different control groups. The more conservative assumption does not materially affect the findings.

2. Experimentals received daily group job search training and supervised job search until a job was obtained. Further detail is given in the **Carbondale Job Club** summary below.

Both controls and experimentals received $1.50 and carfare per session attended.

Sample size: 487 experimentals, 490 controls.

Target population: Registrants for the WIN (Work Incentive) program. Exclusions from this population varied by site. Wichita, Tacoma, and New Brunswick only enrolled "job-ready clients"; New York excluded "non-English speakers, illiterates, and clients already designated to receive training or counseling"; Milwaukee made no exclusions.

Outcome of interest: Employment.

Research components:

Process analysis: After training, the counselors were observed by the experimenters for the first few sessions of their initial Job Club groups to assure general adherence to the experimental program. Experimentals also received all services from a single counselor; controls might receive them from several.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Not conducted.

Major findings:

(1) Employment (over 20 hours/week) after three months:

City	Experimentals	Controls
New York	56%	35%
New Brunswick	63	30
Tacoma	72	39
Wichita	60	39
Milwaukee	54	12
Mean starting wage	$137/wk.	$137/wk.

(2) Effects are statistically significant for all groupings (by sex, age, education, ethnic group) except Hispanics.

(3) Job Club effects are much stronger for mandatory (participation a condition of continued receipt of AFDC) than for voluntary participants. (83% of the sample were mandatories.) This might have to do with the mandatory character or the fact that volunteers generally had children less than six years old, while mandatories had older children.

(4) 21% of all jobs obtained by controls were temporary, compared with 16% of

experimentals' jobs; 25% of controls' jobs were subsidized, compared with 16% of experimentals'.

(5) AFDC payments to experimentals six months after enrollment were reduced by 48%; payments to controls were down by 15%.

Time trends in findings: Follow-up questionnaires had a fairly poor response rate. Six-month differences from those who did respond do not show a drop-off in the experimental effect.

Problems and issues:

1. Job Club counselors were selected for training by their local WIN agency, probably not randomly. Their performance, at least initially, was more closely observed than that of their peers, thus a differential Hawthorne effect is a possibility.

2. At the same time, this observation was clearly necessary to standardize the treatment. Implementing the "Job Club model" on a larger scale would require extensive training and supervision.

Replicability: See **Carbondale Job Club**.

Generalizability: This study would seem to generalize the earlier experiments for an important and relatively homogeneous group.

AFDC JOB COUNSELORS

Information source: George J. Carcagno, Robert Cecil, and James C. Ohls, "Using Private Employment Agencies to Place Welfare Clients in Jobs," Journal of Human Resources, Winter 1982, 132-143.

Funding source: U.S. Department of Health and Human Services, Family Support Administration. Key personnel: Ken Manihan.

Treatment administrator: Michigan Department of Social Services. Key personnel: Robert Cecil.

Evaluator: Mathematica Policy Research. Key personnel: George J. Carcagno and James C. Ohls.

Enabling legislation: None.

Total cost: Research only: $164,400 (1977).

Dates: September 1976-January 1979; data collected September 1976-January 1979; final report December 1979.

Dissemination effort:

Briefings: Dept. of Health and Human Services staff.

Testimony: None.

Distribution of executive summaries: All other state welfare agencies, some congressmen.

Policy deliberations pertaining to treatment: The use of private employment agencies has been incorporated as an option, subject to local administrative discretion, for appropriate AFDC clients in Michigan, according to Cecil, but is seldom chosen.

Location of treatment sites: Wayne County (including Detroit) and Oakland County (including Pontiac), Michigan. Inner-city Detroit had a high unemployment rate and a predominantly black AFDC population. Oakland County is suburban with a predominantly white AFDC population.

Number of treatment groups: Two (with one control group).

Treatment tested:

1. Controls received certain minimal services (counseling without job referral in Oakland County, no counseling in Wayne County) from WIN.

2. Experimentals received an orientation in the Private Employment Agency (PEA) placement program, and, in general, one PEA received information on each subject's education, skills, and interests. The PEA would ask the subject to come for an interview, and if it felt it could match the subject with appropriate jobs, would refer her to employers. PEA fees were paid by the state for successful placements, based on starting salaries, with lower fees where the job

duration was less than 90 days.

Sample size: 2,593 experimentals, 1,691 controls.

Target population: "Inactive" WIN clients: clients not actively receiving employment services from WIN because they were regarded as relatively less employable.

Outcomes of interest: (1) Employment. (2) Duration of employment. (3) Wages. (4) AFDC payment reductions.

Research components:

Process analysis: Conducted with an emphasis on explaining the low placement rate.

Impact analysis: Conducted with probit.

Benefit-cost analysis: Cost-effectiveness only.

Major findings:

1. Placement rates for experimentals were very low, in part because most experimentals never came to the PEA for an interview. In fact, over half never came to the initial orientation.

Employment outcomes (with PEA placements in parentheses):

	Oakland County			Wayne County		
	Experimentals		Controls	Experimentals		Controls
Full-time jobs	19.4%	(3.5%)	19.0%	5.3%	(2.0%)	3.2%
Part-time jobs	8.9	(0.2)	9.5	3.5	(0.0)	2.3
All jobs	27.4	(3.7)	27.4	8.6	(2.0)	5.5

The finding of no significant treatment impact in Oakland County is confirmed by a probit analysis. In Wayne County, the probability of employment, adjusted for personal characteristics, increases by a statistically significant 2.8%.

3. Costs per placement were $1,222 in Oakland County and $2,310 in Wayne County, but the PEA fees were only $341 and $385, respectively. The remainder of the costs were associated with the public administration side, and would have been lower per placement if the placement rate had been higher. In Oakland County, there was no significant difference in cost-effectiveness between the experimental and control treatments. In Wayne County, the experimental treatment was significantly less cost-effective because of its high costs.

Time trends in findings: None reported.

Replicability: Replicable.

Generalizability: The experiment is tested on a large urban population, characteristic of much of the national AFDC population. The findings could not be generalized to a similar program where there were sanctions for nonparticipation.

MASSACHUSETTS WORK EXPERIENCE PROGRAM

Information source: Barry Friedman, Barbara Davenport, Robert Evans, Andrew Hahn, Leonard Hausman, and Cecile Papirno, "An Evaluation of the Massachusetts Work Experience Program," U.S. Dept of Health and Human Services, Social Security Administration, SSA Publication No. 13-11730, October 1980.

Funding source: U.S. Dept. of Labor, Manpower Administration. Key personnel: Howard Rosen.

Treatment administrators: Massachusetts Departments of Employment Security and Public Welfare. Key personnel: MDES: Richard Sullivan and Richard Dill; MDPW: Richard McKinnon.

Evaluator: Brandeis University, Center for Employment and Income Studies. Key personnel: Barry Friedman.

Enabling legislation: None.

Total cost: $760,000 (if CETA-salaried staff not included, $570,000), 1978; research only $300,000.

Dates: January 1978-March 1979; data collected: same; final report October 1980.

Dissemination effort:

   Briefings: U.S. Dept. of Labor and the Health and Human Services Research and Development staff.

   Testimony: U.S. Senate Committee (Moynihan); Massachusetts and other state legislatures; and the National Council on Employment Policy.

   Distribution of executive summaries: DOL.

   Other: "Dozens of interviews," according to Hahn, including some in the Wall Street Journal and the New York Times.

Policy deliberations pertaining to treatment: Final report states that the experiment appeared to have no effect on the incoming administration of Governor Edward King, who had a welfare-reform program in which work experience was not an element.

Location of treatment site: Massachusetts, statewide.

Number of treatment groups: Three (with one control group).

Treatment tested:

   1. Controls received no treatment "other than any normal WIN actions."

   2. Experimental group 1 received a waiver of the 100-hours-per-month-maximum-labor

limitation for AFDC-Unemployed-Fathers program recipients. Instead, they were subject to the "30 and 1/3" rule for six months. Under the latter the first $30 earned does not count against the welfare benefit received, and earnings above $30 are subject to a 67% tax rate.

3. Experimental group 2 received the above treatment and, in addition, were assigned to unpaid work for public or nonprofit agencies three days a week. Failure to report to work could result in sanctions amounting to partial loss of AFDC benefits. The other two days a week were for participation in the agency-assisted job search. Unpaid workers received $30 per month incentive pay and lunch and travel reimbursement. If unable to find regular work within 13 weeks, they were assigned to a second work site.

Sample size: (1) 150; (2) 140; and (3) 725.

Target population: Unemployed fathers receiving AFDC were screened for the following criteria: (1) not already in a special WIN treatment program, (2) unemployed for at least six months, (3) referred unsuccessfully to regular or CETA jobs, (4) "unsuitable for referral to another WIN component," and (5) found on interview to be physically and emotionally able to work.

Outcomes of interest: (1) Employment. (2) AFDC savings.

Research components:

Process analysis: Extensive. Interviews conducted with WIN staff on selection and work-site assignment process, with work supervisors and experimental subjects.

Impact analysis: Conducted with logit regressions.

Benefit-cost analysis: Conducted.

Major findings:

Percentage finding unsubsidized employment over the five quarters: Controls, 28.7%; experimental 1, 30.7%; and experimental 2, 34.2%. These results are not statistically significant.

AFDC payments, average over five quarters: Controls, $839; experimental 1, $864; and experimental 2, $925. (The authors point out that controls also received lower payments on average before the experiment began.)

Received no AFDC for at least one quarter: Controls, 42.3%; experimental 1, 35.2%; and experimental 2, 38.3%.

Time trends in findings: Nonattenders at work sites in experimental group 2 reported finding employment faster than attenders in early quarters; the reverse is true in later quarters. It is possible that for some nonattenders the program either was incompatible with employment not reported to Public Welfare workers, or provided additional incentive to find work.

Problems and issues:

1. The Work Experience program was the subject of intense public controversy for months before its implementation. A court injunction was in force from March 14-April 10, 1978, during which all work performed under it had to be voluntary. Also the ultimately unsuccessful court challenge made the intake process much more selective. Both unemployables and very employables were screened out, and it would be difficult to compare the resulting sample with a population of interest.

2. The screening process was highly discretionary; persons assigned to the sample varied from 15 to 61% of the underlying pool, from one region of the state to another.

3. Cutbacks in CETA staff led to many potential subjects never being called in for an interview. This had different effects across the state, and introduced an additional regional bias.

4. Sanctions were weak and were seldom used. Two-thirds of experimental group 2 never went to assigned work sites, many of them because they were never told to. Some WIN teams in the state made no effort to enforce the sanctions, and the WIN staff in one region seems to have encouraged clients not to cooperate with the program.

5. Additional assistance in job search was minimal. The change in rules from 100 hours to 30 and 1/3 was only effectively communicated to the WIN teams rather late in the experiment, which means it was never communicated to many of the subjects in experimental group 2.

Replicability: The treatment described in official documents is replicable.

Generalizability: Workfare experiments in other states might have similar implementation problems, but the effects here are so severe that the findings probably cannot be generalized.

WIN LABS EXPERIMENTS

Introduction

The U.S. Dept. of Labor financed a group of demonstrations at WIN (Work Incentive) program offices at different sites. Some of these demonstrations were experiments. The Manpower Demonstration Research Corporation (MDRC) coordinated these projects, which collectively were known as the "WIN Labs." The "WIN Labs" collectively, including the nonexperimental projects, cost $2.5 million (1979); costs of separate experiments are not available. The key DOL personnel were Howard Rosen, Merwin Hans, and Gordon Berlin.

MDRC sent copies of the reports listed below, under the individual experiments, to a very large mailing list. DOL officials were routinely briefed about the experimental findings. Some of the findings, particularly those from the Louisville Job Search experiments, were discussed in congressional hearings in 1987, when MDRC made presentations on the Work/Welfare experiments.

Barbara S. Goldman of MDRC, who was the principal investigator of the Louisville experiments, states that the most important form that the dissemination of findings took was in informal meetings with state welfare administrators when the Work/Welfare experiments were being arranged. She feels that the Louisville experiments in particular had "a great deal of influence on the evolution of employment programs within the welfare system." A major emphasis of the Louisville findings is on the feasibility and cost-effectiveness of an enhanced job search for the least employable AFDC recipients. The importance of this emphasis is now widely understood in the policy community, as is the priority that it should have over the placement of relatively advantaged recipients.

LOUISVILLE IMMEDIATE JOB SEARCH ASSISTANCE EXPERIMENT

Information source: Barbara S. Goldman, "Impacts of the Immediate Job Search Assistance Experiment," Manpower Demonstration Research Corporation, June 1981.

Funding source: See Introduction.

Treatment administrator: Kentucky Department of Human Resources. Key personnel: Geralynne Clements.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Barbara S. Goldman.

Enabling legislation: None.

Total cost: See Introduction.

Dates: November 1978-January 1980; data collected November 1978-Summer 1980; final report June 1981

Dissemination effort: See Introduction.

Policy deliberations: See Introduction.

Location of treatment site: Louisville, Kentucky.

Number of treatment groups: Two (with one control group).

Treatment tested:

1. Controls received "the regular services offered to WIN clients under normal procedures." Service delivery delays of two to ten weeks under these procedures are common while approvals for welfare and other social services are pending.

2. Experimentals received services immediately on registering with WIN. The effect of this was to make them immediately eligible for reimbursement of child care, transportation, and lunch expenses and to provide an incentive payment for going to counseling sessions and job interviews.

The most important counseling technique was a counselor-directed, individual job search for four hours a day; participation in this was voluntary (it did not affect the AFDC grant). If they found employment, they continued to be eligible for child care services for up to 90 days, even if they still had not been found eligible for welfare.

At the initiation of the experiment, staff were randomly assigned between controls and experimentals. Subsequent personnel changes could not be kept random, but were not deliberately biased.

Sample size: Experimentals, 811; controls, 808.

Target population: New female WIN registrants (AFDC recipients and applicants). Clients already committed to a job, school, or training could refuse to participate.

Outcomes of interest: (1) Employment. (2) Earnings. (3) AFDC payments.

Research components:

Process analysis: Analysis of factors influencing willingness to participate in counseling programs.

Impact analysis: Comparison of means, linear regression, and logit.

Benefit-cost analysis: Data needed for such an analysis are reported.

Major findings:

(Data are regression-adjusted.)

1. Percent Employed.

Quarter of Follow-up	Experimentals	Controls
First (includes month registered)	35.9%	30.0%
Second	39.1	33.6
Third	37.4	32.9
Fourth	34.7	28.8
Fifth	35.9	30.6

(All differences are statistically significant.)

2. Average Earnings.

Quarter of Follow-up	Experimentals	Controls
First	$218.93	$163.59
Second	327.63	261.10
Third	381.20	346.23
Fourth	388.72	340.39
Fifth	408.14	338.48
Total	1,724.62	1,449.88

(Differences are statistically significant in the first, second, fifth, and last rows.)

3. Average AFDC Payments.

Quarter of Follow-up	Experimentals	Controls
First	$443.02	$448.73
Second	452.84	469.65
Third	402.07	418.41
Fourth	372.92	385.13
Fifth	351.05	376.37
Sixth	337.47	356.75
Total	2,359.37	2,455.03

(Differences are statistically significant in the second, fifth, and last rows.)

4. Sample is too small to reliably test the relative success of the experimental treatment on subgroups of the target population.

5. Estimated incremental administrative costs were about $77-$115 per experimental. The experimental treatment may be marginally cost-effective in terms of AFDC payment reductions. Earnings impacts are two to three times the administrative cost, so the experiment would pass a social benefit-cost test.

Time trends in findings: Displayed above.

Problems and issues:

1. Although participation in the job search counseling was voluntary, the WIN staff involved in the experiment were under considerable pressure to get experimentals to enroll in it, and this may have affected the way they communicated the fact of its being voluntary. "... any inferences about likely participation rates should recognize that participation is likely to be influenced by the type and intensity of encouragement received by the potential participants." About half of the experimentals were either considered unemployable at intake and therefore were not offered the intensive counseling service, or did not participate when offered it.

2. Site self-selection bias possible. Most state WIN programs were not anxious to participate in the experiments.

Replicability: Treatments are documented.

Generalizability: Generalization might be affected by the relative generosity of welfare benefits in other states and the relative employability of those states' WIN populations.

WIN SERVICES TO VOLUNTEERS

<u>Information sources</u>: Ellen L. Slaughter, Paulette Turshak, Gale G. Whiteneck, and Edward C. Baumheier, "Final Report on WIN Services to Volunteers: Denver WIN Research Laboratory Project," Manpower Demonstration Research Corporation, June 1981.

<u>Funding source</u>: See Introduction.

<u>Treatment administrator</u>: Colorado Department of Social Services. Key personnel: Unknown.

<u>Evaluator</u>: Center for Social Research and Development, University of Denver. Key personnel : Ellen L. Slaughter and Edward C. Baumheier.

<u>Enabling legislation</u>: None.

<u>Total cost</u>: See Introduction.

<u>Dates</u>: October 1978-March 1979; data collected October 1978-December 1980; final report June 1981.

<u>Dissemination effort</u>: See Introduction.

<u>Policy deliberations pertaining to treatment</u>: See Introduction.

<u>Location of treatment site</u>: Denver, Colorado.

<u>Number of treatment groups</u>: Four (with two control groups).

<u>Treatments tested</u>: There were two experiments.

A. Effect of recruitment.

A random sample of AFDC recipients with children aged five or younger and not already voluntarily enrolled (volunteers) in WIN.

1. Controls were not subject to a special recruitment effort.
2. Experimentals received letters inviting them to recruitment meetings in their neighborhoods, where WIN services were described.

They were paid $5 if they attended. To register for the services, they still had to go to the WIN office.

B. Effect of special services.

A random sample of walk-in volunteers who were not in either group A1 or A2 was randomly assigned to:

1. Regular WIN services.
2. Special enriched services. Features: (1) Orientations were not focused on AFDC sanctions

for nonparticipation; (2) information sessions on daycare choices; (3) training was available at a larger number of educational institutions; (4) on-the-job training wages were 75% (rather than 50%) subsidized; and (5) the social services and employment services staff were better integrated to improve service delivery.

Sample size: A1, 1,003; A2, 1,003; B1, 114; and B2, 110.

Target population: Female AFDC recipients with children five years old or younger.

Outcomes of interest: (1) Number of volunteers. (2) Employment. (3) Earnings. (4) AFDC payments.

Research components:

Process analysis: Interviews with volunteers and nonvolunteers were conducted.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Cost-effectiveness analysis (budgetary point of view).

Major findings:

Experiment A	Experimentals	Controls
Volunteer WIN registrants	160	20
Incremental cost per recruit	$27.27	---

Employment, earnings, and AFDC payment differences are all statistically insignificant.

In experiment B, the finding is a statistically significant, upward impact on AFDC payments to experimentals. As the preenrollment experimentals' earnings in this fairly small sample were significantly higher than the controls', the most likely reason is that a much higher percentage of experimentals were provided with institutional training than were controls. The training reduced experimental earnings in subsequent quarters by reducing the time available for work. After registration, there are no significant differences in earnings.

Time trends in findings: In B, significant reductions in AFDC payments to experimentals occurred in the third, fourth, and fifth quarters after registration.

Problems and issues:

The sample in B was too small and many of the enhancements in services too subtle to expect statistically significant differences to emerge.

Replicability: Experiment A is replicable. Experiment B is a rather diffuse experiment and aspects of it might not be.

Generalizability: The most frequent reason for registering with WIN was to get help in finding a job. The Denver labor market at this time had very low unemployment, and, therefore, subjects may have felt very little need for WIN services.

MADISON AND RACINE QUALITY EMPLOYMENT EXPERIMENT

Information source: Joan M. Leiman, "The WIN Labs: A Federal/Local Partnership in Social Research," Manpower Demonstration Research Corporation, July 1982.

Summary from above source:

The experiment sought to place women from the Madison and Racine, Wisconsin, WIN programs into better-paying, nontraditional jobs. The centerpiece of the strategy was an employer subsidy, with up to a 100-percent reimbursement for on-the-job training through the first third of a contract (maximum length 50 weeks), declining first to 75% and then to 50% over the remaining term, provided the wage offered was over $4 an hour. The program also offered a peer support group for women adjusting to on-the-job problems. New registrants were randomly assigned to either the treatment or the control group.

"Wisconsin's main problem was the small size of the registrant pool, compounded by the fact that it was very slow to get the marketing and job development efforts underway. The result was a substantial shortfall in the number of women finding employment through the experimental program, so substantial that three-quarters of the way through the project, it was clear that there would be very little probability of seeing any impact from the demonstration. There was also little possibility of increasing the numbers, given the small registrant pool.... A decision was therefore made in the fall of 1979 to phase down the demonstration before its scheduled end, and also to discontinue the Madison/Racine complex as a Lab."

DENVER POSTPLACEMENT SERVICES PROJECT

Information source: Ellen L. Slaughter, Gale G. Whiteneck, and Edward C. Baumheier, "Postplacement Services to WIN Clients: Final Report of a Denver WIN Laboratory Project," MDRC, December 1982.

Funding source: See Introduction.

Treatment administrator: Denver Department of Social Services. Key personnel: We have not been able to obtain these names.

Evaluator: Center for Social Research and Development, University of Denver. Key personnel: Ellen L. Slaughter and Edward C. Baumheier.

Enabling legislation: None.

Total cost: See Introduction.

Dates: April 1980-May 1982; data collected: same; final report December 1982.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: None.

Location of treatment site: Denver, Colorado.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals were called seven times over a six-month period, whether still receiving AFDC or not. Counselors inquired about job-related problems and offered to help. If a job was lost, help was offered in finding a new one. Controls were not called. They were still eligible for WIN services (principally, child care subsidies for 90 days).

Sample size: Experimentals, 270; controls, 281.

Target population: WIN-mandatory AFDC recipients who had recently found full-time, permanent employment.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Recipiency. (4) AFDC payments.

Research components:

Process analysis: Conducted principally through client interviews. The authors found that the average experimental had 8.3 contacts with WIN staff and the average control had 6.6, in both cases mostly connected with the mandatory WIN job search after loss of the initial job. Concerning WIN staff intervention, participants mostly reported that "they talked with [us] about how to solve" a problem. Experimentals were more likely to receive child care subsidies, while controls were more likely to either leave children with older siblings or leave them unattended. Controls were more likely to leave their jobs because of child care conflicts,

but experimentals were more likely to leave their jobs because of conflicts with supervisors and co-workers.

Impact analysis: Comparison of means.

Benefit-cost analysis: Cost-effectiveness only.

Major findings:

1. No impact on job retention beyond one month.

2. Experimental/control differences in earnings, AFDC payments, and recipiency status were statistically insignificant.

3. Costs per experimental were $320 higher than costs per control, and thus the project was not cost-effective.

Time trends in findings: Experimental/control job retention rates are significantly different (at the 10% level) in the first month, insignificantly different thereafter.

Problems and issues: None apparent.

Replicability: Replicable.

Generalizability: There is no obvious reason why this approach should work somewhere else when it did not work in Denver.

LOUISVILLE GROUP JOB SEARCH EXPERIMENT

Information source: Joanna Gould-Stuart, "Welfare Women in a Group Job Search Program: Their Experiences in the Louisville WIN Research Laboratory Project," Manpower Demonstration Research Corporation, April 1982. Carl Wolfhagen with Barbara S. Goldman, "Job Search Strategies: Lessons from the Louisville WIN Laboratory," Manpower Demonstration Research Corporation, November 1983.

Funding source: See Introduction.

Treatment administrator: Kentucky Department of Human Resources. Key personnel: Geralynne Clements and Ruth Harvey.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Barbara S. Goldman.

Enabling legislation: None.

Total cost: See Introduction.

Dates: October 1980-May 1981; data collected through December 1981; final report November 1983.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment site: Louisville, Kentucky.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals received essentially the Job Club treatment, incentive payments of $1.50 per day, and transportation and child care payments. This treatment was available immediately on registration. Controls received "the usual WIN services." This might have included intensive counseling, on-the-job training, or classroom instruction, but for the great majority of controls, it amounted to no services. Controls who did receive services received them after the usual administrative delays, which could be up to 10 weeks.

Sample size: Experimentals, 376; controls, 374.

Target population: Female WIN registrants who were not already employed or in training or schooling, who did not have medical or personal problems preventing them from working, and who volunteered to participate.

Outcomes of interest: (1) Employment. (2) Earnings. (3) AFDC payments.

Research components:

Process analysis: Conducted through observations and interviews. Job search techniques taught were fewer than in the Azrin model--apparently the only one was cold calls from the Yellow Pages. An important finding is that "job-readiness" ratings, an important WIN criterion, were inherently subjective; consequently, job readiness could be affected by changing personal circumstances and group experience. In fact, 32% of non-job-ready experimentals found jobs.

Impact analysis: Comparison of means, OLS, and logit.

Benefit-cost analysis: Cost-effectiveness only.

Major findings:

Over two quarters of follow-up:

	Experimentals	Controls	Difference Significant?
Percentage ever employed	49%	34%	yes, at 1%
Total average earnings	$550	$406	yes, at 5%
AFDC payments	$1,680	$1,710	no

Employment impacts were largest for WIN volunteers; they were generally insignificant for WIN mandatories. Impacts were much larger for those who had not been recently employed than for those who had. The incremental cost was $195 per experimental. This is not cost-effective in budgetary terms, because the AFDC savings are insignificant. However, the Louisville experiment was clearly overstaffed compared to Azrin's, which had similar employment results.

Time trends in findings: Earnings and employment in the second quarter increased over earnings and employment in the first quarter.

Problems and issues:

1. Roughly the first half of the sample (the first six months of intake) were discarded because of dissatisfaction with the initial implementation of the treatment. The treatment procedures were revised, and the findings are for the later sample only.

2. Because the program was voluntary, AFDC savings from one possible source in the Azrin WIN Job Club experiment could not have occurred. Azrin believed that some recipients had employment that they had concealed from welfare officials; since holding a job and going to daylong, supervised workshops and job search simultaneously would be difficult, he speculated that savings even from nonparticipants were possible if the Job Club were mandatory. Even without concealed employment, mandatory attendance would be a disincentive to recipiency. The MDRC report, on the other hand, emphasizes the importance of group reinforcement and solidarity in the frustrating telephone job search, and questions whether a mandatory program would have these elements.

3. Job retention is not reported.

Replicability: This is a replication.

Generalizability: The authors argue that the impact estimates are not very different from Azrin's WIN Job Club impact estimates, when adjusted for differences in sample characteristics and job markets.

DAYTON WAGE-SUBSIDY VOUCHER EXPERIMENT

Information source: Gary Burtless, "Are Targeted Wage Subsidies Harmful? Evidence from a Wage Voucher Experiment," Industrial and Labor Relations Review, October 1985, 105-114.

Funding source: U.S. Dept. of Labor, Office of the Assistant Secretary for Policy, Evaluation and Research. Key personnel: Gary Burtless, Larry L. Orr, and John Cheston.

Treatment administrator: The CETA agency in Montgomery County, Ohio. Key personnel: Not available.

Evaluator: Mathematica Policy Research; DOL, ASPER. Key personnel: The experiment was designed by John A. Burghardt, Gary Burtless, John Cheston, Larry Orr, and Harold Watts. It was canceled by the Reagan administration at an early stage; Gary Burtless analyzed the limited data available, which had been collected by John Cheston.

Enabling legislation: CETA Reauthorization Act of 1978.

Total cost: It is not possible to separate the cost of the experiment from the cost of the Employment Opportunities Pilot Project (EOPP), a large-scale demonstration of employment and training initiatives under the Carter administration which was aborted by the Reagan administration in May 1981. Burtless states that his best guess as to the incremental cost of the experiment, over and above the EOPP, would be well under $500,000 (1980), largely for the research design. A second experiment also planned under the EOPP was never started, again because the new administration canceled it.

Dates: December 1980-May 1981; data collected: same; no final report exists. A Brookings Institution working version of the cited article is dated August 31, 1984.

Dissemination effort:

Briefings: Companies with an interest in the Targeted Jobs Tax Credit.

Testimony: Congressman Rangel, while not calling on Burtless to testify about his findings, did call on other witnesses who criticized them.

Distribution of executive summaries: None.

Other: In a letter to the New York Times, Burtless reported the findings summarized below. A press conference resulted in substantial coverage by the Times, the Baltimore Sun, Business Week, Fortune, and others.

Policy deliberations pertaining to treatment: See comments under "Testimony," above. The Targeted Jobs Tax Credit, despite the findings, continues to cost on the order of half a billion dollars annually, according to Burtless.

Location of treatment site: Montgomery County, Ohio, which includes Dayton.

Number of treatment groups: Three (with one control group).

Treatments tested:

1. Tax Credit Voucher. These experimentals received a Job Club-type treatment. They were also given both vouchers which employers could use to obtain credits on their tax returns and training and written materials in using this subsidy to good advantage in their job search. The credit was good for 50 percent of wages paid in the first year of employment, with the total credit not to exceed $3,000, and 25% of wages in the second, total credit not to exceed $1,500.

2. Direct Cash Subsidy. These experimentals received the same treatment as the first group, except that the subsidy was to be paid quarterly directly by the program operator, without regard to the employer's tax liability.

3. Controls. Controls received the Job Club treatment, but no voucher. They received instead a one-day course in using Job Bank listings at the local employment service (a placebo treatment). They were not told that they were eligible for wage subsidies (although by law they were).

Sample size: Tax credit group, 247; direct cash group, 299; controls, 262.

Target population: There were two: (1) Recipients of general assistance, typically single or, if a member of a couple, childless. "Many were only temporarily destitute"; and (2) AFDC recipients, whose participation was not mandatory. However, many of those referred were WIN mandatories who had been unsuccessful in other WIN activities.

Outcomes of interest: (1) Placement in an unsubsidized job. (2) Initial wage.

Research components:

Process analysis: Not conducted because the experiment was canceled. Data are available only on those who had gone through an eight-week cycle (two weeks of Job Club and six weeks of job search, for experimentals) as of that date.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Not conducted.

Major findings:

1. Job Placement Rates. Controls, 20.6%; tax credit group, 13.0%; direct cash group, 12.7%. The advantage of the controls over the experimentals is statistically significant.

2. Use of the Vouchers. Of 70 voucher-holders who found employment, only 19 worked for firms that requested certification for wage subsidies.

3. Initial Wage Rate. Average initial wages in all three groups were nearly identical.

4. Job-finding rates of AFDC and general assistance groups were almost equal. "When economically disadvantaged workers are clearly identified to potential employers as disadvantaged (by a subsidy offer), their chances of employment are harmed."

<u>Time trends in findings</u>: No data available beyond eight weeks.

<u>Problems and issues</u>:

1. The experiment was conducted during a period of high and growing unemployment in the local area.

2. It is likely that marginally employable welfare recipients are not in a good position to explain and market their own wage subsidies. It is possible that job developers could improve on the results found here, but it is not clear that this improvement would be sufficient to justify the subsidy itself.

3. The absence of process analysis means that no documentation exists on any extraneous influences that might contaminate the findings.

<u>Replicability</u>: Replicable.

<u>Generalizability</u>: The experiment took place in a depressed region and its abrupt termination leaves open some questions about its administration. However, other experiments with wage subsidies (for example, the **Wage-Subsidy Variation** experiment, and the **Florida TRADE Welfare for Work** experiment) have results that are not inconsistent with those reported here.

MONTHLY REPORTING IN THE AFDC PROGRAM

Information source: William L. Hamilton, "Monthly Reporting in the AFDC Program: Executive Summary of Demonstration Results," Abt Associates, September 1985.

Funding source: U. S. Dept. of Health and Human Services, ASPE. Key personnel: John Baine.

Treatment administrator: Welfare Departments in Illinois and Massachusetts. Key personnel: Massachusetts officials have not been with the department for a long while now; in Illinois, Stephen Spence.

Evaluator: Abt Associates. Key personnel: William L. Hamilton and Nancy R. Burstein.

Enabling legislation: None.

Total cost: Operations, $5.5 million; research, $3 million (1982).

Dates: Illinois, October 1981-September 1982; Massachusetts, August 1981-July 1982; data collected: same; final report September 1985.

Dissemination effort:

Briefings: A large-group briefing to various Dept. of Health and Human Services agencies, the Food and Nutrition Service of the Dept. of Agriculture, and others.

Testimony: None.

Distribution of executive summaries: To Dept. of Health and Human Services.

Policy deliberations pertaining to treatment: The finding that monthly reporting makes little difference came out after Congress had passed changes in the law making it mandatory. Since that time, the law has been revised in the direction of flexibility for the states. Hamilton states that state officials pressing for greater discretion probably cited the findings.

Location of treatment sites: Chicago, Illinois; Boston, Massachusetts.

Number of treatment groups: A uniform experimental treatment was tested against controls that varied by state. Illinois had one control group; Massachusetts had two.

Treatment tested: Experimentals had to return a monthly report as a condition of continued receipt of AFDC. The report covered income received in the previous month and household composition. Payments for the following month (e.g., March) were determined by the report for the previous month (January). Thus, the accounting principle was retrospective. (Prospective accounting attempts to match grants to anticipated household needs.) Face-to-face redetermination of eligibility with a caseworker was conducted annually in Massachusetts; Illinois had no redetermination requirement.

Controls: Illinois conventionals faced retrospective accounting for income, prospective accounting for other factors, and semiannual face-to-face redeterminations. Monthly reports

were required only for households with earnings.

Massachusetts had two controls. The conventionals faced retrospective accounting, semiannual redeterminations, and no regular reporting requirement. The voluntaries received a form each month that they had to return only if income or household composition had changed; accounting was prospective, and redeterminations were semiannual.

Sample size: Illinois: 7,000 experimentals, 3,600 controls. Massachusetts: 2,500 experimentals; 2,300 voluntary controls, 5,100 conventional controls.

Target population: AFDC-recipient households.

Outcomes of interest: (1) AFDC payments. (2) Caseloads. (3) Error rate. (4) Administrative costs.

Research components:

Process analysis: Conducted in both states, but more intensively in Illinois. Illinois encountered substantial problems with the automated data processing system that had been developed to support monthly reporting. These problems led to erroneous payments, inappropriate case closings, and a suspension of the policy of closing cases for failing to file reports. The fairness of the experimental treatment was studied with regard to terminations and the reporting burden. The investigators concluded that terminations for noncooperation occurred with roughly equal frequency in Illinois under both the experimental and control systems; the burden of reporting was minor for most experimentals in Illinois (seven minutes once they were used to it).

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Effects on government budgets discussed.

Major findings: Effect on total payments over 12 months, experimentals vs. conventionals: Illinois, +4.3%; Massachusetts, -1.9%. The Illinois figure is statistically significant; but for the second six months, when computer problems had been resolved, the measured impact was 0.6%, which was not statistically significant. The Massachusetts figure is not statistically significant.

Effect on caseload size over 12 months, experimentals vs. conventionals: Illinois, +3.0%, Massachusetts, -2.7%. Both figures are statistically significant, but neither is important. The Illinois figure for the second six months is +0.5% (not statistically significant). The Massachusetts reduction is concentrated around the fourth month of the experiment; "the policy of termination for failure to file was not implemented in Massachusetts until the fourth month of the demonstration... A few months later, the caseloads under monthly reporting and the conventional system were approximately equal."

Effect on average AFDC payment size over 12 months, experimentals vs. conventionals: Illinois +1.2% (statistically significant, but 1.0% and not significant the second six months); Massachusetts +0.8% (statistically significant). Monthly reporting led to more frequent grant adjustments; total increases exceeded total reductions.

Effects of voluntary change reporting in Massachusetts over 12 months, voluntary controls vs. conventionals: No effects were statistically significant.

Effects on error rate in Illinois over 12 months, experimentals vs. conventionals: Effects are not statistically significant.

Effects on administrative cost, experimentals vs. conventionals: Net savings in Illinois of about $1 per case per month; net increase in Massachusetts of about $4.80 per case per month.

Net government expenditure effects, experimentals vs. conventionals: Not statistically significant in either state.

Net government expenditure effects, voluntary controls vs. conventional controls: Net increase of about $4.15 per case per month.

Time trends in findings: Noted in "Major findings."

Problems and issues: Illinois effects affected by computer problems noted above.

Replicability: Replicable.

Generalizability: Sample sizes are very large. The substantial experimental effects in the Denver, Colorado, experiment are shown to be the consequence of the particular control treatment in that state at the time the experiment began. The effects of monthly reporting in large, urban, AFDC populations are shown in these experiments to be minor when compared with more active welfare regimes. Although the effects on rural or medium-city populations are not tested, there is no obvious reason why they should be different.

FOOD STAMP WORK REGISTRATION AND JOB SEARCH DEMONSTRATION

Information source: Robert Lerman and Barry Friedman with Shari Ajemian, Charles K. Fairchild, JoAnn Jastrzab, Jane Kulik, Christopher Logan, Cecile Papirno, and Adam Seitchik, "Food Stamp Work Registration and Job Search Demonstration: Final Report," July 1986. The experiment was conducted in two independent phases, and this report does not include some significant information about the initial phase, which was discussed in a previous final report, dated June 1985.

Funding source: U.S. Department of Agriculture, Food and Nutrition Service. Key personnel: Boyd Kowal.

Treatment administrators: State employment security and social service agencies in 21 locations.

Evaluators: Center for Human Resources, Florence Heller Graduate School, Brandeis University; Abt Associates. Key personnel: Research design, Leonard Hausman; Project director, Jane Kulik; Principal authors, Robert Lerman and Barry Friedman.

Enabling legislation: None.

Total cost: Research only: roughly $3-4 million (1983).

Dates: October 1981-December 1983; data collected through June 1984; final report June 1986.

Dissemination effort:

Briefings: Agriculture Department officials; Senate and House Agriculture Committee staff.

Testimony: Incidental questions and answers at reauthorization hearings.

Distribution of executive summaries: Usual FNS mailing list, and on request.

Policy deliberations pertaining to treatment: Kowal states that the Agriculture Department had recommended that states be allowed flexibility in the enforcement of work registration and job search requirements. The act reauthorizing the Food Stamp program in 1985 allowed for such flexibility, and the experimental findings might have influenced the congressional authors.

Location of treatment sites: Tucson, Arizona; Fresno and San Diego Counties, California; Colorado Springs, Colorado; Washington, D.C.; Pensacola and Sarasota, Florida; Clark and Madison Counties, Kentucky; Portland, Lewiston, and Augusta, Maine; Detroit, Michigan; Albuquerque, New Mexico; Nassau and Niagara Counties, and Schenectady, New York; Toledo, Ohio; Austin, Texas; Portsmouth, Virginia; Cheyenne, Wyoming.

Number of treatment groups: Nine (with one control group).

Treatments tested: Although a total of eight treatments were tested, only one treatment was tested at each demonstration site. The first four treatments were tested during the Initial Demonstration period (October 1981-March 1983), a recessionary period. The remaining four treatments were tested during the Expanded Demonstration period (October 1982-June 1984), a

recovery period. The eight kinds of treatments were:

1. In-Person Registration Model (Cheyenne, Colorado Springs, Sarasota, and Washington). All members of a recipiency household who were not exempt from work were required to register for work in person at the State Employment Security Agency (SESA) and report evidence of registration to the Food Stamp Agency (FSA). In three out of four cases, the SESA added a requirement for certain numbers of job contacts to be made within a specified period of time.

2. Job Club Model (Tucson, Albuquerque, and Detroit). Work registration occurred at the Food Stamp Agency. Nonexempt household members were then called in for assessment by the SESA; job-ready registrants were assigned to a standard Job Club group job search model, which lasted two or three weeks.

3. In-Person Registration/Job Club Model (Austin). Work registration occurred at the SESA, and nonexempt job-ready persons were assigned to the Job Club Model. Findings for this treatment are combined with those of model 2, because only one site was found that implemented it.

4. Food Stamp Agency/Job Club Model (Schenectady, Niagara County, and Toledo). All work registration and job search requirements were completed at the FSA. A special FSA Employment Unit (EU) performed registration, assessment, and supervision. Job-ready registrants were required to make up to 24 employer contacts in an eight-week period, reporting regularly to the EU.

[Note: The next four treatments were not initiated until after October 1982 and, consequently, involve the FSA only, because by 1982 SESAs had been removed from the work registration process by legislation.]

5. Applicant Search Model (Nassau and Fresno Counties). Before applicants were certified to receive food stamps, household members not exempt from work registration were required to complete a specified number of employer contacts. Following certification, the EU monitored continuing employer contacts.

6. Job Club Model (Portland, Lewiston, Augusta, Pensacola, and Portsmouth). All job-ready work registrants were assigned to a two-, three-, or four-week Job Club. In Pensacola, subjects were also required to complete six employer contacts in a two-week period prior to assignment to the Job Club.

7. Group Job Search Assistance Model (Clark and Madison Counties). A two-day Employability Skills Training workshop was followed by an eight-week job search requirement with biweekly group monitoring meetings.

8. Job Club/Workfare Model (San Diego County). After a three-week Job Club, job-ready registrants who did not find a job were assigned to Workfare, under which they were required to repay the household food stamp allotment with work at the minimum wage.

Controls were not subject to any work requirements.

Sample size: Sample sizes for the Initial Demonstration are not reported in the 1986 final report,

but 31,000 persons are said to have been randomly assigned. Key findings (e.g., earnings, employment) are based on follow-up interviews with randomly selected subsamples of the experimental and control populations. Applicant Search model (#5): experimentals, 4,396; controls, 4,116; Job Club model (#6): experimentals, 2,333; controls, 1,633; Group Job Search (#7): experimentals, 870; controls, 586; Job Club with Workfare (#8): experimentals, 2,070; controls, 422.

Target population: All recipients of food stamps who were not exempt from work registration, i.e., able-bodied persons 18-65, not enrolled at least half time in school or training programs, not working 30 or more hours per week, not otherwise incapacitated, and not caring for children under 12. In Nassau and Fresno counties, subjects applying for food stamps and meeting the above criteria had to meet job search requirements before they were certified.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Transfer payments.

Research components:

Process analysis: Conducted. The analysis found that the treatments were implemented as planned, although treatments involving groups (Models 2,3,4,6,7,and 8) sometimes had difficulty matching staff to clients. On average, FSA staff terminated benefits to 23 percent of experimentals for noncompliance, compared with 9 percent of controls. "Contrary to the conventional view of policy analysts, agency staff are indeed willing to conduct assessment interviews, provide job search assistance, and sanction those who fail to comply."

Impact analysis: Conducted by site with tobit, and the results were then pooled.

Benefit-cost analysis: Conducted from taxpayer, recipient, and social perspectives.

Major findings:

Impacts are reported by quarter following random assignment. "Transfers" refers to all transfers.

Model	Earnings (1st Q)	Earnings (2nd Q)	Food Stamps (2nd Q)	Transfers (2nd Q)
1	- $8	+$113	-$53	-$62
2&3	+ 53	+ 59	- 41	- 93
4	+ 11	+ 6	- 4	- 17
5	+ 126	+ 117	- 20	-111
6	+ 76	+ 29	- 13	- 31
7	- 23	+ 54	- 33	- 60
8	+ 136	+ 208	- 59	-117

Benefit-cost analysis showed positive social net benefits for all models except #4. Net social benefits per experimentals range from -$55 for #4 to +$471 for #8. Taxpayers had a net benefit from all models except #4, with transfer savings greater than administrative costs. Recipients had a net loss of income in Models 2&3, 4, and 7, but a net income gain in Models 1, 5, 6, and 8. Since Models 2&3 and 6 are identical in concept, implementation may matter. Implementation was also felt to be important with respect to Model 8: "Given San Diego's extensive experience with Workfare, coupled with its high priority on rigorous implementation of job search and work requirements, the success of the Job Club/Workfare Model might be attributed to factors specific to San Diego as well as to the attributes of the model." Models 1 and 5, however, were effective in a wide range of sites.

Time trends in findings: As shown.

Problems and issues:

1. Previous research had found that many registrants faced little or no actual treatment, owing to agency policy, administrative failure, insufficient resources, or other reasons, despite the legislative mandates for work registration and job search. This accounts for the decision that controls should face no work requirement. However, it might well be the case that the typical U.S. food stamp recipient did face some requirement, however small.

2. All the demonstration sites volunteered to participate in the experiment. Clearly, the findings of the experiment do not apply to agencies which would refuse to administer these treatments or that lacked the resources. In that sense, there is a site-selection bias. According to Kowal, managers at some sites were motivated by the prospect of savings in county payments to General Assistance, which is sometimes administered jointly with food stamps, and in others were simply strongly motivated to establish model programs. Thus, the results are best interpreted as predictions of the probable impact of an experimental treatment if local agency managers chose to implement it thoroughly, rather than enforce no requirement whatever.

3. The Food Stamp program interacts with other support programs (Unemployment Insurance and AFDC, to name two). There is no discussion in the final report about what the consequences would be if the recipients of more than one income maintenance program were then subject to more than one work search requirement.

4. There is no discussion in the final report on the reasons for basing the employment and earnings findings on follow-up interviews rather than administrative records, such as are maintained for unemployment insurance. Follow-up interviews were only planned for about a sixth of the total sample, and the response rate was only 61%. The authors attempt to correct

for potential nonresponse bias, but no consistent technique exists for dealing with it.

Replicability: Replicable.

Generalizability: Site self-selection bias is critical. Local agency managers will not always have the motivation or the ability to implement these programs as well as the agencies who ran them in this demonstration.

THE STATE WORK/WELFARE INITIATIVES

Introduction

In 1981 the Omnibus Budget Reconciliation Act (OBRA) authorized states to employ policies of mandatory unpaid work experience for AFDC recipients under certain circumstances, and to streamline the administration of the Work Incentive (WIN) program.

A series of demonstrations testing how effectively states used these new policies was conducted. Most of the evaluations were performed by the Manpower Demonstration Research Corporation (MDRC) with partial funding from the states, partial funding from the U. S. Dept. of Health and Human Services, and partial funding from the Ford Foundation. The key officer at U.S. Dept. of Health and Human Services was Howard Rolston; at the Ford Foundation the officers were Gordon Berlin and Prudence Brown.

Manpower Demonstration Research Corporation made more than 100 presentations, including briefings and lectures, on their findings in 1986 and 1987. MDRC representatives testified before the Moynihan subcommittee and the full Finance Committee in 1987, and in hearings of the Moynihan subcommittee in May 1989 on regulations to implement the Family Support Act. MDRC findings were prominently reported by the New York Times, the Washington Post, and by United Press International.

The Manpower Demonstration Research Corporation reports were frequently cited in Congress during the framing of the Family Support Act (FSA) of 1988. In an unpublished paper, Erica Baum, who was the principal staff support for Senator Daniel Patrick Moynihan (chairman of the Senate Subcommittee on Social Security and Family Policy of the Committee on Finance) states that MDRC's program evaluations were essential to the passage of the FSA because they were "germane ... timely ... unambiguous ... not subject to challenge on methodological grounds...," consistent across sites, and disinterested. Her account is confirmed by lobbyists and other House and Senate staff members in "'The Remarkable Quango': Knowledge, Politics and Welfare Reform," a paper by Peter Szanton. MDRC findings were cited during the markup of the bill by the Senate Finance Committee in April 1988 and on the Senate floor in June 1988.

Total research expenditures by MDRC were $7.9 million (1985). Some states themselves undertook certain research-related activities, and these costs are not included in that figure.

SAN DIEGO JOB SEARCH AND WORK EXPERIENCE DEMONSTRATION

Information source: Barbara S. Goldman, Daniel Friedlander, and David A. Long, <u>California:</u>
<u>Final Report on the San Diego Job Search and Work Experience Demonstration</u>, Manpower
Demonstration Research Corporation, February 1986.

Funding source: See Introduction. Key personnel: For California, Virginia Hamilton.

Treatment administrator: State Employment Service Agency and the County Welfare Department.
Key personnel: Ray Koenig and Joan Zinser.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Barbara S. Goldman.

Enabling legislation: OBRA.

Total cost: See Introduction. For the second treatment, administrative cost of $636-$727 per
experimental; for the first treatment, $562-$587 per experimental, or about $2.8 million total
(1983).

Dates: Intake October 1982-August 1983; data collected through March 1985; final report February
1986.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction. Led to the **Saturation Work
Initiative Model (SWIM)** experiment.

Location of treatment site: San Diego County, California, countywide.

Number of treatment groups: Three (with one control group).

Treatment tested: Random assignment occurred at the time of application for AFDC. Impacts per
applicant therefore include those who were found ineligible for AFDC (and were therefore
ineligible for programs).

Experimental group 1. Job placement assistance on day of AFDC application, followed by
three weeks of Job Club.

Experimental group 2. Same treatment as group 1, but if still unemployed at the end of three
weeks, required to hold an unpaid work experience job at a public or nonprofit agency for up
to 13 weeks. The hours of unpaid work were set by the family's welfare grant divided by the
minimum wage.

Controls. Minimal WIN services.

Sample size: AFDC-R: Group 1, 856; group 2, 1,502; controls, 873. AFDC-U: Group 1, 831;
group 2, 1,376; group 3, 813.

Target population: AFDC and AFDC-U applicants who were WIN-mandatory. Excluded were

refugees, persons with language barriers, and applicants with children under age six.

Outcomes of interest: (1) Employment. (2) Earnings. (3) AFDC recipiency and payments.

Research components:

Process analysis: Conducted.

Impact analysis: Conducted with OLS regressions.

Benefit-cost analysis: Conducted.

Major findings:

1. Work experience supervisors found the productivity of their assigned subjects roughly comparable to those of regular entry-level employees.

2. Sanctions were applied to from 4 to 8 percent of experimentals, but 1 percent or less of controls.

Findings for AFDC-Regular:

1. Both experimental groups had significantly higher rates of employment (61% to 55%) over six quarters than the controls, but the group 1 (no Workfare) differential faded to insignificance after the third quarter.

2. The Workfare group had $700 more in earnings per subject over six quarters than the control; the non-Workfare group had $251 more in earnings, but this was not significant.

3. Neither treatment had much effect on AFDC recipiency.

4. Over six quarters, the AFDC payments per Workfare experimental were $288 less than those per control; payments to Job Club-only experimentals were $203 less than those to controls, but this difference was not significant.

5. Program gains were largest among those in the sample who had no previous employment experience.

Findings for AFDC-U:

1. "For both program models, there were statistically significant and substantial reductions in welfare payments, but no significant impacts on the employment and earnings of AFDC-U applicants." The Workfare subjects received on average $530 less in AFDC payments over six quarters than did controls; the job-search-only subjects received $470 less.

2. "Sanctioning rates were higher for experimentals than for controls, and those sanctioned faced larger grant reductions than did AFDC's (regulars)."

3. "In general, mandating (Workfare) for AFDC-U's did not improve program outcomes compared to those found for the Job Search program." The benefit-cost analysis findings are

that there were consistent, large net gains to taxpayers and government budgets for both programs and for both applicant groups. AFDC-R applicants also benefitted financially from the Workfare treatment; the job-search-only treatment did not always have positive benefits. AFDC-U applicants were made worse off by the treatments, because they reduced their benefits without increasing their earnings.

Time trends in findings: AFDC-R welfare savings from the Workfare treatment declined over time. AFDC-U welfare savings from the two treatments seem to decline gradually over time.

Problems and issues:

1. Site self-selection. Workfare is not politically acceptable everywhere.

2. The San Diego job market tightened over the course of the experiment. This affected the applicant mix. The study attempts to compare earlier with later cohorts to examine this.

3. The work experience programs seem to have been unusually well run, without any sense of makework or strong client resentment detected in the process analysis. It is not clear that other counties could replicate that success.

Replicability: Replicable, but see above.

Generalizability: Generalization to the full AFDC population tested in the SWIM experiment.

SATURATION WORK INITIATIVE MODEL (SWIM)

Information source: Gayle Hamilton, Interim Report on the Saturation Work Initiative Model in San Diego, Manpower Demonstration Research Corporation, August 1988. Gayle Hamilton and Daniel Friedlander, Final Report on the Saturation Work Initiative Model, Manpower Demonstration Research Corporation, November 1989.

Funding source: See Introduction. Key personnel: For California, Steve Munro.

Treatment administrator: The Welfare Department of San Diego County. Key personnel: John Robbins.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Gayle Hamilton.

Enabling legislation: See Introduction.

Total cost: See Introduction.

Dates: July 1985-September 1987; data collected July 1985-March 1988; final report November 1989.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment site: San Diego, California.

Number of treatment groups: Two (with one control group).

Treatment tested:

1. Experimentals received a fixed program sequence. They were initially assigned to a two-week job search workshop. Those who had not found jobs at the end of two weeks were assigned to three months of unpaid work experience and biweekly Job Club sessions. Those still unemployed after completing the work experience assignment would be assessed and possibly referred to community education and training programs, which were not part of the experiment proper.

2. Controls received no services from the experiment.

Sample size: Experimentals, 1,604; controls, 1,607.

Target population: WIN eligibles (single heads of AFDC households with children aged six or older, principal earners of two-parent AFDC-U households), both registrants and new applicants.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Welfare receipt.

Research components:

Process analysis: Conducted.

Impact analysis: Conducted with OLS.

Benefit-cost analysis: Conducted.

Major findings:

1. "For AFDC (regular) registrants, SWIM led to sustained gains in employment and earnings and sustained reductions in welfare receipt and payments. During the 2 years following random assignment, experimentals had average earnings of $4,932 and controls had average earnings of $3,923, for a program effect of $1,009, a 26 percent increase over the control group mean. Over these 2 years, 63 percent of experimentals were employed at some point compared to 51 percent of controls, a 12 percentage point improvement. The data suggest that most of the earnings gains resulted from increased employment among experimentals rather than greater earnings during employment. For AFDC recipients--the more disadvantaged part of the sample--the employment and earnings impacts were strong and sustained; for AFDC applicants, initial employment and earnings gains declined substantially by the end of the follow-up period."

2. "During the follow-up period, experimentals received $8,590 in welfare payments, $1,097 less than the control group mean payments of $9,687, a saving of 11 percent. By the end of the follow-up period, 48 percent of experimentals were receiving welfare payments compared to 55 percent of controls, a 7 percentage point reduction in welfare use. Both applicants and recipients experienced sustained welfare grant reductions."

3. The experimental treatment appeared to raise earnings and lower welfare payments for AFDC-U recipients. However, sample sizes were small.

4. "Net program costs were $919 per AFDC experimental and $817 per AFDC-U experimental.... Over the five-year period (which includes projections) SWIM produced substantial net savings for governmental budgets, amounting to more than $1,500 per experimental among both AFDC and AFDC-U registrants."

5. From the perspective of all subjects, the treatment had little effect on net income over the five-year period, suggesting that earnings increases and welfare reductions were roughly offsetting. However, some subgroups had net gains and others net losses.

Time trends in findings: Experimental/control differences among recipients do not seem to diminish over time, although these differences do diminish over time among applicants.

Problems and issues:

1. San Diego County volunteered to perform this program. It is a natural outgrowth of existing rigorous welfare employment programs, which enjoy widespread political support. Possible site self-selection bias.

2. Program operated in a tight labor market.

Replicability: Replicable.

Generalizability: See "Problems and issues."

BALTIMORE OPTIONS PROGRAM (Maryland Employment Initiatives)

Information sources: Daniel Friedlander, Gregory Hoerz, David A. Long, and Janet Quint, Maryland: Final Report on the Employment Initiatives Evaluation, Manpower Demonstration Research Corporation, December 1985.

Daniel Friedlander, Maryland: Supplemental Report on the Baltimore Options Program, Manpower Demonstration Research Corporation, October 1987.

Janet Quint with Joseph Ball (deceased), Barbara S. Goldman, Judith M. Gueron, and Gayle Hamilton, Interim Findings from the Maryland Employment Initiatives Program, Manpower Demonstration Research Corporation, February 1984.

Maryland Department of Human Resources, "Final Evaluation of Maryland's Grant Diversion/OJT Demonstration Program," Office of Welfare Employment Policy, August 28, 1987.

Funding source: See Introduction.

Treatment administrator: Maryland Department of Human Resources. Key personnel: Alvin Truesdale.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Daniel Friedlander.

Enabling legislation: OBRA. See Introduction.

Total cost: $1,000 per experimental for the program costs, or $1.4 million (1983).

Dates: November 1982-December 1983; data collected November 1982-March 1985; final report December 1985.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment site: Baltimore County, Maryland.

Number of treatment groups: Two (with one control group).

Treatments tested:

1. The experimental treatment consisted of a packet of options to enhance the employability of the subject, depending on her own perceived needs. The options included training programs, GED tutoring, job search (both group and individual), and work experience with on-the-job training funded in part by diversion of the AFDC grant.

2. Controls received regular WIN services. In fact, very few of them received any formal services.

Sample size: Experimentals, 1,362; controls, 1,395.

Target population: New WIN mandatories (mostly AFDC recipients with no children under six years old) and new applicants for AFDC who were in WIN-mandatory categories.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Welfare receipt.

Research components:

Process analysis: Conducted. Operation of the program components is extensively reported.

Impact analysis: Used ordinary least squares regressions.

Benefit-cost analysis: Conducted from taxpayer, recipient, and social perspectives.

Major findings:

1. Average earnings per control for the three-year follow-up were $6,595 compared with $7,638 for experimentals. The 16% difference is statistically significant.

2. The experimental treatment produced persistent increases in earnings for sample members who lacked recent work experience (over half the sample).

3. The earnings gains of experimentals were not accompanied by aggregate reductions in welfare incidence or grant expenditures.

4. From a budgetary perspective, the experimental treatment cost more than it saved in AFDC payments.

Time trends in findings: Employment impacts decline slightly over time, earnings impacts increase slightly. Insignificance of treatment for welfare receipt is lasting.

Problems and issues:

1. The Baltimore Job Training Partnership Act agency "is nationally recognized and locally influential ... and had lobbied vigorously to run the new Options program." Possible site self-selection bias.

2. The authors suggest several reasons for the paradox of higher earnings without lower benefits. First, Baltimore was somewhat more generous than other areas in counting work-related expenses. Second, some of the differential occurred among individuals who would have moved off welfare in any case. Third, information about earnings was often not communicated to the income maintenance workers either by the Options staff or by the recipients.

Replicability: Package of options is replicable.

Generalizability: There is no obvious limitation on the generalizability of the demonstration.

ARKANSAS WORK PROGRAM

<u>Information source</u>: Daniel Friedlander, Gregory Hoerz, Janet Quint, and James Riccio, <u>Final Report on the WORK Program in Two Counties</u>, Manpower Demonstration Research Corporation, September 1985.

<u>Funding source</u>: See Introduction. In addition to the Ford Foundation, the Dept. of Health and Human Services, and the state, this study received funding from the Winthrop Rockefeller Foundation.

<u>Treatment administrator</u>: Arkansas Department of Human Services. Key personnel: Jerry Evans.

<u>Evaluator</u>: Manpower Demonstration Research Corporation. Key personnel: Daniel Friedlander.

<u>Enabling legislation</u>: OBRA. See Introduction. Special federal waiver obtained to mandate WIN status for some parents of children between the ages of three and six.

<u>Total cost</u>: See Introduction. Administrative cost of $158 per experimental, or about $92,000 (1983).

<u>Dates</u>: Intake June 1983-March 1984; data collected through January 1985; final report September 1985.

<u>Dissemination effort</u>: See Introduction.

<u>Policy deliberations pertaining to treatment</u>: See Introduction.

<u>Location of treatment sites</u>: Pulaski County (Little Rock) and Jefferson County (Pine Bluff), Arkansas.

<u>Number of treatment groups</u>: Two (with one control group).

<u>Treatments tested</u>:

1. Experimentals received a fixed sequence of services, beginning with group job search (Job Club model), followed by individual job search, followed by work experience, i.e., Workfare. Few people ever participated in work experience, which was limited to 12 weeks and 20 to 30 hours per week. The most job-ready individuals could skip the Job Club.

2. Controls received virtually no services.

<u>Sample size</u>: 570 controls, 583 experimentals.

<u>Target population</u>: AFDC applicants and WIN-mandatory recipients. This included recipients with children between three and six, but in practice those with severe employment barriers were excluded from the research sample.

<u>Outcomes of interest</u>: (1) Levels of program participation. (2) Employment. (3) Earnings. (4) Welfare recipiency. (5) Welfare payments.

Research components:

Process analysis: Conducted with emphasis on program participation.

Impact analysis: Conducted with ordinary least squares.

Benefit-cost analysis: Conducted from taxpayer, recipient, and social perspectives.

Major findings:

1. 38% of the experimentals actually participated in at least one part of the program; 5% were sanctioned.

2. Employment rates increased by about five percentage points.

3. Earnings also improved.

4. There were substantial reductions in the incidence of welfare and in the amounts of welfare receipts. For example, in the third and last follow-up quarter, welfare receipts were reduced by about 15%.

5. The program resulted in modest positive net benefits from the social, budgetary, and taxpayer perspectives. There were net losses from the recipient perspective. Budgetary expenditures in running the program were small (about $250 per participant).

Time trends in findings: Short follow-up.

Problems and issues:

1. A high priority was placed on treating the most job-ready clients first. With less creaming, the apparent effectiveness of the program might be lower.

2. It is not clear that the Workfare program can be expanded to treat the entire population.

3. As with most employment programs, private-sector displacement bias is a possibility.

4. Local staff were given considerable discretion to give participation exemptions and to decide which recipients were suitable for which components.

5. Staff turnover in Jefferson County probably reduced the treatment impact, because the Job Club component was less well run.

Replicability: Replicable.

Generalizability: This experiment can be compared to the Job Club experiments, which generally found larger effects. The level of unemployment was high in both counties: 7.7% in Pulaski and 10.5% in Jefferson.

WEST VIRGINIA COMMUNITY WORK EXPERIENCE DEMONSTRATION

Information sources: Daniel Friedlander, Marjorie Erickson, Gayle Hamilton, and Virginia Knox, ·
West Virginia: Final Report on the Community Work Experience Demonstrations, Manpower
Demonstration Research Corporation, September 1986. Joseph Ball (deceased) with Gayle
Hamilton, Gregory Hoerz, Barbara S. Goldman, and Judith M. Gueron, West Virginia: Interim
Findings on the Community Work Experience Demonstrations, Manpower Demonstration
Research Corporation, November 1984.

Funding source: See Introduction. Besides the Ford Foundation, the Dept. of Health and Human
Services, and West Virginia, the Claude Worthington Benedum Foundation.

Treatment administrator: West Virginia Department of Human Services.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Joseph Ball
(deceased) and Daniel Friedlander.

Enabling legislation: OBRA. See Introduction.

Total cost: $277 per experimental, or about $513,000 (1983).

Dates: Intake July 1983-April 1984; data collected through January 1986; final report September
1986.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment sites: 21 counties in West Virginia.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals: Mandatory Workfare in return for AFDC benefits, with no time
limit on the obligation. Controls were excluded from the Workfare program.

Sample size: Experimentals, 1,853; controls, 1,841.

Target population: All female WIN-mandatory AFDC recipients and both new registrants and prior
registrants. As elsewhere, this excludes parents of children under age six. Also excluded were
recipients already enrolled in full-time school or training, people who were already employed,
and WIN volunteers.

Outcomes of interest: (1) Level of participation. (2) Earnings. (3) AFDC recipiency and
payments.

Research components:

Process analysis: Conducted. Major emphasis on clients' attitudes toward jobs and
supervisors' attitudes toward clients.

Impact analysis: Conducted with OLS regressions.

Benefit-cost analysis: Conducted.

Major findings:

1. Negligible effect on earnings.

2. Small reductions in transfer payments.

3. Net social benefit of the program is positive, because the cost of running it is small and the output of experimentals was large. (In fact, their productivity was rated slightly higher than that of regular employees.)

Time trends in findings: Participation rates higher in the first six months.

Problems and issues:

1. The possible differential bias from experimentals migrating out of state to avoid the Workfare obligation while controls remain in-state is not addressed.

2. The difficulty (or lack thereof) of job finding or creation or both is not addressed in report.

3. West Virginia had (and continues to have) a high unemployment rate, which may affect the findings, and might imply a displacement effect.

4. Sanctions for refusal to participate in Workfare were rare.

5. Valuation of program output may be problematic.

Replicability: Replicable.

Generalizability: The West Virginia AFDC population is not representative of the U.S. AFDC population as a whole, nor is its level of unemployment. Also, the state's fiscal distress results in a large number of readily identified community needs for which Workfare is not only appropriate but is the only affordable source of labor that can meet them.

VIRGINIA EMPLOYMENT SERVICES PROGRAM

Information source: James Riccio, George Cave, Stephen Freedman, and Marilyn Price, Virginia:
Final Report on the Virginia Employment Services Program, Manpower Demonstration
Research Corporation, August 1986.

Funding source: See Introduction.

Treatment administrator: Virginia Department of Social Services. Key personnel: Local agency
heads listed in report.

Evaluator: MDRC. Key personnel: James Riccio.

Enabling legislation: OBRA.

Total cost: See Introduction. Administrative cost of $388 per experimental, or about $920,000
total (1984).

Dates: Intake August 1983-September 1984; data collected through August 1985; final report
August 1986.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment sites: 11 Virginia counties: Fairfax, Newport News, Hampton, Chesapeake,
Henry, Martinsville, Carroll, Grayson, Galax, Campbell, and Pittsylvania.

Number of treatment groups: Three (with one control group).

Treatment tested: Job Search/Work Experience. Participants were required to provide proof of
three contacts with potential employers. If these contacts were unsuccessful, participants were
then offered job search assistance, either individual or group, and community work experience,
but in practice these were not mandatory. Group job search did not include a telephone bank
for supervised employer contacts.

All ESP Services. In addition to the above treatment, it was intended that these experimentals
have available greater education or training options. In fact, there were no additional resources
for education or training for this group, so there was in fact no separate treatment.

Controls were not subject to either treatment.

Sample size: Job Search/Work Experience, 1,061; all ESP Services, 1,077; controls, 1,046.

Target population: WIN-mandatory female clients who did not fall into the following excluded
categories: parents of children under six, those already in education or training programs, WIN
volunteers, and those already assigned to nonexperimental treatment.

Outcomes of interest: (1) Participation in services. (2) Employment. (3) Earnings. (4) AFDC

recipiency and payments.

Research components:

Process analysis: Conducted. Emphasis on what treatments were actually received.

Impact analysis: Conducted with OLS.

Benefit-cost analysis: Conducted.

Major findings:

Because treatments in the two experimental groups did not in fact differ, findings reported concentrate on differences between experimentals and controls.

Outcome	Experimentals	Controls
Ever employed, 2nd-4th quarters after random assignment	43.8%	40.5%
Average total earnings, those quarters	$1,119	$1,038
Received any AFDC payment, those quarters	86.0%	86.1%
Total payments received, including quarter of assignment	$1,923	$2,007

The differences are significant in the first and fourth rows.

Differences were larger in urban than in rural areas, although sample sizes in rural areas would make it difficult to find an impact. The authors find increasing, long-term employment gains among applicants and short-term, temporary welfare savings among recipients.

From the subjects' perspective, the treatment resulted in income gains. From the perspective of government budgets, the treatment produced net positive benefits within five years for applicants; costs and benefits were roughly equal for recipients.

Time trends in findings: Employment gains from the treatment among applicants concentrated in later periods.

Problems and issues:

1. An element of site self-selection may bias the findings.

2. Treatments varied across sites, and it is impossible to determine whether this caused the impacts to vary as well.

3. The authors note that Virginia is a relatively low-benefit state, and employable individuals will be less attracted to AFDC than they will be in higher-benefit states.

4. Virginia also has a highly decentralized welfare system. Treatment-standardization problems

encountered in this experiment would be characteristic of policy-implementation problems in decentralized systems.

5. It is not clear why this treatment should be producing long-term rather than short-term employment gains. Benefit-cost analysis based on projections to five years from eighteen months of data may not be robust.

Replicability: Replicable.

Generalizability: Doubtful. See "Problems and issues."

MAINE TRAINING OPPORTUNITIES IN THE PRIVATE SECTOR (TOPS)

Information source: Patricia Auspos, George Cave, and David A. Long, <u>Maine: Final Report on the Training Opportunities in the Private Sector Program</u>, Manpower Demonstration Research Corporation, April 1988.

Funding source: See Introduction.

Treatment administrator: Maine Department of Human Services. Key personnel: Tomie McLean.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Patricia Auspos.

Enabling legislation: OBRA. See Introduction.

Total cost: About $2,600 per experimental, or $770,000 (1984).

Dates: October 1983-June 1985; data collected October 1983-September 1987; final report April 1988.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment: Statewide.

Number of treatment groups: Two (with one control group).

Treatments tested: Experimentals received the following prescribed treatment-sequence: Pre-Vocational Training lasted two to five weeks, stressing personal growth and job-seeking and job-holding skills. Work Experience consisted of 20 hours per week of unpaid work in public or nonprofit sectors for up to 12 weeks. Those who demonstrated motivation and skills acquisition were eligible for on-the-job training (OJT) positions, preferably private-sector. The training period was limited to a maximum of six months and the employer subsidy was set at 50% of wages; the subsidy was funded by grant diversion. Controls could receive other WIN services from the treatment administrator.

Sample size: 297 experimentals, 147 controls.

Target population: The experiment was open only to applicants. Applicants who met the program criteria were then randomly assigned an experimental or a control status. Applicants had to meet the following criteria: (1) single heads of households, (2) AFDC recipients for at least six months, (3) not currently employed, and (4) able to read at the level of the materials used in Pre-Vocational Training. In addition, the intake staff used their discretion to screen out women who had (a) child care, transportation, health, or other problems that could interfere with participation, (b) low motivation, or (c) unrealistic ambitions. About three out of 10 applicants were considered inappropriate and screened out prior to random assignment.

Outcomes of interest: (1) Earnings. (2) Employment. (3) AFDC payments.

Research components:

Process analysis: Conducted.

Impact analysis: Conducted as a difference in regression-adjusted means.

Benefit-cost analysis: Conducted.

Major findings:

1. TOPS had sustained positive impacts on earnings over the entire follow-up period (second through eleventh quarters). Experimentals had earnings of $7,344 on average, controls $5,599, and the difference is statistically significant. However, differences in employment, AFDC recipiency, and AFDC payments were in general insignificant.

2. The authors speculate that the discrepancy between the earnings increase and the nonreduction in AFDC payments may be the result of three factors: (1) relatively generous allowances for work-related expenses; (2) substantial numbers of participants were already working, thus at some point their earnings exceeded the AFDC breakeven-level and further earnings would not result in further benefit reductions; and (3) experimentals were trained in how to become "your own best advocate" within the welfare system.

3. Because there were no AFDC payment reductions, there was no positive budgetary impact. However, TOPS raised the income of experimentals about $3,000 each at a cost of about $1,100. "TOPS can be viewed as a much more efficient means of transferring income to this group than simply raising their AFDC benefits."

Time trends in findings: The earnings effect grows stronger over time.

Problems and issues: High degree of selectivity in the initial screening of the research sample makes it difficult to generalize the results.

Replicability: Replicable.

Generalizability: The Maine AFDC population is unrepresentative of the U.S. AFDC population as a whole. It is mostly white, and historically many recipients worked, often full-time. A well-run OJT program funded by grant diversion in a state with low unemployment could be an effective but very limited part of the overall welfare strategy in other states as well.

FLORIDA TRADE WELFARE FOR WORK

Information source: Florida Department of Health and Rehabilitative Services, Office of the
    Inspector General, Office of Evaluation and Management Review, "Evaluation of the TRADE
    Welfare for Work Program," February 1987.

Funding source: U.S. Department of Health and Human Services, Office of Family Assistance.

Treatment administrators: Florida Department of Health and Rehabilitative Services and local JTPA
    agencies. Key personnel: Jim Clark.

Evaluator: The experiment was designed by Manpower Demonstration Research Corporation,
    which is not associated with the evaluation. The latter was performed by the Department of
    Health and Rehabilitative Services, Office of the Inspector General, Office of Evaluation and
    Management Review. Key personnel: Dawn Case.

Enabling legislation: Florida Public Assistance Productivity Act.

Total cost: $734,363; research only $163,191.

Dates: July 1984-July 1986; data collected through September 1986; final report February 1987.

Dissemination effort:

    Briefings: None.

    Testimony: None.

    Distribution of executive summaries: Dept. of Health and Human Services staff.

Policy deliberations pertaining to treatment: Grant diversion to wage subsidies is used very
    selectively in Florida--500 to 600 cases a year, according to Clark--because it involves
    substantial paperwork burdens for the welfare staff and employers are not very interested in the
    short-term wage subsidy. Clark states that private employers had little interest in the program
    for various reasons.   One of these was the separation of people who hire and supervise staff
    from the accounting department (the people who observed the cost savings were not the people
    who made the relevant decisions). Another was that most employers were much less interested
    in marginal wage savings and much more interested in the reliability of potential employees.

Location of treatment sites: 17 of the state's 24 JTPA agencies, covering most of the population of
    Florida.

Number of treatment groups: Three (with two control groups).

Treatment tested: The WIN regime in Florida consisted of a two-week job search in which clients
    looked for jobs on their own and were expected to complete at least six job applications (clients
    would be subject to sanctioning if they failed to do so). Those who did not find employment
    within two weeks were required to attend job clubs. Those who did not find employment
    within the Job Club time limits were randomly assigned to one of three groups:

1. WIN controls were placed in "extended job search," during which they were expected to fill out six employment applications a month and to address deficiencies in their employability. (In practice, sanctions for all three groups appear to have been rare.) The usual expenditure subsidies for travel, work clothes, and child care were available. Subsidies could continue for up to three months after employment.

2. JTPA controls were referred to the local Job Training Partnership Act agency. If found "appropriate" for on-the-job training (OJT), they could be placed with a private employer, who would receive a 50% subsidy on their wages (always the legal minimum) for a period not to exceed six months. If not found suitable, they were referred back to WIN.

3. TRADE experimentals were also referred to JTPA. If found "appropriate" for OJT, they could be placed with a private employer who received both the 50% JTPA subsidy and an additional $1.10 an hour subsidy from diversion of AFDC grants. Thus, an hour's work, at the minimum wage, would cost the employer 58 cents.

Sample size: TRADE experimentals, 2,617; JTPA controls, 1,024; WIN controls, 934.

Target population: WIN-mandatory and WIN-voluntary female clients. Usual exemptions for children under six, illness, or disability.

Outcomes of interest: (1) Unsubsidized employment. (2) Increase in JTPA placements of AFDC recipients. (3) AFDC payments.

Research components:

Process analysis: Conducted. Based on staff interviews and a survey questionnaire to subjects and employers.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Cost-effectiveness analysis.

Major findings:

1. The evaluators found statistically significant differences between TRADE and JTPA subjects on the one hand, and WIN controls on the other, in months on AFDC, employment, and earnings. They found no differences between TRADE experimentals and JTPA controls. These findings are all based on extremely small samples (see "Problems and issues").

2. Only 9% of the TRADE experimentals were actually placed in OJT, compared with 7.5% of the JTPAs and 5.8% of the WIN controls. One-third of the TRADE placements were without the special $1.10 subsidy. The program goal was OJT placement of one-half of all experimentals.

3. Cost-effectiveness analysis suggests savings from the program, but the analysts themselves say the data are not adequate to support any firm conclusions.

Time trends in findings: Not applicable.

Problems and issues:

1. 27% of experimentals, 25% of JTPA controls, and 23% of WIN controls appeared in the JTPA Information system. The implication for the first two groups is that the majority of "clients were never referred to the SDAs (JTPA agencies), were referred but did not go, or were referred but were not registered...." The WIN controls should not have appeared in the system. The implication of their appearance there is that "participants went to the SDAs on their own, or were inappropriately referred to them." Breakdown of WIN supervision and/or the random assignment system looks very likely, especially since some WIN personnel with TRADE responsibilities reported that they had received no training or direction in the program.

2. The DHRS information system on the subjects of the experiment could not be utilized "due to the lack of accurate and available data in the system on key indicators."

3. For some reason, which the report does not explain, the evaluators obtained the unemployment insurance and AFDC payment records of only a random sample of subjects, not the entire research sample. "Sufficient information was available for only 125 TRADE group participants, 99 JTPA group participants, and 126 WIN group participants." The data used came from the routine quality control audit performed on a random sample of the welfare population.

4. The report is quite vague on what difference was supposed to exist between the treatment of JTPA controls by the SDAs and the treatment of TRADE experimentals. The SDA staffs refused to attempt to place people in OJT who were "inappropriate" because they lacked skills, training, or education. But the lack of these attributes was presumably the rationale for the deep wage subsidy. The leading cause for the lack of placements, however, was that the experimentals never showed up at the SDA offices.

5. The report seems to have been hastily assembled.

Replicability: Not replicable because of the unclarity cited in "Problems and issues," #4.

Generalizability: Interagency coordination failure severely limits the generalizability of the findings. Other wage subsidy experiments, however, also have found very low employer responsiveness.

NEW JERSEY GRANT DIVERSION PROJECT

Information source: Stephen Freedman, Jan Bryant, and George Cave, New Jersey: Final Report of the Grant Diversion Project, Manpower Demonstration Research Corporation, November 1988.

Funding source: See Introduction.

Treatment administrators: New Jersey Department of Human Services and the New Jersey Department of Labor. Key personnel: Sybil Stokes, Rowena Bopp (DHS), and Sally Hall (DOL).

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Stephen Freedman.

Enabling legislation: OBRA.

Total cost: See Introduction. Administrative cost of $921 per experimental, or about $910,000 (1985).

Dates: October 1984-June 1987; data collected through August 1987; final report November 1988.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction.

Location of treatment sites: Nine counties in New Jersey (Atlantic, Burlington, Camden, Essex, Hudson, Mercer, Middlesex, Monmouth, and Passaic) which include all the major cities.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals were allowed to volunteer for OJT with private-sector employers. 50% of wages were subsidized through diversion of the AFDC grant. Controls were not offered this opportunity, but remained eligible for other WIN services.

Sample size: 988 experimentals, 955 controls. Much of the analysis, however, is based on a subsample of 508 experimentals and 488 controls, the "early sample."

Target population: Job-ready, WIN-eligible AFDC recipients.

Outcomes of interest: (1) Employment. (2) Earnings. (3) AFDC recipiency and payments.

Research components:

   Process analysis: Conducted.

   Impact analysis: Conducted with OLS regressions.

   Benefit-cost analysis: Conducted.

Major findings:

1. "Nearly 43 percent of all experimentals worked at some point in an OJT position." The low rate of placement (200 a year) was attributed to various factors, one of them being the high turnover rate among job developers. About half of the placements completed the prescribed subsidy period (averaging ten weeks), and nearly all of those who did were kept on by the employer without subsidy.

2. The treatment led to substantial employment gains in the first two quarters for experimentals versus controls. The impact essentially vanished by the fourth quarter.

3. The treatment "produced a statistically significant earnings gain of $634 during the first year after random assignment. Average earnings for experimentals were 22 percent higher than average earnings for controls." In combination with the first finding, this suggests that "experimentals worked in jobs that either paid more or provided more hours of employment than the jobs in which controls were employed."

4. "Experimentals spent fewer months on AFDC and received $265 less in welfare payments than controls during the first year after random assignment." The difference is statistically significant.

5. Benefit-Cost Analysis: "Over a five-year period, enrollees ... are likely to benefit by an estimated $971 to $1,554 per person.... From the perspective of government budgets, the program can be expected to pay for itself within about two and one-half years. Net savings of between $601 and $1,284 are likely over a five-year period."

Time trends in findings: Experimental/control differences in employment were insignificant in the early sample. In quarters five through seven, however, experimentals averaged $468 more in earnings than controls and $238 less in AFDC payments. This latter difference was declining.

Problems and issues: The early sample was less disadvantaged than the remaining cohorts, because of improvements in the overall unemployment picture in the state and because Hudson and Middlesex counties, which joined the experiment at a later point than the other seven, had higher numbers of very disadvantaged recipients.

Replicability: Replicable in principle, but the treatment is complex.

Generalizability: A well-run OJT program funded by grant diversion in a state with low unemployment could be an effective but very limited part of the overall welfare strategy in other states as well. WIN registration in the nine counties averaged about 80,000 per month. About 20 OJT placements per month were made.

COOK COUNTY JOB SEARCH AND WORK EXPERIENCE

Information source: Daniel Friedlander, Stephen Freedman, Gayle Hamilton, and Janet Quint, "Illinois: Final Report on Job Search and Work Experience in Cook County," November 1987.

Funding sources: Ford Foundation and the Illinois Department of Public Aid. Key personnel: Gordon Berlin, from the Ford Foundation.

Treatment administrator: Illinois Department of Public Aid. Key personnel: Randale Valenti.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Daniel Friedlander.

Enabling legislation: OBRA.

Total cost: Estimated incremental cost per experimental: Group 1, $157; group 2, $127 (about $636,000 and $515,000 [1985], respectively).

Dates: Intake February 1985-September 1985; data collected February 1985-March 1987; final report November 1987.

Dissemination effort: See Introduction.

Policy deliberations pertaining to treatment: See Introduction. Illinois subsequently changed the focus of its WIN program from the techniques employed in this experiment to a greater encouragement of education and training and less frequent application of sanctions.

Location of treatment site: Cook County, Illinois.

Number of treatment groups: Three (with one control group).

Treatment tested:

Experimental group 1 was assigned first to WIN orientation and Independent Job Search (IJS), under which they were expected to contact 20 potential employers a month on their own; progress was monitored in biweekly two-hour group meetings. If unsuccessful in finding work, members were assigned thereafter to any of the other program components: the IWEP (Illinois Work Experience Program, i.e., Workfare), Pre-Employment (educational- or vocational-skills training programs), or Modified Job Search (a holding status). However, the program staff did not emphasize alternatives other than the IWEP. Program staff were evaluated largely on the basis of the grant reductions they achieved, and tended to sanction recipients automatically for failure to satisfy program requirements.

Experimental group 2 was assigned just as group 1 was except for exclusion from the IWEP.

Controls were required to attend orientation (could be sanctioned if they failed to) but were eligible thereafter only for support services if independently involved in educational or training activities.

Sample size: Group 1, 4,050; group 2, 4,057; controls, 3,805.

<u>Target population</u>: WIN-eligible AFDC recipients (but not AFDC-U recipients).

<u>Outcomes of interest</u>: (1) Employment. (2) Earnings. (3) AFDC payments.

<u>Research components</u>:

Process analysis: Conducted.

Impact analysis: Comparison of means and OLS.

Benefit-cost analysis: Conducted.

<u>Major findings</u>:

1. Experimental/control differences in employment and earnings were not statistically significant.

2. Average AFDC payments, six quarters: Group 1, $4,416; group 2, $4,346; controls, $4,486. Differences of $70 and $140, respectively, are statistically significant. "The welfare savings were achieved mainly through closing welfare grants, rather than from reductions in the dollar amounts of grants that remained open. There does not appear to be any clear additional effect from the IWEP component over and above the effect of IJS."

3. "The reduction in AFDC and Medicaid expenditures, combined with the very low cost of the program, led to a net savings for the government. However, the welfare recipients assigned to the program did not benefit financially ... since the losses in AFDC and Medicaid equaled or exceeded any earnings gains that might have occurred."

4. 7.6% of controls were sanctioned for failing to attend the WIN orientation. 11.7% of experimentals had been sanctioned within nine months after intake, either for the same reason or for failing to satisfy subsequent program requirements.

5. The authors can only speculate about the source of the AFDC savings, since they occurred in the absence of any earnings increase. Staff were more familiar with experimentals' activities, and therefore initiated more grant reduction actions because they learned of new employment more quickly. Also, the IJS program required regular attendance at various tasks, and may have served as a deterrent to continued recipiency for experimentals with unreported income. Controls with unreported income had no such deterrent.

<u>Time trends in findings</u>: Group 1 (Workfare) experimentals had higher AFDC payments, a tendency which seemed to grow over time.

<u>Problems and issues</u>:

1. A sufficiently strong deterrent-effect from the treatments might have sent experimentals across state lines, causing a possible attrition bias not addressed here.

2. The higher motivation of the administrative staff to generate AFDC payment savings in this experiment compared with the other experiments is worth noting. It might explain why this experiment detected such savings, while many of the others did not.

Replicability: Replicable.

Generalizability: The demonstration took place in two-thirds of the WIN offices of Cook County, which includes Chicago. This represents a very large urban population.

WASHINGTON COMMUNITY WORK EXPERIENCE PROGRAM

Information source: Hal Nelson, "Evaluation of the Community Work Experience Program," Washington Department of Social and Health Services, Division of Administration and Personnel, Office of Research and Data Analysis, Program Research and Evaluation Section, Report #06-23, June 1984.

Funding source: U.S. Dept. of Health and Human Services, Social Security Administration. Key personnel: Nelson does not recall.

Treatment administrator: Washington Department of Social and Health Services. Key personnel: Gladys McCorkhill and Judith Merchant.

Evaluator: Dept. of Social and Health Services, Office of Research and Data Analysis. Key personnel: Hal Nelson.

Enabling legislation: OBRA.

Total cost: Administrative cost $85,565 (1982).

Dates: October 1982-May 1983; data collected through August 1983; final report June 1984.

Dissemination effort:

Briefings: None.

Testimony: None.

Distribution of executive summaries: To legislature and other agencies.

Policy deliberations pertaining to treatment: There was no effect on subsequent policy, according to Nelson.

Location of treatment sites: Spokane County and Pierce County (which includes Tacoma), Washington.

Treatment tested:

1. Community Work Experience Program (CWEP). Workfare placement with public or nonprofit agencies for four months. Child care and transportation reimbursement were provided. "Persons assigned to CWEP who refused to work were counseled by the CWEP coordinator and if a reasonable settlement was not achieved, these persons could be sanctioned. The sanction discontinued the client's portion of support from her grant for three months."

2. Employment and Training (E&T). A Job Club-based model, with three or four days of group job preparation, followed by five or six days in a phone lab, followed by individual job search.

3. Controls. No treatment. Presumably eligible for some WIN services.

Number of treatment groups: Three (with one control group).

Sample size: CWEP, 64; E&T, 66; controls, 42.

Target population: WIN-mandatory AFDC recipients. Exclusions for persons deemed more or less unemployable.

Outcomes of interest: (1) Employment. (2) AFDC case closures and savings.

Research components:

Process analysis: Not conducted.

Impact analysis: Comparison of means. No tests of statistical significance.

Benefit-cost analysis: Budgetary perspective only.

Major findings:

1. 30% of CWEP clients found unsubsidized employment after participating in it, as did 39% of E&T clients; 14% of controls "were employed at some time during the one-year study period."

2. The author found evidence that CWEP was more useful than E&T to clients with less than a high-school education.

3. Program expenditures exceeded AFDC payment reductions during the study year.

Time trends in findings: Only one year of data.

Problems and issues:

1. The sample sizes, which are substantially smaller than planned in the research design, are too small for useful analysis. Sample sizes were kept low by failure of the welfare staff to refer clients to WIN (which is run by Employment Security, a separate agency) and by discretionary removal of 63 of them from the treatment samples, either by the program staff or the evaluators. The discretionary removal of 37% of the sample appears to open the door to selection bias either by the clients themselves or by others. Much of this was caused by reliance on client follow-up interviews as the primary data source on outcomes.

2. No follow-up interviews were conducted with controls, so the data sources used for experimentals and controls do not match. In fact it is not clear what data source was used for controls.

3. A subsequent doctoral dissertation by Nancy Dickinson found no statistically significant effects on employment from CWEP, according to Nelson. Dickinson conducted a follow-up survey as part of that dissertation.

Replicability: Replicable.

Generalizability: None. Sample is too small.

FREESTANDING HOUSING VOUCHER DEMONSTRATION

Information source: Mireille L. Leger and Stephen D. Kennedy, Final Comprehensive Report of the Freestanding Housing Voucher Demonstration, two volumes, U.S. Department of Housing and Urban Development, Office of Policy Development and Research, May 1990.

Funding source: Dept. of Housing and Urban Development, Office of Policy Development and Research. Key personnel: David Einhorn.

Treatment administrator: 19 Public Housing Agencies (PHAs). Key personnel: None.

Evaluator: Abt Associates. Key personnel: Mireille L. Leger and Stephen D. Kennedy.

Enabling legislation: Housing and Urban/Rural Recovery Act of 1983, P.L. 98-181.

Total cost: Roughly $3 million (1987).

Dates: April 1985-September 1988; data collected: same; final report May 1990.

Dissemination effort:

Briefings: Assistant Secretary level.

Testimony: None.

Distribution of executive summaries: Available for purchase to the public. Copies sent to Congress.

Policy deliberations pertaining to treatment: Einhorn states that the administration likes vouchers, but Congress likes certificates and construction. The results of the experiment show that vouchers are both more flexible (serve more people) and more expensive (for recipients and the government alike) than certificates; there is no clearcut winner. Both sides of the argument can mine the results for evidence supporting their own positions. While the experiment was being conducted, the Reagan and Bush administrations took various incremental actions to replace certificates with vouchers through administrative action.

Location of treatment sites: Atlanta, Georgia; Boston, Massachusetts; Buffalo, New York; Cleveland, Ohio; Dayton, Ohio; Minneapolis, Minnesota; Montgomery County, Maryland; New Haven, Connecticut, New York City, New York; Oakland, California; Omaha, Nebraska; Pittsburgh, Pennsylvania; St. Petersburg, Florida; San Antonio, Texas; San Diego, California; and Seattle, Washington.

Number of treatment groups: Two (with one control group).

Treatment tested:

1. Certificate program (controls). This is the current Section 8 program. It pays a monthly stipend to the landlord on behalf of a tenant living in privately owned, existing housing. The amount of the payment is the difference between the rent (plus certain scheduled utility allowances, if they are not included in the rent) and the tenant's contribution, which is essentially 30 percent of income. Tenants must live in a unit meeting HUD's housing quality criteria, and the rent must be less than or equal to the local Fair Market Rent (set by HUD) and judged "reasonable" by the Public Housing Agency (PHA). From the time of enrollment into the program, tenants have two to four months to find acceptable housing under the program.

2. Housing Voucher program (experimentals). This treatment differs from the current program in the payment formula; the housing unit must still meet HUD quality criteria. The housing assistance payment is equal to $\underline{P} - .3\underline{Y}$, where $\underline{P}$ is the local rental payment standard, initially set equal to the Fair Market Rent, or FMR, and $\underline{Y}$ is income. Thus, the Public Housing Agency no longer sets a ceiling on gross rent. The tenant has, on the one hand, an incentive to obtain housing at a lower cost than the FMR, if it can be found, and, on the other hand, the option to secure housing that costs more than the PHA would allow under the Certificate program.

Sample size: 12,390, evenly divided. Many of the analyses, however, use subsamples of about 4,500.

Target population: Lower-income families certified as eligible for Section 8 who live in large urban areas.

Outcomes of interest: (1) Success rate (percentage of those enrolled who find acceptable units and become recipients). (2) Rent payments. (3) Rent burdens. (4) Program payments. (5) Administrative costs.

Research components:

Process analysis: Not conducted.

Impact analysis: Comparison of means and OLS.

Benefit-cost analysis: Not conducted.

Major findings:

	Voucher	Certificate
Success rate, overall	64.6%	61.0%*
Success rate when P=FMR (PHAs had some discretion about changing P)	64.4	59.5*
Total rent paid by recipients	$463	$437 *
Rent burden as percentage of income:		
at initial payment	34%	31%
at annual recertification	35	31 *
among recipients who		
did not move	28	31 **
moved	39	31
Monthly assistance payments, overall average	$310	$293 **
Initial assistance payment	307	287 **
Payment at recertification	304	298
Administrative cost per slot		
Initial eligibility	$579	$598
Annual ongoing	257	261

* Difference significant at .05 percent level.
** Difference significant at .01 percent level.

Regressions on housing quality appear to show that roughly half of the higher rent payments under the Voucher plan go to improved housing quality, with the other half going to higher landlord income.

Time trends in findings: As noted under "Rent burden" and "Monthly assistance payments" in "Major findings."

Problems and issues: The most obvious problem is the absence of a process analysis. The voucher program changes the budget constraint of the Public Housing Agency as well as the subjects, and the absence of a process analysis means that we do not know how the PHAs responded or whether their responses affect experimental results.

Replicability: Replicable.

Generalizability: Designed for generalizability to the population of large urban PHAs. Two special caveats are (1) the sample is drawn from applicants for the current Section 8 program and (2) more important, many experimentals were renting from landlords with substantial Section 8 experience. If the entire program changed over to vouchers, landlord rent-setting behavior

might change as well. PHAs may have effective monopsony power with respect to a group of Section 8 landlords that tenants shopping individually cannot match.

ILLINOIS ON-LINE CROSS-MATCH DEMONSTRATION

Information source: Nancy Holden, John A. Burghardt, and James C. Ohls, "Final Report for the
    Evaluation of the Illinois On-Line Cross-Match Demonstration," Mathematica Policy Research,
    September 11, 1987.

Funding source: U.S. Department of Health and Human Services, Office of Family Assistance.
    Key personnel: Penny Pendell.

Treatment administrator: Illinois Department of Public Aid. Key personnel: Alan Whitaker.

Evaluator: Mathematica Policy Research. Key personnel: John A. Burghardt.

Enabling legislation: None.

Total cost: Administrative cost, $148,000 (1986); research only: $69,290 (1986).

Dates: May 1986-October 1986; data collected through December 1986; final report September
    1987.

Dissemination effort:

    Briefings: None.

    Testimony: None.

    Distribution of executive summaries: To central management of the Illinois Department of
    Public Aid and to the Dept. of Health and Human Services.

Policy deliberations pertaining to treatment: Whitaker states that the findings on the effects of the
    treatment were disappointing and led the state not to extend further the scope of data collection
    for on-line use by caseworkers.

Location of treatment sites: Seven Illinois welfare offices (four in Cook County and three in other
    counties).

Number of treatment groups: Two (with one control group).

Treatment tested: At the initial determination of eligibility for AFDC and at subsequent face-to-face
    redetermination meetings, a caseworker could immediately call up on a computer screen certain
    information on experimentals: marriage and death information, state payroll and retirement
    pension data, and, in some cases, school records. Caseworkers were required to perform these
    cross-match inquiries for experimentals (odd last-digit identification numbers) and the system
    would not accept inquiries for controls (even last-digit identifications). School district
    information was available for Chicago and Rockford. The tested treatment was an incremental
    increase in the information already immediately accessible to the caseworker, the most
    important of which was wage records from the Illinois Department of Employment Security.
    Under the existing system (the control treatment) the additional information on marriages,
    deaths, and so forth would be periodically updated and circulated to caseworkers in hard copy.

<u>Sample size</u>: New applicants: experimentals, 5,305; controls, 5,489. Ongoing cases: experimentals, 20,429; controls, 20,323.

<u>Target population</u>: AFDC applicants and recipients.

<u>Outcomes of interest</u>: (1) AFDC applications denied. (2) Savings in payments.

<u>Research components</u>:

Process analysis: Conducted through interviews and computer records to determine whether caseworkers actually used the system and whether they perceived it as useful. Workers who were using a fully automated system and who had access to school district records found it useful, and some of them succeeded in evading the system block on even numbers to use it on controls (thereby contaminating the control sample). Computer records showed that this happened in one office only; the authors note that although this produces a downward bias in the measured effect of the treatment in that office, it is itself evidence of perceived treatment effectiveness. Many workers did not understand the data format for state payments, marriages, and deaths.

Impact analysis: Comparison of means, OLS.

Benefit-cost analysis: Cost-effectiveness analysis.

<u>Major findings</u>: An asterisk denotes a difference that is statistically significant at the 90% confidence level.

	Experimentals	Controls
New applications denied	39.5%	40.5%
For applicants:		
Mean AFDC payments	$625.08	$609.34
Mean food stamps paid	238.24	231.49
For ongoing cases:		
Mean AFDC payments	1,571.02	1,574.22
Mean food stamps paid	774.63*	782.20

Three Cook County offices had access to school records and had a fully automated information system. In these offices, denials of new applications increased from 23.9% to 26.4% (statistically significant at the 90% level) and benefit payments were lower by $11.41 (not statistically significant).

Although some downward bias due to the contamination discussed under "Process analysis" was present, the authors do not believe it was large enough to change the analysis significantly.

Under most reasonable assumptions about benefits and costs, benefits of the system exceed costs when school records are available, but the authors do not claim that the benefits are very precisely measured.

<u>Time trends in findings</u>: There are no time trends, but there are curious discrepancies in treatment impacts across offices, which the authors discuss but cannot explain.

Problems and issues:

1. The inclusion of offices that were not fully automated (and where, accordingly, use of the additional data was more difficult) added little to the value of the experiment. Apparently it was not anticipated before the experiment that only the school records would be of much use to caseworkers.

2. The contamination of the control sample, previously noted, was not the only result of a vulnerable computer system. In particular, the authors had special trouble designating the new applicant sample, for reasons discussed in the report.

Replicability: Replicable.

Generalizability: The sample size is very large, but most results are not statistically significant. That would seem to imply that the savings are very small; on the other hand, the system costs seem to be small as well.

DIGEST OF THE SOCIAL EXPERIMENTS:

II. The Unemployed

GENERAL EDUCATION IN MANPOWER TRAINING

Information sources: William F. Brazziel, "Effects of General Education in Manpower Programs," Journal of Human Resources, volume 1, number 1, 1966, 39-44. Also Cooperative Research, HEW, Office of Education, "Reeducating Unemployed Workers."

Funding sources: U.S. Department of Labor, Office of Automation; HEW, Office of Education; and an anonymous donor. Key personnel: David Kerrico, Education.

Treatment administrator: Norfolk Division, Virginia State College. Key personnel: William Cooper, Hampton University.

Evaluator: Norfolk Division, Virginia State College. Key personnel: William F. Brazziel.

Enabling legislation: None.

Total Cost: $1.75 million (1964); research only, roughly 25%.

Dates: January 1964-June 1965; data collected through June 1966; final report 1966.

Dissemination effort:

Briefings: Labor Department.

Testimony: House Education and Labor Committee and the President's Committee on Rural Poverty.

Distribution of executive summaries: On request.

Other: Frequent presentations to universities and State Departments of Education.

Policy deliberations pertaining to treatment: Brazziel states that this experiment still informs policy; it was cited by Secretary of Labor Arthur Goldberg and assistant secretaries.

Location of treatment site: Norfolk, Virginia.

Treatment tested:

(1) Control. No treatment.

(2) Placebo. "Simulated type of occupational information and guidance," received daily. No further description in report, and Brazziel does not remember.

(3) Technical Education. One-half day of technical education, and one hour per day of supervised (but not guided) study. Classes were in auto mechanics, sheet metal, masonry, electronics, and maintenance technology (upkeep of buildings). Received $30/week stipend. Program placed graduates with employers.

(4) General Education. Went to same courses as group (3) and, in addition, one-half day of

systematic instruction in reading improvement, language arts, number skills, and occupational information. Received $30/week stipend. Program placed graduates with employers.

Number of treatment groups: Four.

Sample size: 45 in each of the four groups.

Target population: Male, unemployed for three months or more, laid off from previous work, typically for automation-related reasons.

Outcomes of interest: (1) Employment. (2) Salary per week. (3) Mobility.

Research components:

Process analysis: Considerable stress on making reading and math work both job-related and adult-oriented. Special materials prepared. Average gains of three years in reading and arithmetic for general education group. In follow-up surveys, some group (4) participants volunteered attribution of their subsequent success to reading, language, and math courses.

Impact analysis: Conducted. Comparison of means.

Benefit-cost analysis: Not conducted.

Major findings:

From follow-up interviews, one year later:

Group	Employment Rate	Average Weekly Salary	% Promoted
General Ed	95%	$83*	31
Technical Ed	74	71	25
Placebo	63	50	12
Control	59	46	12

*Statistically significant difference from technical education group, at 1% level.

Time trends in findings: Only one follow-up reported; Brazziel does not know if any were conducted subsequently.

Problems and issues: (1) Self-selection of site. (2) An enthusiastic faculty who developed special instructional materials and therefore may not be replicable elsewhere.

Replicability: In principle, fully replicable. Principles are (1) use of job-related materials in general education, (2) "andragogy not pedagogy," and (3) adequate stipends.

Generalizability: (1) Brazziel says having the program on a college campus seemed to make a positive difference in subject attitudes. (2) Jobs were available in Norfolk, but were mismatched to worker skills. The results may not generalize to the common inner-city situation in which transportation to jobs is costly.

CARBONDALE JOB FINDING CLUB

Information source: Nathan H. Azrin, T. Flores, and S. J. Kaplan, "Job-Finding Club: A Group-Assisted Program for Obtaining Employment," Behavior Research and Therapy, 1975, 17-27.

Funding source: Illinois Department of Mental Health. Key personnel: None.

Treatment administrator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Evaluator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Enabling legislation: None.

Total cost: Estimate of $200 per experimental cost increment (administration only) or $12,000 (1973); cost of research only: N/A.

Dates: 1973; data collected 1973; final report 1975.

Dissemination effort:

   Briefings: Department of Labor officials and presentations to professional meetings.

   Testimony: None.

   Distribution of executive summaries: None.

Policy deliberations pertaining to treatment: Azrin states that the study was cited by then-Congressman Paul Simon, who placed an article about it in the Congressional Record and recommended the use of the Job Club method as a general tool in government programs.

Location of treatment site: Carbondale, Illinois.

Number of treatment groups: Two (with one control group).

Treatment tested:

   (1) Controls. No treatment.

   (2) Experimentals. Daily group meetings to teach job search methods and develop positive job search attitudes through group reinforcement. Subjects received supervision in job search until successful. Elements of the treatment included the buddy system, secretarial services for resumes and letters of recommendation, a telephone bank, and job leads from other clients.

   Experimentals were matched one-for-one with controls by an overall criterion of probable employability based on age, sex, race, education, marital status, desired position and salary level, number of dependents, and current financial resources. Once matched, a coin flip determined which member of the pair would be a control and which an experimental.

Sample size: 60 experimentals, 60 controls.

Target population: Unemployed persons not receiving unemployment benefits.

Outcome of interest: Employment.

Research components:

Process analysis: To eliminate attrition bias, those experimentals matched with nonresponding controls were dropped from the sample for reporting purposes.

Impact analysis: Conducted as difference in means or medians.

Benefit-cost analysis: Not conducted.

Major findings:

	Experimentals	Controls
Employed (more than 20 hrs./wk.) within 2 months of beginning treatment	90%	55%
Employed within 3 months of beginning treatment	92%	60%
Mean starting wage	$2.73	$2.01
Median time until job found	14 days	53 days

(Includes those who did not find jobs in the period.)

These figures exclude experimentals who attended less than five sessions and their matched controls. No data are available on those excluded. All differences shown are statistically significant.

Time trends in findings: Results are only reported through three months.

Problems and issues:

1. Results may be biased by self-selection, since only data on those who chose to attend five or more sessions are presented. The matching process does not control for self-selection except to the degree that the observed variables capture it.

2. The sample is small and quite heterogeneous.

3. Displacement bias seems very likely.

4. Success of the Job Club method probably varies with the size of the informal (unadvertised) job market, and this may vary among communities.

Replicability: Treatment has been replicated in subsequent studies, and a training manual for counselors has been published.

Generalizability: Findings are striking, since Carbondale was a high-unemployment community. The sample excluded persons receiving unemployment insurance benefits because the authors believed some of them were likely to lack motivation to find employment until their benefits ran out. They suggest, however, that if participation were a condition of receiving benefits, it would motivate job search in this group too.

CARBONDALE HANDICAPPED JOB FINDING CLUB

Information source: Nathan H. Azrin and Robert A. Philip, "The Job Club Method for the Job Handicapped: A Comparative Outcome Study," Rehabilitation Counseling Bulletin, 23, December 1979.

Funding source: Illinois Department of Mental Health. Key personnel: None.

Treatment administrator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Evaluator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Enabling legislation: None.

Total cost: Not available (experiment was one activity of an ongoing research lab); research only: N/A.

Dates: 1974-75; data collected: same; final report 1979.

Dissemination effort: See **Carbondale Job Club**.

Briefings: None.

Testimony: None.

Distribution of executive summaries: None.

Policy deliberations pertaining to treatment: None.

Location of treatment site: Carbondale, Illinois.

Number of treatment groups: Two (with one control group).

Treatments tested:

(1) Controls received two days of group lectures, discussions, and role-playing in job search. The authors state that this is the common format of the only other standardized method of job counseling.

(2) Experimentals received daily group job search training and supervised job search until a job was obtained. A fuller description is in **Carbondale Job Club**.

Sample size: 80 experimentals, 74 controls.

Target population: Unemployed persons with "severe employability problems": physically or mentally handicapped persons, ex-prisoners or mental patients, welfare clients, substance abusers, and alcoholics; and other long-term job seekers.

Outcome of interest: Employment.

Research components:

Process analysis: "A principal difference between the two programs was that the comparison clients were informed of the need for (certain) actions; the Job Club clients were required to perform them under supervision."

Impact analysis: Conducted as a difference in means or medians.

Benefit-cost analysis: Not conducted.

Major findings:

	Experimentals	Controls
Employed after 2 months at a job over 20 hours/wk. (based on survey with 100% response rate)	90%	20%
Employed after 6 months (based on survey with 32% response rate)	95%	28%
Mean starting wage	$3.01	$3.08
Median starting wage	$2.61	$2.20
Median time to find job (successful seekers only)	10 days	30 days
Percent of days worked out of all available days over a 3-month period	89%	23%

These data exclude both experimentals and controls who attended less than two sessions. Differences in employment are significant. The difference between mean and median starting wages is due to a single high-wage control. All jobs found were unsubsidized.

"The program was found superior to an alternative program, but the salaries were not extraordinary, and some clients required weeks and months of continued diligent supervision and guidance."

Time trends in findings: There was no tendency for differences to narrow between two months and six months.

Problems and issues:
1. Experimental design eliminates any possibility of self-selection bias (because both experimentals and controls had to attend at least two sessions to be included in sample), but the six-month follow-up is probably vulnerable to attrition bias.

2. The sample is small, and the clients' problems, although severe, are heterogeneous.

Replicability: See **Carbondale Job Club.**

Generalizability: See **Carbondale Job Club.**

U.S. EMPLOYMENT SERVICE EFFECTIVENESS OF COUNSELING PILOT STUDY

Information source: Jacob Benus, Arden R. Hall, Patty Gwartney-Gibbs, Marilyn Coon, Caren Cole, Diane Leeds, and Doug Brent, "The Effectiveness of Counseling in the U.S. Employment Service: A Pilot Study; Analytic Results," Stanford Research Institute, August 1977. This study has never been published.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: William Showler.

Treatment administrator: U.S. Employment Service, three locations.

Evaluator: Stanford Research Institute. Key personnel: Jacob Benus.

Enabling legislation: None.

Total cost: Roughly $250,000 (1976).

Dates: November 1975-February 1976; data collected through August 1976; final report August 1977.

Dissemination effort:

Briefings: DOL Staff.

Testimony: None.

Distribution of executive summaries: None

Policy deliberations pertaining to treatment: None, according to Benus. The study was explicitly designed as a pilot program for a full-scale national study. The national study was canceled, however, after SRI reported the results. He believes that the cancellation occurred because the results were not supportive of the counseling program. The U.S. Employment Service counseling program is still operating.

Location of treatment sites: Salt Lake City, Utah; Minneapolis, Minnesota; and West Palm Beach, Florida.

Number of treatment groups: Four (with two control groups).

Treatments tested:

1.a. Persons determined by Employment Service interviewers to need counseling and who received it (ES experimentals).

1.b. Persons determined by ES interviewers to need counseling who did not receive it (ES controls).

2.a. Persons determined by SRI interviewers (not ES interviewers) to need counseling and who

then received it from the usual ES counselors (SRI experimentals).

2.b. Persons determined by SRI interviewers (not ES interviewers) to need counseling and who did not receive it (SRI controls).

Random assignment occurred after determination of need. The reason for the second set of experimentals/controls was the investigators' belief that ES interviewers tend to refer people to counseling not on the basis of their need for it but on the basis of their low placement-potential. People with a higher placement-potential might need counseling in order to achieve that potential, whereas low-potential individuals might still not benefit from employment counseling.

Sample size: 481 experimentals (receiving counseling), 439 controls.

Target population: Clients of the U.S. Employment Service determined by interviewers to need employment counseling.

Outcome of interest: Duration of unemployment.

Research components:

Process analysis: Not conducted. This is a black box experiment.

Impact analysis: Conducted with OLS regressions. Other analytical methods were also used.

Benefit-cost analysis: Not conducted.

Major findings:

1. Counseling had no significant impact on the duration of unemployment. This finding was the same for both ES and SRI subjects.

2. Counseling also had no significant impact on wages, job prestige, percentage of observation period employed, job satisfaction, or the number of job search methods used.

Time trends in findings: Data series too short to find any.

Problems and issues:

1. Because this study was explicitly designed as a pilot for a more ambitious evaluation, the period for which the investigators had data is no more than ten months for any subject, and as little as six months for some.

Replicability: None. The experiment was undertaken to demonstrate the feasibility of a larger study, which was never funded. The investigators did not inquire into the content of the counseling.

Generalizability: The sites selected were not representative of the U.S. Employment Service nationally.

JOB CLUB BEHAVIORAL SUPERVISION TEST

Information source: Nathan H. Azrin, V. A. Besalel, I. Wisotzek, M. McMorrow, and R. Bechtel, "Behavioral Supervision Versus Informational Counseling of Job Seeking in the Job Club," Rehabilitation Counseling Bulletin, March 1982, 212-218.

Funding source: Illinois Department of Mental Health. Key personnel: None.

Treatment administrator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Evaluator: Anna Mental Health Center. Key personnel: Nathan H. Azrin.

Enabling legislation: None.

Total cost: Not possible to separate from other research.

Dates: Roughly 1980-1981; data collected: roughly the same period; final report March 1982.

Dissemination effort:

   Briefings: None.

   Testimony: None.

   Distribution of executive summaries: None.

Policy deliberations pertaining to treatment: None.

Location of treatment site: Carbondale, Illinois.

Number of treatment groups: Two (with one control group).

Treatments tested:

   (1) Controls were taught all skills and techniques used in the job search under the Job Club model. See **Carbondale Job Club**.

   (2) Experimentals received this information and were supervised in using it; for example, in telephoning employers and friends and writing resumes.

Sample size: Experimentals, 186; controls, 133.

Target population: Unemployed persons of all sorts, especially (but not solely) referrals from the local employment service.

Outcome of interest: Employment.

Research components:

   Process analysis: This experiment is itself a form of process analysis.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Not conducted.

Major findings:

Six months after enrollment:

	Controls	Experimentals
Obtained jobs of 20+ hrs./wk.	70.6%	87.6%*
Mean hrs./wk. worked	33.6	36.9
Days required to find job	60.7	32.1*
Mean salary/hr.	$3.93	$4.99

(Asterisk indicates the difference is statistically significant.)

Time trends in findings: Only six-month data are reported.

Problems and issues:

1. Attrition is higher in the controls than in the experimentals, and there is no analysis of it. (The initial sample included 196 experimentals and 150 controls.)

2. This study in particular would have benefitted from a cost-benefit analysis, to show whether the experimental difference was worth the supervision cost.

3. As with all the Job Club experiments, the possibility of displacement exists. See also the comments on the other Job Club experiments.

Replicability: Replicable. See **Carbondale Job Club**.

Generalizability: Apparently generalizable; there does not seem to be any community-specific effect here.

BUFFALO DISLOCATED WORKER DEMONSTRATION PROGRAM

Information source: Walter Corson, Sharon Long, and Rebecca A. Maynard, "An Impact
Evaluation of the Buffalo Dislocated Worker Program," Mathematica Policy Research, March
12, 1985. It should be emphasized that this "experimental" evaluation was not so planned from
the beginning. Certain services, discussed below, were rationed by lottery, and the authors of
the report used this random assignment after the fact to assess program impact.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key
personnel: Beverly Bachemin and George Koch.

Treatment administrator: Worker Re-employment Center. Key personnel: Harry Reeverts, Mark
Cosgrove, and Edie Rifenburg.

Evaluator: Mathematica Policy Research. Key personnel: Walter Corson, Sharon Long, and
Rebecca A. Maynard.

Enabling legislation: None.

Total cost: The impact analysis was conducted as part of a six-site program evaluation, most of
which was nonexperimental. It would be very difficult to disentangle the experimental and
nonexperimental costs.

Dates: October 1982-September 1983; data collected through July 1984; final report March 1985.

Dissemination effort:

Briefings: A panel on Technology and Employment of the National Academy of Sciences and
a group of program operators in New Jersey.

Testimony: None.

Distribution of executive summaries: U.S. Department of Labor and to people who requested
it.

Policy deliberations pertaining to treatment: Corson states that the findings have been cited by the
NAS panel, in Technology and Employment: Innovation and Growth in the American
Economy, edited by Richard Cyert and David Mowry.

Location of treatment site: Buffalo, New York.

Number of treatment groups: Two (with one control group).

Treatment tested: The "experiment" took place in the context of factory shutdowns and large-scale
layoffs in the Buffalo area. Laid-off workers from six large "target plants" were recruited for
reemployment-related services. Because resources were limited, treatment administrators
rationed these services through a lottery system: at random, workers were either notified or not
notified of the availability of program slots. The program offered testing and assessment, a
four-day job search workshop, a job search resource center, classroom training for new

careers, on-the-job training, job development services, relocation assistance, and Targeted Jobs Tax Credit eligibility determination. These component services were not assigned randomly.

Sample size: The analysis sample has 586 experimentals and 210 controls.

Target population: Laid-off workers from four steel plants, an automobile assembly plant, and a petroleum refinery. Workers over the age of 55 were excluded ex post from the analysis sample, because no such workers opted to receive the services offered.

Outcomes of interest: (1) Participation. (2) Employment. (3) Wages. (4) Receipt of transfer payments.

Research components:

Process analysis: Conducted. Some unexpected differences in background variables between experimentals and controls were noted, but were judged not to affect the results. The treatment was an expensive one, averaging $1,975 per participant. Participation was fairly low, with 27.7% of experimentals choosing to receive services.

Impact analysis: Conducted with OLS and a Heckman selection term for participation. The inverse Mills ratio is set equal to zero for controls; a probit analysis is performed on the discrete choice to participate or not to participate among experimentals, and the inverse Mills ratio is derived for each experimental from that probit. The impact of the experimental treatment per participant is then calculated as the coefficient on a dummy variable (one if participant experimental, zero if nonparticipant experimental or control) in an equation which includes among other control variables the inverse Mills ratio. The authors present alternative estimation models and argue that the one they use is the most efficient. Also, because of significant nonresponse rates, the observations are weighted so that the analysis sample of experimentals resembles the population of controls.

Benefit-cost analysis: Not conducted.

Major findings:

Impacts on participant experimentals in the first six months after recruitment:

	Impact estimate	Mean for participants
Proportion of time employed	+0.33	0.57
Probability of ever being employed	+0.31	0.72
Average hours employed per week	+13.6	23.7
Average weekly earnings	$115	$174

All of these impacts are significant at the 95% level with a one-tail test.

Weekly amount of food stamps	-$5.10	$3.60
Percentage of time receiving public assistance	-10.7	2.3
Weekly amount of public assistance	-$9.20	$1.10

All of these impacts are significant at the 90% level with a one-tail test. Public assistance includes AFDC, SSI, and general assistance. Effects on unemployment insurance receipt and Supplemental Unemployment Benefit are not significant.

Time trends in findings: Experimental/control employment differences narrow over time.

Problems and issues:

1. The most serious problem is the high nonresponse rate, nearly 46% among controls, over a third of them refusals. The rate of refusals is higher among controls than among experimentals; nonresponse bias might well exist, and the weighting method will not eliminate it.

2. One of the great advantages of random-assignment experiments is that they do not require the use of econometric adjustments for self-selection like the Heckman procedure, which is not robust to deviations from the assumptions of the model concerning the distribution of the random disturbance. The use of the Heckman self-selection procedure here is driven by a desire to estimate impacts per participant, rather than per experimental, where the number of participants is small with respect to the number of experimentals. Average impact per experimental is the relevant focus for policy purposes, however, because a program would be made available to some population at some average cost, and would have some average benefit per individual in that population. Ex ante, the policymaker probably knows fairly well the number of persons who will be eligible, but can only guess the number who will participate in a voluntary program.

3. The Buffalo economy was quite depressed at the time of the experiment, the unemployment rate rising from 9.7% to 12.6% over the course of 1982.

Replicability: Replicable.

Generalizability: Limited by the small sample size, the possibility of nonresponse bias, and the particularly depressed condition of the local economy.

DELAWARE DISLOCATED WORKER PILOT PROGRAM

Information source: Howard S. Bloom, "Lessons from the Delaware Dislocated Worker Pilot Program," Evaluation Review, April 1987, 157-177.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: None.

Treatment administrator: Delaware Department of Labor. Key personnel: Dennis Carey. (Carey was the state secretary of labor. He is now with the Hay Group in Washington, D.C.).

Evaluator: Bloom Associates. Key personnel: Howard S. Bloom and Susan Philipson Bloom.

Enabling legislation: None.

Total cost: Research only: $10,000 (1983).

Dates: January 1983-July 1983; data collected through December 1983; final report 1984.

Dissemination effort:

Briefings: To the Delaware Secretary of Labor.

Testimony: None.

Distribution of executive summaries: To the Delaware Department of Labor.

Other: Experiment was discussed in the local press during the time it was conducted, and the findings were also reported there.

Policy deliberations pertaining to treatment: Howard Bloom does not think the findings have had much impact on policy.

Location of treatment sites: All of the three counties in Delaware.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals received four basic services in sequence: job search workshops, regular individual counseling sessions, services of job developers, and retraining. The retraining services were a last resource, and only 13 out of 65 experimentals received them.

Sample size: Experimentals, 65; controls, 110.

Target population: Volunteers were taken from the population of all Unemployment Insurance (UI) claimants who had been receiving benefits for seven to 12 weeks, attended an orientation, and applied for entry in the program. Those without at least 10 years of education or access to transportation were screened out, as were those who expected to be recalled by their most recent employer. Separate assignment lotteries were held in each of the state's three counties to even out the workload of program staff in the different locations.

<u>Outcomes of interest</u>: (1) UI benefit payments. (2) Earnings.

<u>Research components</u>:

Process analysis: An analysis of participation was conducted; it generally found that nonparticipants had better than average employment prospects than participants, and that early dropouts had higher past earnings than did completers, but lower earnings after the program started. Program staff also commented unfavorably on one of the key concepts of the program, which was to have high-school guidance counselors with very limited special training perform the counseling tasks. The program was also evaluated with no preliminary starting-up period.

Impact analysis: Conducted with OLS.

Benefit-cost analysis: Not conducted.

<u>Major findings</u>: The program had no statistically significant impact on UI payments or earnings. The impacts reported were in the wrong direction (higher UI payments, lower earnings).

<u>Time trends in findings</u>: None.

<u>Problems and issues</u>: The sample size is extremely small. In this case, the underlying population also appears to be extremely small--only 965 workers in the entire state had been unemployed for seven to 12 weeks, and those who applied for program services were only about a third of the total. Statistically significant results could not have been expected from a sample this size unless the treatment impacts were quite large.

<u>Replicability</u>: Replicable.

<u>Generalizability</u>: Probably none, because the sample is too small and the implementation, specifically the use of high-school guidance counselors, seems questionable.

CLAIMANT PLACEMENT AND WORK TEST DEMONSTRATION

Information sources: Terry R. Johnson, Jennifer M. Pfiester, Richard W. West, and Katherine P. Dickinson, "Design and Implementation of the Claimant Placement and Work Test Demonstration," SRI International, May 1984.

Walter Corson, David A. Long, and Walter Nicholson, "Evaluation of the Charleston Claimant Placement and Work Test Demonstration," Mathematica Policy Research, July 31, 1984.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: Norm Harvey and William Showler.

Treatment administrator: South Carolina Employment Security Agency. Key personnel: Agency head, not named in report.

Evaluators: SRI International; Mathematica Policy Research; and Bloom Associates. Key personnel: Susan Philipson Bloom (Bloom Associates) and Terry R. Johnson (SRI) helped design the evaluation. Walter Corson and Walter Nicholson (MPR) were the principal investigators.

Enabling legislation: None.

Total cost: Administration, $25,000 (1983); research only: roughly $200,000 (1983).

Dates: February-December 1983; data collected: same; final report July 31, 1984.

Dissemination effort:

Briefings: None.

Testimony: None.

Distribution of executive summaries: A special letter reporting the findings was sent to all states.

Policy deliberations pertaining to treatment: The report states that the state of South Carolina was planning for statewide implementation of some of the demonstration treatments. Showler believes, however, that the Employment Security Agency head changed, and the required cooperation of the Employment Service (ES) and the unemployment insurance branches of the agency was no longer forthcoming. Very substantial federal budget cuts to the Employment Service in 1984 would have made this project difficult to implement in any state thereafter, because the Employment Service would not have been able to perform the function that it did in this experiment.

Location of treatment site: Charleston, South Carolina.

Number of treatment groups: Four (with one control group).

<u>Treatment tested</u>:

Group A. Controls. The treatment of controls differed slightly from prior practice in that prior practice had a theoretical requirement for ES registration. Controls had no ES registration requirement and did not receive special job development efforts, although they could use the ES services voluntarily. They were required (as were experimentals) to come in periodically for eligibility reviews at the UI office.

Group B. Improved work test, but regular Employment Service (ES) services. These experimentals were mailed notices at the same time as they received their first week's UI check to come to the Employment Service office to register their availability for work. This practice differed from the prior practice in that (1) the registration requirement was delayed so that those who never received a check did not come into ES offices, and (2) the registration was required as of some definite date. In general, failure to register would be taken as possible evidence of unavailability for work and, therefore, ineligibility for UI payments. New procedures were implemented to match ES and UI records so that this rule would be routinely enforced.

Group C. Improved work test and enhanced placement interviewer services. These experimentals received the same notice as group B. In addition, when they reported to the ES, an interviewer would attempt to develop a job for the subject unless he or she was a union member, was not job-ready, or was on layoff for some definite period. Group C subjects were also called in for a renewed job-placement attempt if still unemployed after nine weeks.

Group D. Improved work test, enhanced placement interviewer services, and job search workshops. In addition to group C experimentals, three-hour job search workshops were mandated for group D experimentals who were still receiving UI benefits four to five weeks after receiving enhanced placement interviewer services.

<u>Sample size</u>: A, 1,485; B, 1,493; C, 1,666; D, 1,277.

<u>Target population</u>: New Unemployment Insurance (UI) claimants who had received an initial UI check, excluding those whose employers said they were on layoff for some definite period.

<u>Outcomes of interest</u>: (1) Employment. (2) UI payment reductions.

<u>Research components</u>:

Process analysis: Conducted. The process analysis indicates that the experiment was conducted essentially as planned. Roughly 25% of the experimentals failed to register on time with the ES the first time, and among experimentals subject to subsequent call-ins, about 9% of those eligible failed to respond. Nonresponse was more common among men than women, and varied inversely with age and education. Nonresponses by men and women were not homogeneous (i.e., could not be captured simply by an intercept term), and there was a cohort effect (the longer the call-in policy was in effect, the more likely subjects were to respond). 85% of experimentals received some ES service, compared with 35% of controls. Over 62% of group C and D members received some attempt at job development, compared with 33% of group B and 9% of group A members.

Impact analysis: OLS, comparison of means. Probit conducted but not reported, since the

results were similar to OLS.

Benefit-cost analysis: Cost-effectiveness analysis.

<u>Major findings</u>:

	A	B	C	D
Percentage of subjects with nonmonetary determination (eligibility ruling)	5.8	13.4	16.7	18.4
Percentage of subjects with a denial (ruled ineligible)	4.2	7.4	9.2	8.7

OLS confirms that all experimental treatments had a statistically significant positive effect on the denial rate compared with the experience of controls. This effect is substantially higher in C and D than in B, but differences between C and D are negligible.

Treatment effects on employment and wages as recorded in the UI wage reporting system are weak, inconsistent, and usually not statistically significant.

Effects on weeks of UI payments (coefficients of OLS dummy variables for treatment group; this measures the treatment effect on experimentals by comparison with controls):

	B	C	D
Men	-0.83*	-1.15*	-1.14*
Women	-0.20	0.31	-0.15

An asterisk indicates statistical significance at the 95% confidence level, on a two-tail test. OLS results control for cohort (week applying for UI).

Much of the difference between men and women results from the strong treatment effect on construction workers, who are mostly male. The experimental treatments reduced the weeks of UI received by male construction workers by about two weeks (the effects on female construction workers are about the same, but are not statistically significant because of small numbers). "A possible explanation for this result may relate to the casual, part-time nature of some construction employment (particularly during slack periods) and to the relatively low wage replacement rates that UI provides to construction workers."

All experimental treatments were inexpensive: $4.72, $13.17, and $17.58 per subject increments for treatments B, C, and D, respectively. The corresponding average reductions in UI payments were $52.93, $58.71, and $73.14. All experimental treatments are therefore cost-effective, with the most cost-effective being treatment D.

<u>Time trends in findings</u>: The variable representing cohort has a negative and statistically significant effect on weeks of unemployment. However, it is not possible to distinguish between the effect of falling unemployment rates and the effect of learning about new registration requirements for UI. Since experimental treatments differ in the times at which interventions occur, treatment

effects are also indistinguishable from time effects.

Problems and issues:

1. Unemployment in Charleston fell from 8.9% to 6.6% in 1983. The improving economy reduced UI claims and increased job orders at the ES office.

2. If a claimant stated that he or she had failed to report as required because of illness or lack of transportation, there would have been a denial of UI on the grounds that they were not able to work or available for work. On the other hand, claims that the summons to register at the ES had been lost in the mail were always accepted, even when there had been no difficulty receiving the UI check sent under a separate cover. It was sent under a separate cover only because of the experimental character of the demonstration (controls were not supposed to receive a call-in, and it was apparently too complex to insert a call-in with some checks and not with others); experimental denial rates therefore slightly understate what could be expected upon implementation of the policy.

3. If the treatment effect is primarily centered on construction workers, it would not be surprising if no wage or employment effects were noted from UI data because much of the industry is not covered or escapes reporting requirements.

4. In the cost-effectiveness analysis, instead of estimating the reduction in UI payments directly, the authors take the average UI weekly payment ($96.24) and multiply it by the experimental treatment effect on weeks of receipt. This procedure fails to take into account the possibility that the part of the population whose behavior is changed by the treatment will not have the same average weekly benefit as the overall population.

Replicability: Replicable.

Generalizability: The authors are cautious about generalizability. They think that South Carolina Job Service/UI procedures are similar to those in most other states. The Charleston labor force has a higher percentage of blacks, however, than does the U.S. labor force. The importance of the construction industry, where much of the treatment impact was concentrated, varies across the country. State UI laws and regulations also vary; the maximum weekly payment in South Carolina, for example, was $118.

ILLINOIS UNEMPLOYMENT INSURANCE INCENTIVE EXPERIMENTS

Information sources: Stephen A. Woodbury and Robert G. Spiegelman, "Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois," American Economic Review, September 1987, 513-530. By the same authors: "The Illinois Unemployment Insurance Incentive Experiments," final report to the Illinois Department of Employment Security, W. E. Upjohn Institute, February 1987.

Funding sources: Illinois Department of Employment Security (using Wagner-Peiser grants); and W. E. Upjohn Institute for Employment Research. Key personnel: DES: Sally Ward; Upjohn: Robert G. Spiegelman.

Treatment administrator: Illinois DES. Key personnel: Sally Ward.

Evaluator: W. E. Upjohn Institute for Employment Research. Key personnel: Robert G. Spiegelman and Stephen A. Woodbury.

Enabling legislation: None.

Total Cost: $800,000 (1985); research only, $200,000.

Dates: Mid-1984 to early 1985; data collected from mid-1984 to mid-1985; final report February 1987.

Dissemination effort:

Briefings: State DES personnel and U.S. Department of Labor, Employment and Training Administration personnel.

Testimony: None.

Distribution of executive summaries: To DES.

Policy deliberations pertaining to treatment: None.

Location of treatment: 22 Job Service offices in northern and central Illinois.

Number of treatment groups: Three (with one control group).

Treatments tested:

1. Claimant experiment. A $500 bonus was offered to an eligible claimant of unemployment insurance payments if he/she could find a job within 11 weeks and hold that job for four months.

2. Employer experiment. The same, except that the $500 bonus would be paid to the claimant's employer.

3. Controls. Standard unemployment insurance eligibility.

Sample size: 4,186 claimant experimentals; 3,963 employer experimentals; 3,963 controls.

Target population: Persons who are (1) eligible for 26 weeks of Unemployment Insurance (UI) benefits; (2) between 20 and 55; and (3) registrants with Job Service offices who were not on definite layoff, not eligible for a union hiring hall, or not recent veterans or federal employees.

Outcomes of interest: (1) Reductions in unemployment spells. (2) Net program savings.

Research components:

Process analysis: Not conducted.

Impact analysis: Conducted as difference in means.

Benefit-cost analysis: Conducted from a budgetary perspective.

Major findings:

1. A $194 reduction in 52-week benefit payments to the average claimant experimental was found, by comparison with the average control. The comparable $61 reduction for employer experimentals was not statistically significant.

2. A 1.15-week reduction in insured unemployment over 52 weeks was found for the average claimant experimental. The 0.36-week reduction for employer experimentals was not statistically significant.

3. The experimental treatments did not appear to curtail productive job search activity, because no statistically significant change in subsequent earnings was found between the control and experimental groups.

4. The employer experiment did result in a $164 reduction in benefit payments to white women which was statistically significant. The effects of the employer experimental treatment among blacks (male and female) and white males were not significantly different from zero. The claimant experimental response was statistically significant for whites of both sexes, but not for blacks of either sex.

5. 13.6% of claimant experimentals received a bonus; 25% qualified for one. 2.8% of employer experimentals obtained a bonus for their employers; 22.8% of them could have obtained one for their employers.

6. The ratio of benefit payments reductions to bonus cost for the claimant experiment is 2.32, which is statistically significant. The ratio for the employer experiment is 4.29 (not significant). If 100% of those eligible for bonuses had claimed them, the benefit-cost ratio would have been 1.26 for claimant experimentals, 0.53 for employer experimentals.

Time trends in findings: Reductions in the initial unemployment spell may be slightly offset by increases in subsequent unemployment, but this is not statistically significant.

Problems and issues:

(1) A potential displacement effect among nonparticipants in the program clearly exists.

(2) Long-term market bias effects are conceivable if workers adjusted to a permanent bonus program by undergoing frequent short unemployment spells with accompanying state bonuses instead of infrequent long spells.

(3) The very low rate at which the bonuses were claimed by those who qualified for them is puzzling.

Replicability: The treatments are clearly replicable.

Generalizability: The 22 sites involved in the experiment represented very diverse labor markets, so locality effects should not be present. Spiegelman notes that roughly the first half of workers subject to the experiment would have been eligible for extended (38-week) federal unemployment payments if their unemployed status had lasted longer than 26 weeks, whereas subsequent claimants were not eligible for them. Experimental response seems to have been stronger in the first group, but this is not conclusively established.

**NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT
DEMONSTRATION PROJECT**

Information source: Walter Corson, Paul T. Decker, Shari Miller Dunstan, and Anne R. Gordon, with Patricia Anderson and John Homrighausen, "The New Jersey Unemployment Insurance Reemployment Demonstration Project: Final Evaluation Report," U. S. Department of Labor, Employment and Training Administration, Unemployment Insurance Occasional Paper 89-3, April 1989.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: Stephen Wander and Wayne Zajac.

Treatment administrator: New Jersey Department of Labor. Key personnel: Frederick Kniesler, Nancy Snyder, and Roger Emig.

Evaluator: Mathematica Policy Research. Key personnel: Walter Corson, Paul T. Decker, and Shari Miller Dunstan.

Enabling legislation: None.

Total cost: Research only: $1.27 million (1987).

Dates: July 1986-Fall 1987; data collected through July 1988; final report April 1989.

Dissemination effort:

Briefings: State of New Jersey, Department of Labor officials; Kniesler has made frequent presentations to meetings of employment officials from other states.

Testimony: None.

Distribution of executive summaries: The Occasional Paper mailing list, which is extensive.

Other: A report was carried in the UI program letter of the Department of Labor. The New Jersey program received an award for innovation from a national association of employment security professionals.

Policy deliberations pertaining to treatment: Wander states that this experiment related to an ongoing debate in Congress over what would be the appropriate government response to dislocated workers: a passive income maintenance approach, or an active intervention approach. Elements of the New Jersey model (early identification of dislocated workers, referrals to job search assistance) were adopted in the Economic Dislocation and Worker Adjustment Assistance Act. To encourage early intervention, needs-based stipends under that act are available to workers after the 13th week of a UI claim only if they are in a training program. MPR found that the additional costs per claimant from adding the Reemployment Bonus as designed in New Jersey to the Job Search Assistance program were greater than the additional UI savings. Wander states that this finding has led to other experiments with different bonus designs.

<u>Location of treatment sites</u>: 10 Unemployment Insurance (UI) offices in New Jersey. The sites were chosen to represent the New Jersey UI recipient population, and were in Paterson, Hackensack, Jersey City, Butler, Bloomfield, Newark, Elizabeth, Perth Amboy, Burlington, and Deptford.

<u>Number of treatment groups</u>: Four (with one control group).

<u>Treatment tested</u>: Both sexes are in the population. "He" is used for brevity.

1. Job Search Assistance (JSA) only. Four weeks after the subject received his first weekly UI payment, he was directed to come to an orientation and testing session. The following week the subject was (with certain exceptions) expected to attend a job search workshop lasting five days (half-day sessions). The week thereafter he was supposed to attend an individual session with a counselor to provide an assessment of his employment prospects. An employment resource center with job listings, telephones, and literature was set up in the UI office and he was expected to use it regularly. If he failed to do so, he would be recontacted every two weeks and reminded of this obligation. Failure to comply with these expectations would be grounds for termination of UI payments.

2. JSA plus Training and Relocation. The same treatment as JSA only, but in addition, at the individual counseling session the subject was informed of the availability of funds for vocational-training courses or expenses of relocation and job search in another area. (The option was training or relocation, but not both.) Counseling on training options was provided to those interested. As anticipated, less than 1% of those offered the relocation option accepted assistance for that purpose; the incremental effect of this treatment over JSA only is essentially the training effect.

3. JSA plus Reemployment Bonus. The same treatment as JSA only, but, in addition, at the individual counseling session the subject was offered a reemployment bonus. The maximum bonus was equal to half of the remaining UI entitlement at the time of the interview (the average of the maximum bonus was $1,644). The bonus declined at the rate of 10% per week until UI was no longer available to the subject. The bonus was not available if the new job was with the subject's last employer or a relative, or if it was temporary, seasonal, or part-time. The subject received 60% of the bonus if employed four weeks, and the balance if employed 12 weeks.

4. Controls faced the usual obligation to look for work, but the use of existing job search services was voluntary.

<u>Sample size</u>: Controls, 2,385; JSA only, 2,416; JSA plus Training and Relocation, 3,810; JSA plus Reemployment Bonus, 2,449.

<u>Target population</u>: The treatments were intended for dislocated workers. The following types of UI claimants were therefore screened out of the sample: those who never received the first payment (they found work or were ineligible), those who had worked less than three years for their previous employer, those less than 25 years old, those who had definite recall dates from the last employer, those hired through a union hiring hall, and certain types of special claimants (e.g., ex-Armed Forces, ex-federal government, interstate movers).

<u>Outcomes of interest</u>: (1) UI payments. (2) Employment. (3) Earnings.

Research components:

Process analysis: Conducted. Essentially the treatments were delivered as planned. Many subjects were excused from portions of the JSA treatments because they could not read, speak, or understand English well enough to benefit from them.

Impact analysis: Conducted with OLS, logit.

Benefit-cost analysis: Conducted from claimant, agency budget, government budget, and social perspectives.

Major findings: An asterisk denotes an effect significantly different from zero at a two-tailed 90% confidence level, and often at higher confidence levels.

	JSA Only	JSA w/T&R	JSA w/Bonus
Change in UI benefits	-$87*	-$81*	-$170*
Change in weeks of UI payments, benefit year	-0.47*	-0.48*	-0.97*
Change in probability of exhausting benefits	-0.028*	-0.017	-0.037*
Change in weeks employed, benefit year:			
1st quarter	2.3*	1.9*	2.8*
2nd quarter	4.9*	2.8*	5.0*
3rd quarter	4.2*	2.2	2.3
4th quarter	2.8	1.7	0.6
Change in earnings			
1st quarter	$125*	$82	$160*
2nd quarter	263*	103*	278*
3rd quarter	171	83	131
4th quarter	49	77	22

Subgroup analysis indicates that "the treatments were less successful for individuals who faced hard-core, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers."

Benefit-cost analysis: Incremental costs were $155 per subject for JSA only, $377 for JSA plus Training and Relocation Assistance, and $277 for JSA plus Reemployment Bonus. From a social perspective, benefits exceed costs for all three experimental treatments (by $581, $44, and $565, respectively), with most of the benefit going to subjects in higher income categories. For the government budget as a whole, savings exceeded costs in the JSA only and bonus treatments, but not in the training treatment (although long-term benefits from training may be underestimated); for the Labor Department specifically, all experimental treatments result in net cost increases. The bonus treatment resulted in $122 in additional costs per claimant above the JSA only treatment, but only $83 per claimant in additional UI payment savings.

Time trends in findings: As shown above.

Problems and issues: The major issue is covered under "Generalizability," below. Subgroup analysis seems to consist mostly of the use of interaction terms (e.g., a dummy for industry

multiplied by a dummy for treatment) in the regression. This type of analysis assumes that the slope coefficients on continuous variables, like prior earnings, are identical for all groups. Separate regressions for distinct groups might have been useful.

Replicability: Replicable.

Generalizability: It is unlikely that a real social program would be set up using the particular eligibility criteria selected for this experiment. Only about one-fourth of the UI claimant population were eligible for the experiment. It is noteworthy that for the subgroups most typical of the population the experimental treatments were intended to assist (older workers, blue-collar workers), the treatments were less successful.

DIGEST OF THE SOCIAL EXPERIMENTS:

III. Disadvantaged Youth

PUBLIC VS. PRIVATE SECTOR JOBS DEMONSTRATION PROJECT

Information source: James F. Gilsinan, "Information and Knowledge Development Potential," Evaluation Review, June 1984, 371-388.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart and Joseph Seiler.

Treatment administrator: CETA prime sponsors in five sites. Key personnel: None.

Evaluator: Center for Urban Programs, Saint Louis University. Key personnel: James F. Gilsinan and E. Allen Tomey.

Enabling legislation: Youth Employment and Demonstration Projects Act (YEDPA) of 1977.

Total cost: About $600,000 (1979).

Dates: Summer 1978-Spring 1979; data collected Spring 1980; final report 1981.

Dissemination effort:

Briefings: Presentations at conferences of YEDPA contractors and academic conferences.

Testimony: Gilsinan believes the experiment was discussed in testimony by Taggart to Congress; it was also discussed by Andrew Hahn in testimony to the House committee chaired by Congressman Augustus Hawkins.

Distribution of executive summaries: U.S. Department of Labor.

Policy deliberations pertaining to treatment: The findings are ambiguous about the value of private-sector job creation. Gilsinan states that officials of the Reagan administration were disappointed by them, and that there was minimal policy impact.

Location of treatment sites: Portland, Oregon; St. Louis, Missouri; Philadelphia, Pennsylvania; New York City; and rural Minnesota.

Number of treatment groups: Two (with no control group).

Treatment tested: Participating public- and private-sector employers were provided youths fully subsidized by the program at 100% of the minimum wage for 25 weeks. Employers were encouraged to place the young worker in an unsubsidized position at program's end, but where this was infeasible, program operators attempted to develop a different unsubsidized job with another employer. Young people from the same site were grouped according to age, race, sex, and a reading test score, and were randomly assigned to either a public- or private-sector job slot.

Sample size: Public-sector workers, 1,366; private-sector workers, 1,470.

Target population: 16-21 year-old low-income youths who were not in school.

Outcome of interest: Employment.

Research components:

Process analysis: Conducted. The analysis concluded that a much greater effort was needed to develop private sector worksites for the demonstration.

Impact analysis: Conducted by comparison of means, OLS.

Benefit-cost analysis: Not conducted.

Major findings: All findings are subject to some doubt because of heavy attrition in responses to follow-up surveys, and attrition may not have been random across treatments. 60% of the sample could not be found at the first follow-up, which occurred 90 days after program termination. At the second follow-up, 240 days after termination, the nonresponse rate was 65%. In the comparison of means, Gilsinan does not state that any differences were statistically significant, although some of them appear to be.

Program flow data:

Stage	Public	Private
1. Random assignment	1,366	1,470
2. Starting program	1,034	1,092
3. Reporting to worksite	892	879
4. Early termination	530	675
(% of line 2):	51.1	61.8
5. Completing program	504	417
(% of line 2):	48.9	38.2

Thus, subjects assigned to the private sector were more likely to quit or be fired than their public counterparts.

Outcomes for those completing the program, at completion:

	Public	Private
Unsubsidized employment	39.2%	52.5%
Other positive (schooling, etc.)	12.6	5.1
"Nonpositive" outcomes	47.1	40.4

Outcomes for completers, 90 days after program completion:

	Public Responses	Total	Private Responses	Total
Unsubsidized full-time job	50.2%	28.6%	64.0%	32.4%
Unsubsidized part-time job	16.0	9.1	16.6	8.4
Education or training	25.6	14.7	17.5	8.9
"Nonpositive" outcomes	8.0	4.6	1.9	1.0
Unknown		43.1		49.4

Thus, private-sector experience was more likely to lead directly to employment than public-sector experience for those who completed the program. This is confirmed in OLS regressions using discrete outcomes (employment, etc.) as the dependent variables. For example, private-sector assignment raised the probability of unsubsidized employment immediately after completion by 14%, an effect which is statistically significant at the 5% level.

Time trends in findings: The 240-day follow-up results are similar to the 90-day results.

Problems and issues: "(T)he high dropout rate clouds the issue of whether those who dropped out of the program and were never heard from again failed to gain meaningful employment." The failure to collect data on the subsequent experience of those terminated is a flaw in the experimental design; some of them may have dropped out in favor of better jobs, for example, and there is no information on whether the private-sector group of dropouts had a better career path than the public-sector group.

Replicability: Replicable.

Generalizability: This project is unique; it is the only source of experimental information on the relative effects of job creation in government as opposed to business. The findings seem to indicate a trade-off between a higher attrition rate in business and lower prospects of a permanent job in government. However, the flawed design, discussed above in "Problems and issues," limits the confidence that can be placed in the findings.

WILKES-BARRE YES WORKSHOP PROGRAM

Information source: Andrew Hahn and Barry Friedman, "The Effectiveness of Two Job Search Assistance Programs for Disadvantaged Youth," with the assistance of Cecilia Rivera-Casale and Robert Evans, Center for Employment and Income Studies, Florence Heller Graduate School for Advanced Studies in Social Welfare, Brandeis University, Waltham, Massachusetts, 1981.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart, Joseph Seiler, and Gordon Berlin.

Treatment administrator: Youth Employment Service. Key personnel: Joey Kelly.

Evaluator: Center for Employment and Income Studies, Florence Heller Graduate School. Key personnel: Andrew Hahn and Barry Friedman.

Enabling legislation: Youth Employment and Demonstration Projects Act of 1977.

Total cost: $164,162 (1980).

Dates: May 1979-October 1980; data collected: same; final report 1981.

Dissemination effort:

   Briefings: DOL.

   Testimony: None.

   Distribution of executive summaries: DOL.

Policy deliberations pertaining to treatment: None. According to Hahn, the experiment was poorly implemented.

Location of treatment site: Wilkes-Barre, Pennsylvania.

Number of treatment groups: Three (with no control group).

Treatments tested:

   1. Individual career counseling and job placement services (Job Bank only). The Job Bank included a job developer who would actively look for jobs for youths from employers in the community, and would try to locate specific kinds of jobs sought by youths if they were not already listed.

   2. Career counseling, job placement services, and special job search skills workshops (Job Bank and workshop).

   3. Career counseling and job search skills workshops (workshop only). No subjects were paid for participating.

Sample size: Job Bank only, 138; Job Bank and workshop, 140; workshop only, 123.

Target population: Youth ages 16-21, unemployed, and from low-income families (CETA-eligible).

Outcome of interest: Employment.

Research components:

Process analysis: Conducted. Focused on (1) low levels of enrollment, which were attributed to problems with outreach to targeted population, completion of income verification forms, and competition with CETA-subsidized jobs; (2) actual levels of participation, which were low in the group workshops, with typically only two or three young people present, apparently owing to lack of interest; and (3) effects of personnel turnover. The treatment organization was not really in place prior to the commencement of the experiment, so difficulties in starting up may have affected the impact analysis.

Impact analysis: Comparison of means, logit, and OLS.

Benefit-cost analysis: Not conducted.

Major findings:

(The data source used was follow-up surveys. The authors attempt to control for differential attrition bias by reporting both the results for all respondents and the results for respondents who completed all surveys. The pattern of response among the latter is much the same as among the former. Follow-ups were sent roughly once a quarter. The question being answered is essentially, Have you found a job since intake? Percentage answering "yes" can go down as well as up because of a less-than-complete response on each follow-up.)

| | Job-Finding Rates | | |
Survey	Job Bank Only	Job Bank & Wkshp.	Wkshp. Only
1st	70.7%	70.8%	70.8%
2nd	77.4	68.9	76.0
3rd	89.5	90.2	84.2
4th	92.3	83.3	89.5

Logit and OLS confirm that differences in outcome among the experimental groups were statistically insignificant.

Available data seem to indicate that the Job Bank and Workshop group, despite their apparent lack of incremental success in finding jobs, were more likely to have first-time jobs that were full-time and unsubsidized, and that their median earnings were higher; but the data were too incomplete for statistical testing.

Time trends in findings: Data too incomplete; response to surveys falls over time.

Problems and issues:

1. Treatment administrators believed that individual counseling was the most effective component of their program, but all three groups received individual counseling and therefore this component is untested.

Replicability: It is not clear that there was a prescribed workshop format with a content different from the individual counseling available to all subjects. In this sense the treatment is not replicable.

Generalizability: Probably none.

CAMBRIDGE JOB FACTORY

Information source: Andrew Hahn and Barry Friedman, "The Effectiveness of Two Job Search Assistance Programs for Disadvantaged Youth," with the assistance of Cecilia Rivera-Casale and Robert Evans, Center for Employment and Income Studies, Florence Heller Graduate School for Advanced Studies in Social Welfare, Brandeis University, Waltham, Massachusetts, 1981.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart, Joseph Seiler, and Gordon Berlin.

Treatment administrator: Cambridge Job Factory Program. Key personnel: Joseph Fisher.

Evaluator: Center for Employment and Income Studies, Florence Heller Graduate School. Key personnel: Andrew Hahn and Barry Friedman.

Enabling legislation: Youth Employment and Demonstration Projects Act of 1977.

Total cost: $202,940 (1979).

Dates: June 1979-July 1980; data collected through October 1980; final report 1981.

Dissemination effort:

Briefings: DOL.

Testimony: Hahn has testified about this experiment, as well as about other research, before Hawkins's committee in the House.

Distribution of executive summaries: DOL.

Policy deliberations pertaining to treatment: Hahn states that these findings have become part of a body of research relied on by youth assistance program operators. The gist of its message is that short-term programs have short-term benefits.

Location of treatment site: Cambridge, Massachusetts.

Number of treatment groups: Two (with one control group).

Treatment tested:

1. Experimentals. A four-week cycle in which participants were "hired and paid to get a job." The program components included minimum wage payments for attendance (which could be docked or terminated); a one-week Job Club program with special group problem-solving exercises; three weeks of supervised job search; and a bonus (two days' pay) for finding a job within the first three weeks.

2. Controls were told that because of funding limitations, no slots were open for them.

Sample size: 203 experimentals, 165 controls.

Target population: Unemployed youth ages 15-21. Recent high-school graduates, dropouts, and graduating seniors (no in-school youth) from CETA-eligible (low-income) families.

Outcomes of interest: (1) Employment. (2) Earnings.

Research components:

Process analysis: Conducted. It showed that enrollment targets for graduating seniors were difficult to meet (and some of those enrolled were only temporarily unemployed), but that graduate and dropout participation targets were feasible. It also showed that without stipends paid to participants, it was not feasible to run the program. (However, this was shown only for a cycle of graduating seniors.)

Impact analysis: Comparison of means, logit, and OLS.

Benefit-cost analysis: Not conducted.

Major findings:

(The data source used was follow-up surveys. The authors attempt to control for differential attrition bias by reporting both the results for all respondents and the results for respondents who completed all surveys. The pattern of response among the latter is much the same as among the former. Follow-ups were sent roughly once a quarter. The question being answered is essentially, Have you found a job since intake? Percentage answering "yes" can go down as well as up because of a less-than-complete response on each follow-up.)

	Job-Finding Rates	
Survey	Experimentals	Controls
First	63.6%	47.7%
Second	77.1	73.2
Third	79.3	78.0
Fourth	79.2	81.8

(The question being answered here is, Are you now employed?)

	Employment Rates	
Survey	Experimentals	Controls
First	63.1%	47.7%
Second	55.1	55.1
Third	56.4	51.4
Fourth	64.7	50.0

Logit and OLS confirm that the experimental effect on job-finding is statistically significant in the first quarter.

Cost per experimental was $715. Cost per one net new job (where new jobs are the expected

experimental/control differential at the close of the first quarter) was $4,468.

Time trends in findings: Apparent above.

Problems and issues: The first graduating seniors cycle included many students who either planned to go on to other education or had (before the program started) signed up for CETA-subsidized employment commencing shortly after the treatment ended.

Replicability: Replicable.

Generalizability: The sample is probably a representative low-income urban population, but the size is small and the targeting of graduating seniors problematic.

CAREER ADVANCEMENT VOUCHER DEMONSTRATION PROJECT

Information source: Clark, Phipps, Clark, and Harris, "Advanced Education and Training--Interim Report on the Career Advancement Voucher Demonstration," Department of Labor, Employment and Training Administration, Youth Knowledge Development Report 5.3, May 1980. The names of the authors are not given in this report, which was obtained from Bill Showler of the DOL. The only copy of the final report known to exist is in the files of Kenneth Clark and Associates, 615 Broadway, Hastings-on-Hudson, N.Y. 10706, 914-478-1010. The position of the firm is that its reports are proprietary to the client; on application to Dr. Clark, permission might be granted to inspect the report, but not to copy it. All copies sent to the funding agency, the Department of Labor's Office of Youth Programs, have apparently been lost. As of this writing, we have not been able to inspect this report.

Funding source: U. S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart.

Treatment administrator: Negro Scholarship Service-Fund for Negro Students (Atlanta); CETA prime sponsor (Pittsburgh); three "semi-autonomous" projects in Little Rock, Washington, and El Paso. Key personnel: Unknown.

Evaluator: Clark, Phipps, Clark, and Harris. Key personnel: Unknown.

Enabling legislation: None.

Total cost: $1.8 million (1980).

Dates: The period covered by the cited report is April 1, 1979, to March 31, 1980; data collected: unknown; date of final report: unknown.

Dissemination effort: None.

Briefings: None.

Testimony: None.

Distribution of executive summaries: A panel of the National Academy of Sciences reviewed the project reports and implicitly decided they had no useful scientific content because they did not include them in their own report. See the **Alternative Youth Employment Strategies** summary for detail and citation.

Other: The report states that an article titled "GI Bill for the Poor Being Considered by U.S." ran in the Washington Star in May of 1977. This was well before the actual experiment.

Policy deliberations pertaining to treatment: The research question for the Department of Labor was whether CETA funds should be used for higher education rather than job creation. Support for postsecondary education of low-income youths was a lawful use of CETA funds, but prime sponsors were not using their funds for this purpose. The experiment was designed to test whether the beneficial impact of grants for education would exceed those of subsidized employment. For several reasons, the experiment as conducted could not have addressed that

question. See "Process analysis." This experiment was terminated after two years by the Reagan administration, before the most important data could have been collected. Although the experimental treatment tested has substantial policy interest, it is clear that this termination was justified, at least in a narrow sense, in that the information being gathered could not have answered the research needs of the sponsor.

Location of treatment sites: Washington, D.C.; Pittsburgh, Pennsylvania; Atlanta, Georgia; Little Rock, Arkansas; and El Paso, Texas. The effectiveness of the experimental treatment was hypothesized to vary with the unemployment rate. El Paso and Pittsburgh had fairly high rates; the other three cities had fairly low rates.

Number of treatment groups: Five (with one control group).

Treatment tested: The experimental treatment was financial support for two years of a full-time college program (accredited, offering A.A., B.A., or both) plus counseling. In principle, the support was limited to schools charging $2,500 or less in tuition and located within commuting distance of the project site; but in practice, this was waived in Little Rock. Four variants of this treatment were randomly assigned in all five sites: (1) Voucher (no counselor approval for academic program required) plus assistance with involvement in college life; (2) Voucher without such assistance; (3) Nonvoucher (counselor approval required for academic program) plus assistance with involvement in college life; and (4) Nonvoucher, but no assistance with involvement in college life. The variants were to test the ability of the subjects to choose realistic programs without guidance (guidance would be more expensive and would reduce diversity) and the sociological finding that dropouts had little contact with the campus apart from attendance in class. The control treatment was designed to be regular CETA-subsidized employment.

Sample size: 490 experimentals (1: 125; 2: 120; 3: 125; 4: 120), 205 controls.

Target population: CETA-eligible low-income youths, 16-21 years old, who (1) were out of school, (2) had obtained a high school diploma or GED, (3) had had at most eight months of participation in other CETA programs, and (4) had a desire to attend college. (The last condition is emphasized because it critically affected the experiment.) In some sites there were additional screens. In Atlanta and Washington, subjects had to have a test score of 80 or better on an aptitude test (GATB). In El Paso, subjects needed proof of legal residency, letters of recommendation, and SAT or ACT scores. In Little Rock, they needed at least a C average in high school. In Pittsburgh, there were no additional requirements.

Outcomes of interest: (1) College attendance. (2) Employment. (3) Earnings. (4) Effectiveness of strategies to integrate subjects into ordinary college life.

Research components:

Process analysis: Control group youths failed to enroll in CETA programs. To be exact, only 20 out of 205 controls enrolled in CETA. The experiment, therefore, could not shed any light on whether college was more likely than subsidized employment to raise wages and employment among some representative group of low-income youths. The reasons for this failure were the recruitment sources and the identity of the program operators. Most subjects were recruited by high-school guidance counselors; many of them had no particular interest in subsidized employment or noncollege training. With the exception of Pittsburgh, the CAVD

projects were "semi-autonomous" from the local CETA programs. The local projects did not have any CETA jobs to provide, and the coordination problem with the CETA prime sponsors was apparently not solved. "The current manner in which the local sites relate to their respective regional offices does not particularly facilitate learning how to fit the CAVDP into the existing CETA system."

Impact analysis: None.

Benefit-cost analysis: None.

Major findings: The major substantive finding that could be drawn from this experiment would be the effect of the scholarship offer on enrollment. This turns out to be significant, but is not a focus of the report.

The following figures are calculated from the report.

Attending college, October 1979

	Experimentals		Controls	
Atlanta	78	(89%)	22	(82%)
El Paso	109	(98%)	36	(77%)
Little Rock	103	(100%)	12	(50%)
Pittsburgh	77	(85%)	6	(32%)
Washington	94	(95%)	15	(56%)

The control percentages have been calculated as percentages of controls whose status was known. The project had no information on about 30 % of the controls. About 5% of the experimentals were attending college without support from the project, usually because they had chosen colleges that they could not commute to from the project city.

First-semester dropout rates from college among experimentals are reported to be low, but are not compared with those of controls or of first-semester freshmen in general.

The effects of the voucher/nonvoucher experimental treatments on the diversity of colleges chosen are inconsistent.

Time trends in findings: None.

Problems and issues: Like other YEDPA experiments, this one tests an interesting idea on a large population. It then dissipates much of its value with a needlessly complex experimental design, an inadequate data collection strategy, and a severely flawed implementation. The flawed implementation can be blamed on the severe haste with which the program was set up.

Replicability: The experimental treatment is replicable and the control treatment is replicable, but whether the two treatments can be simultaneously assigned to different members of the same population with reasonable results must be doubted. It appears that low-income young people who are primarily interested in college and low-income young people who are primarily interested in subsidized employment are fundamentally different groups.

Generalizability: This is the only experiment of its kind. The findings indicate that there is a substantial population of young poor people who would attend college if they received scholarships that are not presently available. The findings do not indicate whether they will finish their programs if they receive such scholarships. It needs to be pointed out that the populations of the five cities differ by design, and it would not be possible to compute from the findings of this experiment how large the national population is that would respond to such scholarships.

WAGE-SUBSIDY VARIATION EXPERIMENT

Information source: Joseph Ball (deceased) and Carl Wolfhagen, "The Participation of Private Businesses as Work Sponsors in the Youth Entitlement Demonstration," Manpower Demonstration Research Corporation, 1981.

Funding source: U.S. Department of Labor, Employment and Training Administration, Office of Youth Programs. Key personnel: Robert Taggart.

Treatment administrator: Detroit Employment and Training Department. Key personnel: William Diaz does not recall.

Evaluator: Manpower Demonstration Research Corporation. Key personnel: Joseph Ball (deceased) and Carl Wolfhagen. The prime author of the evaluation of the entire project was William Diaz.

Enabling legislation: The Youth Employment and Demonstration Projects Act of 1977 authorized a very large nonexperimental demonstration, the Youth Incentive Entitlement Pilot Project (YIEPP). This experiment was a small part of YIEPP.

Total cost: Cannot be separated from YIEPP.

Dates: Treatment administered January-May 1980; data collected: same; final report March 1981.

Dissemination effort:

Briefings: Diaz does not recall.

Testimony: House Education and Labor Committee.

Distribution of executive summaries: Broadly published.

Policy deliberations pertaining to treatment: Diaz does not know of any.

Location of treatment site: Detroit, Michigan.

Number of treatment groups: Two.

Treatments tested:

1. A 100% wage subsidy was offered to businesses that would employ disadvantaged 16-19-year-olds who would be assigned by the project. Youths were guaranteed minimum wage employment if they stayed in school and maintained satisfactory school performance.

2. A 75% wage subsidy was offered. A list of over 1,000 possible employers was compiled, and the firms were randomly assigned to one treatment or the other. Payrolling was handled by the project administrator.

Sample size: (1) 519 businesses; (2) 569 businesses.

Target population: Private-sector businesses within commuting distance of the impact neighborhoods of the project.

Outcome of interest: Agreement to participate in youth employment.

Research components:

Process analysis: Interviews with subjects on reasons for participating or not participating.

Impact analysis: Difference in means. Also some logit.

Benefit-cost analysis: Not conducted.

Major findings:

Treatment	Contacted	Agreed to Participate	Rate
75% subsidy	569	44	7.7%
100% subsidy	519	81	15.6%

The difference is statistically significant, but "... wage considerations did not appear to be a first priority for most businesses."

Most important reasons for agreeing: "Chance to do something for disadvantaged youth; cheap or no-cost labor."

Most important reasons for not agreeing: "Not enough work for them to do; work inappropriate for teenagers."

Time trends in findings: Not applicable.

Problems and issues:

1. The project could not offer wage subsidies beyond August 1980.

2. "Both local economic conditions and the diligence of the call-back effort (to employers) appeared to affect the yield (of participants)."

3. Current or former YIEPP employer participants were excluded from the sample. The bias could be in either direction (current participants are presumably positive responders, former participants presumably negative), but since current participants exceeded former participants, the probable direction of the bias is to reduce participation rates in both treatments.

4. The experiment occurred during a serious recession in Detroit.

5. Displacement bias is a virtual certainty, and the authors conclude that the more real work a job involved the more likely it was that a wage-subsidy placement would displace an unsubsidized worker.

Replicability: Replicable.

Generalizability: Very limited, since only two levels of the subsidy were tested and, moreover, tested in the context of a serious local recession. Diaz has argued, however, that the results point to a very limited efficacy for a sub-minimum wage.

SUMMER EXPERIMENTAL YOUTH TRANSITION PROJECT

Information source: The original report appears to have been lost. It is by Susan Radcliffe, "Summer Experimental Youth Transition Project Analysis and Evaluation Report," Mayor's Office of Manpower Resources, Research and Evaluation, Baltimore, May 1981. The data are discussed in an unpublished paper by Elise Bruml, "Self-Directed Group Job Search: the Results," July 13, 1981.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Not available.

Treatment administrator: City of Baltimore, Mayor's Office of Manpower Resources. Key personnel: Not available.

Evaluator: City of Baltimore, Mayor's Office of Manpower Resources. Key personnel: Susan Radcliffe.

Enabling legislation: None.

Total cost: Not available. Payments to participants must have been at least $130,000 (1980).

Dates: July 1980-September 1980; data collected: same; final report May 1981.

Dissemination effort:

 Briefings: Internal briefings for Mayor's Office of Manpower Resources.

 Testimony: Unknown.

 Distribution of executive summaries: To DOL.

Policy deliberations pertaining to treatment: This experiment tested different forms of subsidized job search among disadvantaged young people. Reagan administration cutbacks in this area were comprehensive. In particular, the director of the Mayor's Office of Manpower Resources, Marian Pines, has no recollection of this experiment.

Location of treatment site: Baltimore, Maryland.

Number of treatment groups: Four (with one control group).

Treatment tested: Model 1 received a standard Job Club treatment: one week of job search skills workshop followed by three weeks of supervised group job search using a telephone bank. Model 2 also received a one-week skills workshop, followed by three weeks of individual job search assisted by a professional job developer. Model 3 was the same as the last three weeks of model 2 (i.e., no workshop). Model 4 received three weeks of "standard MOMR transition services," mostly counseling, if desired. Experimentals in models 1-3 received minimum-wage payments over the duration of the experiment.

Sample size: (1) 65, (2) 110, (3) 110, (4) 100.

<u>Target population</u>: Graduating low-income high-school seniors who had at least six months' experience in the Youth Incentive Entitlement Pilot Project. YIEPP was a major nonexperimental project guaranteeing full-time summer and part-time school-year jobs to disadvantaged youths, provided they remained in school.

<u>Outcome of interest</u>: Employment.

<u>Research components</u>:

Process analysis: Conducted. The observers noted that the staff assigned to the workshop phase of the group 2 model did not adhere to the prescribed presentation. Thus, the intended pure test of the effectiveness of supervised group job search as compared with individual job search did not occur.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Unknown.

<u>Major findings</u>:

1. For all subjects, the job-finding rate by the end of four weeks was:

Model 1: 26%*
Model 2: 12%
Model 3: 10%
Model 4: 7%

*The difference from the combined job-finding rate of the three other groups is statistically significant at the 1% level.

2. Of those who were functionally illiterate, the job-finding rates were:

Model 1: 50%
Model 2: 26%
Model 3: 17%
Model 4: 0%

Bruml does not provide the gross number of those who were functionally illiterate.

<u>Time trends in findings</u>: Only four weeks of data collected.

<u>Problems and issues</u>: See "Process analysis."

<u>Replicability</u>: This is a replication.

<u>Generalizability</u>: Impossible to evaluate. Findings can be compared with those of the **Cambridge Job Factory**, a similar intervention with a similar population.

ALTERNATIVE YOUTH EMPLOYMENT STRATEGIES PROJECT

Information source: Susan Sadd, Mark Kotkin, and Samuel R. Friedman, "Alternative Youth
    Employment Strategies Project Final Report," Vera Institute of Justice, August 1983. This
    report is unpublished. A "Special Summary" is available for free from Vera. The complete
    report may be purchased for $21.85.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert
    Taggart.

Treatment administrators: Three agencies--in Albuquerque, New Mexico (Office of Comprehensive
    Employment and Training Administration); in Miami, Florida (South Florida Employment and
    Training Consortium); and in New York City (Court Employment Project). Key personnel:
    Claire Haaga of Vera was in charge of management and coordination among sites.

Evaluator: Vera Institute of Justice. Key personnel: Susan Sadd.

Enabling legislation: Youth Employment and Demonstration Projects Act of 1977.

Total cost: $4.8 million (1981) for implementation; research only: roughly $1.5 million (1981).

Dates: July 1980-September 1981; data collected through May 1982; final report August 1983.

Dissemination effort:

    Briefings: None.

    Testimony: Has been cited by Andrew Hahn in congressional testimony to the Hawkins
    committee in the House.

    Distribution of executive summaries: To DOL.

Policy deliberations pertaining to treatment: The experiment was conceived, designed, and
    implemented within an unusually frenzied political context documented in Richard F.
    Elmore's "Knowledge Development Under the Youth Employment and Demonstration
    Projects Act, 1977-1981," in Youth Employment and Training Programs: The YEDPA Years,
    edited by Charles L. Betsey, Robinson G. Hollister, Jr., and Mary R. Papageorgiou, National
    Academy Press, Washington, D.C., 1985. Billions of dollars were appropriated by Congress
    throughout the Carter administration to deal with the emerging problem of minority youth
    unemployment. Significant congressmen were well aware that no treatment for this
    population was known to "work," and there was enormous pressure and funding to find
    alternatives that might. The experiment was finished under the Reagan administration, which
    cut back extensively on subsidized employment programs.

Location of treatment sites: Albuquerque, New Mexico; Miami, Florida; and New York City.

Number of treatment groups: Four (with one control group).

Treatments tested: All subjects receiving one of three experimental treatments could remain in the

program for up to 26 weeks.

Model 1. Full-time work experience with counseling and placement services. The work was in entry-level jobs with government or nonprofit agencies on the one hand, or in a supported work environment in which crews of participants performed building rehab, landscape, and clean-up work for community organizations on the other. Jobs were 35 hours per week.

Model 2. Basic education or vocational education or prevocational training with counseling and placement services. Educational option selected by the subject, in consultation with counselors. Payment was based on 30 hours a week participation, equivalent to payment in model 1 because the stipend was not taxed.

Model 3. Balanced and complementary part-time work experience and part-time training, with counseling and placement services. Payment based on 30 hours a week participation.

Controls. Received no services from the project, but $10 for completing the intake interview. Both controls and experimentals received $10 per follow-up interview.

The assignment process was complex. Subjects were randomly assigned to be either controls or experimentals (where experimentals would be in any of the three models). The first 225 experimentals in each of the three sites chose which model they wanted for themselves, with guidance from a counselor. In every case the client received either his first or second choice. The remaining experimentals in each site were randomly assigned.

Sample size: Controls, 1,137; experimentals, 1,082. Of the latter, 357 were in model 1, 355 in model 2, and 370 in model 3. The experimentals assigned randomly were 186 in New York, 151 in Miami, and 70 in Albuquerque.

Target population: Persons 16-21 years old, unemployed, YETP-eligible (low-income), out of school, and who were "high-risk," defined as having a prior involvement with the juvenile or criminal justice systems "or a substantial likelihood for such involvement in the future." At least 50%, by design, were to be on referral from the juvenile or criminal justice systems.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Arrests.

Research components:

Process analysis: Conducted. Probation officers in Albuquerque were uncooperative about referring clients to a random-assignment program where half of those referred received no services. 56% of the New York subjects were justice-system referrals, as were 49% of the Miami subjects, but only 29% of the Albuquerque subjects were. The Albuquerque sample is therefore unrepresentative of the target population: it was 41% female (compared with 26% in New York and 35% in Miami) and 43% had high-school diplomas or GEDs (11% in New York, 16% in Miami). Documentation of low-income status tended to screen out the high-risk target population. Criminal justice referrals were more likely than referrals from other sources to fail to bring documents with them and fail to keep subsequent intake appointments. There was no starting-up period whatever prior to intake; the initial cohort received lower quality services as a consequence. Model 3 in most cases could not be implemented as designed owing to insufficient preparation time; most participants received either mostly training or mostly work experience. The Labor Department's budget

commitment to the experiment was uncertain throughout the life of the project; "no provision was made for assuring program staff of employment beyond the end of the data collection period," and "an atmosphere of imminent doom developed in the last few months," which probably affected the quality of service delivery, especially when important staff left the project early. (This would have been most serious in Albuquerque; funding from other sources continued for a while in New York and Miami.) Budget uncertainties caused job developer positions, vital to the placement service, to go unfilled for long periods in Miami, and they were never filled in Albuquerque or New York.

Impact analysis: Conducted as difference in means, and with OLS and logit.

Benefit-cost analysis: Not conducted.

Major findings:

1. Employment data from eight-month follow-up:

"Have you worked" in the eight months since ... (for experimentals, exit from the program; for controls, intake)?"

	Overall	Albuquerque	Miami	NYC
Experimentals	51%	64.4%	47.2%	43.9%
Controls	41%	55.3	41.6	27.1

The overall difference is statistically significant, and is significant at all individual sites except Miami (the only site where job development was properly implemented).

2. Weekly earnings, most recent job (includes zeros for those who had no jobs):

	Overall	Albuquerque	Miami	NYC
Experimentals	$63.16	$68.39	$62.37	$59.12
Controls	47.67	52.82	56.12	32.78

The overall difference is statistically significant. The difference is almost entirely due to higher employment rates for experimentals, not to higher wages for those employed.

3. Percentage of the eight-month follow-up spent working: experimentals, 26.2%; controls 19.2%. The difference is highly significant.

4. In a regression, the effects of the three different models were insignificantly different from each other, except that those model 1 subjects who were employed had higher earnings during the follow-up than both model 2 and model 3 subjects who were employed.

5.There was no effect on arrests. Three-quarters of both experimentals and controls stayed out of trouble during the data collection period.

Time trends in findings: Only data from an eight-month follow-up were available for analysis.

Problems and issues:

1. One could argue that the selection of the treatment model by experimentals themselves almost necessarily prevented the evaluation from distinguishing the true treatment effects from unobserved participant attributes; thus, the results of the experiment could not have illuminated program design choices very much, even if the three different models had produced significantly different results. In designing the experiment, however, "the research staff came to believe that random assignment to model would substantially increase the rate at which participants failed to show up or dropped out of the program prematurely.... this would result in fewer people having a meaningful experience with the program. Moreover, since there was no reason to assume that the increased dropout rates would be uniform for the three models, there was no theoretical assurance of group equivalence at the start of the program."

2. The contradiction between the positive effects of model 1, as compared with controls, and the findings of the **National Supported Work Demonstration** project on the employment and earnings of young dropouts and ex-offenders in supported work is striking. The contradiction is even more striking because in New York City the same agency was administering both the **NSWD** treatment and the AYES treatment. The report does not address the discrepancy.

3. It is not possible to distinguish a site effect from an ethnic effect. For example, 79% of Albuquerque subjects were Mexican-American. About three-quarters of subjects in New York and Miami were black (but some of the Miami subjects were Haitian).

4. The labor market in the country was steadily worsening throughout the course of the experiment, especially for young unskilled workers.

5. Using logit on the dichotomous variable, "have you worked in the past 8 months," models 2 and 3 did significantly better than model 1. "The different conclusions reflect the fact that the logistic regression was applied to the entire 8-month sample ($N > 1300$) and is a more powerful statistical technique. Statistical significance is not hard to achieve with so large an N, but the size of the effect seems quite small." It might also reflect self-selection, given the assignment process. Some findings of the process analysis tend to support the proposition that this group needs education more than work experience. For example, "it was the impression of job developers and senior staff that many, if not most, terminating participants in AYES were not job-ready..." mostly because they were functionally illiterate.

Replicability: Replicable.

Generalizability: The AYES project was reviewed with other YEDPA projects by a committee from the National Academy of Sciences (see "Policy deliberations" for citation). Their primary reaction was to the finding of no significant differences among the three experimental models. "In several other studies, similar null findings for alternative treatments were also found. Indeed, this is the one finding that was fairly robust throughout the studies of labor market preparation programs we reviewed." Comparing the results of the 26-week program with the 10-12-week program, they found evidence, subject to some caveats, that "the same marginal gains in employment can be achieved as well by a shorter program."

CAMBRIDGE JOB SEARCH VOUCHER PROGRAM

Information source: Cecilia Rivera-Casale, Barry Friedman, and Robert Lerman, "Can Employer or Worker Subsidies Raise Youth Employment? An Evaluation of Two Financial Incentive Programs for Disadvantaged Youth," Center for Employment and Income Studies, Florence Heller Graduate School for Advanced Studies in Social Welfare, Brandeis University, September 1982. Only one copy of this report is known to exist. We have inspected it, courtesy of Barry Friedman and Andrew Hahn of the Florence Heller Graduate School.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart.

Treatment administrator: Cambridge Office of Manpower Affairs (CETA prime sponsor). Key personnel: Patricia Tankard and Timothy Reidy.

Evaluator: Center for Employment and Income Studies, Florence Heller Graduate School, Brandeis University. Key personnel: Cecilia Rivera-Casale, Barry Friedman, and Robert Lerman.

Enabling legislation: Funded under the Youth Employment and Demonstration Projects Act of 1977.

Total cost: Administration, $272,625 (1981).

Dates: November 1980-February 1982; data collected through April 1982; final report September 1982.

Dissemination effort:

Briefings: None.

Testimony: Andrew Hahn would have reviewed these findings in testimony to the House committee chaired by Hawkins, along with findings from other studies.

Distribution of executive summaries: None.

Other: See **Cambridge Job Factory** experiment.

Policy deliberations pertaining to treatment: Friedman is unaware of any.

Location of treatment site: Cambridge, Massachusetts.

Number of treatment groups: Three (with one control group).

Treatment tested: Full-treatment experimentals. This group received a Job Club model treatment over four weeks; the treatment combined a week-long workshop in job search skills with supervised group job search organized around a phone bank. For each hour spent in this Job Factory, subjects received the minimum wage. Those who obtained jobs were paid a supplemental bonus of $1.50 an hour (up to 48 hours a week) for the first two weeks on the job, $1.00 an hour for the following ten weeks. If the subject left one job for another during

the 12 weeks, the subsidy would carry over to the new job.

Voucher-only experimentals received no help with job search, but were entitled to the same 12-week subsidy if they found a job.

Controls received neither job search assistance nor a subsidy.

Hours worked were verified by pay stubs or special employer records. There was generally a four-week lag in voucher payments.

Sample size: Full-treatment experimentals, 161; voucher-only, 130; controls, 108.

Target population: CETA-eligible (low-income) Boston area youth, ages 16 to 22.

Outcomes of interest: (1) Employment. (2) Earnings.

Research components:

Process analysis: Conducted. Later intake cycles were affected by turnover of top staff and office relocation. Payment of wages to subjects for attendance in the Job Factory treatment were believed to have brought in a number of "program hustlers" with no real interest in finding unsubsidized employment.

Impact analysis: Conducted with OLS and logit.

Benefit-cost analysis: Cost per new job calculated.

Major findings:

Data come from three follow-up surveys, taken at four, 12, and 20 weeks after intake.

1. Difference in probability of having worked at all since the previous follow-up (or since intake), logit estimation:

	1st Follow-up	2nd Follow-up	3rd Follow-up
Experimentals (both groups) vs. controls	.035	.123*	.190*
Full-treatment vs. voucher-only	.142*	.008	-.150*
Actual control mean	.343	.446	.512

*Statistically significant at one-tail, 5% level test.

"A voucher paid to workers consistently raised employment of disadvantage youth... the voucher impacts rose over time ... the combined Job Factory plus voucher treatment produced employment gains in the initial period after program start-up but the combined treatment did no better and sometimes worse than the voucher alone in later periods."

2. Effect on wage rates of those who worked, OLS estimate, in dollars:

	1st Follow-up	2nd Follow-up	3rd Follow-up
Experimentals dummy variable	-0.1183	-0.4620	-0.7830*
Full-treatment dummy	0.1997	0.5570*	0.5150*
Actual control mean	3.81	4.07	4.40

("Experimentals dummy" is a measure of the effect of the voucher alone. "Full-treatment dummy" is a measure of the additional effect of the Job Factory. The number of observations are 90, 72, and 89 among the three follow-ups, respectively.)

*Statistically significant at one-tail 5% level test.

The only data on job retention and hours worked comes from voucher payments to experimentals. Differences between full-treatment and voucher-only groups are not significant.

3. Cost per new job (net over controls):

Full-treatment, 1st follow-up:	$6,739
ever found job:	$8,611
Voucher-only, 1st follow-up:	$2,220
ever found job:	$793

"The evidence from this experiment does not suggest any positive long-term benefits" from the Job Factory treatment, although short-term employment effects are noted.

Time trends in findings: As noted above.

Problems and issues:

1. Findings about the Job Factory do not generalize to Job Club treatments where participants are not paid to attend.

2. No attention is paid to potential attrition bias, and sample attrition is considerable.

Replicability: Replicable.

Generalizability: Findings are for one job market only.

WILKES-BARRE JOB SEARCH VOUCHER PROJECT

Information source: Cecilia Rivera-Casale, Barry Friedman, and Robert Lerman, "Can Employer or Worker Subsidies Raise Youth Employment? An Evaluation of Two Financial Incentive Programs for Disadvantaged Youth," Center for Employment and Income Studies, Florence Heller Graduate School for Advanced Studies in Social Welfare, Brandeis University, September 1982.

Funding source: U.S. Department of Labor, Office of Youth Programs. Key personnel: Robert Taggart.

Treatment administrator: Youth Employment Service. Key personnel: Frank Hines and Richard Borofsky.

Evaluator: Center for Employment and Income Studies, Brandeis University. Key personnel: Cecilia Rivera-Casale, Barry Friedman, and Robert Lerman.

Enabling legislation: Funded under the Youth Employment and Demonstration Projects Act of 1977.

Total cost: Cost of operations $181,044 (1981).

Dates: January-December 1981; data collected: same; final report September 1982.

Dissemination effort:

　　Briefings: None.

　　Testimony: Andrew Hahn would have reviewed these findings in testimony to the House committee chaired by Hawkins, along with findings from other studies.

　　Distribution of executive summaries: None.

　　Other: See **Cambridge Job Factory** experiment.

Policy deliberations pertaining to treatment: Friedman is unaware of any. The Targeted Jobs Tax Credit (TJTC) continues to exist, although an implication of the experiment is that the TJTC is ineffective. See comment on the **Dayton Wage-Subsidy** experiment.

Location of treatment site: Wilkes-Barre, Pennsylvania.

Number of treatment groups: Three (with one control group).

Treatments tested:

　　1. Voucher and TJTC. Job developers visited these employers, informed them about the Targeted Jobs Tax Credit (a wage subsidy in the form of a reduction on personal or corporate federal income tax available for employers of low-income 18-24 year olds), and also made available to them a special subsidy (the voucher) for hiring 16- and 17-year-olds. The voucher

subsidy was $1.80 per hour for the first three months a youth worked for the firm, and $1.00 per hour for the next five months. The intention was to stimulate the hiring of youths from the Wilkes-Barre YES program, although the TJTC could not be restricted to them.

2. TJTC only. Job developers visited these employers, informed them of the availability of the TJTC, and encouraged them to hire program youths.

3. Control employers were not contacted by job developers.

A sample of 375 employers was drawn and stratified according to size, location, and the intensity of youth employment in their industries. Random assignment was performed within strata.

Program youths attended individual and group counseling sessions to develop career goals and job search skills.

Sample size: (1) 125; (2) 125; (3) 125. Total 375.

Target population: Potential employers.

Outcome of interest: Employment of low-income youths by employers who received wage subsidies.

Research components:

Process analysis: Conducted. The program was affected by high staff turnover. The turnover itself was increased by the near certainty that the Labor Department would terminate the program at the end of the period.

Impact analysis: Not conducted, for reasons that will be obvious from "Major findings."

Benefit-cost analysis: Not conducted.

Major findings:

Impact of voucher. Of 125 firms contacted, three took advantage of the voucher. Another firm, outside the sample, asked to use the subsidy and was allowed to; the four firms hired five workers.

Impact of TJTC. Precisely one firm from each group used the TJTC. The voucher-group firm employed seven youths; the other two firms hired one each.

Time trends in findings: None.

Problems and issues:

1. It is not clear why the Wilkes-Barre YES organization was selected to run this experiment. The previous Workshop experiment, run by YES and evaluated by the same team from Brandeis, was poorly implemented. It was plagued by many of the same factors that are discussed in the process analysis of this experiment, and ceased operation when the DOL grant

ended in December 1981.

2. The unemployment rate in Wilkes-Barre in December 1981 was 11.7%. High unemployment among all workers would in any case have resulted in poor job prospects for teenagers ("On the average employers report 11 applicants for each entry level opening ... among all ages"). It could be argued that a wage subsidy would be most effective in the special case in which overall unemployment is very low but youth unemployment is much higher than overall unemployment. Wilkes-Barre in 1981 did not fit this special case.

Replicability: Replicable.

Generalizability: This is one of four small-sample experiments that have addressed the effectiveness of wage subsidies paid to employers of low-income youths. The others were in Dayton, Baltimore, and Detroit. The findings do not support the proposition that wage subsidies paid to employers stimulate employment.

DIGEST OF THE SOCIAL EXPERIMENTS:

IV. The Mentally Impaired

TRAINING IN COMMUNITY LIVING

<u>Information sources</u>: Leonard Stein, Mary Ann Test, and Burton Weisbrod, "Alternative to Mental Hospital Treatment," <u>Archives of General Psychiatry</u>, Volume 37, April 1980, 392-412. Burton Weisbrod, "Benefit-Cost Analysis of a Controlled Experiment: Treating the Mentally Ill," Institute for Research on Poverty Reprint #444, University of Wisconsin-Madison, 1981.

<u>Funding source</u>: National Institute of Mental Health. Key personnel: None.

<u>Treatment administrator</u>: Mendota Mental Health Institute. Key personnel: Leonard Stein and Mary Ann Test.

<u>Evaluators</u>: Mendota Mental Health Institute, Institute for Research on Poverty. Key Personnel: Burton Weisbrod.

<u>Enabling legislation</u>: None.

<u>Total cost</u>: Not published, and Stein does not recall; research only: not published.

<u>Dates</u>: 1972-1975; data collected 1972-1976; final report April 1980.

<u>Dissemination effort</u>:

   Briefings: Presentations to psychiatric groups.

   Testimony: State legislators.

   Distribution of executive summaries: None.

<u>Policy deliberations pertaining to treatment</u>: Stein thinks it was cited in legislative deliberations in Wisconsin, but does not recall any specifics.

<u>Location of treatment site</u>: Madison, Wisconsin.

<u>Number of treatment groups</u>: Two (with one control group).

<u>Treatments tested</u>:

   (1) Controls treated in hospital as long as necessary, and referrals made on release to community agencies. Outpatient follow-up was available.

   (2) Experimentals were seldom hospitalized initially and received 24-hour, on-call care from specially trained staff members focusing on coping skills (laundry, shopping, grooming, finding work, problem solving on the job, constructive use of leisure) over a 14-month period. Staff assertively sought out patients when they missed appointments or failed to show up for work; monitored use of medication; and counseled family members and employers.

<u>Sample size</u>: 62 experimentals, 60 controls.

<u>Target population</u>: Residents of Dane County, Wisconsin, aged 18-62, voluntarily seeking admission to a mental hospital, with any diagnosis other than severe organic brain syndrome or primary alcoholism.

<u>Outcomes of interest</u>: (1) Reductions in institutionalization. (2) Reductions in unemployment. (3) Increases in earnings.

<u>Research components</u>:

Process analysis: Not conducted. Researchers could not say to what extent the effects observed were primarily due to increased compliance with prescribed medications.

Impact analysis: Comparison of means.

Benefit-cost analysis: Formal social benefit-cost analysis conducted.

<u>Major findings</u>:

(1) Significant reduction in time spent in psychiatric institutions. Significant differences extending over the first 16 months.

(2) No significant differences in time spent in penal or general medical institutions.

(3) Significant increase (through 20 months) in time spent in unsupervised living situations.

(4) Significant reduction (through 28 months) in time spent unemployed, mostly achieved through substantial increase in time spent in sheltered employment.

(5) Significant increases (through end of observation period) in nonsheltered earned income.

(6) Net social benefits of $399 per patient. (Valued costs exceeded valued benefits by $6,128 for controls, $5,729 for experimentals.) However, the reduction in transfer payments of $564 per patient is essentially treated as a social benefit, which is not customary.

<u>Time trends in findings</u>: All differences except in nonsheltered earnings tended to erode after treatment ended.

<u>Problems and issues</u>: The main employment effect comes from increasing use of sheltered employment. Where sheltered employment opportunity is less available than in the treatment community, this result may not generalize. Also, the use of sheltered placements raises questions about (1) possible displacement of other persons who might have found work in the sheltering agencies and (2) whether increased subsidies were needed by these sheltering agencies, which are not included as costs. If they were, the analysis did not include them.

<u>Replicability</u>: Requires specially trained, "assertive," around-the-clock staff. See also comments on sheltered employment above.

<u>Generalizability</u>: Investigators could not extrapolate the findings to larger or smaller communities, or to different labor-market conditions.

JOB PATH

Information source: Sally T. Hillsman, Janet Weinglass, and Arlene Silberman, "Fostering Independence in Developmentally Disabled Adults: Supported Work as a Rehabilitative Mechanism," paper presented at the annual meeting of the American Orthopsychiatric Association, Boston, Massachusetts, April 1983. The paper is unpublished. It can be obtained from the Vera Institute.

Funding source: A private foundation. Hillsman does not recall which one.

Treatment administrator: Job Path, an arm of the Vera Institute of Justice. Key personnel: Arlene Silberman.

Evaluator: Vera Institute of Justice. Key personnel: Sally T. Hillsman and Janet Weinglass.

Enabling legislation: None.

Total cost: Not available.

Dates: 1978-1979; data collected through 1980; final report 1983.

Dissemination effort:

Briefings: Federal and state officials.

Testimony: Hillsman thinks so.

Distribution of executive summaries: None.

Other: Presentations at professional organizations.

Policy deliberations pertaining to treatment: Hillsman states that the treatment and the findings together have had a powerful impact on policy, and have changed the nature of support services to the developmentally disabled in New York and across the country.

Location of treatment site: New York City.

Number of treatment groups: Two (with one control group).

Treatment tested: The experimental treatment placed subjects in supported work assignments, initially in the public sector, for 35 hours a week. They were paid a subsidized minimum wage while learning food-service, clerical, mailroom, maintenance, housekeeping, and messenger skills. Supervisors were assisted by Job Path counselors. Expectations were gradually increased over time, with transfers to more demanding job sites, sometimes going from the public to the private sector. Counseling and supervision were structured to give subjects feedback on how well they were doing, and weekly group meeting for trainees provided mutual support. Controls were returned to the referral agencies from which they had come; six of the controls subsequently were allowed to enter the experimental treatment after the research intake was completed, and this complicated the interpretation of some of the

research findings.

Sample size: 60 experimentals, 60 controls.

Target population: Mentally retarded adults who did not hold unsubsidized employment. (Many of them did work in sheltered workshops.)

Outcome of interest: Employment.

Research components:

Process analysis: Not conducted in the usual sense; this was more like a pilot program. The authors report very heavy job development work had to be performed (2,020 telephone contacts, etc.) to develop 71 training sites and 34 unsubsidized jobs. They also report on subjective responses of experimental subjects to the changes in their lives resulting from working in nonsheltered employment.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Not conducted.

Major findings: In general the authors do not report whether differences are statistically significant.

Employment.

Six months after intake: 44% (24 out of 54) of experimentals had full-time, unsubsidized jobs; usually this was their last Job Path job, and the employer had hired them without subsidy. 20% (13 out of 54) of controls had full-time jobs without subsidies. Another 24% of controls had part-time, unsubsidized jobs.

Twelve months after intake: 61% of experimentals held full-time, unsubsidized positions. 24% of controls held such jobs; another 12% had part-time jobs without subsidy.

Fifteen months after intake: 61% of experimentals held unsubsidized jobs (hours unspecified), while 30% of controls did.

Eighteen months after intake: 72% of experimentals and 42% of controls had unsubsidized employment, a difference that is statistically significant.

Wages.

Fifteen months after intake: Average weekly earnings of employed experimentals, $146; of employed controls, $117.

Benefits.

Most employed experimentals had private health and dental insurance, paid sick days and vacations, and workman's compensation coverage. Most employed controls did not. Sample sizes declined over time.

<u>Time trends in findings</u>: Given above.

<u>Problems and issues</u>:

1. Small sample size. Apparently this is the reason the authors generally do not report statistical significance.

2. New York City. This is a more difficult environment for the independent mentally retarded than some other areas; on the other hand, there is a larger variety of employers.

3. A deteriorating labor market over the course of the experiment.

4. Pilot project staff are frequently more able or more enthusiastic than staff in regular projects.

<u>Replicability</u>: Replicable.

<u>Generalizability</u>: Not generalizable, because of items mentioned in "Problems and issues." For generalizable implications, see **Structured Training and Employment Transitional Services Demonstration (STETS)** experiment and **Transitional Employment Training Demonstration (TETD)**.

**STRUCTURED TRAINING AND EMPLOYMENT TRANSITIONAL SERVICES
DEMONSTRATION (STETS)**

Information source: Stuart Kerachsky and Craig Thornton, "Findings from the STETS Transitional
Employment Demonstration," Exceptional Children, April 1987, 515-521.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key
personnel: None.

Treatment administrator: Manpower Demonstration Research Corporation. Key personnel:
Judith M. Gueron.

Evaluator: Mathematica Policy Research. Key personnel: Stuart Kerachsky and Craig Thornton.

Enabling legislation: None.

Total cost: $2.5 million (1983) for service delivery; research only: $1.2 million (1983) for impact
and benefit-cost analysis.

Dates: November 1981-June 1984; data collected through October 1984; final report 1985.

Dissemination effort:

Briefings: DOL officials.

Testimony: None.

Distribution of executive summaries: To the whole MDRC mailing list, which is extensive.

Other: Findings were informally discussed with Department of Education officials concerned
with related programs.

Policy deliberations pertaining to treatment: Thornton states that the Department of Labor has done
nothing with the findings. Although results of the impact and benefit-cost analysis were
positive, MDRC, which has widely publicized other research, was disappointed with them,
because the percentage of those subjects who found employment was still small.

Location of treatment sites: Cincinnati, Ohio; Los Angeles, California; New York City; St. Paul,
Minnesota; Tucson, Arizona.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals received a three-phase treatment of up to 18 months. Phase 1
consisted of initial training and support services in a low-stress work environment, with paid
employment of up to 500 hours. Phase 2 was essentially a period of on-the-job training
(subsidized or unsubsidized) in local firms and agencies, emphasizing job performance and
work stress that resemble the demands faced by nondisabled workers in the same types of jobs.
By design, phase 2 jobs were intended to be potentially permanent jobs in which participants
would continue after the withdrawal of program support. Phase 3 consisted of up to six months

of follow-up services to those workers who had made the transition into unsubsidized, competitive employment. Controls received no STETS services.

Sample size: Experimentals, 236; controls, 231.

Target population: 18-24 year-olds with IQ scores between 40 and 80, limited prior work experience, and no severe secondary handicaps.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Transfer recipiency. (4) Payments.

Research components:

Process analysis: Conducted. The authors believe the evidence suggests that ongoing programs would have an impact greater than that found in the full sample; the employment behavior of experimentals and controls was most different, and the administrative cost per participant was lowest, during the "steady-state period," defined as the five months during which client intake reached its maximum monthly rate and during which operations were relatively smooth.

Impact analysis: Conducted with OLS. Probit and tobit were also used where appropriate and did not yield substantively different results than those yielded using OLS.

Benefit-cost analysis: Conducted from participant, taxpayer, and social perspectives.

Major findings:

1. Employment in regular (unsubsidized) job:

	Month 6	Month 15	Month 22
Experimentals	11.8%	26.2%*	31.0%*
Controls	10.7	16.8	19.1

2. Average weekly earnings from regular job (includes zeros):

Experimentals	$11.81	$26.90*	$36.36*
Controls	9.81	16.31	20.55

3. Average weekly earnings from any job (includes zeros):

Experimentals	$52.39*	$37.91*	$40.79*
Controls	25.93	26.48	28.41

4. Percentage in any training (including STETS)

Experimentals	61.7%*	20.6%*	16.6%*
Controls	40.6	28.4	29.1

5. Percentage in any schooling

	Month 6	Month 15	Month 22
Experimentals	7.5%*	6.2%	8.0%
Controls	15.7	10.1	11.4

6. Percentage receiving any cash transfers (most commonly SSI, SSDI)

	Month 6	Month 15	Month 22
Experimentals	31.7%*	44.5%*	49.6%
Controls	43.1	51.5	52.0

7. Average monthly income from transfers (includes zeros)

	Month 6	Month 15	Month 22
Experimentals	$80.23	$114.78	$126.53
Controls	99.98	138.72	136.08

8. Average weekly personal income (including earnings, transfers, and other regular sources)

	Month 6	Month 15	Month 22
Experimentals	$71.72*	$67.22	$71.59
Controls	50.94	59.67	62.39

* Experimental/control difference is statistically significant at the 5% level.

Subgroup analysis found that the more retarded (the lower the IQ) the subject, the greater the impact, with essentially no impact on the slightly retarded.

From a social perspective, total benefits will outweigh the costs. The taxpayer investment will be repaid within four and a half years in lower outlays; from the social perspective, the investment will pay for itself after two and a half years.

Time trends in findings: Shown above.

Problems and issues: The subgroup analysis finding that the more retarded the subject, the greater the impact, is precisely the opposite of the finding in the **Transitional Employment Training Demonstration**. Thornton explains this as a difference in the sample: IQ's in STETS ranged from about 40 to about 80, while IQ's in TETD were, as worst, too low to test, at best, somewhat over 70. He believes that higher-IQ individuals who get into this type of program tend to have severe secondary problems inhibiting employment, while the severely retarded are probably unemployable. He believes the intervention is most likely to succeed with those between the extremes; on the other hand, he cautions that simply using t-tests for the statistical testing of this hypothesis can be misleading.

Replicability: Replicable.

Generalizability: The study findings "are based on only five judgmentally selected urban sites, whose programs were specially designed and implemented for this demonstration. We cannot be certain whether other program operators in other sites who operate ongoing programs under different social, political, and economic conditions would have similar experiences.... It is also problematic whether similar programs could be efficiently and effectively operated in rural

areas or even in more dispersed labor markets." Findings may be compared with the previous **Job Path** experiment and the subsequent **Transitional Employment Training Demonstration.**

TRANSITIONAL EMPLOYMENT TRAINING DEMONSTRATION (TETD)

Information source: Craig Thornton and Paul T. Decker, "The Transitional Employment Training Demonstration: Analysis of Program Impacts," Mathematica Policy Research, July 1989.

Funding source: U.S. Dept. of Health and Human Services, Social Security Administration. Key personnel: Aaron Prero.

Treatment administrator: Eight training organizations. Key personnel: Too many to list.

Evaluator: Mathematica Policy Research. Key personnel: Craig Thornton.

Enabling legislation: Social Security Disability Amendments of 1980.

Total cost: Research only: $1,271,307 (1987).

Dates: June 1985-June 1987; data collected through December 1988; final report July 1989.

Dissemination effort:

Briefings: None.

Testimony: None.

Distribution of executive summaries: Federal officials and researchers in this area.

Other: As of May 1990, Thornton was working on a journal article about the findings.

Policy deliberations pertaining to treatment: One of the findings was that program costs could not be funded out of savings in the Social Security budget, and Thornton states that this implies that no short-run change in policy will result. Long-term policy impacts cannot be judged at the time of this writing.

Location of treatment sites: 13 sites: Dover, Delaware; Harrisburg, Lancaster, Philadelphia, and York, Pennsylvania; Monmouth County, New Jersey; Chicago, Illinois; Boston, Massachusetts; Los Angeles, California; Milwaukee, Wisconsin; Portland, Oregon; and Chippewa, Dunn, Eau Claire, and Pepin Counties, Wisconsin.

Number of treatment groups: Two (with one control group).

Treatments tested:

1. Experimentals were placed in unsubsidized, potentially permanent jobs; provided specialized on-the-job training that was phased out over time; and were able to receive postplacement support and follow-up as necessary. These "core services" were to be provided within one year of intake into the experiment; subsequent services were to be arranged as necessary but had to have funding from some source other than the demonstration.

2. Controls received none of these services, but were free to seek other services in the

community.

One of the goals of the experiment was to test various approaches to delivery of service. Providers were competitively selected to represent different methods and philosophies of service delivery.

Sample size: Experimentals, 375; controls, 370.

Target population: Mentally retarded SSI recipients. They had to apply to enter the experiment, and had to be between 18 and 40 years old. The average IQ score was 57. Intake workers excluded persons who had severe emotional problems, or who would otherwise not benefit from the treatment, prior to random assignment.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Wage rates. (4) SSI payments.

Research components:

Process analysis: Conducted. It showed that the treatment was substantially implemented as planned, and (among other things) that transportation barriers were often as serious or more serious than the lack of job skills.

Impact analysis: Conducted with raw means and OLS.

Benefit-cost analysis: Conducted.

Major findings:

1. Two-thirds of experimentals were placed on jobs. One half of those placed (one-third of experimentals) were "successfully stabilized" on a potentially permanent job. These results were consistent with the initial expectations of the program designers.

2. By the third year after enrollment, 45% of experimentals were in unsubsidized jobs, as compared with 30% of controls. (Experimentals spent 32% less time than controls in sheltered workshops.)

3. Estimated treatment effects on earnings over three years ("percent change" represents the increase over the raw mean of the controls):

Year	Impact	Percent change
1	$665	108%
2	909	96
3	742	63

4. Effects on SSI payments were small, on the order of $240, or 2%, over three years. Earnings increased but generally remained below the income disregards in SSI regulations.

5. Earnings impacts varied considerably across sites. The New Jersey project raised earnings by $2,000 a year over three years; it tried to place experimentals in light manufacturing and assembly jobs. Projects in Portland and Los Angeles were exceptions to the general rule of

treatment impacts declining over time.

6. Treatment impacts rose with IQ scores. Persons with IQ's over 70 had an earnings gain of over 200%, while the impact on those with IQ's under 40 had a gain that was not statistically significant.

7. Average treatment costs per person enrolled were $5,600.

8. The treatment raised the net income of experimentals, but SSI savings did not offset the costs from an SSI-budget perspective. From a government-budget perspective, the costs and benefits were about equal, because the program costs were offset by reductions in the use of sheltered (and subsidized) workshops. Targeting services to currently sheltered workers would mean that the program would have a neutral effect on budgets. From a social perspective, the benefits exceeded the costs.

Time trends in findings: Note the impact reductions in "Major findings," #3; most projects showed impacts declining over time.

Problems and issues:

1. Those enrolled in the experiment represent about 5% of those who were sent initial invitation letters; two-thirds of those responding were screened out at intake. They therefore represent not the population of retarded SSI recipients, but that part of the population who volunteered for these services, and were thought to have some probability of benefiting from them. In addition, transitional employment (and some of the agencies) were unfamiliar; only the more adventuresome members of the population would have left the well-established, sheltered worksites. As agencies become more established, this will be less true. Findings from the experiment will not necessarily reflect the impacts of the treatment on a less adventuresome population.

2. The training organizations were competitively selected from 80 providers who applied. These agencies were chosen to reflect different treatment approaches; generally, the less rigid treatment approaches appeared to have the most success. The findings are from a mix of successful and unsuccessful programs, all conducted by the agencies that were judged to offer the best versions of alternative approaches; findings from a mix of agencies offering the same, relatively successful, approach, but with different degrees of competence, might not be the same.

3. In determining benefits and costs, it turns out to be important whether the alternative is sheltered employment or no employment. Budgetary savings are possible if the alternative is sheltered employment, but otherwise do not occur.

Replicability: The eight providers of treatment had different methods and philosophies, each of which could be replicated.

Generalizability: The comments above under "Problems and issues" generally reflect Thornton's caveats. The experiment represents a reasonably large-scale national test; the only region excluded was the South.

DIGEST OF THE SOCIAL EXPERIMENTS:

V. Persons Charged with or Convicted of Crimes

LIVING INSURANCE FOR EX-OFFENDERS (LIFE)

Information sources: Charles D. Mallar and Craig Thornton, "Transitional Aid for Released Prisoners: Evidence from the LIFE Experiment," Journal of Human Resources, Spring 1978, 208-236. Kenneth J. Lenihan, Unlocking the Second Gate: The Role of Financial Assistance in Reducing Recidivism Among Ex-Prisoners, R&D Monograph 45, U.S. Department of Labor, Employment and Training Administration, 1978.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: Howard Rosen.

Treatment administrator: Bureau of Social Science Research. Key personnel: Kenneth J. Lenihan.

Evaluators: Bureau of Social Science Research and Mathematica Policy Research. Key personnel: BSSR: Kenneth J. Lenihan; MPR: Charles D. Mallar and Craig Thornton.

Enabling legislation: None.

Total cost: $230,000 (1973); research only $30,000.

Dates: Fiscal years 1972-1974; data collected 1972-1975; final report 1978.

Dissemination effort:

Briefings: A conference including Labor and Justice Department officials, and academic criminologists, leading to the later **Transitional Aid Research Project** experiment.

Testimony: None.

Distribution of executive summaries: Department of Labor.

Other: Talks at research institutes.

Policy deliberations pertaining to treatment: Lenihan states that a California legislator, citing LIFE results, eventually obtained financial assistance for released prisoners.

Location of treatment site: Baltimore, Maryland.

Number of treatment groups: Four (with one control group).

Treatment tested:

(1) Controls. No treatment.

(2) Financial Aid. $60 a week for 13 weeks, conditional on not being reimprisoned. If the subject had earnings above $40 a week, 50% of those earnings was subtracted from the weekly payment and deferred to a later week, thus slightly extending the 13-week period.

(3) Job Placement Services. Staff members worked full-time finding job openings, chauffeuring

experimentals to interviews and helping them fill out job applications, and advocating on experimentals' behalf with employers and bureaucrats.

(4) Financial Aid and Job Placement. Experimentals received both job service and financial aid.

Sample size: (1) 108; (2) 108; (3) 108; (4) 108.

Target population: Male ex-offenders returning to Baltimore from prison, nonaddicts, with records of multiple prior offenses, at least one of them for theft; under 45; and having less than $400 in savings.

Outcomes of interest: (1) Rearrest. (2) Employment. (3) Earnings.

Research components:

Process analysis: Extensive interviews of experimentals.

Impact analysis: Conducted both as difference in means and by regression and probit.

Benefit-cost analysis: Conducted from several perspectives with upper and lower bounds on confidence.

Major findings:

(A) Job placement services had no statistically significant impacts. The remaining findings are reported for financial experimentals (groups 2 and 4) versus financial controls (groups 1 and 3).

(B)	Experimentals	Controls
New arrests, all theft crimes	48	66
Estimated new arrests, from regression with other factors	48.6	66
Estimated new arrests, from probit with other factors	45.5	66
New arrests, all crimes	107	123
In school or training, first quarter	3.7%	1.4%
second quarter	4.2%	2.0%
Employed full-time, fourth quarter	54.7%	49%

All of the differences above are statistically significant; however, schooling differentials are not significant after the second quarter and employment differentials are not significant in the first, second, and third quarters. There are no statistically significant differences in weekly earnings in any quarter.

(C) Benefit-cost analysis from a social perspective: Lower Bound: $108,565 benefits, $27,000

costs; 4.02 benefit-cost ratio. Upper Bound: $870,431 benefits, $16,200 costs; 53.73 benefit-cost ratio.

Benefit-cost ratio from other perspectives:

	Lower	Upper
Budgetary	0.49	2.67
Nonparticipant	0.77	3.99
Participant	1.93	3.76

Time trends in findings: The difference in arrests was 16 in the second year, compared with 18 in the first year, indicating the effect did not disappear. However, the second-year data is of a lower quality since it relies on Baltimore area court records, and some subjects had left the area.

Problems and issues: The primary grounds for uncertainty are the value of increased output and the size of the losses from theft. Benefit-cost ratios reflect various assumptions about the social discount rate, the rate of decline in the effect on recidivism, the costs of the judicial system, direct losses from theft, displacement effects, and the pattern of change in the dependence on welfare payments.

Replicability: Replicable.

Generalizability: The sample selection criteria were deliberately chosen in order to assemble a group that was likely to show a strong response. Since a policy would necessarily embrace a larger group, the **Transitional Aid Research Project** (TARP) was funded in order to determine whether the experimental effects would be repeated in a wider population. Lenihan notes that Baltimore had fairly inexpensive inner-city housing at the time of the experiment, and thus the experimental response was obtainable there at a lower cost than in some other cities.

TRANSITIONAL AID RESEARCH PROJECT (TARP)

Information source: Peter H. Rossi, Richard A. Berk, and Kenneth J. Lenihan, Money, Work and Crime: Experimental Evidence, Harcourt Brace Jovanovich, 1980.

Funding sources: U.S. Department of Labor, Employment and Training Administration, and the Department of Justice, Law Enforcement Assistance Administration. Key personnel: DOL: Howard Rosen; DOJ: Howard Rosen.

Treatment administrators: Texas Department of Corrections, Georgia Department of Corrections, and the Employment Security Agency. Key personnel: Coordination and experimental design were performed by Kenneth J. Lenihan. To identify persons at different agencies with TARP-related responsibilities, contact Sheldon Olson for Texas and Lois Sanders for Georgia.

Evaluators: Social and Demographic Research Institute, University of Massachusetts at Amherst, and the Group for Research on Social Policy, University of California at Santa Barbara. Key personnel: Peter H. Rossi and Richard A. Berk.

Enabling legislation: None.

Total cost: $3.4 million (1976). ($2.6 million federal; the rest is an estimate of the states' administrative expenses.)

Dates: January-December 1976; data collected December 1975-June 1977; final reports for each state were submitted in 1978.

Dissemination effort:

> Briefings: None.

> Testimony: None.

> Distribution of executive summaries: To Labor Department.

Policy deliberations pertaining to treatment: None, according to Lenihan.

Location of treatment sites: Georgia and Texas, statewide, with limited exceptions.

Number of treatment groups: Six (with two control groups).

Treatments tested: Treatments varied in whether or not there were guaranteed incomes to subjects for a period of time following release. Payment treatments varied by the number of weeks of payments guaranteed and by the tax rate on earnings. Georgia and Texas unemployment insurance rules applied, accounting for differences in the size of payments and the maximum amount of untaxed, earned income permitted (weekly forgiveness amount). The Georgia and Texas unemployment rules are summarized as a 100% tax rate above the forgiveness amount; this is an oversimplification.

Treatment	State	Max. Wkly. Payment	Max. No. Weeks	Max. Allowance	Forgive-ness	Tax Rate
1	Georgia	$70	26	$1,820	$ 8	100%
	Texas	63	26	1,638	15.75	100
2	Georgia	70	13	910	8	100
	Texas	63	13	819	15.75	100
3	Georgia	70	13	910	---	25
	Texas	63	13	819	---	25

4. Job placement services in both states, with grants up to $100 for tools, work clothes, etc. No eligibility for payment.

5. Interviewed controls in both states. $15 payment for each of the four interviews in Georgia; $10 average payment for each of the four interviews in Texas. No other payments.

6. Noninterviewed controls in both states, followed through arrest and FICA earnings records; no payments, did not know they were part of an experiment.

Sample size: Group 1: GA: 176; TX: 175; 2: GA: 199; TX: 200; 3: GA: 201; TX: 200; 4: GA: 200; TX: 200; 5: GA: 200; TX: 200; 6: GA: 1,031; TX: 1,000.

Target population: All prisoners released from state prisons with the following exceptions: (1) those returning to a small number of very remote rural counties; (2) those returning to some other state or country; (3) those for whom arrest warrants or detainers for other offenses were pending prior to release.

Outcomes of interest: (1) Recidivism. (2) Employment and earnings.

Research components:

Process analysis: Wide-ranging interviews with members of the first five groups were conducted, in some cases with their family members. However, the researchers do not have any data about the content of the subjects' interactions with the employees of the employment security agencies. This is important for group 4, where virtually nothing is known except expenditures for tools and work clothes; it is also important for group 3, the low-tax group, because interviews showed no particular understanding of the tax system by that group or by the others. Tests were also performed on the possibility of underreporting of employment by the payment groups against unemployment insurance records; no such pattern was found.

Impact analysis: Conducted both as difference in means and with regression and other statistical techniques.

Benefit-cost analysis: Not conducted.

Major findings:

(1) No statistically significant differences in recidivism were found.

(2) Employment and earnings, Georgia

Variable	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Mean wks. worked, first yr. after release	12.3	17.4	17.7	19.6	24.3	N/A
Mean weeks worked, first 14 wks.	2.4	2.8	3.1	4.9	5.9	N/A
Mean weeks worked, wks. 15-24	2.5	4.0	4.5	5.2	6.5	N/A
Percentage with some earnings	53.7	60.8	62.4	62.5	65.2	61.2
Mean earnings	$1,064	$1,525	$1,433	$1,088	$1,553	$1,531

In general, experimental groups have statistically significant differences from control group 5 in the first two rows and the fourth.

(3) Employment and earnings, Texas

Variable	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Mean wks. worked, first yr. after release	20.8	27.1	24.6	29.3	28.3	N/A
Mean wks. worked, first 14 wks.	4.0	4.9	5.2	7.0	7.1	N/A
Mean wks. worked, wks. 15-24	4.5	7.3	6.8	7.4	7.6	N/A
Percentage with some earnings	67.4	78.6	69.7	73.1	66.1	66.2
Earnings	$1,922	$2,215	$2,242	$2,069	$1,960	$2,043

In general, experimental-group differences from group 5 are statistically significant for all rows except the last one.

"The TARP payments, as administered, did not decrease arrests for property-related offenses in either state.... TARP payment eligibility exerted a clear and strong work-disincentive effect...." Emphasis in the original.

(4) The authors set up and tested an elaborate multiple-equation system with the experimental data. Their conclusion is that the effect of the payments themselves was to reduce recidivism, but that the negative effects of the high tax rate led to low employment, which in turn led to higher recidivism.

Time trends in findings: Data are for one year.

Problems and issues:

  1. In Georgia, a "speeded-up" commutation procedure went into effect shortly before the experiment began. The effect of this change was to increase the number of prisoners in the Georgia sample who would be expected to exhibit an experimental effect if one existed; it tended to accelerate the release of older offenders with more than one prior theft conviction.

Replicability: As in many other experiments, "job placement services" is not replicable. The other portions of the experiment appear to be.

Generalizability: This experiment was planned to generalize the results of the **LIFE** experiment. It did not confirm those results.

COURT EMPLOYMENT PROJECT

Information source: Sally Hillsman Baker and Susan Sadd, "Diversion of Felony Arrests, An Experiment in Pretrial Intervention: An Evaluation of the Court Employment Project," summary report, Department of Justice, National Institute of Justice, June 1981.

Funding source: National Institute of Justice. Key personnel: Joel Garner.

Treatment administrator: Court Employment Project, Inc. Key personnel: Ennis J. Olgiati (deceased), Bruce Eichner, and Rae Linefsky.

Evaluator: Vera Institute of Justice. Key personnel: Sally Hillsman Baker and Susan Sadd.

Enabling legislation: None.

Total cost: $750,000 (1977).

Dates: January 1977-March 1978; data collected through 1979; final report June 1981.

Dissemination effort:

Briefings: Justice Department and officials of New York state and New York City.

Testimony: Numerous presentations to academic, professional, and criminal justice organizations.

Distribution of executive summaries: Persons active in this field.

Other: Substantial coverage in Corrections Journal, a major professional publication.

Policy deliberations pertaining to treatment: Hillsman believes there have been three impacts. (1) It was the first successful implementation of random assignment in court settings for a long time. It supported the belief of the NIJ staff (notably Garner) that random assignment was a feasible and necessary research tool, and led to further experiments. (2) It fostered skepticism, especially among policymakers in New York, that the various "alternatives" to detention or custody were in fact alternatives, and subjected alternative projects to higher standards in proving their effectiveness. (3) It has caused pretrial diversion, as a strategy, to be largely abandoned in New York City. It has not had the same effect in the rest of the country, although she feels the experiment has exposed pretrial diversion as conceptually flawed.

Location of treatment sites: Brooklyn and Manhattan.

Number of treatment groups: Two (with one control group).

Treatment tested: Prosecutors would refer persons charged with felonies to the project, and an "approximately" random assignment, described below, would occur. Experimentals were offered a delay and eventually a dismissal of their cases if they agreed to attend counseling sessions over a four-month period and if they complied with the agreement. The counseling included job referral and placement services, needs assessment, and referral to services in their neighborhoods

(for GED tutoring, substance abuse, etc.). This option was not open to controls. The approximately random process was adopted to make the experiment politically acceptable. The experimental period was randomly divided into periods of time of varying length, and project staff did not know when the current subperiod would expire. Quotas were preset, proportional to the length of the subperiods: new participants who entered before the quota was reached were experimentals; the overflow were controls.

Sample size: Experimentals, 410; controls, 256.

Target population: Persons charged with felonies selected by prosecutors. Three-quarters of the felonies were property crimes.

Outcomes of interest: (1) Disposition of case. (2) Employment. (3) Education or training activity.

Research components:

Process analysis: Conducted through interviews with prosecutors, implementation staff, and subjects. The key finding in the process analysis was that large numbers of controls either would not have been prosecuted, or, if prosecuted, would not have been convicted, or, if both prosecuted and convicted, would have faced minor penalties. Prosecutors used pretrial diversion to skim off a group who were not in serious trouble but "needed supervision." Defense attorneys cooperated with the tactic partly to economize on their own scarce resources. A second finding was that the CEP staff felt they had not been successful in job development.

Impact analysis: Conducted as a difference in means. OLS findings were not qualitatively different and were not reported.

Benefit-cost analysis: Not conducted.

Major findings: Charges were dropped against 72% of experimentals, as against 46% of controls. However, only 6.6% of controls were convicted of criminal charges, while 1.9% of experimentals were. Thus, the experimental treatment did not significantly conserve court resources. Effects on employment were negligible. At a six-month follow-up, the average experimental had been employed for 1.29 months, the average control for 1.41 months. Effects on education and training were negligible.

Time trends in findings: A 12-month follow-up also found no effects of treatment.

Problems and issues: The assignment process was accepted as effectively random by a national panel of referees.

Replicability: Replicable.

Generalizability: The report emphasizes that the **Court Employment Project**, which was set up by Vera, was one of the earliest pretrial diversion programs; it conformed its policies to standards set by a national professional organization. Thus, there is no obvious reason that the finding that pretrial diversion is ineffective and conceptually flawed should not have general validity.

EX-OFFENDER RESEARCH PROJECT

No written information source is known to exist.

According to Lafayette Grisby, this experiment was intended to determine the effects of guaranteed jobs on the behavior of ex-offenders on their release from prison. It was located in Baltimore, the contractor was Blackstone Associates, and $2.5 million (1981) was initially allocated to the project.

The project was terminated within a few months of initiation because of a funding cutback, long before any results could have been observed.

DIGEST OF THE SOCIAL EXPERIMENTS:

VI. Substance Abusers

WILDCAT EXPERIMENT

Information source: Lucy N. Friedman, "The Wildcat Experiment: An Early Test of Supported Work in Drug Abuse Rehabilitation," National Institute on Drug Abuse, 1978.

Funding sources: New York City Addiction Services Agency, Department of Employment; Dept. of Health and Human Services, National Institute on Drug Abuse; Dept. of Justice, Law Enforcement Assistance Administration; and the Labor, Employment, and Training Administration of the U. S. Dept. of Labor. Key personnel: Friedman does not recall.

Treatment administrator: Vera Institute of Justice, which set up the Wildcat Service Corporation. Key personnel: Herbert Sturz and Kenneth Marion.

Evaluator: Vera Institute of Justice. Key personnel: Lucy N. Friedman.

Enabling legislation: Waiver of regulations to permit diversion to wages of welfare funds otherwise payable to participants.

Total cost: $36.2 million (1975)--does not include research cost. Also includes the payments to treatment participants who were not in the initial sample; research only: N/A

Dates: July 1972-June 1976; data collected July 1972-June 1978; final report 1978.

Dissemination effort:

Briefings: Conference organized by the Ford Foundation, featuring Robert McNamara, with representatives of the U.S. Department of Labor, Dept. of Health and Human Services, and officials of New York City and New York state. Friedman also addressed other conferences.

Testimony: None.

Distribution of executive summaries: Persons active in the drug field.

Policy deliberations pertaining to treatment: The findings from this experiment, according to both the Wildcat report and the National Supported Work Demonstration report, led the Ford Foundation and the Department of Labor, with support from other sources, to initiate the creation of the Manpower Demonstration Research Corporation to oversee a national experimental evaluation of the supported work concept.

Location of treatment site: New York City.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals were randomly selected for work from volunteers. They were placed in small work crews with persons of similar background, and confronted with graduated demands for productivity, graduated rewards for performance, sympathetic but firm supervision, and consistent daily communication of management expectations. One crew member was the designated crew chief, and there were additional supportive services. Typical work: office/clerical, messenger, and building maintenance; usually the work was performed for the

city government.

Sample size: Experimentals, 194; controls, 207.

Target population: Substance abusers, at least 18 years old, enrolled in drug abuse treatment for at least three months, currently unemployed, receiving public assistance, unemployed at least 12 of the past 24 months, and not intoxicated at interview.

Outcomes of interest: (1) Employment. (2) Earnings. (3) Rearrest. (4) Drug and alcohol use.

Research components:

Process analysis: Conducted with open-ended interviewing. Self-reported earnings, arrests, and drug use tested against data available from tax, police, and drug treatment clinic sources.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Conducted from a taxpayer perspective.

Major findings:

(1)	Experimentals	Controls
Weeks worked in 3 years	101	46
Earnings	$12,236	$4,968
Weekly earnings (if working) in unsubsidized jobs	$133	$108
Never received welfare payments over 3 years	46%	6%
Probability of arrest, first year	19%	31%

All of these differences are statistically significant.

(2) No significant impact was found on drug or alcohol use.

(3) Experimentals were more likely to marry and to stay married.

(4) Average taxpayer expenditures on experimentals: $13,127; average taxpayer benefits: $15,405; benefit-cost ratio: 1.12.

Time trends in findings: All experimental effects diminish over time. For example, at the end of the first year, 74% of experimentals were working, compared with 30% of controls; at the end of three years, 49% of experimentals were working, compared with 36% of controls. Rearrest differences also vanish.

<u>Problems and issues</u>:

1. The benefit-cost evaluation is most sensitive to the valuation of the services provided, although conservative methods seem to have been used.

2. Displacement of outside contractors and of other workers is possible. Wildcat made a policy of not bidding for work performed by members of public service unions; on the other hand, it frequently offered services to city agencies at zero cost.

<u>Replicability</u>: Crew chiefs required special training. Project supervisors apparently had to possess entrepreneurial skills of a high order.

<u>Generalizability</u>: The **National Supported Work Demonstration** was performed in order to see whether the findings could be generalized.

The Wildcat and **NSWD** participants were mostly addicted to heroin and were receiving methadone maintenance. Friedman feels that the findings of Wildcat and **NSWD** could probably be applied to users of other drugs, like crack cocaine.

JOB SEEKERS' WORKSHOP

Information sources: Sharon Martinelli Hall, Peter Loeb, Joseph Norton, and Ray Yang, "Improving Vocational Placement in Drug Treatment Clients: a Pilot Study," Addictive Behaviors, 1977, 227-234 (article 1). Sharon Martinelli Hall, Peter Loeb, Kristin Coyne, and James Cooper, "Increasing Employment in Ex-Heroin Addicts I: Criminal Justice Sample," Behavior Therapy, 1981, 443-452 (article 2). Sharon Martinelli Hall, Peter Loeb, Michel Le Vois, and James Cooper, "Increasing Employment in Ex-Heroin Addicts II: Methadone Maintenance Sample," Behavior Therapy, 1981, 453-460 (article 3).

Funding source: U.S. Department of Health and Human Services, National Institute on Drug Abuse, U.S. Public Health Service. Key personnel: None.

Treatment administrator: University of California, Behavioral Treatment Research. Key personnel: Sharon Martinelli Hall.

Evaluator: University of California, Behavioral Treatment Research. Key personnel: Sharon Martinelli Hall.

Enabling legislation: None.

Total cost: Roughly $120,000 (1979) for the latter two studies.

Dates: First study, 1976; second and third studies, 1978-1979; data collected three-month follow-ups only; final report 1981.

Dissemination effort: The authors initially relied on the publication of the articles listed above for dissemination. They subsequently obtained a grant from the NIDA to test the effectiveness of alternative dissemination modes through an experiment involving random assignment of 174 drug treatment centers; the alternatives were on-site workshops, expenses-paid conferences, mailing literature only, and a no-treatment control. See J.L. Sorensen, Sharon Martinelli Hall, Peter Loeb, T. Allen, E.M. Glaser, and P.D. Greenberg, "Dissemination of Job Seekers' Workshop to Drug-Treatment Programs," Behavior Therapy, 1988, 143-155. The control and mailed-literature-only clinics never adopted the **Job Seekers** treatment for their patients, although some success in adoption was obtained where center staff received training in person. "... (N)o programs in the study, when initially contacted, were aware of the specific Job Seekers' Workshop, despite the fact that it was the subject of previous research and publication."

Policy deliberations pertaining to treatment: Hall states that the experimental treatment tested has not been widely adopted, sometimes because treatment professionals are not aware of it and sometimes because it requires greater skill levels than many treatment centers have.

Location of treatment site: San Francisco, California.

Number of treatment groups: Two (with one control group).

<u>Treatments tested</u>: There were three experiments.

A (article 2). Random assignment was performed by use of the date of the workshop the subject chose, relying on the fact that experimental and control workshops alternated randomly according to a schedule not known at intake. Controls attended a three-hour meeting during which they learned about employment resources that were available (e.g., union halls); they discussed their job interests with group leaders, and leaders made appropriate suggestions. Experimentals attended a similar meeting. They then attended a workshop lasting eight hours over three days, corresponding to the initial phases of the Job Club model but with much less supervised job search. Much more attention was placed on appropriate interview behavior than on obtaining interviews (probably because of the favorable local job market). Interviews in which participants role-played were set up two days after the workshop for both groups.

B (article 3). Subjects were stratified according to sex, parole/probation status (yes or no), and past job history. Members of each stratum were randomly assigned to experimental or control treatments. These treatments were the same as in A, although the experimental treatment took 12 hours over four days.

C (article 1). The pilot study had simple random assignment, with an experimental treatment that was a little longer than that in A or B, mostly because of two days at the end with no preset structure, where subjects could identify areas in which they wanted more work.

In all three experiments, self-reported employment was verified with other sources.

<u>Sample size</u>: Experiment A: 35 experimentals, 20 controls; experiment B: 30 experimentals, 30 controls; experiment C: 23 experimentals, 26 controls.

<u>Target population</u>: For experiment A, the target population were parolees or those on probation with documented histories of heroin abuse. Those who were psychotic, illiterate, or anticipated serving jail time in the next three months were screened out. For experiments B and C, the target populations were patients at methadone maintenance clinics, with the same exclusion criteria as in A. For experiment C, the target population was the same as for B.

<u>Outcomes of interest</u>: (1) Employment. (2) Performance at simulated interview.

<u>Research components</u>:

Process analysis: Not conducted.

Impact analysis: Conducted as a difference in means.

Benefit-cost analysis: Not conducted.

<u>Major findings</u>:

Experiment A. 86% of experimentals found full- or part-time employment over the three-month follow-up, compared with 54% of controls. This difference is significant at the 1% level.

Experiment B. At the end of three months, 52% of experimentals had found jobs, compared

with 30% of controls. The difference is not quite significant. "Regardless of [experimental or control] condition, subjects who reported no job history in the 5 years prior to the study start failed to find employment."

Experiment C. At three-month follow-up, 50% (nine of 18) of experimentals were employed, compared with 14% (three of 23) of controls, a statistically significant difference. This difference may be partly biased by the dropping of three experimentals who failed to attend workshops from the sample.

Time trends in findings: "Life Tables" are presented in articles 2 and 3 showing the differences in job-finding success over 12 weeks for each group. In A, the difference continues to increase up to 10 weeks; in B, the difference is stable after three weeks. There is no follow-up beyond three months.

Problems and issues: Differences in job-finding success are reported, but not relative wages or tenure on the job.

Replicability: Replicable.

Generalizability: The samples are small, and the San Francisco job market is very favorable to job seekers.

TRANSITION PROJECT

Information source: Ellen Rossman, Don Des Jarlais, Sherry Derren, and Holly Robinson, "An Evaluation of a Corporate-Based Job Preparation Training Program, the Transition Project," National Association on Drug Abuse Problems (NADAP), 1982. The paper is unpublished. Copies are available from the NADAP.

Funding source: U.S. Department of Labor, Employment and Training Administration. Key personnel: Robinson does not recall.

Treatment administrator: NADAP. Key personnel: Holly Robinson.

Evaluator: NADAP. Key personnel: Don Des Jarlais.

Enabling legislation: None.

Total cost: Research only: $187,000 (1980).

Dates: 1980; data collected through 1981; final report 1982.

Dissemination effort:

Briefings: New York state officials.

Testimony: Presentations to professional organizations.

Distribution of executive summaries: Department of Labor.

Policy deliberations pertaining to treatment: Robinson states that there was probably no impact on policy. The evaluation found that the program tested was ineffective, at least in the absence of comprehensive follow-up services. New York state officials, who had continued funding after the Reagan administration terminated the DOL research grant, had expected better results. Robinson feels that agencies like hers should disseminate findings like these much more widely than they do in order to educate policymakers that there will be no cheap, quick solution for people affected simultaneously by addiction, poverty, and other problems.

Location of treatment site: New York City.

Number of treatment groups: Two (with one control group).

Treatment tested: Experimentals were able to attend two meetings a week over seven weeks, one at their own drug-treatment site, one at a corporation to which their group had been assigned. Company personnel would explain what types of jobs were available in that firm, what the entry-level job requirements were, what they looked for in job applications and interviews, how job performance was evaluated, and opportunities for advancement and benefits; experimentals practiced interviewing with people who made hiring decisions and observed employees at work. Lessons were reinforced at counseling sessions at the treatment site. Five corporations (a bank, a pharmaceutical, and three insurance companies) participated. There was no expectation that the corporation would hire the experimentals; the purpose was orientation to the rewards and

demands of corporate life. Controls were not eligible for these services, although they were told about NADAP job-placement services. All subjects were paid for their time in research interviews.

Sample size: 146 experimentals, 78 controls.

Target population: Substance abusers who (1) had been involved in drug treatment for at least six months, (2) were in good standing within treatment program guidelines (e.g., no evidence of recent substance abuse), (3) had no major time conflicts with participation (child care, criminal justice, medical), (4) could read at least on a sixth-grade level, (5) no more than six months work experience in past twelve months, (6) were considered nearing job readiness by counselors, but still having substantial barriers to employability, and (7) volunteered to participate.

Outcomes of interest: (1) Employment. (2) Wages. (3) Education or training activity. (4) Drug or alcohol use. (5) Criminal behavior.

Research components:

Process analysis: Evaluations of the value of the program by participants, corporate staff, and treatment center staff. Attendance at sessions was on average about 50% of planned attendance.

Impact analysis: Used OLS, but reported only unadjusted means to facilitate exposition.

Benefit-cost analysis: Not conducted.

Major findings:

1. Treatment effects on employment and wages were not significant.

2. Comparing only employed experimentals and employed controls, experimentals earned higher wages at their longest full-time job ($212 vs. $193) and were more likely to work in a company with over 100 employees (36% vs. 21%); but these results are not statistically significant, possibly because there were only 52 experimentals and only 25 controls reached during the follow-up who had held full-time jobs.

3. Differences in enrollment in academic or vocational training were insignificant, but experimentals were more likely to choose academic training, while controls were more likely to choose vocational training.

4. Subgroup analysis did not find statistically significant differences. The evaluators came to believe that the experimental/control differences were greatest in subgroups where controls had the poorest outcomes, i.e., the most disadvantaged.

5. Controls were more likely than were experimentals to report that their principal activity in the previous year was illegal (10% vs. 3%, a significant difference). The most common illegal activity was drug dealing.

6. There was no significant difference in alcohol or drug use. Most participants were in

methadone maintenance, which only inhibits heroin use, but the majority in both groups had used cocaine since treatment. There was no significant difference in self-reported criminal activity.

Time trends in findings: The patterns in six-month and 12-month follow-ups are similar.

Problems and issues:

1. Small sample size does not permit finding statistical significance for subtle effects, if they are present.

2. Treatment counselors were usually not well trained in group counseling skills, which limited the usefulness of the clinic sessions.

Replicability: Replicable.

Generalizability: See "Problems and issues."

DIGEST OF THE SOCIAL EXPERIMENTS:

VII. Multiple Target Groups

RAND HEALTH INSURANCE STUDY

Information sources: Willard G. Manning, Joseph P. Newhouse, Naihua Duan, and three others, "Health Insurance and the Demand for Medical Care: Evidence from a Randomized Experiment," American Economic Review, 77:3, June 1987, 251-277. Robert H. Brook, John E. Ware, Jr., William H. Rogers, Emmett B. Keeler, and six others, "Does Free Care Improve Adults' Health? Results from a Randomized Controlled Trial," New England Journal of Medicine, 309:23, December 8, 1983, 1426-1434. John E. Ware, Jr., Robert H. Brook, William H. Rogers, Emmett B. Keeler, and five others, "Comparison of Health Outcomes at a Health Maintenance Organization with those of Fee-for-Service Care," Lancet, May 3, 1986, 1017-1022. R. Burciaga Valdez, Robert H. Brook, William H. Rogers, John E. Ware, Jr., and six others, "Consequences of Cost-Sharing for Children's Health," Pediatrics, 75:5, May 1985, 952-961.

Funding source: U.S. Department of Health and Human Services, Assistant Secretary for Planning and Evaluation. Key personnel: Larry L. Orr and James Schuttinga.

Treatment administrator: Rand Corporation. Key personnel: Rae Archibald.

Evaluator: Rand Corporation. Key personnel: Joseph P. Newhouse.

Enabling legislation: None.

Total cost: $136 million in 1984 dollars.

Dates: November 1974-January 1982; data collected: same; the final report had not been finished as of spring 1990.

Dissemination effort:

Briefings: "Hundreds of them," according to Newhouse, to professional organizations, trade groups, public-policy groups, and policymakers.

Testimony: House Subcommittee on Public Health and the Environment; Senate Committee on Aging; Armed Services Committee, Subcommittee on Health.

Distribution of executive summaries: Final report not complete; the AER article is a good summary.

Policy deliberations pertaining to treatment: The AER article notes that some private firms quoted the findings when instituting changes in their employee insurance policies.

Location of treatment sites: Dayton, Ohio; Seattle, Washington; Fitchburg, Massachusetts; Franklin County, Massachusetts; Charleston, South Carolina; and Georgetown County, South Carolina.

Number of treatment groups: 14. (For most analyses, however, these 14 groups are grouped into five.)

Treatment tested: Insurance benefits varied over two dimensions, the Maximum Dollar Expenditure

(MDE) and the coinsurance rate (CR). The MDE was the upper limit on the annual out-of-pocket medical expenses for which the family was responsible. It was set at 5%, 10%, or 15% of income, up to a maximum of $1,000 (1973 money; this limit was held constant in real terms). The coinsurance rate was the percentage of expenditures below the MDE for which the family was responsible. It was set at 0% (free care), 25%, 50%, and 95%. An "individual deductible" (ID) plan had a coinsurance rate of 95% for outpatient care only, but the MDE was limited to $150 per person (fixed in nominal terms) or $450 per family. Inpatient care was free in the ID plan.

To obtain uniformity in the experiment, all participant families were induced to sign over the benefits from their existing insurance plan to the experiment. The inducement was the financial maintenance (where necessary) of the plan by the administrator and a guaranteed payment equal to the difference between the MDE assigned to them and the maximum deductible of their previous coverage. They therefore could not be financially worse off by participating and would in most cases have somewhat higher incomes.

Participant families were randomly assigned to three-year or five-year treatments.

The experiment also randomly assigned Seattle participants between a Health Maintenance Organization (HMO) and a Fee-for-Service (FFS) plan.

Sample size: Numbers are individuals. Free Care: 1,893; 25% coinsurance: 1,137; 50% coinsurance: 383; 95% coinsurance: 1,120; ID plan: 1,276; total, 5,809.

Target population: Representative samples of the populations of the site areas with the following exclusions: (1) persons over 61 years; (2) persons with incomes in excess of $25,000 (1973 money); (3) those in jails or institutionalized; (4) those eligible for the Medicare disability program; (5) military personnel and their dependents; and (6) veterans with service-connected disabilities.

Outcomes of interest: (1) Total expenditures on health care. (2) Relative demand for services by poor and nonpoor. (3) Quantifiable health differences. (4) Effect of HMO organizational structure on expenditures and care.

Research components:

Process analysis: Careful analysis of characteristics of persons who refused to participate in the experiment. Although the rate of refusal rose with the coinsurance rate, the authors report that the refusers do not appear statistically different from the participants. Attrition was very small. There was no analysis of the content of medical care received.

Impact analysis: Conducted with sample means and sophisticated analyses using regression and other methods.

Benefit-cost analysis: Not conducted but see comment on generalizability.

Major findings:

(1) Predicted Annual Per Capita Use of Medical Services, by Plan (from a four-equation system designed to reduce the effects of individual catastrophic cases on the estimates; sample means generally show the same patterns; standard errors in parentheses):

Plan	Likelihood of Any Use	One or More Admissions to Hospital	Medical Expenses (1984 dollars)
Free care	86.7%	10.37%	$777
	(0.67)	(0.42)	(32.8)
25% CR	78.8	8.83	630
	(0.99)	(0.379)	(29.0)
50% CR	74.3	8.31	583
	(1.86)	(0.4)	(32.6)
95% CR	68.0	7.75	534
	(1.48)	(0.354)	(27.4)
ID	72.6	9.52	623
	(1.14)	(0.529)	(34.6)

"Our findings decisively reject the hypothesis that increased coverage of outpatient services, holding constant the coverage of inpatient services, will reduce expenditure."

(2) Predicted Annual Use of Medical Services, by Income Group:

Plan	Lowest Third	Middle Third	Highest Third	Corrected t
Likelihood of any use				
Free care	82.8%	87.4%	90.1%	5.90
25% CR	71.8	80.1	84.8	6.28
50% CR	64.7	76.2	82.3	4.86
95% CR	61.7	68.9	73.8	4.64
ID	65.3	73.9	79.1	7.09
Likelihood of one or more hospital admissions				
Free care	10.6%	10.1%	10.4%	-0.35
25% CR	10.0	8.4	8.0	-2.75
50% CR	9.1	8.1	7.8	-1.66
95% CR	8.8	7.4	7.1	-2.46
ID	9.3	9.4	9.9	0.68
Medical expenditures, 1984 dollars				
Free care	$788	$736	$809	0.53
25% CR	680	588	623	-1.47
50% CR	610	550	590	-0.49
95% CR	581	494	527	-1.41
ID	609	594	670	1.38

"Corrected t" is the t-test on the hypothesis that the population value for the upper third of households is the same as the population value for the lower third of households, corrected for

intertemporal and intrafamily correlation. An absolute value of 1.96 or higher indicates that the probability of this hypothesis being true is 5% or less.

(3) For the sample as a whole, the only statistically significant health gains from the free-care plan over the cost-sharing plans were for high blood pressure and the correction of nearsightedness. For the 25% of the sample judged to be in the poorest health, there was a 10% greater risk of dying, other things equal, in the cost-sharing plans than in the free-care plan.

Authors note that gains in health were mostly due to improved control of blood pressure, and a targeted program of free hypertension screening could accomplish the same result.

(4) "For the typical child participant, we could not discern significant differences in health status between those who received free care and those insured by the cost-sharing plans.... Taking all the measures together, the direction of estimated effects favored neither the free plan nor the cost-sharing plans."

(5) "1673 individuals aged 14 to 61 were randomly assigned to one HMO or FFS plan in Seattle. For non-poor individuals assigned to the HMO who were initially in good health, there were no adverse effects. Health outcomes in the two systems of care differed for high and low income individuals who began the experiment with health problems. For the high income initially sick group, the HMO produced significant improvements in cholesterol levels and in general health ratings by comparison with free FFS care. The low income initially sick group assigned to the HMO reported significantly more bed-days per year due to poor health and more serious symptoms than those assigned free FFS care, and a greater risk of dying by comparison with pay FFS plans."

(6) Annual Use of Medical Services Per Capita, by HMO and FFS Status (HMO critics have long claimed that the apparent cost savings from the HMO organizational mode were partly due to self-selection; persons less likely to demand care are more likely to choose HMOs. To test this, HMO experimentals were randomly assigned to the HMO; HMO controls were assigned to it randomly but had already been enrolled in it anyway; the randomly-assigned FFS group included people who before the experiment had been HMO members):

Plan	Likelihood of Any Use	One or More Admissions to Hospital	Imputed Expenditures (1984 dollars)
HMO experimental	87.0%	7.1%	$434
HMO control	91.1	6.4	432
Free FFS	85.3	11.2	640

"Our results ... show no evidence of selection in the single HMO that we studied; those previously enrolled at the HMO (the Controls) used services at approximately the same rate as those who were not previously enrolled (the Experimentals)."

Time trends in findings: No differences between three-year and five-year groups are reported.

Problems and issues:

1. "(T)here are no easy, quantitative measures of health in large populations." (Relman, in editorial accompanying Brook et al.)

2. Poor families in this study reached their MDE quickly, since it was a function of income, and thereafter care was free. This will distort any projections from these findings to the effects of cost sharing on the poor when the deductible is set at a higher level and/or is not a function of income. These reservations do not apply to comparisons between the free-care plan and the ID plan, where the limit was not a function of income; however, the ceiling in the ID plan was still fairly low.

3. "The fact that there was greater variation in the amount of care used by children between sites than between payment groups suggests that very different types of care were provided in different places. Unless one knows what care was delivered, it is difficult to come to conclusions about its relation to outcomes." (Haggerty, in an editorial accompanying Valdez et al.)

4. Although the refusal group was not statistically different from the participant group in observed characteristics, the rate of refusal rose with the coinsurance rate (25% of those offered the 95% plan refused), leaving open the question of differences in unobserved characteristics.

5. Published reports do not generally report the effects of differing levels of MDE on the variables of interest. Given the experimental design, this is a strange omission.

6. There could be a problem of underreporting of small claims, especially at the 95% coinsurance rate and in the high MDE plans. At 95%, filing a claim on a $50 office visit would have a reward of $2.50; healthy people might not bother.

7. The HMO study is clearly dependent on the characteristics of the HMO used, and perhaps on the characteristics of competing FFS physicians in Seattle as well.

Replicability: Would appear to be broadly replicable.

Generalizability:

1. The key finding of the experiment is that the price elasticity of health care is substantial, even when price changes are compensated (in this case, overcompensated) by income supplements. Under standard economic theory, the provision of subsidized medical care will therefore result in an important loss in social welfare owing to the use of resources in medical care that have less value to the consumer than the money it costs to provide them. This has a clear bearing on the design of any national health insurance plan. Using strong assumptions, like competitive medical care prices and no externalities, the authors of the AER article calculate the "deadweight" loss in wasted resources in moving from a national 95% plan with $1,000 MDE to a national free-care plan. Their estimate is between $37 and $60 billion; expenditures on these services in 1984 by the under-65 population were around $200 billion.

2. In an early article (Newhouse et al., New England Journal of Medicine, 305:25, Dec. 17, 1981, 1501-1507) the authors note some limits to generalizability. An increase in subsidy to

ambulatory care would result in increases in the quantity of services demanded sufficient to exceed the short-run capacity of the medical-care delivery system; this might well lead to a nonprice rationing of care, which would upset many of the conclusions in these articles. Large, long-run increases in capacity might not be allowed by the government for cost reasons, so this generalizability problem might be a long-run problem as well. On the other hand, a slackening in services demanded brought on by increased cost-sharing would in some theories cause physicians to "induce demand" by suggesting more costly therapies to their patients; this theory, however, is controversial.

NATIONAL SUPPORTED WORK DEMONSTRATION

Information sources: Board of Directors, Manpower Demonstration Research Corporation, Summary and Findings of the National Supported Work Demonstration, MDRC (1980); Peter Kemper, David A. Long, and Craig Thornton, The Supported Work Evaluation: Final Benefit-Cost Analysis, MDRC (1981).

Funding sources: U.S. Department of Labor (Education and Training Administration); U.S. Dept. of Justice (Law Enforcement Assistance Administration); U.S. Dept. of Health and Human Services (Office of Planning and Evaluation, National Institute on Drug Abuse); U.S. Dept. of Housing and Urban Development (Office of Policy Development and Research); U.S. Dept. of Commerce (Economic Development Administration); and the Ford Foundation. Key personnel: Labor: Howard Rosen and Fritz Kramer; Dept. of Health and Human Services: Mike Barth and Bill Barnes; Ford Foundation: Stan Breznoff; and NIDA: Deborah Hastings Black.

Treatment administrator: Manpower Demonstration Research Corp. Key personnel: Judith M. Gueron.

Evaluators: Primary: Mathematica Policy Research, Inc. Secondary: Institute for Research on Poverty, University of Wisconsin-Madison. Key personnel: Robinson G. Hollister, Jr., Peter Kemper, David A. Long, and Craig Thornton.

Enabling legislation: AFDC sample required waivers to Social Security Act.

Total cost: $82.4 million (but this includes $10.6 million in sales of goods and services produced), in 1977 dollars; research $11.1 million.

Dates: March 1975-December 1978; data collected April 1975-mid-1980; final report 1981.

Dissemination effort:

Briefings: Executive agencies.

Testimony: Congress.

Distribution of executive summaries: Gueron says it was "not distributed as widely as MDRC reports are now."

Policy deliberations pertaining to treatment: Gueron states that supported work is a component of AFDC reform programs in California and Massachusetts.

Location of treatment sites: Atlanta, Georgia; Chicago, Illinois; Hartford, Connecticut; Jersey City, New Jersey; Newark, New Jersey; New York City; Oakland, California; Philadelphia, Pennsylvania; San Francisco, California; and several sites in Wisconsin.

Treatments tested: (1) Controls received no treatment. (2) Experimentals offered employment in a structured work experience program involving peer group support, graduated increase in work standards, and close sympathetic supervision, for 12 to 18 months. Local agencies contracted

with MDRC to employ the experimentals in a broad range of activities, with pay starting at the minimum wage (or slightly higher, depending on local market conditions), and bonuses and merit increases for workers who met increasing work standards. Agencies maintained a high ratio of supervisors to participants (1:8 to 1:12), and implemented different on-site methods for crew interaction and shared responsibility. Typical work activities were construction, building maintenance, and child day care.

Number of treatment groups: Two.

Sample size: 3,214 experimentals, 3,402 controls.

Target populations: (1) Long-term recipients of AFDC (30 of last 36 months, no children under six); (2) Ex-addicts following drug rehabilitation treatment (within past six months); (3) Ex-offenders, 18 or over, incarcerated within past six months; (4) Young school dropouts, 17-20, not in school past six months, at least 50% having delinquent or criminal records.

Outcomes of interest: (1) Increases in posttreatment earnings. (2) Reductions in criminal activity. (3) Reductions in transfer payments. (4) Reductions in drug abuse.

Research components:

Process analysis: Conducted. Results do not, however, appear in the summary volumes but in earlier reports.

Impact analysis: Comparison of means; regressions performed in earlier work produced similar results.

Benefit-cost analysis: Conducted from taxpayer, subject, and social perspectives. Results very sensitive to assumptions about the social costs of criminal activity, somewhat sensitive to extrapolation of earnings effects.

Major findings:

(1) Major positive effect on earnings of AFDC-recipient group.

(2) Minor increase in earnings of ex-addicts, and major reduction in criminal activity.

(3) No discernible effects on young dropouts.

(4) No clear effects on ex-offenders.

(5) Benefits exceeded costs for AFDC recipient and ex-addict groups by $8,000 and $4,000, respectively. Costs exceeded benefits for young dropouts. For ex-offenders, the bulk of findings show costs substantially exceeding benefits (experimentals were arrested more frequently) but a small-sample, three-month follow-up shows the reverse tendency.

Time trends in findings: Earnings differences showed very little decay over time among AFDC recipients; criminal activity differences fell over time among ex-addicts.

Problems and issues:

1. Local organizations competed to win these contracts. Some projects were not funded, and one was discontinued for poor performance. Thus, self-selection of sites might bias the results.

2. Displacement effects could occur in two ways. First, the greatest impact seemed to be for AFDC recipients in periods of high unemployment. Second, the agencies competed for local-government contracts, set up small businesses, and so on, and might have displaced other businesses.

3. Both controls and experimentals underreported arrests. A research finding that underreporting seemed to be of the same magnitude between controls and experimentals is critical to results.

Replicability: Treatment seems to require development of entrepreneurial local project management.

Generalizability: The large number of sites and subjects adds power to the findings. However, site and contractor self-selection are certainly present.

AFDC HOMEMAKER-HOME HEALTH AIDE DEMONSTRATIONS

Information source: Stephen H. Bell, Nancy R. Burstein, and Larry L. Orr, "Overview of Evaluation Results," Abt Associates, December 1987.

Funding source: Health Care Financing Administration. Key personnel: Kathy Ellingson-Otto.

Treatment administrators: Social service agencies in seven states. Key personnel: Joann Barham (Arkansas); Darlene Goodrich (Kentucky); Sybil Stokes (New Jersey); Joe Capobianco (New York); Ruth Ann Sieber (Ohio); Mary Frances Payton (South Carolina); and David Chavez (Texas).

Evaluator: Abt Associates. Key personnel: Larry L. Orr, Stephen H. Bell, and Nancy R. Burstein.

Enabling legislation: Omnibus Budget Reconciliation Act of 1980.

Total cost: Research only: $8 million (1984).

Dates: January 1983-June 1986; data collected: same; final report 1987.

Dissemination effort:

> Briefings: To HCFA officials and presentations to professional organizations of economists and public-policy academics.

> Testimony: None.

> Distribution of executive summaries: On request.

Policy deliberations pertaining to treatment: Orr states that this is one of several studies that dampened enthusiasm for home care. Although the findings suggest positive effects from the component that trained and employed AFDC recipients, these findings have not been utilized at the federal level, perhaps because the funding agency for the experiment is not the agency with responsibility for AFDC.

Location of treatment sites: Arkansas, Kentucky, New Jersey, New York, Ohio, South Carolina, and Texas.

Number of treatment groups: This was a two-component experiment. There were two AFDC-recipient groups (one control) and two elderly/impaired groups (one control).

Treatment tested: Experimental elderly/impaired subjects (clients) could receive up to 100 hours per month of homemaker and home health aide services as needed for the duration of the demonstration. These services were free if their incomes were less than two times the AFDC standard of need in their state; persons with incomes above that level were charged on a sliding scale. Control elderly/impaired subjects could not receive these services. Experimental AFDC subjects (trainees) received a four- to eight-week course of training to become a homemaker-home health aide, followed by a year of subsidized employment. Wages averaged

$3.84 per hour, and hours averaged 75 per month. Controls did not receive this training, nor did they receive subsidized employment.

Sample size: Elderly/impaired: roughly 9,500 experimentals, 9,500 controls; AFDC: roughly 4,750 experimentals, 4,750 controls.

Target population: Elderly/impaired subjects had to be elderly or disabled, at risk of institutionalization, and home health aide services could not "reasonably or actually" be available to them. AFDC subjects had to be currently eligible for AFDC and to have received it for the past 90 days. They could not have been employed as a homemaker-home health aide during that period, and they had to have applied to enter the program.

Outcomes of interest:

For the clients: (1) Changes in other informal or paid in-home care. (2) Changes in survival. (3) Changes in hospital and nursing home utilization. (4) Changes in Medicare and Medicaid reimbursements. (5) Changes in health outcomes.

For the trainees: (1) Employment. (2) Earnings. (3) AFDC and food stamp payments and recipiency.

Research components:

Process analysis: The process analysis was generally limited to observing that the treatments were delivered as planned to the groups for whom they were designed. One salient finding, however, was that intake workers' ratings of AFDC subjects' potential had little value in selecting applicants who would most benefit from the demonstration.

Impact analysis: Conducted with OLS.

Benefit-cost analysis: Conducted from social, taxpayer, client, and trainee perspectives.

Major findings:

An asterisk denotes results that are statistically significant at the .05 percent level.

Clients

Experimental effect on hours per week of care ("total" is not the sum of formal and informal; the effects are estimated from three different regressions):

	Formal	Informal	Total
Arkansas	4.92*	-0.62	4.20*
Kentucky	1.20*	-0.47	0.72
New Jersey	3.47*	-0.55	3.26*
New York	4.21*	0.34	4.83*
Ohio	1.58*	-0.63	1.09*
South Carolina	8.29*	-1.42*	7.05*
Texas	4.02*	-0.19	4.26*

Survival: There was no significant impact on mortality.

Percentage of period spent in hospitals: The only statistically significant effect was in New York and in the wrong direction (clients spent an additional 4.5% of their time in hospitals).

Percentage of period spent in nursing homes: There was no significant impact on institutionalization.

Experimental effect on Medicare/Medicaid Reimbursement:

	Medicare	Medicaid
Arkansas	-$ 39	-$40*
Kentucky	+ 32	+ 1
New Jersey	- 240*	- 30
New York	- 30	data not collected
Ohio	- 66	+ 13
South Carolina	+ 25	- 59*
Texas	+ 26	0

Health and functioning: Clients were slightly less likely than controls to be completely dependent, communicated somewhat better, and their medical conditions were less likely to have gotten worse during the demonstration period.

Trainees

Effects are computed per participant, by dividing the effect per experimental by the fraction of assigned experimentals who actually entered training.

Experimental effect on average monthly earnings over a 30-month follow-up period:

Arkansas	$122*
Kentucky	148*
New Jersey	216*
New York	39
Ohio	210*
South Carolina	140*
Texas	141*

In the following tables, postdemonstration-year 1 is the 12 months following the time when the

typical trainee left subsidized employment. Year 2 is the next 12 months.

Experimental effect on percentage employed:

	Year 1	Year 2
Arkansas	3%	31%*
Kentucky	0	2
New Jersey	9*	12 *
New York	- 2	-13
Ohio	6*	13 *
South Carolina	3	3
Texas	- 3	28 *

Experimental effect on hours worked per month:

	Year 1	Year 2
Arkansas	3	24*
Kentucky	3	11
New Jersey	15*	22*
New York	- 9	-10
Ohio	14*	25*
South Carolina	4	- 2
Texas	1	48*

Experimental effect on earnings per month:

	Year 1	Year 2
Arkansas	$10	$101*
Kentucky	28*	161*
New Jersey	81*	126*
New York	- 36	12
Ohio	68*	105*
South Carolina	26*	22
Texas	8	215*

Experimental effect on percentage receiving AFDC or food stamps (during a typical follow-up period month):

	AFDC	Food Stamps
Arkansas	-13*	- 1
Kentucky	-28*	- 3*
New Jersey	-26*	-11*
New York	- 3*	5*
Ohio	-27*	-15*
South Carolina	-39*	- 8*
Texas	-11*	- 1

Experimental effect on dollars received monthly per participant:

	AFDC	Food Stamps
Arkansas	-$ 31*	-$ 16*
Kentucky	- 52*	- 17*
New Jersey	- 106*	12*
New York	- 2	6*
Ohio	- 84*	- 28*
South Carolina	- 68*	- 42*
Texas	- 18*	- 8*

Time trends in findings: Shown above for trainee earnings. Savings in AFDC and food stamps drop sharply over time.

Benefit-cost analysis: The unit of analysis chosen is dollars per hour of service. The analysis is dominated by the failure of the treatment to reduce the usage of hospitals and nursing homes. Trainees and clients are net gainers by the treatment, but taxpayers are worse off.

Net social benefit in dollars per hour of service:

Arkansas	-$ 9.67
Kentucky	- 4.47
New Jersey	+ 15.75
New York	- 40.00
Ohio	+ 13.47
South Carolina	- 0.39
Texas	- 3.68

Problems and issues: Orr states that there are several reasons for New York having been an extreme outlier. The first is turnover at the upper management level, with half a dozen persons holding the project director position at one time or another. The second is that contract negotiations with local providers took too long, and implementation was too slow; in some ways the demonstration never properly got off the ground. The third is that New York has a very heavy turnover in its AFDC population; controls would catch up to experimentals more quickly there than elsewhere for other interventions as well.

Replicability: Replicable.

Generalizability: This is a massive demonstration, but the sites are not a representative sample of the United States. States were selected for their strong interest in home care. In some ways, this makes the negative results on the client side more striking.

APPENDIX

Social Experiments in Progress During
the Summer of 1990

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
THE LOW-INCOME POPULATION				
The Minority Female Single Parent Demonstration 1984/1993	Gordon, Anne R.; Burghardt, John A.; and Zambrowski, Amy. 1990. The Minority Female Single Parent Demonstration: Report on Short- Term Economic Impacts. Princeton, N.J.: Mathematica Policy Research, Inc.	Education, job skills training, employability training, counseling, child care, other support services	Minority single mothers with children of any age; 72% on AFDC or other public assistance at baseline	Employment, earnings, job characteristics, welfare receipt and payments, family formation, home environment, educational attainment, child care use
Pennsylvania Saturation Work Program 1986/1991	Hogarth, Suzanne; Martin, Roger; and Nazar, Kathleen. 1989. Pennsylvania Saturation Work Program Impact Evaluation. Draft report. Harrisburg, Pa.: Department of Public Welfare.	Ongoing participation requirement, enhanced case management, day care	Applicants and recipients: AFDCs with children 6 or older; AFDC-UPs with children of any age	Employment, earnings, welfare receipt and payments

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Teenage Parent Demonstration 1987/1992	Field, T.; Widmayer, S.; Greenberg, R.; and Stoller, S. 1982. "Effects of Parent Training on Teenage Mothers and Their Infants." Pediatrics, 69: 703-707.	Education, job skills training, job search, life skills instruction, support services, child support enforcement	Teenage recipients of AFDC who have one child of any age or are pregnant with first child	Employment, earnings, welfare receipt, education attainment and achievement, family formation, establishment of paternity and child support, parenting skills
Riverside (Calif.) Case Management 1988/1992	Manpower Demonstration Research Corporation. 1988. Research Design for a Special Study of Case Management in the Riverside Country GAIN Program. Unpublished internal document.	Test of GAIN activities with case management provided with registrant-to-staff ratios of approximately (1) 50:1 or (2) 100:1	Applicants and recipients: AFDCs with children 6 or older; AFDC-UPs with children of any age	Employment, earnings, job characteristics, welfare receipt and payments, household income, educational attainment, attitudes
California GAIN (Greater Avenues for Independence) 1988/1992	Riccio, James; Goldman, Barbara S.; Hamilton, Gayle; Martinson, Karin; and Orenstein, Alan. 1989. GAIN: Early Implementation Experiences and Lessons. New York: MDRC.	Education and job search followed by assessment and further education, unpaid work experience, job skills training, or on-the-job training	Applicants and recipients: AFDCs with children 6 or older; AFDC-UPs with children of any age	Employment, earnings, job characteristics, welfare receipt and payments, household income, educational achievement and attainment, attitudes

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
New York Comprehensive Employment Opportunity Support Centers 1988/1992	Werner, Alan; and Nutt-Powell, Bonnie. 1988. Evaluation of the Comprehensive Employment Opportunity Support Centers: Volume 1: Synthesis of Findings. Cambridge, Mass.: Abt Associates.	Assessment followed by pre-employment training, job skills training, education, job search assistance, support services including on-site child care	AFDC recipients with children under 6	Employment, earnings, welfare receipt and payments
Food Stamp Employment and Training Program 1988/1990	Puma, Michael; Burstein, Nancy R.; Merrell, Katie; and Silverstein, Gary. 1990. Evaluation of the Food Stamp Employment and Training Program: Final Report. Bethesda, Md.: Abt Associates.	Job search, job club, education, job skills training, unpaid work experience	Food stamp recipients	Employment, earnings, job characteristics, household income, food stamp and welfare receipt and payments
North Carolina Guaranteed Child Care Demonstration 1988/1991	North Carolina Department of Human Resources. (No date.) Child Care Recycling Fund Demonstration Project: Pretest and Evaluation. Unpublished internal document.	Offer of a guarantee of transitional child care, compared to transitional child care on a space-available basis	AFDC recipients with children aged 1-5	Employment, welfare receipt and payments

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
New York Child Assistance Program (1988/1993)	Werner, Alan. 1990. <u>Implementing the New York State Child Assistance Program</u>. Cambridge, Mass.: Abt Associates.	Raises income ceiling for AFDC benefits and reduces benefit reduction rate. Food stamps cashed out. Reduces administrative burden on single parent families with valid child support orders.	AFDC caretakers in families with absent parent	Employment, earnings, child support orders, receipt of child support payments, welfare receipt and payments, total household income
New Chance 1989/1995	Quint, Janet C.; and Guy, Cynthia A. 1989. <u>New Chance: Lessons from the Pilot Phase</u>. New York: MDRC.	Education, employability training, job skills training, work internship, family planning and health education, health services, parenting edcuation, life skills training counseling, job placement, on-site child care in most sites	AFDC recipients 17-21 who gave birth as teens and are dropouts, with children of any age	Employment, earnings, educational attainment and achievement, welfare receipt, fertility, parenting skills, child outcomes
Ohio LEAP 1989/1995	Long, David A.; and Bloom, Dan. 1989. <u>Design of the Project Learn Evaluation</u>. Unpublished document prepared for the Ohio Department of Human Services. New York: MDRC. (LEAP was previously known as Project Learn.)	Required school attendance, financial incentives and penalties, case management, child care, transportation, guaranteed summer job	Custodial parents on AFDC, under 19, without high school diploma or GED, with children of any age	Employment, earnings, welfare receipt, educational attainment, health, family formation, attitudes

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Ohio Transitions to Independence 1989/1993	Bell, Stephen H.; Hamilton, William L.; and Burstein, Nancy R. 1989. <u>Ohio Transitions to Independence Evaluation: Design of the Cost-Benefit Analysis</u>. Cambridge, Mass.: Abt Associates.	Assessment, job club, unpaid work experience, subsidized employment, education, job skills training	AFDC recipients with children 6 or older and AFDC-UP recipients	Employment, earnings, job characteristics, welfare receipt, payments, duration, and recidivism, child support payments, family formation
Illinois Early Access Program 1989/Unknown	Interagency Low Income Opportunity Advisory Board, Executive Office of the President. (No date.) <u>Special Terms and Conditions</u>. Unpublished internal document.	For most recipients, job search followed by assessment, job club, pre-employment training, education, job skills training, work experience; for a few recipients, education followed by other services	AFDC recipients with children 3-5	Employment, welfare receipt and payments, Medicaid receipt
Illinois Career Advancement Demonstration 1989/Unknown	Interagency Low Income Opportunity Advisory Board, Executive Office of the President. (No date.) <u>Special Terms and Conditions</u>. Unpublished internal document.	Payment for training-related expenses (transportation, child care, fees other than tuition), materials paid to $300	Former welfare recipients earning less than 185% of standard of need	Employment, earnings, welfare receipt and payments, Medicaid receipt, food stamps

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Montgomery County (Ohio) Demonstration 1989/1993	None. (Evaluation being conducted by Abt Associates.)	Transitional Medicaid and child care (effective 4/1/90, controls also could receive these services); mandatory assessment; access to employment-related activities	AFDC recipients with children under 6	Employment, earnings, job characteristics, welfare receipt, duration, and recidivism, Medicaid and child support payment, family formation
Texas Transitional Child Care and Medicaid Study 1989/1993	Interagency Low Income Opportunity Advisory Board, Executive Office of the President. (No date.) Special Terms and Conditions. Unpublished internal document.	Offer of extended transitional child care and Medicaid benefits for those who become employed (effective 4/1/90, controls also could receive these services)	AFDC applicants and recipients (age of youngest child not specified)	Employment, earnings, child care use and cost, welfare receipt, payments, and recidivism
San Diego Food Stamp Cash-Out Demonstration 1989/1993	Ponza, Michael; Fraker, Thomas; and Ohls, James. June 1990. Final Evaluation Plan for the San Diego Food Stamp Cash-Out Demonstration. Princeton, N.J.: Mathematica Policy Research.	Substitution of cash (checks) for food coupons	Food stamp recipients	Food use, household expenditures, and nutrient availability; program participation; program administrative costs; fraud and theft.

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Alabama Food Stamp Cash-Out Demonstration 1989/1991	Anneciemnecki, Anne; Fraker, Thomas; Martini, Alberto; Ohls, James; Ponza, Michael; and Quinn, Elizabeth. June 1990. <u>Final Evaluation Plan for the Alabama Cash-Out Demonstration</u>. Washington, D.C.: Mathematica Policy Research.	Substitution of cash (checks) for food coupons	Food stamp recipients	Food use, household expenditures, and nutrient availability; program participation; administrative costs; fraud and theft
Florida Project Independence 1990/1994	Manpower Demonstration Research Corporation. 1989. Unpublished internal documents.	Job search for "job-ready" enrollees; assessment followed by education, training, or unpaid work experience for others and completers of job search without a job	AFDC applicants and recipients with children 3 or older	Employment, earnings, job characteristics, welfare receipt and payments, family composition, educational attainment
Child Care Plus Demonstration 1990/Unknown	New Jersey Department of Human Services. 1989. <u>Child Care Plus: A Demonstration of Enhanced Child Care Options for Low-Income Families</u>. Draft report. Trenton, N.J.: New Jersey Department of Human Services.	Test of 3 programs: (1) high-quality developing child care and parenting support; (2) child care (at rate paid by N.J. REACH) until child enters first grade; (3) N.J. REACH-supplied child care	AFDC recipients with a child under 3	Maternal employment, earnings, welfare receipt and payments, use of education and training, use of child care

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Memphis Nurse Home-Visitation Program 1990/1998	Olds, David L; Belton, Jann; Cole, Robert; Foye, Howard; Helberg, June; Henderson, Charles R., Jr.; James, David; Kitzman, Harriet; Phelps, Charles; Sweeney, Patrick; and Tatelbaum, Robert. (No date.) Nurse Home-Visitation for Mothers and Children: A Research Proposal. Unpublished document. Rochester, N.Y.: New Mothers Study, Department of Pediatrics, University of Rochester.	Test of 2 programs: nurse visits for Group 1 during pregnancy, for Group 2 until child is 2; referrals to services; encouragement to resume school or work	Disadvantaged first-time mothers; oversampling women under 18	Maternal education, employment, welfare receipt, Medicaid, food stamps, fertility, health habits, infant care, service use
Alabama Avenues to Self-Sufficiency Through Employment and Training (ASSET) Program 1990-1994	None. (Evaluation being conducted by Abt Associates.)	AFDC, food stamps, and Low Income Heating and Energy Assistance benefits combined into a single cash grant. AFDC employment and training requirements and child support payment requirements applied to former food stamp-only cases.	AFDC and food stamp-only recipients	Employment and earnings, welfare recipiency, child support orders and collections, household expenditures, administrative costs

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
THE UNEMPLOYED				
National JTPA Study 1987/1993	Doolittle, Fred; and Traeger, Linda. 1990. Implementing the National JTPA Study. New York: MDRC.	JTPA services, including job search, job skills training, on-the-job training	Economically disadvantaged adults and out-of-school youths	Employment, earnings, household income, welfare receipt, family composition, educational attainment, criminal behavior
Washington Re-employment Bonus Demonstration 1988/1991	Spiegelman, Robert; O'Leary, Christopher; and Kline, Kenneth. 1990. Washington Reemployment Bonus Demonstration Experiment: Preliminary Findings. Kalamazoo, Mich.: W.E. Upjohn Institute.	Cash bonus if U.I. claimant obtained job within specified period after filing for U.I. and also retained job for specified period	Unemployment Insurance claimants likely to exhaust U.I. benefits	Length of unemployment, cost of U.I., quality of new job, relative cost-effectiveness of alternative combinations of bonus levels and bonus qualification periods
Tacoma (Wash.) Unemployment Insurance Work Search Experiment 1986/1991	Johnson, Terry R.; and Klepinger, Daniel. 1990. Experimental Evidence on the Effectiveness of the Washington Alternative Work Search Demonstration. Seattle, Wash.: Battelle Human Affairs Research Centers.	Four alternative treatments, including a control. Treatments vary in the degree to which claimants are monitored in meeting the U.I. work search requirement and are provided assistance in searching for work.	Unemployed workers initially filing claims for Unemployment Insurance	Length of insured unemployment, quality of new job, costs associated with administrative work search requirement.

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
Pennsylvania Bonus Demonstration 1987/1991	Corson, Walter; Dunstan, Shari Miller; and Kerachsky, Stuart. 1989. <u>The Pennsylvania Reemployment Bonus Demonstration Project: Interim Report</u>. Princeton, N.J.: Mathematica Policy Research.	Reemployment bonuses paid to U.I. claimants upon their reemployment	U.I. claimants	Employment, earnings, U.I. receipts, net effects on costs
Washington State and Massachusetts Unemployment Insurance Self-Employment Demonstrations 1989/1993	Orr, Larry; Johnson, Terry R.; Montgomery, Mark; and Hojnacki, Marie. 1989. <u>Design of the Three-States Self-Employment Demonstration</u>. Washington, D.C.: Abt Associates.	Combining U.I. payments with support services and training to assist claimants in starting small businesses. Also, participants in Washington state will receive lump sum payments equal to their remaining U.I. entitlement.	Potential U.I. payment exhaustees who volunteer to participate	Earnings, from both regular- and self-employment U.I. claims, number of businesses started, length of time business remains in existence

DISADVANTAGED YOUTH

Name Starting Yr./Yr. Final Report Expected	Information Source	Treatment(s) Tested	Target Population(s)	Outcome(s) of Interest
JOBSTART 1985/1993	Auspos, Patricia; Cave, George; Doolittle, Fred; and Hoerz, Gregory. 1989. <u>Implementing JOBSTART: A Demonstration for School Dropouts in the JTPA System</u>. New York: MDRC.	Education, job skills training, job placement, support services	Dropouts 17-21, reading below eighth-grade level, JTPA eligible; 50% of female sample on AFDC at baseline	Employment, earnings, educational attainment, welfare receipt, family formation, criminal behavior

Sources: Information for twenty of the experiments listed in this table was obtained from Gueron, Judith M. and Pauly, Edward, <u>The Effects of Welfare-to-Work Programs: A Synthesis of Recent Experimental Research</u>, New York, Manpower Demonstration Research Corporation, August 1990, Tables 3.1-3.5. Information on the remaining seven experiments was obtained from reports published by the organizations conducting the evaluations and from telephone conversations with the staffs of these organizations.

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