U.S. Department of Health & Human Services

ASPE

OFFICE OF THE ASSISTANT SECRETARY
FOR PLANNING AND EVALUATION

OVERVIEW OF THE FINAL REPORT OF THE SEATTLE-DENVER INCOME MAINTENANCE EXPERIMENT
05/01/1983

HOME • OVERVIEW OF THE FINAL REPORT O...

Search for...

OVERVIEW OF THE FINAL REPORT OF THE SEATTLE-DENVER INCOME MAINTENANCE EXPERIMENT

The Seattle-Denver Income Maintenance Experiment (SIME/DIME) was the last in a series of four, large-scale income maintenance experiments undertaken in the late 1960s and early 1970s to measure the disincentive effects of cash transfers on the market work of those eligible for them.

The purpose of the summary is to provide an overview of the rationale behind the income maintenance experiments in general and SIME/DIME in particular; a brief description of SIME/DIME experimental design; and a synopsis of the SIME/DIME research results.

THE POLICY CONTEXT

In the mid 1960s, a consensus emerged among policy analysts and that there were potential problems with the existing set of transfer programs available to those in need. Programs were believed to be fragmented and characterized by variations in benefit levels and administrative access among different types of families. In particular, the working poor in two-parent families were largely excluded. In 1966, the Aid to Families with Dependent Children-Underemployed Fathers (AFDC-UF) program covered less than 100,000 families, although one-third of all the poor

were in two-parent families where the husband worked full-time all year round. The existing set of transfer programs was also asserted to be anti-family. For example, one possible way a poor father could help his family would be to leave his wife and children — thus enabling them to apply for AFDC.

A variety of policy reform proposals were being developed during this period, such as the Heineman Commission's 1969 proposal for a federal negative income tax with universal eligibility. Since a major objective of such proposals was to extend coverage to the working poor, it was considered important to determine if the benefits of increased coverage would be offset by reductions in work. The major reason for the income maintenance experiments was to measure how strong the work disincentive for such a program might be.

Other policy concerns of the period also shaped the design of SIME/DIME. First, the 1960s and early 1970s saw a rapid increase in rates of divorce and separation, and hence, the proportion of female-headed families. This development focused attention on whether a negative income tax with universal eligibility would increase marital stability. Second, "Great Society" programs were not conceived primarily as income supported, but rather as a way to increase the ability of the poor to be economically self-sufficient. This led to the question of whether a job counseling and education or training subsidy program administered simultaneously with a negative income tax could offset the reduction in work that might result from the negative income tax alone.

Four income maintenance experiments were undertaken, starting with the New Jersey Experiment in 1967, including the Rural (1968) and Gary (1971) Experiments, and ending with SIME/DIME, the subject of this overview. SIME/DIME was the largest of the four experiments — indeed larger than the other three combined — and lasted for the longest period of time. For this reason, its results can be viewed with the most confidence.

The next section of the overview discusses the potential of social experimentation as a policy research tool. The third section describes the design and administration of SIME/DIME, and the rest of the overview presents the major research findings that resulted from the analysis of SIME/DIME data.

SOCIAL EXPERIMENTATION

A social experiment is a field test of one or more social programs — or, to use the phraseology of the natural sciences, a test of one or more "treatments." A social experiment is a field test in the sense that families or individuals are actually enrolled in a pilot social program offering some type of special benefit or service. It is "experimental" in the sense that families or individuals are enrolled in each of the tested programs on the basis of a random assignment process, for example, the flip of a coin. To draw conclusions about the effects of the treatment, it is necessary to collect information about the people who are enrolled in each experimental program and about those who receive no special treatment (called the control group), and then to compare them on the basis of the collected information. If the composition of each of the groups is determined by a random process and the groups are composed of similar families or individuals, any differences in measured behavior between the groups can be attributed to the effects of the programs being tested.

It is the random assignment procedure that gives social experimentation its advantage as a policy research tool. Most non-experimental date sources are simply observations of behavior or situations resulting from a myriad of unspecified and indeterminate influences. With such non-experimental data, one can observe factors that appear to change simultaneously or sequentially. However, at best, only tentative conclusions can be drawn about causality. Since experimentations can deliberately inject a new element into an environment, keeping everything else the same, subsequent changes can be attributed to the influence of the new element within known statistical confidence intervals.

This cause-effect characteristic does not, of course, guarantee that social experiments will produce definitive findings; the treatment may turn out to have no effect or to have an effect that is not precisely measurable. And even if there are definitive findings, there is no guarantee that these findings will be those that were expected by the designers of the experiment or will be useful for policy purposes. The scientific success of the experiment depends on several important factors:

- How precisely the propositions to be tested can be formulated;
- How well the experiment is designed to test those propositions;
- How large the experimental and control groups are and how long is the period over which they experience the treatment;
- How much difference the treatment makes to the environment faced by the experimental group compared to the otherwise identical environment faced by the control group — in other words, how strong the treatment is; and
- The extent to which unforeseen or uncontrollable situations and events distort the observed experimental-control comparison.

It is worth expanding briefly on each of these factors.

THE PROPOSITIONS TO BE TESTED

In order to test whether something is the cause of something else, it is important not only to define the treatment and the hypothesized effect clearly and unambiguously, but also to be able to spell out the logic of the mechanism connecting the treatment to the hypothesized effect. If the issue of policy interest cannot be given prior shape in this manner, the unique analytic strength of the experimental approach is not worth its expense.

The primary focus of SIME/DIME — the effect of income maintenance on work effort or labor supply — was a good subject for experimentation from this point of view. Economists had developed a rigorous and widely accepted theory of how changes in effective wage rates and unearned income caused changes in individual labor supply, i.e., the number of hours people worked. The direction of the expected experimental effect was clear and so was the expected cause-effect mechanism. An increase in unearned income or a decrease in the effective wage rate were hypothesized to lead to decreases in labor supply. By the late 1960s, analyses of non-experimental survey data had yielded numerical estimates of the magnitude of such decreases. However, the wide range of available estimates suggested that social experimentation was an appropriate research strategy to improve the accuracy of the numerical estimates.

THE DESIGN OF THE EXPERIMENT

Given a will-defined treatment, an expected effect, and an expected cause-effect mechanism, the next step is to design an experiment that will clarify the cause-effect relationship. Developing such a design is complex. The basic aim, however, is straightforward; to introduce deliberate and systematic variations in the strength of the treatment and in the characteristics of those people exposed to it, so that the resulting pattern of behavioral responses permits identification of how the effects vary both as the treatment changes *and* as the characteristics of those exposed to the treatment change. If the number of treatment variations is numerous in comparison to the number of enrolled families or individuals, the experiment may fail to detect the impact of each of the tested variations with adequate precision. In this case the experiment will have failed in its basic purpose.

For this reason, experiments cannot be designed to test everything. While large-scale social experiments can and do collect a great deal of information on many issues, the most precise data collected will relate to the central issues motivating the experimental design. In the case of SIME/DIME, the experiment was in fact designed to test the effect of two different kinds of social programs on participant work effort. As has been noted, the two policies were a variety of cash transfer or negative income tax programs and several combinations of job counseling and education or training subsidy programs. Many other topics were examined in SIME/DIME — in particular, the relationship between cash transfers and marital stability — but it should be kept in mind that the statistical advantages of the experimentally generated data are somewhat greater for the labor supply analysis.

SIZE OF SAMPLE AND STRENGTH OF TREATMENT

Other things equal, the larger the sample size, the greater the ability of an experiment to detect treatment effects and the greater the statistical confidence that can be placed in the findings. Having a larger sample size than all the other income maintenance experiments combined, SIME/DIME produced the results to which one could attach the greatest degree of confidence.

The "strength" of the cash transfer treatment was greater in SIME/DIME than in the other experiments, both in terms of the generosity of the benefit levels and in terms of the length of the treatment period. The more generous the benefit levels, the greater the expected effects on behavior. Given this expectation, the more generous the benefit level, the smaller is the treatment group size required to detect the effect of the treatment at any given level of statistical confidence. In addition, to the extent that it takes time for families to learn what the treatment means and to make adjustments in behavior, longer treatment periods will also increase the likelihood of detectable response.

With respect to treatment generosity, however, a caveat is in order. Although a stronger treatment is more likely to advance scientific understanding of labor supply behavior, it will not yield better estimates of the probable labor supply responses to a national cash transfer program if the benefits proposed in that program differ substantially from those in the experiment. Care must be exercised, therefore, in drawing policy implications from the experimental results.

THE TREATMENT-CONTROL COMPARISON

We have been discussing random assignment of treatment to subjects in an experimental study. One "treatment" that is frequently tested is no treatment at all, in order words the "control" treatment. Families or individuals enrolled in the control group are not eligible for any special benefit or services, but provide the same information to the experimenters as that provided by the enrollees in the experimental treatments. The control families, of course, do not exist in a vacuum. They are eligible for and participate in ongoing programs similar to the experimental treatments. It is possible to imagine a situation in which, midway through an experiment, a regular government program is implemented that is exactly like the experimental treatment. In such a case, all the difference in behavior between the experimental and control groups might disappear. This is not to say that the treatment has lost its effect — merely that the difference in environments experienced by experimental and control families has disappeared. In the case of SIME/DIME, for example, the members of the control group were potentially eligible for the AFDC and AFDC-UF programs and Food Stamps as well as for a variety of job training programs. Any observed effects of the experiment, therefore, must be interpreted as the differential effects of the experimental treatment compared to existing government programs. Thus any observed experimental-control differences in outcomes must be interpreted as estimates of the effect of replacing the early 1970s status quo with the experimental programs.

In addition to such external influences, certain occurrences within the experimental environment may pose problems for the interpretation of the results. Two that are inevitable in any experiment are sample attrition and mismeasurement of behavior. Attrition occurs when members of the experimental and control groups drop out of the sample and stop providing information to the experimenters. Mis-measurement occurs when information used to measure the effect of the treatments turns out to be inaccurate. Since most of the information used to evaluate SIME/DIME was supplied in personal interviews, the predominate form of mismeasurement here consisted of misreporting of income or hours worked and earning information.

If we could guarantee that the incidence of attrition and misreporting were random and thus identical for the experimental as well as the control groups, the observed experimental responses would provide undistorted estimates of treatment effects. But the incentives to drop out of the experiment and to report income incorrectly may differ for the two groups. With respect to the misreporting bias, experimentals might be expected to underreport in comparison with controls, because the less income they report the larger will be their benefit. Of course, controls receiving AFDC face a similar incentive *vis-a-vis* the welfare office, but less so with respect to the SIME/DIME interviewers. Controls' incentive to underreport earnings to the welfare office, however, may be relatively lower then experimentals', since AFDC benefit levels were lower than SIME/DIME benefit levels. If controls report to the interviewers a higher fraction of their earnings than do experimentals, then the effect of the misreporting bias is to overestimate the actual work reduction effect. Of course, if experimentals report a higher fraction of their earnings than controls the actual work reduction effect is underestimated. With respect to attrition, people in the control group and in low-benefit treatment might be expected to drop out with greater frequency than those on the high-benefit plans — because they have less to lose by leaving the experiment. But those on the high-benefit plans can be expected to have a larger behavioral response to the experiment. Therefore, attrition may cause the observed experimental-control difference to overestimate the actual effect, unless the difference in attrition rates can be explained by measurable family characteristics and these characteristics are controlled for in the analysis.

Additional difficulties in interpreting results may arise in an experiment like SIME/DIME, where different types of treatments were tested in combination. While SIME/DIME was designed so that the independent effects of the two different types of treatment (cash transfers and job counseling/training subsidies) could be separately measured, the separation of effects introduces additional complexity, and thus, potential controversy into the analysis of the experimental data.

Finally, there are inherent limitations in social experiments regardless of design quality. Because experiments have finite durations, they may not completely represent the conditions of a fully implemented, permanent national program. Participants may alter their behavior in response not only to the program options being tested, but to the experiment itself, a phenomenon known as the "Hawthorne Effect". Furthermore, participants, knowing the experimental conditions are only temporary, may not respond in the same way they would to a permanent program. For example, given the opportunity to participate in an income maintenance experiment, individuals may use it to increase their schooling; also a temporary activity.

THE DESIGN AND IMPLEMENTATION OF SIME/DIME

SIME/DIME was launched in Seattle, Washington, in 1970 and extended in 1972 to a second site in Denver, Colorado. The prime contractors for SIME/DIME were the States of Washington and Colorado, which subcontracted with SIR International for the design, operation, and research evaluation of the experiment. SRI International, in turn, subcontracted with Mathematica Policy Research (MPR) for the administration of the cash transfer or negative income

tax (NIT) treatment and all the field data collection from both experimental and control groups. For the administration of the job counseling/training subsidy treatment, SRI subcontracted with the Seattle Central Community College and the Community College of Denver.

The experiment involved almost 5,000 families. In order to test whether, and if so to what extent, behavioral responses varied by duration of treatment, the families in the experimental groups were also randomly assigned to a 3-year or 5-year treatment duration. The four basic treatment combinations were

1. NIT only,
2. Counseling/training only,
3. NIT and counseling/training,
4. no treatment.

The NIT and counseling treatments are described briefly below.

CASH TRANSFER (NIT) TREATMENTS

The cash transfer treatment tested in SIME/DIME, as in the previous income maintenance experiments, consisted of a series of negative income tax plans. A negative income tax is simply a cash transfer program in which there is (a) a maximum benefit (called the guarantee) for which a family is eligible if it has no other income and (b) a rate (called the benefit reduction or tax rate) at which the maximum benefit is reduced as other income rises. The combination of a guarantee and a tax rate defines an income level (called the breakeven level) at which the benefit falls to zero. Families whose incomes rise above this breakeven level no longer receive benefits, although they retain program eligibility and regain benefit entitlement should their income fall below the breakeven level at some future date.

In all the experiments, more than one version of the negative income tax treatment was tested. This was to provide information on how behavioral responses might differ as program structure varied — making the results useful for predicting response to a wide range of cash transfer plans. Table 1 shows the plans tested: three guarantee levels and four tax rates, combined in such a way as to produce 11 negative income tax plans in all. The three guarantee levels for a family of four in 1971 dollars were $3,800, $4,800, and $5,600.[1] The dollar guarantee levels varied with family size (as does the poverty line) with larger families qualifying for higher guarantee levels under a given NIT plan. In the three earlier income maintenance experiments, only constant tax rates were tested (that is, tax rates that remained the same for every level of family income below the breakeven). SIME/DIME tested two constant tax rates: 50% and 70%. In addition to constant tax rates, however, SIME/DIME also tested two declining tax rate schedules. These rates were 80% and 70%, respectively, for the first $1,000 of non-experimental income and then declined by 5 percentage points for each additional $1,000 of non-experimental income.[2]

Table 1. Negative Income Tax Plans Tested in SIME/DIME

INITIAL GUARANTEE[1]	TAX RATE			
	50 PERCENT	70 PERCENT	70 PERCENT (DECLINING)	80 PERCENT (DECLINING)
$3,800	X	X	X	X
$4,800	X	X	X	X

$5,600	X	X	—	X	

Footnote:
1. 1971 dollars.

A major point to keep in mind about the cash transfer plans tested is that they constituted a rather generous benefit range compared with transfer programs now in place or contemplated in most policy proposals. The SIME/DIME treatments averaged out to a negative income tax plan with a maximum benefit for those with no other income of about 115% of the poverty line and an effective marginal tax rate of about 50%. (The effective marginal tax rate was this low because families assigned to the declining tax plans typically face rather low marginal rates). With respect to breakeven level (that is, the income level below which experimental families receive positive benefits), 88% of the two-parent families had breakeven levels above 150% of the poverty line, and 58% were above twice the poverty line. For female-headed families the proportions were 83% and 43% respectively.[3]

The relative generosity of the average benefit received by experimental families as compared with the average received by the control group can be seen in Table 2, which shows the average amount of experimental and other transfers received over the course of the experiment, expressed in 1971 dollars.[4] It should be noted that, over the life of the experiment, there was a rising income trend among the families independent of the experiment, leading to a decline for both groups in the percentage of families with incomes low enough to receive transfer benefits of any kind.

Table 2. Average Transfers Received by SIME/DIME Families
[In 1971 dollars]

	EXPERIMENTAL GROUPS		CONTROL GROUPS		
	SAMPLE SIZE[1]	AVERAGE TRANSFER	SAMPLE SIZE	AVERAGE TRANSFER	
YEAR	HUSBANDS				
1	1,393 (440)	$1,361	1,071	$263	
2	1,294 (415)	$1,296	990	$220	
3	1,183 (392)	$1,276	851	$210	
4	(374)	$1,233	804	$195	
5	(340)	$990	562	$165	
Year	**Wives**				
1	1,408 (445)	$1,455	1,089	$295	
2	1,343 (427)	$1,524	1,026	$327	

3	1,253 (416)	$1,564	895	$383
4	(406)	$1,618	849	$397
5	(372)	$1,323	600	$348
Year	**Single Female Heads**			
1	1,102 (304)	$2,105	699	$1,171
2	1,066 (294)	$2,185	649	$1,089
3	997 (286)	$2,032	562	$1,014
4	(278)	$1,908	525	$932
5	(260)	$1,776	381	$748

Footnote:
1. The sample sizes for the 5-year sample are in parentheses.

Note: For experimental families, the transfer total includes SIME/DIME payments plus AFDC, AFDC-UF, and bonus value of food stamps as reported in interviews. For control families, the transfer total includes AFDC, AFDC-UF, and the food stamp bonus value reported in interviews. The husband and wife sample sizes differ because of family splits.

Source: *Final Report*, Volume 1, Part III, Chapter 4, Tables 3.1 and 3.2.

COUNSELING/TRAINING SUBSIDY TREATMENT

There were three variants of the counseling/training subsidy treatments: counseling only, counseling combined with a 50% subsidy for approved education[5] or training courses, and counseling combined with a 100% subsidy for approved education or training. Within a family enrolled in the counseling/training subsidy programs, every family member aged 16 and older was eligible for the experimental counseling and training subsidies. The counseling component of the treatment can be characterized as voluntary, informational, and non-directive. Including in the training of the SIME/DIME counselors, all of whom had extensive previous counseling experience, was a special three-day workshop in non-directive counseling at the start of the experiment. Within this non-directive framework, the counseling had three main features: self-assessment, labor market assessment, and job search assistance.

Self-assessment consisted non only of getting sample members to evaluate their own employment records and work skills, if any, but also of encouraging them to explore their long-range hopes and unfulfilled expectations without regard to the feasibility of these goals. Feasibility was discussed at a later state. Labor market assessment consisted of providing counselors with detailed reports on occupations of interest to clients including specific tasks involved, credentials and experience required, usual hiring channels, and career potential of the occupation. Although job openings were indicated in cases where that information happened to be available, such information was not usually provided to counselors. The job search assistance component did not involve direct job placement or referrals but, rather, general help in preparing resumes and in practicing job interview skills.

The culmination of the counseling process was the formulation of a "plan of action". Although the counselor did at this stage examine with the participants the obstacles in the way of successfully completing the proposed plan, the participant selected the goal and the counselor helped the participant assess the assets and liabilities of the participant's choice.

The training subsidy component of the treatment was also flexible and geared to providing maximum freedom of choice with respect to the type of education or training subsidize. Two subsidy plans were tested. In one, individuals who chose to receive job-related training were reimbursed for 50 percent of the direct costs of training (tuition, fees, materials, transportation, and child care expenses); in the other, clients were reimbursed for 100 percent of training cost. In both subsidy plans, the amount of training subsidized was limited to the cost of the least expensive local institution providing the desired training. Within that constraint, individuals were free to use the subsidy at the institution of their choice. The requirement that training be "job related" was liberally interpreted so that individuals could receive subsidized training that was required for ambitious career goals (like professional and managerial positions). There was no fixed limit on the amount of training that would be subsidized, either with respect to cost or duration, except that subsidies were limited to the three- or five-year period of experimental eligibility.

ADMINISTRATION OF THE TREATMENTS

The cash transfer treatment, as has been noted, was defined by a guarantee and a tax rate. The amount of payment actually received by a family was calculated according to information regarding non-experimental income, assets, and expenses contained in a monthly Income Report Form submitted by the family to the SIME/DIME payments office. This information was used to calculate the benefit to be paid by check to the family in the subsequent month.

Such a reporting and payment system differed from what was then common state welfare policy, where benefits were calculated for a future period based on projected income and the expected expenses for that future period.[6] Another important difference between the payment calculations used in the income maintenance experiments and those still prevailing under state welfare policy is that, in the former, the underlying basis for experimental benefits was the income received over the previous year. This ensured that families with the same annual income received the same total benefit over the course of a year. For those families whose income remained below their breakeven level throughout the year (most of the sample), this constraint made no difference to the amounts received each month. For those whose incomes fluctuated around their breakeven levels — so that in some months they were above it and in some months below it — receipt of benefits in any month depended not only on whether the family income was below the breakeven level for the most recent month, but also on the family's non-experimental income over the previous twelve-month period. A third important difference in SIME/DIME was the monthly Income Report Firm. The conventional welfare system had no regular reporting form for clients.

Families were enrolled into the experiment in a face-to-face interview at which they were told about the guarantee level and tax rate of the negative income tax plan to which they had been assigned, how the benefit payment was calculated, and how long the experiment would last for them. The filing and record keeping responsibilities of the sample families, the specifics about what counted as income and what were deductible expenses, the details of the payment calculation formula, and the conditions under which the experiment could withhold benefit checks were all spelled out in considerable detail in a set of rules and regulations given to each family. Penalties for misrepresentation were also specified, and appropriate audit and appeal procedures were set up. Although, as in any program, many cases of misreporting did occur, the incidence of deliberate fraud seems to have been low and repayments were usually achieved through appropriate deductions from future payment streams.

Throughout their period of experimental eligibility, families in the treatment group who were also eligible for AFDC or AFDC-UF were allowed to choose which program they wished to receive benefits from in any given month. They were not however, allowed to receive checks from both SIME/DIME and AFDC simultaneously. A major part of the auditing function thus involved making sure that families were not benefiting from both SIME/DIME and AFDC in the same payment period.

The administration of the counseling/training subsidy treatment involved informing families of their eligibility for the counseling/training subsidy program at the time of the enrollment interview mentioned above. A counselor then contacted them and arranged individual or group sessions for those individuals who desired counseling.

DATA COLLECTION

The primary data for analysis of behavioral responses to the experiment came from a series of face-to-face interviews administered three times a year, for the duration of the treatment period and at least one year beyond. These took about forty minutes each, were administered in the families' homes by specially trained interviewers, and were identical for both experimental and control group families.

Detailed questions on every aspect of labor force participation, earnings, and job change for the period since the previous interview were included in each questionnaire. The database thus contained a continuous work history on each family member (age 16 and over) from two years prior to enrollment until at least one year after treatment ended. In addition to the regular labor force core questions, each questionnaire included modules covering other aspects of behavior. In all, 53 questionnaire modules were developed, covering additional economic information such as consumption and wealth as well as family functioning, education, and social and psychological attitudes.

In addition to the rich body of information from these periodic interviews, Social Security and Internal Revenue Service records were collected so that validation studies of the interview-reported earnings information could be conducted.

THE SAMPLE

The composition of the SIME/DIME sample was defined partly with reference to the likely population to be included in any national negative income tax program and partly to assure that important questions about the magnitude of the overall work effort response and about possible differential responses by different population groups could be satisfactorily answered.

The sample was restricted to families with heads between 18 and 58 years of age at enrollment in order to focus on those at least potentially in the prime-aged labor force. For this reason, families with disabled heads were excluded. In addition, eligibility was limited to families with total earnings of less than $9,000 a year if one head was employed, or earnings of $11,000 a year if both husband and wife were employed. These earnings cutoffs were a compromise between the need to include those, although above their breakeven income level at enrollment, could be expected to fall below it during the period of the experiment, and the wish to exclude people whose incomes were so high that potential eligibility for cash transfers appeared highly unlikely. These earnings cutoffs do raise the potential problem of sample selection bias in the results.

To be able to detect differences by family type, both one-parent families with a dependent child present and couples were included in the sample. For convenience, the latter type of family is frequently referred to as two-parent, although there was no requirement that such families contain a dependent child. Nor was there a requirement that the two family heads be legally married, although for convenience they are often referred to as husband and wives. Three ethnic groups were included in the SIME/DIME sample — Blacks, Whites, and (in Denver only) Chicanos. All families were residents of selected low-income census tracts in Seattle and Denver.

The experimental sample was designed using a sophisticated mathematical procedure to yield the maximum amount of useful information within a fixed budget. To achieve this purpose, enrolled families were divided into a large number of different types or "strata." For example, families were divided into seven different income levels, depending on their average income over a number of years prior to the start of the experiment. They were further subdivided according to race and the number of family heads present (one or two). In addition to including a large number of family types or strata, the experiments tested a variety of treatment combinations; twelve NIT treatment (the eleven NIT plans tested plus the control treatment) combined with four counseling/training subsidy treatment (three experimental plans plus the control treatment), in combination with two different periods of experimental eligibility (three and five years). In the interest of economy, the experiment did not test every possible combination of these treatments, but a large number were tested.

After determination of the treatment combinations to be tested and family types to be enrolled, the essence of the sample design problem is to determine the number of families of each family type to be assigned to each treatment combination. For a particular family type and treatment combination, this number is know as the "cell size". Obviously, the average experimental cost of families in a given cell will depend both on the characteristics of the family and the generosity of the treatment combination. Low-income families, for example, will receive higher NIT benefits under a given plan than high-income families. Families assigned to high-guarantee NIT plans or to 100 percent training subsidy plans will cost more to enroll than families in the control group or in low-guarantee and less generous training subsidy plans. It should also be mentioned that not every treatment combination tested was of equal interest or importance. For example, certain guarantee/tax-rate combinations were considered more feasible than others (basically, those in the middle range were considered the more policy-relevant), and consequently the designers put greater emphasis on obtaining precise behavioral response estimates for these combinations.

Taking account of the cost differences of the various cells and their varying degrees of policy relevance as well as their relative contributions to statistically precise measurement of the response pattern, the mathematical procedure alluded to earlier was used to determine the cell sizes in SIME/DIME. A family of a given type which had been selected for the sample was then randomly assigned to a particular treatment combination on the basis of the cell size computed by this mathematical model. Since assignment to particular treatment combinations was completely random for all families of a given family type, the hoped-for result was that measured differences in the behavior of people subject to different treatment combinations measured the effect of the treatment differences.

In all, 4,800 families were enrolled in SIME/DIME including control families. The way the initial sample was distributed by family structure and race and by assignment to site, treatment, and treatment duration can be seen in Table 3. Note that the "pure" control group (that is, the group eligible for neither the cash transfer nor the counseling/training subsidy treatments) accounts for less than one quarter of the sample because the same comparison group can be used for all the treatment variants. The groups eligible for the cash transfer treatment only and the counseling/training subsidy treatment only were slightly smaller than the control group. The group eligible for both a cash transfer and a counseling/training subsidy treatment is about twice as large as either group receiving a single type of treatment.

Table 3. The Distribution of the SIME/DIME Sample at Enrollment

SITE	SAMPLE SIZE
Seattle	2,042
Denver	2,758
Family Structure	
2-parent families	2,769
1-parent families	2,031
Ethnic Group	
White	2,071
Black	1,862
Chicano	867
Treatment/Control	
Negative income tax (NIT) only	946
Counseling/training subsidy only	1,012
NIT plus training/counseling	1,801
Control group	1,041
Treatment Duration	
3 years	2,638
5 years	1,121
Control group	1,041

Source: Murarka, B. A. and R. G. Spiegelman, "Sample Selection in the Seattle and Denver Income Maintenance Experiments," SRI International Technical Memorandum 1, July 1978, p. 53, as quoted in the
— Final Report
, Volume 1, Part I.

Families were located through a intensive survey effort, which first identified the areas in Seattle and Denver that would be most fruitful in terms of the expected yield of eligible families and then canvassed those areas on an individual dwelling-unit basis.

During the period of the experiment, in spite of strenuous efforts to keep track of sample families and persuade them to continue in the experiment, some families dropped out. Over the first thirty months of the experiment, 20% of the originally enrolled husbands, 15% of the originally enrolled wives, and 15% of single heads of families dropped out. The husband-wife differences are due to differential drop-out rates in the cases of couples that split up.

SIME/DIME RESULTS

INTRODUCTION

The major SIME/DIME results are presented under three headings. First, the effects of the experiment on work effort or labor supply are presented. The primary focus is the effect of eligibility for a negative income tax plan, but the analysis sample used to obtain these findings includes all families enrolled in the experiments, including those families enrolled only in the counseling/training subsidy treatment. Second, the effects on hours worked and earnings of the counseling/training subsidy treatment are discussed specifically. Third, the effects of the experimental treatments on "marital stability" are described. Recall that marital stability is not used in the literal sense here since couples were not required to be legally married.

For the marital stability research, the analysis sample included those who were eligible for the negative income tax and counseling/training subsidy treatments alone plus those who were eligible for both, as well as those eligible for neither — the control group. Many other types of behavioral response to the experimental treatments have also been analyzed and are discussed in the *Final Report*. Tables of Contents for Volume I (the research findings) and Volume II (the administration of the experiment) are included at the end of this overview as a guide for readers wishing to pursue issues not covered here.

In all cases, unless otherwise noted, the results presented in this overview are experimental-control differences, adjusted by statistical methods to control for the variation in sample characteristics across treatment combinations.

CASH TRANSFER EFFECTS ON LABOR SUPPLY

The labor supply results for husbands, wives, female family heads, and youth will be summarized in turn. For each group, the overall response to the SIME/DIME negative income tax plans taken together is described first. A discussion of differences in response among the different NIT plans follows.

As mentioned above, the samples used for the labor supply analysis include families that were eligible for the counseling/training subsidy treatment. There is no statistically significant evidence that the counseling/training subsidy treatment altered the effect of the negative income tax treatment on labor supply behaviors. Consequently, the labor supply analysis can statistically separate the effect of the two treatments, and the results described in this section can be interpreted as the effect of just the negative income tax plans on work effort.

The NIT treatment was administered for two different lengths of time (three and five years) to get some information both on how long the family members took to adjust their behavior to the change and on whether a long-term program might have a different effect from a shot-term one. Obviously the experiment could not go on indefinitely. The hope was, however, that any differences between the responses of the three- and five-year sample wold help predict long-term program effects. In addition, interview information was collected for at least one post-experimental year in order to measure any effects of the cessation of benefits.

HUSBANDS

The results for husbands show that the combination of negative income tax plans tested in SIME/DIME — which, as already mentioned, represents on average a relatively generous cash transfer program with a guarantee of 115% of the poverty line and a tax rate of 50% — has a significant negative effect on hours worked per year. Table 4A shows the findings by experimental year and duration of treatment. The best measure of the overall labor supply effect for the combination three- and five-year samples is probably the disincentive effect as measured in the second year — after all the experimentals have had time to adjust to the treatment but before the three-year families start preparing for the treatment to end.

This percentage reduction in hours of labor supplied for the three- and five-year families combined, as measured in the second experimental year, is about 9%. For the three-year sample, the maximum labor supply response is a 7.3% decline, occurring in both the second and third years. For the five-year sample, the maximum response is a 13.6% decline, occurring in the fourth year of the five-year treatment period. These maximum percentage responses represent in absolute terms a decline of about 133 and 234 hours of work per year, respectively.

Table 4. Labor Supply Response of Husbands:
A. Overall NIT response
(percentage difference in annual hours worked)

	YEAR				
SAMPLE GROUP	1	2	3	4	5
3-year sample	-1.6	-7.3	-7.3	-0.5	-0.2
5-year sample	-5.9	-12.2	-13.2	-13.6	-12.3
Total Sample	-3.1	-9.0	-9.3	—	—

B. Second year response, by NIT plan
(percentage difference in annual hours worked)

	TAX RATE

GUARANTEE	50 PERCENT	70 PERCENT	70 PERCENT (DECLINING)	80 PERCENT (DECLINING)
$3,800	-6.7	-5.6	-10.0	-8.9
$4,000 [$4,800?]	-8.8	-1.5	-14.5	-9.9
$5,600	-11.8	-10.4	—	-8.7

Source: Derived according to the formula in the *Final Report*, Volume 1, Part III, Chapter 5, footnote 3 and data in Tables 3.4 and 3.9.

The larger response for husbands enrolled in the five-year group suggests that the response to a long-term national program of comparable generosity might be higher than the response measured by the three-year sample, or even for the combined three- and five-year sample. It should be noted, however, that no general statement about the effect of treatment duration on response can be made, since this effect depends critically on the generosity (i.e., the guarantee/tax rate combination) of the NIT.

By the end of the first post-treatment year, labor supply for NIT-eligible husbands had again returned essentially to the same level as that for controls, indicating strongly both that the observed response was indeed a result of the treatment and that husbands can adjust their labor supply fairly rapidly to changed incentives. Average work reductions were observed to be larger among Black and Chicano men than among White men. However, these results were not statistically significant. Similarly, work reductions were observed to be larger in Denver (which had a tight labor market during the experiment) than in Seattle (which had high unemployment during most of the experiment). Again, these results failed to be statistically significant.

When the results for all 11 negative income tax plans are estimated separately, as shown in Table 4B, we begin to see how the pattern of response changes with changes in the negative income tax plan. As the guarantee becomes more generous, the labor supply response becomes generally more negative. Response does not, however, change in any clear pattern as the tax rate changes. This result may at first appear surprising. However, recall that plans with higher tax rates — and greater associated work disincentives for NIT recipients — also have lower breakeven levels. Consequently, higher tax plans will have fewer recipients, and a smaller fraction of the population will be affected by their work disincentives.

When the plans are grouped according to relative overall generosity as measured by the breakeven level (not shown), the magnitude of the labor supply reductions for husbands increases as the generosity of the plan increases. Findings by plan again indicate greater responses for the five-year sample than the three-year sample. In addition, although the response is uniformly greater for the five-year sample than the three-year sample, the difference between the two is less for the lower than for the higher generosity plans.

What form did the decrease in annual hours worked take? Was it mainly that people worked with the same regularity but for fewer hours each week, or was it that they spent more time not working at all — that is, unemployed or out of the labor force? Experimental husbands did work significantly fewer weeks than control husbands: annual weeks worked were 2.8 less during the second year. Most of this reduction came about through a significant increase in

unemployment.[7] Weeks unemployed for experimental husbands were on average 2.2 more than for controls. One interpretation of this result might be that the cash transfer program enabled husbands to take more time to find a better job. However, other findings from the experiment show that husbands in the experimental group did not find measurably better jobs than their control counterparts, at least as judged by the wage rate. Disregarding the distinction between unemployment and out of the labor force, a safer conclusion is that NIT eligibility induced men who were out of work to spend more time between jobs than men in the control sample. For a few men, the time spent out of employment was increased quite considerably. For example, during the second experimental year the proportion of men in the NIT-eligible group who worked at least one week during the year dropped by 7 percent in comparison to that observed in the control group. For those experimentals who did work, there was a significant reduction in the proportion who worked full time, but no significant impact on the proportion working overtime or only part time.

A question remains as to whether the missing observations of those who dropped out during the experiment and/or possible misreporting bias cause the estimates presented above to be distorted in any measurable way. This is, by its very nature, a difficult question to answer. Examination of other earnings records (particularly from Social Security, but also, on a more fragmentary basis, from the Washington and Colorado Department of Employment Security) on both experimental and control families suggests that for husbands any attrition and misreporting bias is probably small.

WIVES

The labor supply response of wives to the SIME/DIME negative income tax treatment was significantly negative and larger in percentage terms than the response of husbands, at least when that response is measured with SIME/DIME interview data. As shown in Table 5A, the average work reduction for the three- and five-year samples taken together, as measured in the second experimental year, is about 20%. For the three-year sample the maximum effect was a decrease in annual hours of 16.5% in hours worked, occurring in the second year. For the five-year sample, the maximum effect was a decrease in annual hours of approximately 27.1%, occurring in the fourth year. In absolute terms these decreases — just over 100 hours a year and just over 200 hours per year, respectively — are smaller than for husbands. The larger percentages decreases should be interpreted in the context of the smaller average hourly commitment of these women to market work than the average hourly commitment of their husbands. Wives readjusted at the end of the experiment as quickly as husbands, with the wives in the three-year sample even showing a tendency to work more then comparable control families during the second post-experiment year.

Table 5. Labor Supply Response of Wives:
A. Overall NIT response
(percentage difference in annual hours worked)

	YEAR				
SAMPLE GROUP	**1**	**2**	**3**	**4**	**5**
3-year sample	-4.0	-16.5	-15.2	-2.0	+13.4
5-year sample	-15.1	-26.5	-21.6	-27.1	-24.0
Total Sample	-8.1	-20.1	-17.4	—	—

B. Second year response, by NIT plan
(percentage difference in annual hours worked)

| | TAX RATE | | | |
GUARANTEE	50 PERCENT	70 PERCENT	70 PERCENT (DECLINING)	80 PERCENT (DECLINING)
$3,800	-24.7	-13.2	-1.6	-19.7
$4,800	-29.3	-23.2	-20.7	-18.7
$5,600	-28.1	-40.1	—	-12.6

Source: Derived according to the formula in the *Final Report*, Volume 1, Part III, Chapter 5, footnote 3 and data in Tables 3.5 and 3.9.

When response is estimated separately for the eleven plans tested (see Table 5B), the response for wives, as for husbands, generally increased in magnitude as the guarantee became more generous and, again as for husbands, showed a somewhat inconsistent pattern with respect to the tax rate. When plans are arrayed by generosity of the breakeven level, wives show the expected pattern of generally increasing response magnitude as generosity increases. The five-year responses are uniformly, although not significantly, larger than the three-year responses for wives. The treatment duration differences that do exist are again smaller for less generous NIT plans than for plans with higher breakeven levels.

There are some interesting differences between the behavior of wives and husbands with respect to the form the decrease in work actually took. Weeks worked per year were about 3.8 less for experimental wives than for control wives in the second experimental year. Virtually all of this difference took the form of an increase in weeks out of the labor force (rather than weeks unemployed as was the case with husbands).[8] The probability of working at least one week during the year was also significantly less for experimental wives during the second experimental year. With respect to changes in the amount worked by those who do work, there was a significant reduction in the probability of both full-time and part-time work.

Analyst have investigated the importance of misreporting and attrition biases on the experimental results for wives. Employment and earnings checks with the same records used to validate the results for husbands suggest that both the attrition bias and the misreporting bias for wives may be substantial — with both working in the direction of exaggerating observed experimental control differences. Once again, incomplete date makes it impossible to come up with precise estimates of the magnitude of the biases, though examination of the Social Security and unemployment insurance records suggests that as much as half of the measured response may be attributable to differential attrition bias and that a large fraction of the remaining response may represent misreporting bias. The reader should therefore be cautioned that the observed labor supply response for wives might overstate the work reduction that actually occurred.

FEMALE FAMILY HEADS

As with the previous two groups, female family heads responded to the SIME/DIME negative income tax treatment by reducing work effort significantly. The overall response of the three-year and five-year samples taken together, as measured in the second treatment year, was 14%. Their maximum response was larger in absolute and percentage terms than that of either husbands or wives. For the three-year sample (see Table 6A) the maximum reduction was about 22%, occurring in the final treatment year; for the five-year sample the maximum response was about 32%, also occurring in the final treatment year. These correspond to absolute reductions of about 220 and 405 hours per year, respectively. These maximum responses, it should be noted, are about double the average response.

Unlike husbands and wives, female heads do not seem to have responded differently to the three- and five-year treatments, since the responses for the two samples measured over the same period of time are not significantly different. However, their adaptation both to the experiment and to its end appears to have been slower, suggesting that female heads adjust more slowly to changes in financial incentives than do husbands or wives. This suggest that the response observable in any brief-duration experiment, such as three or five years duration, will understate the work reduction of female family heads relative to a permanent program.

Table 6. Labor Supply Response of Female Heads:
A. Overall NIT response
(percentage difference in annual hours worked)

| | YEAR | | | | |
SAMPLE GROUP	1	2	3	4	5
3-year sample	-5.5	-14.1	-21.6	-8.9	-7.7
5-year sample	-7.9	-15.0	-21.2	-28.3	-31.8
Total Sample	-6.3	-14.3	-21.4	—	—

B. Second year response, by NIT plan
(percentage difference in annual hours worked)

| | TAX RATE | | | |
GUARANTEE	50 PERCENT	70 PERCENT	70 PERCENT (DECLINING)	80 PERCENT (DECLINING)
$3,800	-7.0	-19.4	-2.5	-16.0
$4,800	-21.7	-11.7	-20.9	-10.5
$5,600	-6.9	-23.7	—	-25.1

Source: Derived according to the formula in the *Final Report*, Volume 1, Part III, Chapter 5, footnote 3 and data in Tables 3.6 and 3.9.

When the responses are calculated separately for the eleven plans tested (see Table 6B) no obvious pattern of variation between the labor supply response and either guarantee or tax rate is found.

How did the average reduction in annual hours worked manifest itself in patterns of employment for female family heads? First, as with the other two groups, there was a significant reduction in weeks worked. For female heads this reduction was higher than for husbands but lower than for wives. As with wives, but not husbands, this was accounted for by dropping out of the labor force rather than by being unemployed. The probability of working at all during the year also decreased significantly for female heads — again, the magnitude of the labor force participation decline is between those for husbands and for wives. With respect to changes in full-time, part-time, or overtime work by those who worked, female heads reacted more like the husbands than the wives. The only significant change for those that worked was a decrease in full-time work.

With respect to the question of possible misreporting and attrition bias in the observed responses, comparison with other data records suggests that there was no misreporting bias. There is, however, evidence of moderate attrition bias that goes in the other direction from that observed for wives. When interpreting the results for female heads, therefore, it should be kept in mind that the observed responses might underestimate slightly the actual reduction.

YOUTH

Based on their own interview-reported hours and earnings, both male and female youth (at least age 16 but under age 21) appeared to respond to the negative income tax plans tested in SIME/DIME by reducing their labor supply significantly. For the three- and five-year samples for males taken together, the observed reduction was substantial; reported hours worked per week decreased on average by about 24% and the proportion of the year during which they reported working decreased by about 17%. Although the proportional reductions in work effort are large for both young men and women, the absolute reductions are quite small, since average work effort among teenagers is low.

When the labor supply response of youth is estimated for the three- and five-year samples separately, the differences between the two are not generally significant. However, the fact that the observed effects for the five-year sample are larger (and, in the case of young men, sometimes much larger) than for the three-year sample suggests that the work effect of a permanent program might be larger than the observed response in this limited-duration experiment. Differentiating response by plan yields the same lack of tax rate effect as appeared for the other groups; and for youth there is no systematic guarantee effect either.

Labor supply response does vary in an important respect according to whether the youth remained dependents or set up separate households. For males who continued to live at home and for males who left their original family to form new, two-parent families (both eligible for continued payment), the observed work disincentive is substantial. By the second half of the third experimental year, hours worked by the new husbands had declined by about 33% (mainly accounted for by increased unemployment), and the hours worked by those continuing to live at home had declined by about 43% (about equally accounted for by increased unemployment and increased time out of the labor force). But for young men who left their parental family to live as single individuals, there is no significant work disincentive effect. For females there is a significant work disincentive (about a 42% decrease in hours worked) only for those who continue to live with their parental family. For none of the youth groups was the decrease in hours worked accompanied by an increase in time spent in school. The observed work reductions tend to be largest for those who are not in school, but labor supply declines are not restricted to youths in that group.

GENERALIZING TO THE NATIONAL POPULATION

As emphasized earlier in this overview, the observed labor supply responses represented so far are effects that are specific to the population enrolled in SIME/DIME and relate only to NIT programs actually tested in that experiment. In order to predict from the observed SIME/DIME responses the effects of national programs of differing generosity, the SIME/DIME analysts developed a more general form of the work disincentive response.

The most noteworthy pattern they found is that, although the decrease in hours worked by participants gets larger the higher the tax rate (holding the guarantee constant), the work disincentive for the U.S. population as a whole gets smaller. This is because a higher tax rate implies a lower breakeven income level and a smaller number of participants. The positive labor supply response among those losing eligibility is big enough to offset the larger negative response among the program participants.

<div style="border:1px solid #b5d16a; padding:10px;">

COUNSELING/TRAINING SUBSIDY EFFECTS

</div>

A unique feature of SIME/DIME among the four NIT experiments was the testing of a labor market counseling and training subsidy program in addition to a negative income tax transfer program. The rationale for including these programs was to determine whether increased labor market information and increased education and training could offset the decline in work effort that was predicted to occur due to the negative income tax program. As described at the beginning of this overview, the SIME/DIME counseling treatment was informational and non-directive in nature, and types of training deemed appropriate for subsidization were very flexibly defined. The expectation was that the counseling would lead, at least eventually, to better labor market match of skills to jobs and thus higher wage rates, earnings, and possibly, job status. The training subsidy was expected to lead to the same general outcomes, possibly after a brief period of decreased labor market activity in the short run as the extra training was acquired.

PARTICIPATION

There was substantial participation in the counseling and subsidized training programs, particularly among single women. Many of those who participated in the counseling program demonstrated interest in obtaining additional training, including even some who were not eligible for a training subsidy. There was marked diversity in the goals of those who planned to seek training. A majority chose relatively modest occupational and training objectives, but a substantial minority chose quite ambitious objectives that presumably held lower prospects for successful attainment.

Table 7 shows the proportions of the SIME/DIME sample that participated in the counseling and training subsidy programs. Participation rates for counseling rose consistently as the amount of subsidy offered increased. Husbands and wives participated at similar rates (nearly 40% for the counseling only group, just over 50% for the 50% subsidy group and about 60% for the 100% subsidy group). Female heads participated in counseling at uniformly higher rates (54% for the counseling only group, 64% for the 50% subsidy group, and 72% for the 100% subsidy group). A similar pattern emerges for participation in the training subsidy programs, though the percentages are lower. Just over 20% of husband and wives chose to participate in the 50% subsidy option and 36% chose to participate in the 100% subsidy option. For female heads, the figures are 35% and 47%, respectively.

Table 7. Rates of Participation in the Counseling/Training Subsidy Treatments

	COUNSELING ONLY			COUNSELING AND 50 PERCENT SUBSIDY			COUNSELING AND 100 PERCENT SUBSIDY		
	H	W	FH	H	W	FH	H	W	FH
Number of eligibles	510	510	374	671	670	481	391	392	313
Percent attending at least 1 counseling session	39.8	38.2	54.0	51.4	52.8	64.4	60.6	56.6	71.9

Of those participating: ▪ Average number of sessions	4.8	4.9	5.8	6.6	6.4	7.3	6.5	7.4	7.7
▪ Percent receiving some subsidy	—	—	—	21.0	21.3	34.9	36.3	36.5	46.6
Of those receiving subsidy: ▪ Average amount	—	—	—	$363	$401	$650	$666	$954	$857
▪ Average number of academic quarters subsidized	—	—	—	4.0	3.6	4.1	3.8	3.9	4.1

Footnote:
1. Demographic groups are husbands (H), wives (W), and female heads (FH).

Source: *Final Report*, Volume 1, Part IV, Chapter 3, Table 4.1.

For those who participated in counseling, the average number of sessions also increased as the amount of subsidy offered increased — from the 4.8-to-5.8 range for the counseling only group to the 6.5-to-7.7 range for the 100% subsidy range group. The average amount of the subsidy for those who chose to participate range from a low $363 for husbands on the 50% subsidy plan to a high of $954 for wives on the 100% subsidy plan. The average number of academic quarters subsidized ranged between 3.6 and 4.1.

With respect to the amount of schooling received, counseling-only did not make a difference. The subsidies did tend, however, to increase the amount of schooling received — mildly for husbands, more strongly for wives, and most strongly for female heads. For the latter two groups, the effects on schooling were generally stronger the more generous the subsidy.

IMPACT ON LABOR MARKET PERFORMANCE

The counseling/training subsidy treatment did not have the expected positive effect for most groups. The effects on average annual earnings and hours of work are shown in Table 8. Earnings declined during the experimental period and, quite unexpectedly, the negative results tended to continue into the post-program period as well, though these post-program reductions were not statistically significant. Not all results for all groups are statistically significant, but the negative pattern shows clearly through the estimates — in earnings, hours worked, and wage rates.

Table 8. Effect of Counseling and Training Subsidies on Annual Earnings and Hours of Work.

Experimental Year

	1		4		6	
	EARNINGS	HOURS	EARNINGS	HOURS	EARNINGS	HOURS
Husbands						
Counseling Only:						
• 3-year sample • 5-year sample	+$6 -31	-3.0 -2.8	-$5 -185	+24.1 -80.6	— -$239	— -115.6
Counseling and 50% subsidy:						
• 3-year sample • 5-year sample	-248[2] -398[1]	-71.7[2] -105.6[1]	-101 -161	+22.4 73.7	— -33	— -18.7
Counseling and 100% subsidy:						
• 3-year sample	-317[2]	-88.8[1]	-245	-37.9	—	—
Wives						
Counseling Only:						
• 3-year sample • 5-year sample	+21 -187[3]	-8.6 -77.1[3]	-215 -534[1]	-61.8 -197.9[1]	— -430[3]	— -125.8
Counseling and 50% subsidy:						
• 3-year sample • 5-year sample	+6 -255[1]	-14.9 -107.8[1]	-199 -124	-63.9 -43.1	— -301	— -52.1
Counseling and 100% subsidy:						
• 3-year sample	-37	-10.0	-222[3]	-96.1[3]	—	—
Female Heads						
Counseling Only:						

■ 3-year sample ■ 5-year sample	+148 -123	+36.1 +31.8	+144 +184	+13.2 +81.1	— +426	— 183.4[3]
Counseling and 50% subsidy:						
■ 3-year sample ■ 5-year sample	-32 -114	-4.6 -81.3	-207 -89	-98.1 -77.9	— -37	— -112.8
Counseling and 100% subsidy:						
■ 3-year sample	-11	-20.7	-45	-35.7	—	—
Footnotes: 1. Significant at the 1 percent level. 2. Significant at the 5 percent level. 3. Significant at the 10 percent level.						
Source: *Final Report*, Volume 1, Part IV, Chapter 3, Tables 4.5 - 4.7.						

Table 8 shows the results for the three- and five-year samples for years 1, 4, and 6, to indicate both in-program and post-program effects. For husbands, the three-year counseling only program had virtually no impact on earnings. In contrast, the five-year counseling program had a predominately negative impact on earnings, even in the sixth year when experimentals were no longer eligible for counseling. In addition, eligibility for counseling resulted, for those husbands who were working, in consistently lower wage rates than those of their control counterparts. The training subsidy programs led to substantial and significant first year decreases in earnings and hours. The 50% subsidy for example, led to a $248 decrease in annual earnings for the three-year sample and a $398 decrease for the five-year sample. The 100% subsidy, only administered to the three-year sample, led to a $317 decrease. In year 4, the decrease in earnings was smaller and not significant; in year 6, it had practically disappeared.

Wives show a similar pattern of response to the counseling-only program with even larger negative effects. Both the three-year and five-year counseling programs resulted in lower earnings and hours of work for wives in every year, and these effects are generally significant for the five-year program. For example, in the first post-program year, wives

eligible for the five-year counseling treatment earned $430 less than did comparable controls, a 19% reduction. Wives eligible for the training subsidy programs also had lower earnings and worked fewer hours than did controls. These effects tend to be significant in the early years, but are also large and negative in the later years.

For single female heads, the counseling only program probably did have positive results on earnings. Although not statistically significant, the effect on earnings is substantial: in the fifth year, female heads eligible for the counseling only program earned between $275 and $300 more per year than did controls, approximately a 10% increase. Furthermore, the counseling only program did have some significant post-program impacts on the wage rates and hours of work of female heads.

For the combined counseling/training subsidy program, in contrast, the effects were negative for female heads, as for the other groups. As for husbands and wives, female heads eligible for subsidies generally had lower earnings and worked fewer hours than did comparable controls even in the post-program period.

It would be instructive to be able to separate out the effect of the counseling from the effects of the training subsidies, to test the possibility that the counseling was the driving negative influence. While no definitive conclusion on this point can be drawn, because counseling was a prerequisite to the subsidy program, it is possible to infer the separate impact of subsidies when added to an existing counseling program. Here the results for female heads suggest strongly and those for husbands suggest weakly that the addition of a training subsidy program causes even larger earnings and hours reductions than counseling by itself. For wives, the negative impact of counseling plus subsidies is approximately equal to the impact of counseling by itself.

POSSIBLE REASONS FOR THE EFFECTS

How did programs intended to improve the employment and earnings experiences of eligibles actually lead to lower earnings? If the results had indicated no effects, one might be able to explain the results by hypothesizing that very little actually happened in the counseling and training programs. The findings of negative effects, however, cannot be so dismissed. Something did occur in counseling and in training that actually reduced the earnings prospects of participants.

Analysis of date gathered on the objectives of participants suggest that a substantial fraction of the subsidized training was oriented to achieving ambitious career goals. For an important fraction of participants, the goals were evidently overambitious, and the training did not translate into higher earnings even though it may have provided immediate satisfaction to the participants. Evidently, the SIME/DIME counseling/training subsidy program induced short-run reductions in earnings without supporting the type of training or education that would enable participants to secure better paying jobs, at least during the one- to three-year follow-up period. This "training ineffectiveness argument" would explain a zero treatment response, but it doesn't really offer an explanation of the obvious negative response. Perhaps the counseling and training experiences of those with ambitious upward mobility goals actually made it more difficult to pursue a career consisting of a series of relatively low-paying jobs. In any case, a different type of counseling might have resulted in training and education decisions that were less ambitious, but this is entirely conjectural.

EFFECTS ON MARITAL STABILITY (9)

Although, as mentioned earlier, the experiment was not explicitly designed to test effects on marital stability, much attention was paid to the issue in terms of both data collection and analysis. To the extent that the effects of the experiment on marital dissolution were expected to depend on the guarantee and the tax rate, and to the extent that they were expected to differ according to income, ethnicity, and family type, the experimental design was well suited to

the task of determining such effects. But to the extent that response might depend on other factors — such as presence or absence of children, degree of stigma attached to the programs, or tp some unanticipated combined effect of the cash benefit and counseling/training subsidy treatment — the design was not optimal for testing those hypotheses. This nonoptimality implies only that the response is measured with less statistical efficiency than with an optimal design, not that the design affects the validity of the response analysis.

EXPECTED EFFECTS

Previous research on the possible determinants of marital stability had not reached as well defined a consensus on the expected effects of cash transfers on marital dissolution and the chain of causation through which these effects may occur, as was the case with respect to labor supply. There was some theoretical basis in the literature for believing both that cash transfers would reduce marital dissolution and that they would increase it.

The "conventional wisdom" at the time the experiment began, however, was relatively unambiguous. The widely held view then was that the welfare system might be contributing to marital dissolution. AFDC was restricted largely to one-parent families. The AFDC-UF program for which two-parent families with children were eligible was not available in every state and, even where available, was so highly restrictive in its eligibility requirements that few two-parent families actually participated. The empirical evidence on whether AFDC increased marital dissolution was mixed, but the policy presumption was that, if it did, a negative income tax for which both one-parent and two-parent families were eligible would be stabilizing influence. A negative income tax with universal eligibility would be available to all families as soon as their incomes fell below a certain level, regardless of who was part of the family. Therefore, the argument went, the incentive to leave one's family in order to make them eligible for cash transfers would no longer exist.

THE ANALYSIS

As already noted, two-parent families with children did not have to be legally married, or even claim to be, in order to be eligible for SIME/DIME benefits. All they needed was to be living together on a continuing basis. Unmarried couples without children were ineligible for SIME/DIME. The SIME/DIME rules also permitted persons who had left their original partner to retain SIME/DIME eligibility and continue receiving negative income tax payments if their current income were below the breakeven level given their new family size. Thus, if an original two-parent family split up, the experiment permitted both halves of the original family to continue receiving (separate) NIT payments. If a member of the original couple formed a new continuing relationship, the new person was counted after a short waiting period as an eligible family member in computing SIME/DIME benefits.

During the first three years of SIME/DIME, roughly one in five of the couples married at enrollment were observed to break up. Table 9 shows the proportion of original marriages that dissolved during the first three payment years, broken down by duration of treatment and ethnic group.

The methodology used in statistical analysis of the SIME/DIME data is complex and need not be spelled out in this overview. A number of points are worth keeping in mind as the analytical results are discussed below. First, the estimation methodology properly places substantial emphasis not only on the number of events but also on their timing.[10] Second, the analysts place more emphasis on their findings for the smaller number of families in the five-year treatment group than for the larger number of families in the three-year treatment group, arguing that the longer treatment more closely approximates a permanent program. In addition, as the raw date in Table 9 suggest, the statistical analysis demonstrates that the different ethnic groups react differently to the treatments, and hence needs to be analyzed separately, thus reducing further the size of the samples used in the analysis.

Third, the treatment group on which the statistical results are based includes those who received both the negative income tax treatment and the counseling/training subsidy treatment, as well as those on the negative income tax treatment only. The statistical procedure used in the marital dissolution analysis to adjust for the counseling/training subsidy treatment is basically the same as that used in the labor supply analysis. Although the interaction between the NIT and counseling/training subsidy treatments is statistically significant for Whites (but not for Blacks or Chicanos), the analysts conclude that the unsystematic nature of the interactions is most plausibly explained by sampling variable. An additional finding that leads the authors to discount the importance of the interaction between the two types of treatment is that when they used their statistical methodology to re-estimate rates of marital dissolution in the New Jersey income maintenance experiment, they found the NIT effects to be similar in New Jersey (where there was no counseling or training subsidy) and SIME/DIME.

Table 9. Proportions of Original Marriages Observed to End
During the First 3 Years of SIME/DIME[1]

	BLACKS	WHITE	CHICANOS
Control Group	0.205 (435)	0.145 (608)	0.185 (200)
NIT Treatment Group	.278 (504)	.203 (691)	.222 (338)
▪ 3-year Sample	.270 (333)	.198 (479)	.223 (238)
▪ 5-year Sample	.292 (171)	.212 (212)	.220 (100)
Footnote: 1. The number of original couples is shown in parentheses.			
Source: *Final Report*, Volume 1, Part V, Chapter 5, Table 5.3.			

THE OVERALL EFFECT

Table 10 shows the estimated effect of the negative income tax treatment for the three-year treatment and five-year treatment families, by ethnic group. The rate of marital dissolutions among Chicanos is unaffected by the NIT. As can be seen, the overall effect on marital dissolution rates is positive and substantial for Black and White families in both the three-year and five-year NIT treatment groups.

To confirm the observed effects for Blacks and Whites, the analysis differentiated the dissolution rates during the experiment from those occurring in the period after the treatments ended. For this analysis, three time periods were used — enrollment to three years later, three years after enrollment to five years after, and five years after enrollment to seven years after (i.e., the post-experimental years for the five-year treatment group). The impact of the NIT on marital dissolution effects for both the three-years and five-years treatments are again positive and significant for Blacks and Whites, but not for Chicanos. In the post-treatment period, the experimental-control difference disappears altogether for both the three-year and five-year treatment groups. Thus, the observed response in dissolution is clearly attributable to the experiment.

A separate analysis of the experimental effect on remarriage concludes that the NIT treatments did not affect remarriage rates for single White or Black women, but did reduce the rate by over 60 percent for single Chicano women. Furthermore, after an analysis of how sample attrition may have biased estimates of the change in dissolution rates, the analysts concluded that the unadjusted estimates for Whites and Blacks, which range in Table 10 from 40 percent to 50 percent, should be reduced about 10 percentage points for Blacks and about 5 percentage points for Whites.

Table 10. Estimated Percentage Change in Marital Dissolution Rates Caused by the NIT Treatments: All Marriages[1]

	BLACKS	WHITES	CHICANOS
3-year sample	+47[2]	-41[2]	+19
5-year sample	+43[2]	+43[2]	+2
Number of marriages	1,203	1,714	698

Footnotes:
1. Includes relationships entered into after the start of the experiment; 3-year treatment effect is estimated over a 3-year period and 5-year effect over 5 years.
2. Significant at 1 percent level.

Source: *Final Report*, Volume 1, Part V, Chapter 5, Table 5.5.

EXPLANATION OF EFFECTS

What is the reason for the experimental effect on Black and White marriages and single Chicano women, given that this effect is the differential effect of the SIME/DIME NIT treatment compared to the effect of the public assistance (AFDC) option present in the control environment? The analysts began explaining the increase in marital dissolution rates by noting a surprising general pattern of the experimental effects when estimated separately by NIT plan. There was no perfectly consistent pattern, but what pattern there was suggested the paradox that lower marital dissolution rates are associated with higher guarantees.

Grouping by guarantee confirms that the experimental effect tends to decrease as the generosity of the guarantee increases. In fact, for White couples the effect is statistically significant only for the low $3,800 guarantee (an 82 percent increase in the rate of marital dissolution), and for Blacks the effect is statistically significant only for the $3,800 guarantee (a 60 percent increase) and the medium $4,800 guarantee (a 91 percent increase). For neither Black nor White is the effect significant for the high $5,600 guarantee. This pattern of effects is especially striking in view of the fact that the $3,800 guarantee most closely approximates the generosity of the AFDC and AFDC-UF programs available to the control group.

Recognition of two aspects of the situation may account for this pattern of findings. First, according to the existing literature, an increase in cash transfers can be expected to have two opposing effects on the marital dissolution rate. Increases in family income tend to stabilize marriages, giving rise to an *income* effect; but a cash transfer program that provides financial alternatives to marriage for low-income women also tends to destabilize marriage, causing an *independence* effect. Depending on the strength of the two effects, which are opposite in direction, a negative income