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THE IMPACT OF INCOME MAINTENANCE ON THE MAKING AND BREAKING OF MARITAL UNIONS: INTERIM REPORT

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ABSTRACT

The purpose of this preliminary study was to determine the systematic effects of income maintenance on the probability of marriage and of marital dissolution. Extensive analyses are presented of the impact of income maintenance on the dissolution and formation of marriages during the first 18 months of the Seattle and Denver Income Maintenance Experiments. The study focus is on women enrolled as heads of families (either with or without a male head). For each spell of singleness and of marriage women had during the first 18 months, we estimate the impact of income maintenance on the probability that they will change their marital status.

Two different procedures are used for assessing impacts of income maintenance. The first procedure employs a linear probability model with observations from each six-month period pooled into a single equation. The second procedure employs a stochastic model of rare events in which the rate at which an event occurs is assumed to depend log-linearly on the set of exogenous variables. This model is estimated by maximum likelihood. We report the results from both procedures to determine the robustness of our findings.

The overall impact of income maintenance is to raise the rate of marital dissolution. For each of the three race-ethnic samples, the effect is strongest for the support levels closest to the control situation, which includes AFDC and food stamps. This pattern of effects persists when a variety of interactions of support levels with individual characteristics are estimated. The magnitude of the impacts of income maintenance is very large over the range of specifications examined.

The impact on remarriage differs greatly by race-ethnicity. For Black women, the rate of remarriage increases with the level of support. The opposite pattern holds for Chicanas. For Whites there is no discernible impact of income maintenance on the rate of remarriage.

Because there was evidence that attrition (dropping out of the experiment) was related to both the experimental treatment and marital status changes, we conducted an analysis to determine the sensitivity of our experimental results to attrition. We concluded that while attrition could cause us to underestimate the rates of marital status change among the controls, any bias produced could not account for the observed experimental effects. We also conducted an analysis to determine the degree to which our definition of marriage, which counts a couple who separates and later reconciles as a dissolution and remarriage, might affect our results. We concluded that the definition of marriage used does not substantially inflate the experimental impacts observed and even under a more restrictive definition we would have observed significant experimental impacts.

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I INTRODUCTION

This report is one of a series on the marital status change response of families enrolled in the financial and manpower treatments of the Seattle and Denver Income Maintenance Experiments (SIME/DIME). An earlier memorandum[1] presented preliminary estimates of the response in Denver over the first 18 months of the experiment. The present report contains a much more extensive analysis of responses in both Seattle and Denver for the same period. This analysis also differs from the earlier report in that:

- We analyze all marital status changes during the period (not just the first change).
- We use two different analytical models to evaluate experimental impacts.
- We present preliminary estimates of dynamic response to experiment treatment.
- We investigate the possible consequences of our results on attrition from the experiment.

Since this is an interim report, the results should not be considered as final, even for the period covered.

We continue to focus exclusively on the marital status changes of those women who were heads of families (either with or without a male head) at the time of enrollment. For each spell of singleness and of marriage that they had during the first 18 months, we estimate the impact of income maintenance on the probability that they will end the spell (i.e., remarry or dissolve a marriage). We concentrate on female heads because so few unmarried male heads were enrolled in the experiment. In future reports we will investigate the impact of income maintenance on the subsequent marital status changes of enrolled male heads and of those enrolled individuals who were not heads at the time of enrollment (e.g., teenage children).

Although we estimate the impacts on marital status change of both the financial and manpower treatments, we concentrate on the former since it has the greater policy interest. Later studies will relate labor supply responses to marital status change responses, and present a more comprehensive analysis of the effects of the manpower treatments.

This preliminary study was to determine whether or not income maintenance has a systematic effect on the probability of marriage and of marital dissolution. To do this, we conducted a relatively simple analysis, relying heavily on the experimental nature of the design. In particular, we did not analyze the way in which marital status change responses depend on other responses (e.g., labor supply changes) to income maintenance, nor did we create a model of the behavioral processes that link income maintenance to changes in marital status. The results of these analyses will be reported in future papers.

A review of the previous research on the relationships between income and marital events is presented in Section II. The data and methodology of the report, including the design of the Seattle and Denver Income Maintenance Experiments and the analytical models used to assess experimental impacts, are described in Sections III through V. The results of our analyses of the impacts of income maintenance on marital dissolution and remarriage are described in Sections VI and VII, respectively. Section VIII contains an analysis of the likely impact of attrition on our findings. These findings include an analysis of attrition during the first 18 months and an analysis of the sensitivity of our estimated impacts to attrition. In section IX, we summarize our main findings.

II INCOME AND MARITAL EVENTS: REVIEW OF PREVIOUS RESEARCH

In this section, we review theoretical arguments and previous empirical research on the relationship of income to marriage and the dissolution of marriage. The results cited provide the substantive motivation for some of our analyses and serve as a basis from which to evaluate our findings.

We first discuss dissolution of marriage, then marriage. Since most women that are enrolled in the study have been married at least once, this ordering reflects the stages of marital decision making. Moreover, remarriage has not been studied as intensively as dissolution of marriage.

A. Income and Dissolution

The sociological literature on dissolution of marriage has focused primarily on the quality of interpersonal relations in the marriage and on the fit among the attributes of the partners (e.g., statuses, personality factors, and cultural backgrounds). There is, however, an important line of research--tracing back at least 25 years--on the relationship of socioeconomic status (SES) to dissolution of marriage. This research has produced one well-established empirical generalization:

> When dissolution is available equally to all social classes, there is an inverse relationship between socioeconomic status and the probability of marital dissolution.[2]

Any attempt to explain the generalization reveals the complexity of the relationship of SES to dissolution. Consider Goode's widely cited explanations for the lower propensity to divorce in the upper socioeconomic status:

(1) The network of social and of kin relations is more extended, more tightly organized, and exercises greater control over the individual.

(2) The income differentials between the wife and husband are greater in the upper strata than in the lower strata; consequently the wife has more reason to maintain the marriage if she can.

(3) Toward the upper strata, far more of the husband's income is committed to long-term expenditures, from which he cannot easily withdraw to support an independent existence.

(4) The husband in the lower strata can easily escape the child-support payments and other postdivorce expenditures because his life is more anonymous and legal controls are less effective.

(5) The strains internal to the marriage are greater in the lower strata: marital satisfaction scores are lower, romantic attachment between spouses is less common, the husband is less willing to share household tasks when the wife is working, and so on.

These propositions intermingle three possible effects. One hypothesis is of a broad social class effect [Propositions (1), (3), (4), and (5)] that states that patterns of social relations, consumption habits, values, and orientations toward marriage differ among social classes and that observed variations in dissolution rates are a function of these differences. A second effect is one that concerns only the financial independence of spouses on the marriage (Proposition 2). This effect compares income levels in the marriage with those in singleness (or perhaps, in other marriages). It is not clear, in Goode's (or most other) discussions, whether these propositions [particularly (3), (4), and (5)] involve a third effect that is only attributable to family income. Many investigators have recently argued for pure income effects. For example, Cutright[3] proposes:

> It is reasonable to expect that male income will be more important to marital stability than social status derived from years of education or occupational position because income is more directly linked to consumption than either of the other two status indicators. Consumption is daily activity, and provides the wife with a constant empirical monitoring of how well her husband is doing in his role as bread winner. A satisfactory level of consumption should help the wife maintain her own feelings of competence in her role of wife and homemaker, and should act to reinforce her positive view of her husband. Of course, the husband's view of himself as an adequate provider may also be directly linked to his evaluation of his current earnings and prospects for future income growth.

Thus the literature on the relationships of risk of marital dissolution and socioeconomic variables offers three broad propositions:

(1) Family social class is inversely related to risk of marital dissolution (social class effect).

(2) Financial independence (of the lower income partner) is positively related to risk of marital dissolution (independence effect).

(3) Family income is inversely related to risk of marital dissolution (income effect).

All three hypotheses have a degree of plausibility and each, if true, can explain the observed relationship between income and dissolution rates. To understand the possible effects of income maintenance on dissolution rates it is essential to separate the hypotheses into two parts: (1) class versus current income (family income and independence) effects, and (2) the two different effects of income variations.

Much sociological literature suggests that the relationship of income to dissolution reflects only the operation of social class effects. If so, only interventions that alter social class will affect dissolution. The stronger version of this argument holds that class-linked behavior patterns are set relatively early in life. The weaker version holds that major, long-term shifts in resources during adulthood change class-linked behaviors. According to both versions a permanent, national income maintenance program might affect dissolution rates, but neither condition would be met by a short-term program of modest transfer payments. If the fundamental relationship involves patterns of marital behavior and decision-making linked to social class, income maintenance experiments would likely have no effects on dissolution rates.

The research that examines the alternative possibility--that variation in family income affects the risk of dissolution--focuses on family income and does not distinguish among the male's earnings, the female's earnings, and nonwage income. We expect that the higher the family's income, the greater the benefits received by the family members. These greater benefits should tend to lower the probability of dissolution. We refer to this as "a pure income effect" because this tendency should hold whatever the source of the income.

Increases in family income also change the difference between benefits received from within the family and the potential benefits received from outside the family. The source of the income is important in making this comparison. If the increase in family income is from an increase in the male's earnings, the female becomes more economically dependent on her husband. Thus, an increase in the male's earnings should lower the probability of dissolution because it increases the female's dependence as well as raises the total family welfare. On the other hand, if the increase in family income is from an increase in the female's earnings or an increase in nonwage income available from outside of the marriage (including both AFDC and income maintenance), the female becomes less dependent upon her husband. Thus, an increase in the female's earnings or in transferable nonwage income should tend to raise the probability of dissolution through its effect on the woman's independence as well as to lower the probability of dissolution becauses it raises the total family welfare.* Since income maintenance will both increase income and increase independence, we cannot predict its net effect. These opposing effects are discussed more fully in Section VI.

We are aware of no research that empirically distinguishes between the effects of pure income and independence on marital dissolution.† At best, empirical researchers have contrasted the explanatory power of measures of family income with education and other measures of social class backgrounds. Many published analyses of census materials[3,6,7,8] relate family income, education, and other variables to marital status (married, divorced, never married, widowed). Unfortunately, analyses of census data typically involve three complications in methodology that greatly diminish their value. First, reliance on current marital status treats divorce as a <u>state</u> occupied by individuals rather than as an <u>event</u> characterizing unions. The measurement of divorce is thereby confounded with rates of remarriage and lengths of duration between marriages.‡ Second, for at least some variables (notably income), the direction of causal effect is obscured by the cross-sectional analysis of current marital state. Persons currently divorced may have been in that state for some time and their current levels on socioeconomic variables may be quite different

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*Our argument on this point basically follows that of Becker.[4,5] Where we have referred to independence and income effects, Becker discusses the effects of changes in full income on relative gains to being married rather than single. Where our analysis focuses on comparisons of being married to becoming single, Becker compares the relative gains accrued to a particular marriage to those available in another match.

†An exception to this is work reported in Ross and Sawhill,[9] which has recently come to our attention.

‡As a simple example, consider two populations with identical dissolution rates. If one population remarries instantly while the second never remarries, the second population will always have a fraction divorced while the first will not.

than the levels during the previous marriage. In fact, the dissolution of marriages appears to hamper occupational achievement (and presumably income) for males.[10] If this is so, the cross-sectional relationship between income and marital status includes both the effects of income on risk of divorce and the effects of divorce on income. Finally, because of their cross-sectional nature, these studies compare families with different levels of income rather than the impact of changes in income on the probability of dissolution of the marriage. For these reasons, we conclude that most empirical literature provides a weak foundation for modeling the effects of class and income on risk of dissolution.

A few studies have avoided one or more of these problems in methodology. Glick and Norton[11] obtained marital histories and measures of current socioeconomic variables on a sample of 28,000 households from the 1967 Survey of Economic Opportunity. The use of marital histories permits specific rates to be calculated that avoid the confounding of dissolution with remarriage. However, the measurement of income and education at the end of the histories (1967) confounds, to some extent, causes and consequences of dissolution as well as prevents inferences based on changes in income. Despite this limitation, the results they presented are interesting. They reported evidence of an education and a male income effect on dissolution rates. Although no multivariate analysis is reported:[11]

> The probabilities of divorce by income and education can be properly interpreted as consistent with the hypothesis that income is more significant than education in determining which men obtain a divorce, particularly during the first ten years of marriage.

But, since income is more likely than education to be causally affected by changes in marital status, this conclusion may be misleading.

Results on changes in marital status from the first four years of the Panel Study of Income Dynamics[12] in which approximately 5,000 families were interviewed annually for seven years, beginning in 1968, partially support Cutright's contention concerning the causal priority of income. A modification of Automatic Interaction Detection (AID) analysis--which divides the sample sequentially into groups, with the largest differences in their distributions being in family composition--yielded a distinctively nonadditive relationship of 1967 income to divorce probability. For those families in which the male head was 18 to 34 years of age and the marriage less than five years old in 1968, the probability of divorce decreased with family income; but for marriages of more than five years, the probability of divorce increased with family income.* This result is unexpected and perhaps should not be taken seriously, since it does not involve simultaneous controls for education, race, and other variables.

As we have seen, most nonexperimental research provides shaky support for the hypothesis that variations in current income affect chances of dissolution. We now consider the results of three other income maintenance experiments. These results are particularly instructive since the research designs avoid the difficulties in methodology that hamper nonexperimental research. Moreover, the experimental variations of income permit stronger inferences concerning income effects.

The first income maintenance experiment was conducted on a sample of 1,160 two-headed households residing in New Jersey or in Scranton, Pennsylvania. Analysis of the probability of at least one marital dissolution during the three-year experiment by Knudsen et al.[13] showed

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*These researchers also report that changes in family income relative to needs (indexed by family size) are related to the risk of dissolution.[12] The divorce rate for those whose income decreased relative to needs was 13.7% between 1968 and 1972 compared to 7.5% for those with no change or an increase (see their Table 2.9). Unfortunately, income change is measured after dissolution so their analysis confounds the effects of divorce on income and income on divorce.

support for both class and income effects and puzzling experimental effects. The nonexperimental findings parallel those mentioned above: preexperimental family income (dichotomized at $4,500 per year) had a strong negative association with divorce, while the education of male heads had lesser effects in the same direction. Concerning the effects of income maintenance treatments, Knudsen et al. reported, "We found no evidence of changes in household composition among experimental families indicative of major disruptions in family life." This conclusion is misleading, in our view, since it refers to effects on all types of composition changes (e.g., dissolution, death, birth, and addition or loss of nonhead members). Clearly, the range of events that cause a composition change are too heterogenous to justify the conclusions drawn.

Fortunately, the report enables us to obtain a few results on changes from two-headed families to female-headed family. These changes parallel the causes of dissolutions considered by other researchers. For these events, the New Jersey data show that the effects of preexperimental variables are similar to those reported above. The probability of dissolution declines with preexperimental income and with the education of the male head.

Their experimental findings are more puzzling. The experimental plans (defined in terms of a support level of 50%, 75%, 100%, or 125% of the poverty level and tax rates of 30%, 50%, and 70%) are grouped according to the breakeven point. The breakeven point is the amount of family wage and nonwage income at which the income maintenance transfer becomes zero. Plans were grouped into three categories: low, medium, and high. Logistic regressions of dissolution/nondissolution on dummy variables representing the plans, and a variety of preexperimental characteristics of families were estimated. Each regression shows a strikingly nonmonotonic pattern of plan effects.* The largest positive effect is for the low plan, with somewhat smaller positive effects for medium plans. In each case, the high plans are indistinguishable from the controls. These findings are paradoxical because those plans that are most like the nonexperimental environment (which includes AFDC) have the largest effects, while those plans that depart most from the preexperimental situation yield no effect. The authors attempt no interpretation of these findings.

A second experiment--the Rural Income Maintenance Experiment--conducted on a sample of 600 North Carolina and Iowa two-headed households, reported very low dissolution rates over a three-year period for both experimental and control families: 0.0175 and 0.0181 per year, respectively.[13] Clearly there was not enough variation in the outcome to draw dependable inferences from this experiment.

Finally, the Gary Income Maintenance Experiment reported preliminary results on marital dissolutions for 674 families over a three-year period.[15] Unfortunately, difficulties in data retrieval restricted the analysis to investigating whether persons married at enrollment were married three years later, without distinguishing whether they were married to the same person. Therefore, to some extent, dissolution effects are confounded with remarriage effects. Not surprisingly, there is no strong pattern of experimental plan effects.

We conclude that no strong inferences can be drawn from the three previous income maintenance experiments. Each of the three studies was hampered by a relatively small sample which is particularly damaging to the analysis of rare events such as dissolutions. Further, an extensive

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*The effects are large relative to the effects of other variables. But, since no standard errors are reported, we cannot rule out the possibility that pattern reflects only sampling error.

analysis of marital dissolution has not yet been reported from any of the experiments. As a result, we have no firmer basis for proposing the effects of income variations on dissolution rates than we had before the experimental research.

B. Remarriage

Virtually all theoretical and empirical work on factors affecting rates of marriage focus on first marriages, particularly on the timing of first marriages. As a result, we know very little about the remarriage process. Lacking any better framework, we organize our discussion in terms of the three hypotheses made in Section II-A.

It is not obvious that the probability of remarriage varies among social classes. Census materials reveal a U-shaped relationship of women's education to the probability of never marrying. The highest rates of remaining single are for women with the lowest and, particularly, the highest levels of educational attainment.[7,16,17] Goode[18] reports the same pattern for remarriage. These data are now quite dated, however, and the relationship may have changed in the interim.

While there is doubt concerning the existence of a class effect, it seems much more likely that variations in income affect the rate of remarriage. Consider the effect of a woman's resources (financial and otherwise) on the probability that, having divorced, she will remarry. The larger and the more stable her resources outside the marriage, the more likely it is that she can satisfy material needs outside of marriage. That is, the greater her own resources, the less she foregoes by remaining single. In particular, women with low levels of resources (i.e., a combination of low asset levels, low wage rates, and little history of labor force participation) almost certainly have much to gain economically from marriage to wage-earning men. Conversely, financially more independent women have lower expected economic gains from marriage. This argument parallels closely that made in Section II-A concerning independence of women. We expect, then, that _increases to a woman's wage and nonwage income make her less dependent on marriage and thereby lowers the probability of remarriage._

A small amount of empirical research supports the independence hypothesis. In two senses women having children (particularly young children) are more financially dependent on financial support from men than those who do not. The presence of children in the family lowers the standard of living (e.g., per capita income) when income level is controlled. Also, women with young children face more constraints in working (at the least their wage rates are lowered by the cost of child care). Not surprisingly, women with children are more likely to remarry.[16,18] A second finding that supports the independence hypothesis is that divorcees who are employed are less likely to remarry.[18]

Income variations might also have an effect opposite to that implied by the independence effect. As a woman's resources increase, she might become a more desirable mate (a _dowry_ effect). If so, the number and variety of marriage offers she receives would rise, and the probability that she will receive an acceptable offer would increase. We know of no empirical research on the remarriage process that bears on this hypothesis. There is, however, an ample ethnographic literature on the economics of arranged marriages supporting this view.

To complete the picture of the paucity of evidence on remarriage, we note that none of the other income maintenance experiments has reported any analysis of the issue.

III THE DESIGN OF THE EXPERIMENT

The Seattle and Denver Income Maintenance Experiments (SIME/DIME) are designed to measure the effects of a negative income tax program and of a manpower training program on family stability and the labor supplied by family members.[19]

A. The Experimentally Manipulated Variables

Under the SIME/DIME negative income tax program, a family's support level depends on the program to which it is assigned and the number of family members. The support (or guarantee) is the amount of money available to the family over the period of a year, assuming the family had no other source of income. The amount of the actual grant to the family depends on both the support and the family's other income. As the other income increases, the grant declines at a rate stipulated by the program.

The support levels used are \$3,800 per year, \$4,800 per year, and \$5,600 per year (all in constant 1971 dollars) for a primary family consisting of four persons. To provide roughly equivalent real support on a per capita basis, these levels are adjusted by a family size index.

The tax function determines how the grant to the family changes in response to other income available to the family. If G represents the amount of the grant, S the support level, Y the amount of taxable income received by the family, and t(Y) the tax function, then:

$$G = S - t(Y) \cdot Y$$

The tax function t(Y) depends on two quantities, t', the initial tax rate on the first dollar of income and, r, the rate of decline of the tax rate with increases in income:

$$t(Y) = t' - r \cdot Y$$

The initial tax rates used are 0.50, 0.70, and 0.80; the rates of decline used are zero (i.e., a constant tax rate) and 0.000025 (i.e., a tax rate that declines by 2.5% per $1,000 of earned family income). Of the six possible tax systems resulting from combinations of these initial tax rates and rates of decline, only four are actually used: the 50% constant tax rate, the 70% constant tax rate, the 70% declining tax rate, and the 80% declining tax rate. Combining the three support levels with the four tax systems gives a total of 12 negative income tax treatments (also called financial plans). The $5,600 per year support level with a 70% declining tax rate is not used, because under this combination the declining tax does not exhaust support before a zero tax rate is reached. Table 3-1 describes the treatment levels used in the experiments.

In designing the experiment, it was anticipated that families who viewed the income maintenance program as transitory would adjust their behavior differently from those who viewed the program as permanent. Since it is crucial that an income maintenance experiment measure long-term responses, such as would be expected on a permanent national program, the experiment must be long enough that once a period of initial adjustment to the program elapses, sufficient time remains on the experiment for families to reasonably be expected to make long-term behavior adjustments. Although it was expected that a three-year period would be sufficient for this purpose, a portion of the families was enrolled for five-year periods to ensure that long-term adjustments would in fact be observed.

Table 3-1

FINANCIAL TREATMENTS

Financial Treatment	Support Level	Initial Tax Rate	Rate of Decline of Tax Rate
F_0	$ 0	0 %	0 %*
F_1	3,800	0.50	0
F_2	3,800	0.70	0
F_3	3,800	0.70	0.000025†
F_4	3,800	0.80	0.000025
F_5	4,800	0.50	0
F_6	4,800	0.70	0
F_7	4,800	0.70	0.000025
F_8	4,800	0.80	0.000025
F_9	5,600	0.50	0
F_{10}	5,600	0.70	0
F_{11}	5,600	0.80	0.000025

*Controls.

†Tax rate declines at an average rate of 2.5% per $1,000 income.

Another aspect of the experiment is the manpower program, which has four treatment levels. The first treatment, M1, provides only counseling services. M2 provides counseling plus a subsidy of 50% of the direct costs of any training taken during the experiment. M3 consists of counseling plus 100% of the direct costs of training. Counseling, which is voluntary in both Denver and Seattle, is provided by local community college counseling staffs. In the fourth, or control, treatment, no counseling nor subsidies were provided. Families were assigned so there would be all combinations of financial treatments and manpower programs, and with control families being neither on a financial nor a manpower program. No five-year families are on M3, which avoided the possibility of providing full support for four years of college.

Individuals are eligible for the treatments throughout the entire experimental period. When a marital dissolution occurs both spouses are eligible for the treatment in their new families, with the guarantee adjusted to their new family sizes. When new members join an existing family through marriage or birth they are eligible for the treatment and the guarantee is adjusted to reflect the new family size.

B. The Assignment Problem

Families were enrolled in the Seattle and Denver experiments on the basis of information gathered during preexperimental interviews conducted during 1970 in Seattle and during late 1971 and early 1972 in Denver. The interviews were from house-to-house canvasses of lower income areas in the two cities to identify households eligible for the experiment.

Participation in the experiment was limited to: (1) families who are likely to be eligible for a national program if one is initiated, and (2) families whose responses would be particularly important in forming policy. The eligibility requirements were:

- The family unit had to contain at least two members, consisting either of a husband and wife or of an adult and a dependent child. These groups were considered to be the most likely targets of a national income maintenance program.
- The male head of a two-parent family or the head of a one-parent family had to be at least 18 but not more than 58 years of age. This restriction was based on the a priori assumption that the time horizons (and hence experimental response) of heads between 18 and 58 years of age would differ significantly from those older and younger. Analyses of families with older or younger heads was therefore beyond the scope of this experiment.
- Earnings of the family in 1970 had to be less than \$9,000 for a family of four with one working head, and less than \$11,000 for a family of four with two working heads. The maximum income for families with other than four members was obtained by using standard of living differentials. This restriction was based on the assumption that the alternatives of the negative income tax program were not sufficiently attractive to families with higher incomes to have a detectable effect on their behavior.
- The family heads could not be permanently disabled. This requirement was based on the a priori assumption that the labor supplied by disabled persons is essentially zero and thus not subject to a reduction by a negative income tax program.

Thus, the families selected are a nonrepresentative sample because (1) families with high incomes are not included, (2) few never-married adults are included, and (3) few unmarried males are included. The first restriction is not a disadvantage for assessing the effects of income maintenance, but it is a restriction from the more general perspective of understanding the dynamics of the relationship between income and marital stability. The second restriction means that we will be unable to make inferences about the effects of income maintenance on the formation of first marriages, except for the dependents of families in the experiment. The third restriction forces us to focus the analysis on the experience of females. The only unmarried males in the experiment are dependents of enrolled families, males who have ended a marriage since being enrolled as part of a family with both husband and wife present, and the few unmarried males who were enrolled with dependents. It is unlikely that these are representative of the population of unmarried males.

The next step in solving the assignment problem was to stratify the eligible families along two major dimensions having particular policy importance: (1) family type (one or two parent family), and (2) race/ethnicity (Black, Chicano, or White). The decision was made to allocate families to experimental treatments (financial plans) in such a way that 75% of the total predicted payment costs would come from the three-year program, the remaining 25% would come from the five-year program, and the total predicted payments would be equal for the three different racial groups. Payments were predicted on the basis of family type, race, normal earnings of the family, and the generosity of the financial plan.[20]

An important consequence of the assignment model is that families varying in type, race, and normal earnings were not randomly assigned to different financial plans. In particular, financial families (those on a financial plan) differ in a nonrandom way from control families (those not on a financial plan). For this reason, the effects of the income maintenance experiment cannot be accurately assessed through direct comparison of control families with financial families, but must be analyzed through multivariate techniques that take into account the stratification of the sample resulting from the assignment model.

The final assignment model required a total of 5,202 families, distributed as follows:

	Black	White	Chicano
Seattle	1,012	1,156	--
Denver	1,012	1,156	866

Roughly 60% of the total number of families have two parents.

Each family was given a preenrollment screening interview, was interviewed again at the time of enrollment in the program, and was reinterviewed regularly at approximately four-month intervals throughout the experiment. In each interview, we collected information on family composition and detailed economic data. Interviews also contained questions on other topics that varied from one time to the next. More complete descriptions of the design of the experiment, the sample, and data collection may be found in other research reports.[21,22]

IV VARIABLES

In this section we describe the variables used in the analysis. We specify our unit of analysis; discuss our definitions of marriage, divorce, and remarriage; and describe the explanatory variables that are used.

A. Units of Analysis

For many reasons, we analyze the marital status changes of women who were originally enrolled as heads of families with one or two heads. About 60% of the women studied were married at the time of enrollment. Because we have chosen to examine the stability of marriages formed during the experiment, as well as marriages existing at the time of enrollment, persons, not families, must be the units of analysis. When heads change their marital status, we cannot continue to study the family's response to income maintenance. We can, however, continue to study the response of individual family members. We have used as units of analysis only persons originally enrolled as heads of primary families, because we lack data on heads of other types of families (e.g., secondary families). We have not used the men who were originally enrolled as heads of primary families as units of analysis for two main reasons. Since almost all originally enrolled male heads were married at enrollment, data on the original marriages of the originally married women provide essentially the same information as data on the men. In addition, it is very difficult to gather data on enough cases to study the stability of families with an unmarried male head because few unmarried males were initially enrolled and because males whose marriages dissolve have a high rate of attrition from the experiment.

B. Defining Marriage, Dissolution, and Remarriage

Following the SIME/DIME Rules of Operation, we consider a marital relationship to be any permanent relationship between a man and a woman involving both coresidence and pooling of resources. The key defining element is intended or presumed permanence: we exclude relationships that are intended to be casual or temporary. The definition includes both legal marriages and consensual unions.

The fact that we include both legal and nonlegal marriages means that the events of dissolution and formation that we enumerate are different from those that appear in official reports. If the rate of dissolving and forming nonlegal unions is different than for legal marriages the rates of dissolution and formation that we record will differ from those given by vital statistics. For this reason, we make no attempt to compare our rates with those of any official source.

Including both legal and nonlegal marriages offers substantial advantages. From a policy perspective, what matters, presumably, is the pooling of resources and intended permanence. As long as two people pool their resources so that their subsidy differs from that calculated for each separately, it should not matter whether or not they are legally married. Moreover, dissolution of a long-term consensual union would appear to involve substantial emotional and financial costs to those involved.

Our design suffers a weakness with respect to possible differences in rates of formation and dissolution of marriages between legal and consensual unions. We cannot distinguish unambiguously between the two types of unions. Since all we know is that the couple is "married" (by our definition), we cannot control for any differences between the two states in analyzing dissolution and remarriages. Given this situation, most marriages observed at the beginning of the experiment were of a duration long enough to rule out casual cohabitations (see Table 4-1). Only 3% of the marriages in Seattle and 4% in Denver were less than a year in duration at the time of enrollment.

Table 4-1

MARRIAGE DURATIONS AT ENROLLMENT

	Duration in Years							Number of Marriages
	0-1	1-2	2-3	3-5	5-10	10-20	20+	
Seattle								
Percent of enrollment marriages	3.2	8.2	8.6	14.1	23.4	24.4	17.1	802
Denver								
Percent of enrollment marriages	4.2	10.0	10.3	15.4	25.6	23.7	10.7	1,568

Marriage and consensual unions are handled differently during the experiment. When an enrolled adult legally marries, the new spouse becomes eligible for the experimental treatment immediately. For members of consensual unions to become eligible, the couple must sign an affidavit declaring the intended permanence of the relationship, and the new member must wait three months before receiving the experimental treatment. Given these procedures, we are reasonably sure that we have not included casual liaisons in our enumeration of marriages.

We record marital status changes from the periodic interviews. Each periodic interview is preceded with the list of all family members present at the last interview (along with identification number, date of birth, and relationship code). Our interviewers record and date all changes--both additions and deletions. For each change, those involved are asked to state whether or not the change will be permanent. The dissolutions and remarriages we analyze are those changes of marital status that are reported as permanent. Since we are following enrolled female heads, we use their reports of permanence.* So for our purposes, a dissolution occurs when a husband and wife separate and the wife reports the change will be permanent. A remarriage is defined analogously.

C. Reconciliations

Not surprisingly, forecasts concerning permanence of marital separations contain considerable error. In Table 4-2 we show the frequency with which dissolutions are followed by reconciliation (remarriage to the same spouse without any intervening marriage). We cannot calculate the frequency of reconciliation for marriages that are dissolved late in the 18-month observation period, since reconciliations that follow within a reasonable time will lie outside the observation period. Therefore, we calculate the frequency with which marriages that dissolve in the first four experimental quarters are followed by a reconciliation within six months and the probability that a dissolution in the first two quarters will be followed by a reconciliation within 12 months. As we see in Table 4-2, rates of reconciliation within six months range from 7.3% to 12.3%. Most of the dissolutions we record do not result in reconciliation within a six-month period. If we employ a 12-month maximum separation period, the conclusion is not much altered.

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*Since we also attempt to follow the separated male heads we also obtain marital status change reports from them. In cases in which the female head drops out of the experiment but the male head remains, we rely on male head responses to date any first marital status change.

Table 4-2

PERCENTAGE OF DISSOLUTIONS ENDING IN RECONCILIATION BY RACE AND EXPERIMENTAL QUARTER FOR FEMALES MARRIED AT ENROLLMENT

	Experimental Quarter				Total for First Four Quarters
	1	2	3	4	
Blacks					
Number of dissolutions	65	24	32	20	141
Percent reconciling					
Within 6 months	7.7%	8.3%	6.2%	15.0%	8.5%
Within 12 months	12.3	8.3	--	--	--
Whites					
Number of dissolutions	45	29	23	27	124
Percent reconciling					
Within 6 months	2.2%	17.2%	13.0%	0 %	7.3%
Within 12 months	6.7	20.7	--	--	--
Chicanas					
Number of dissolutions	24	16	11	14	65
Percent reconciling					
Within 6 months	4.2%	12.5%	27.3%	14.3%	12.3%
Within 12 months	8.3	25.0	--	--	--

Using our procedures, each reconciliation is recorded as a remarriage (and the initial separation as a dissolution). Since some of the durations between initial separation and reconciliation are brief, the reader may question whether or not we are making overly fine distinctions. Our procedures give somewhat inflated rates of movement into and out of marriage and not all of the events we record have equal policy significance, but we have found no alternative procedure that overcomes this problem without incurring even greater costs.

In Appendix A we consider alternatives for handling reconciliations and their consequences. We also estimate the impact of our treatment of reconciliations on our estimates of marital status change. Our rates are inflated at most by 33%; the modal effects are only about 20%. Given the disadvantages of alternatives we continue to treat reconciliations as remarriages in this analysis.

D. Explanatory Variables Included in the Analysis

In the analytical models used in this report, a measure of change in marital status is hypothesized to depend upon three functions: a background function $Z(\cdot)$, an experimental function $X(\cdot)$, and an interaction function $W(\cdot)$.[23]

The background function $Z(\cdot)$ is a linear combination of variables determined before starting the experiment. Since assignment of experimental treatment was not random, the background function includes the assignment variables. In addition, the background function includes other variables (e.g., age and education) that theory and previous research have suggested are determinants of marital stability. These variables are included primarily to improve the statistical efficiency of estimated effects of experimental treatments. The experimental treatment function $X(\cdot)$ is a linear function of treatment variables, e.g., support levels. The interaction function $W(\cdot)$ is a linear combination of interactions among financial treatment variables and background variables. The interaction function permits us to measure the differential response to experimental treatments of individuals with different characteristics.

The variables contained in the background and experimental functions are described in detail below.

1. Background Variables

Background variables refer to social and economic characteristics of the sample. These variables fall into two categories: variables that were used in the assignment process, and variables that are related to marital instability according to previous theory and research.

a. Assignment Variables

The first assignment variable describes race-ethnicity. There are three race-ethnic groups: Blacks, Whites, and Chicanas. Efforts were made to exclude all other minority groups to provide relatively homogenous groups for the study. The Denver sample contains approximately equal proportions of all three races, while the Seattle sample contains roughly equal numbers of Blacks and Whites. Our analyses control for this assignment variable by estimating separation equations for each race.

The family's normal income, which is the expected income of the family in the year before the experiment, is described by a series of dummy variables. (The expected income is based on normal family circumstances and normal regional conditions of the economy and employment situations.) Income includes all money and in-kind earnings from paid work and family businesses, and income from property; it omits transfer payments from public agencies or private individuals. For use in the assignment process, family incomes were adjusted by a family size index and grouped to give families with roughly equal income equivalent income levels. The seven earnings level categories are:

Earnings Level	Income*
E1	Less than $1,000
E2	$1,000 to $2,999
E3	3,000 to 4,999
E4	5,000 to 6,999
E5	7,000 to 8,999
E6	9,000 to 10,999
E7	11,000 to 12,999

Because of the income restrictions for eligibility (see Section III), the two highest earnings level categories, E6 and E7, contain only families with two working heads, and hence contain no families headed by females who were unmarried at the time of enrollment.† A residual category (E0) was created to capture families not previously assigned to an earnings level. This mainly consists of families that experienced changes (such as marriage or divorce) between the preenrollment interview and enrollment.

b. Other Background Variables

Five background variables were selected to examine effects of important social characteristics on marital stability. Female education in years was defined as the last year of formal education completed by the female head before enrollment. AFDC is a dummy variable that equals one if AFDC was received by an enrolled family during 1970-1971 for Denver or during 1969-1970 for Seattle; otherwise the variable equals zero.

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*Converted by a family size index, which calculates the family's equivalent income for a family size of four members.

†In our analysis of remarriage in Section VII, the single women who appear with normal earnings levels, E6 or E7, were enrolled while married and had a marital dissolution.

Age is based on the age of the female head of household at enrollment. In the pooled linear probability model, the age variable is the age at the start of the period under observation. In the instantaneous rate model, age refers to age at the time of enrollment. The number of children is defined by dummy variables (each having the values of 0 or 1), according to the ages of the children. The two children variables are: children in the home who are ages zero to five years, and children in the home that are ages zero through nine years.

The female wage variable was observed wage in dollars per hour for those females employed during the preexperimental period. For those women unemployed before enrollment, the wage variable is a value predicted in the labor supply study.[23]

The sample sites were chosen for comparability. The major differences between sites are (1) a higher rate of unemployment in Seattle, matched by a close approximation to the national average in Denver, and (2) the addition of the Chicano subpopulation in Denver. In the merged sample, the site is represented by a dummy variable that equales one if the site is Denver and zero if the site is Seattle.

2. Experimental Variables

We have previously described the three components of the experimental treatment: financial plan, manpower training level, and length of time on the experiment. Each component is represented separately by a set of dummy variables that are coded one if the woman receives that particular treatment and otherwise coded zero. Financial treatment is represented either by a series of dummy variables representing a combination of support and tax rate (see Table 3-1), or by separate dummy variables for each support level and tax rate. In the latter case, the support level variables refer to the 50% constant tax rate when the tax-rate variables are included.

3. Time Variables

Two types of time variables are used in analyses with the pooled linear probability model. The first is a dummy variable that equals one if the first day of the observation period fell in either the second or third quarter of the year (April through September) and otherwise equals zero. The second set of time variables is a series of dummy variables that indicate whether or not the observation period fell in the first experimental six months, the second experimental six months, or the third experimental six months.

E. Explanatory Variables Excluded from the Impact Analysis

We have excluded from the analyses we present in this report explanatory variables that depend on choices made by individuals after the start of the experiment. The main reason for excluding such endogenous variables is to permit us to detect the total experimental impact on marital status change. Endogenous variables that are hypothesized to affect marital status change but have been excluded from our analyses are variables that describe:

- The labor supply choices of family members during the experimental period.
- Actual income maintenance payments received (which depend on labor supply choices).
- Characteristics of husbands of married women (which probably depend on experimental treatments in the case of marriages formed during the experiment).
- The duration in the married or unmarried state.

Because these endogenous variables are excluded from our analyses, we are only able to estimate a total impact of financial treatments. In particular, we are unable to estimate the separate income and independence effects discussed in Section II.

Since marital status at the time of enrollment is used in assigning families to experimental treatments, there is good reason to include this variable in our analyses. At the same time, we must consider this variable to be endogenous. Since we analyze dissolution and remarriage separately, variation in enrollment marital status is possible only because a woman has changed marital status during the experiment. For women who are not in their original marital status, the value of this variable depends on a choice made during the experiment. Thus, we have excluded this variable from our analyses.

V ANALYTICAL MODELS OF MARITAL STATUS CHANGE

We must find an appropriate analytical model* of dissolution and remarriage if we are to have confidence in our assessment of the impact of income maintenance treatments on marital stability. There is no obvious solution to this problem. Each income maintenance experiment has been analyzed with a different model.[1,13,14,15]

The choice of analytical models is difficult in the case of changes in marital status because most people change their marital status only a few times within their lives; therefore, a relatively small percentage of people enrolled will change their marital status during the Seattle and Denver Income Maintenance Experiments. Consequently, to detect the impact of income maintenance on marital status changes, either the impact must be very large or the analytical model must be one that efficiently uses available data.

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*The term analytical model refers to the set of basic assumptions about the form of relationships between a measure of marital status change and other explanatory variables.

A. Desirable Features

An analytical model of marital status change should have the following properties:*

(1) The model should be based upon valid measures of change in marital status. In particular, it should distinguish between continuous marriage to one spouse and several marriages. For example, the marital stability of a woman who is continuously married to one man for 12 of the 18 experimental months studied should not be treated as identical to the marital stability of a woman who is married for a total of 12 months on three different occasions.

(2) The model should permit the effects of experimental treatments to be estimated, while controlling other background variables. As indicated in Section III, certain background variables, caused by nonrandom assignment to experimental treatments, <u>must</u> be controlled to interpret correctly any findings of the impact of income maintenance treatments on <u>any</u> outcome of interest. It is also highly desirable to include other background characteristics as control variables to improve the efficiency of the estimates of experimental effects.

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*Since the present study is not intended to untangle the joint impact of income maintenance on marital stability and on labor supply, results reported in this document depend on analysis of reduced-form equations and exclude explanatory variables whose values were not determined after enrollment. The objective of determining total impact also affects the analytical models used; in particular, simultaneous equations models were considered as inappropriate for the impact analysis and are not discussed in this document.

(3) The model should permit a test of change over time in the impact of income maintenance on marital status change. It has been argued previously[19] that responses to experimental time will vary with length of exposure to the experimental treatment. People need time to find the experimental regime believable and to acquire a practical understanding of its consequences for their lives. In addition, previous research leads us to expect marital status changes to depend on the person's age, the duration of the marriage, and the season of the year.

(4) The model should take advantage of the continuous nature of the available information on marital status.

(5) The model should permit the use of information on people who drop out for as much of the experiment as such information is available. For example, if a woman drops from the experiment after 12 months, but her marital status is known until that time, the information that <u>is</u> available should be used in the analysis. This makes efficient use of resources and minimizes the danger of mistaken conclusions resulting from nonrandom differences between people who drop and those who do not.

(6) The model should be one in which effects of experimental treatments (and also of other variables) can be estimated by methods that are unbiased and efficient.

(7) The model should permit conclusions about experimental impact that are relatively insensitive to incorrect specification of the relationship between marital status change and causal variables. The possibility of erroneous conclusions caused by misspecification has been greatly

reduced by the experimental design, which produces approximate statistical independence between experimental treatments and background variables, except for those variables used as a basis for assignment to treatments. It is also desirable, however, that conclusions based on the analytical model do not depend heavily on an assumption that the measure of marital status change has a particular probability distribution.

B. Alternative Models

The number of alternative models of marital status change that we can review here is limited. However, we consider those models that were used to analyze the impact of income maintenance on change in marital status in the other Income Maintenance Experiments.* We also describe the two analytical models that we use in this report, as well as a few others that we considered but eventually rejected. In the following discussion we mention only the main disadvantages of a rejected model; however, in Table 5-1 we summarize the degree to which each model possesses each desirable feature.

The first model that we considered is the one used by Knudsen et al.[13] in their report of results of the New Jersey Income Maintenance Experiment. The dependent variable in this analytical model is the logarithm of the ratio of (1) the proportion of transitions from a given family type to some other family type within the specified period of an experiment, and (2) to the proportion of families of the first type who do not change

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*We concentrate on the models used in the New Jersey and Gary Experiments. Because few changes in marital status occurred in the Rural Experiment, Middleton et al.[14] report only raw rates for each treatment.

Table 5-1

DESIRABLE FEATURES OF AN ANALYTIC MODEL OF MARITAL STABILITY

Type of Model	Uses All Marital Status Changes	Controls for Background Variables	Permits Dynamic Inferences	Utilizes Timing of Events	Utilizes Partial Observations	Possesses Desirable Statistical Properties	Insensitive to Incorrect Specification of Probability Distribution
Logit model of Knudsen et al.	Yes, in practice	Very limited	No	No	No	Yes	Yes(?)
Multiple regression model with y = 1 if married at time t, 0 otherwise	No	Yes	No	No	No	No	Yes
Multiple regression model with y = proportion of days married in a period	No	Yes	No	Yes	Yes	Yes	Yes
Multiple regression model with y = 1 if marital status changes during a period = 0 otherwise	Yes	Yes	No	Yes	No	No	Yes
Multiple regression model with y = 1 if marital status changes during a period = 0 otherwise (Observations pooled over time)	Yes	Yes	Yes	Yes	Yes	No	Yes
Loglinear model of the instantaneous rate of a change in marital status	Yes	Yes	Yes	Yes	Yes	Yes	Unknown

type within the same period. In this model, the dependent variable is hypothesized to be a linear function of a set of dichotomous variables describing experimental treatments and background variables.

Although this model might give reasonably good estimates of impacts on change in marital status, it lacks a number of other desirable features. In particular, given the sample sizes available in SIME/DIME, the model permits only a small number of background variables to be controlled.* Moreover, it does not enable us to measure differential response to experimental treatment over time. Consequently, this model is not appropriate for our analysis.

All but one of the other models that we considered were multiple regression models; they differ primarily in the specification of the dependent variable. In each multiple regression model, the dependent variable is hypothesized to be a linear combination of other explanatory variables. The explanatory variables usually include background variables $Z(\cdot)$ and experimental treatment variables $X(\cdot)$, and may also include interactions between background and treatment variables $W(\cdot)$. Thus, the general form of these models, which closely resembles that used by Kurz et al.[23] is for a measure of marital status change y to have the following relationship to the explanatory variables:

$$y = Z(\cdot) + X(\cdot) + W(\cdot) \tag{1}$$

The variables contained within these functions are described in Section IV.

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*Using data from the New Jersey Experiment, whose sample size was considerably smaller than in SIME/DIME, Knudsen et al.[13] could partition effects among only four variables at a time. Three of these variables were always specified as experimental treatment variables; the fourth varied among different background characteristics.

The first multiple regression model that we review is one in which the dependent variable equals one if a woman is married at some point in the experiment (e.g., 18 months after enrollment) and otherwise equals zero. This is essentially the model used by Henry[15] in reporting preliminary results of the impact of the Gary Income Maintenance Experiment on marital stability; however, Henry analyzed only data on families who were married at enrollment.*

We have rejected this model primarily because it treats multiple changes (e.g., separation followed by a remarriage) identically with no change in status. In addition, this model does not permit us to test for a dynamic response to the experimental treatment or to make full use of our data, since it ignores all information on marital status between enrollment and the selected point in experimental time.

A second possible multiple regression model has as its dependent variable the proportion of time within some given period in the experiment when a woman is married. This model is preferable to the first regression model because it uses the continuous information on marital status available to us. However, like the first model, it does not distinguish between a woman who is married to several men sequentially and another woman who is married the same length of time to one man. In addition to its failure to deal directly with multiple marital status changes, this model does not allow a test for different responses to experimental treatments over time.

The third alternative is to estimate separate multiple regression equations for married and single women with a dependent variable that equals one if a woman changes her marital status before the end of the

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*Limitations on the data that were available prevented the use in the preliminary analysis of the Gary Experiment of alternative models with more desirable features than the one actually used.

observation period and otherwise equals zero. This model was used in an earlier preliminary report on the impact of income maintenance on marital stability during the first 18 months of the Denver experiment.[1]

This model has a number of desirable features: it deals directly with marital status change, it is multivariate, and it rests on knowledge of women's continuous marital histories. On the other hand, this model cannot represent dynamic responses; it does not allow us to use the partial histories of women who drop from the experiment but are not observed changing their marital status; it does not use all available data since it ignores changes in marital status subsequent to the first change. Consequently, we have rejected this model for the present report.

C. Models Used in This Research

We employed two models in analyzing impacts of income maintenance. In this section, we consider the properties of these models in more detail.

1. A Pooled Linear Probability Model

For each period and for each marital status we defined a model of the following form:

$$\underline{y}_t = \underline{Z}_t\underline{\alpha} + \underline{X}_t\underline{\beta}_t + \underline{W}_t\underline{\gamma}_t + \underline{\varepsilon}_t \qquad (2)$$

where

$$\underline{y}_t = \left(y_{1t},\ldots y_{N_t t}\right),\ y_{it} = \begin{cases} 1 \text{ if spell i ends in a marital status change during period t} \\ 0 \text{ otherwise} \end{cases}$$

$t = 1,2,\ldots,T$

T denotes the number of time periods

N_t denotes the number of distinct spells in the particular marital status that fall in period t

$\underline{Z}_t$ denotes a matrix of observations on background variables $(N_t \times K)$

$\underline{X}_t$ denotes a matrix of experimental treatments $(N_t \times J)$

$\underline{W}_t$ denotes a matrix of interactions $(N_t \times L)$

$\underline{\alpha}$ is a $(K \times 1)$ vector of coefficients

$\underline{\beta}_t$ is a $(J \times 1)$ vector of coefficients

$\underline{\gamma}_t$ is a $(L \times 1)$ vector of coefficients

$\underline{\varepsilon}_t$ is a $(N_t \times 1)$ vector of disturbances.

The observations for which Equation (2) is defined are spells of marriage (for the dissolution analysis) or of singleness (for the remarriage analysis) within the period. In one experimental period, an individual may have more than one spell of marriage or of singleness. For example, consider a woman with the marital history shown in Figure 5-1. She was married for the first eight months on the experiment, single for three months, and then married for the remainder of the time of observation. In the first six-month period, the woman whose history is shown in Figure 5-1 provides one observation for the dissolution equation. In the second six-month period she provides three observations: two observations for the dissolution equation (one for each spell of marriage in the period) and one for the remarriage equation. In the third six-month period she supplies one observation to the dissolution equation. The number of observations provided by each woman in a period is the number of spells observed. Thus, N_t, the total number of observations for a period is the sum of the observations contributed by each woman.

The fact that a woman may enter a state after the beginning of a period introduces a slight complication. Presumably a woman who enters a state late in the period should have a lower probability of marital

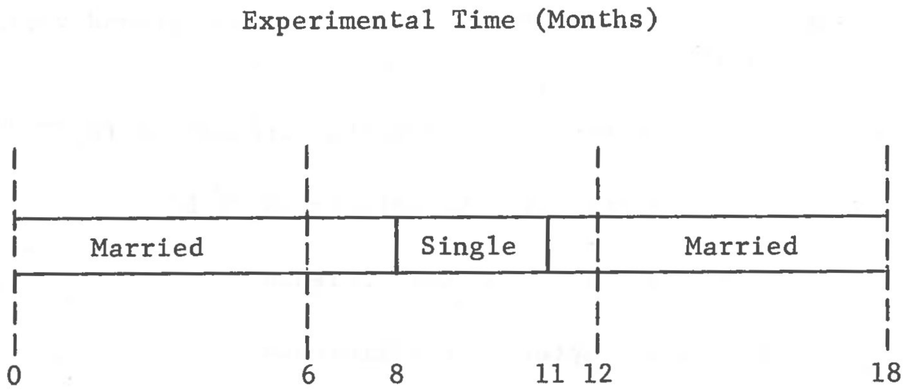

FIGURE 5-1 HYPOTHETICAL MARITAL HISTORY DIVIDED INTO THREE OBSERVATIONAL PERIODS

status change in that period than an otherwise identical woman who is exposed to the risk of change for the entire period. Consequently, we must adjust for the length of exposure. We assume that the probability of a change is proportional to the period of exposure:

$$\underline{y}_t = \underline{p}_t(\underline{Z}_t\underline{\alpha} + \underline{X}_t\underline{\beta}_t + \underline{W}_t\underline{\gamma}_t + \underline{\varepsilon}_t) \tag{3}$$

where

$$\underline{p}_t = \begin{bmatrix} p_{1t} & & & 0 \\ & p_{2t} & & \\ & & \ddots & \\ 0 & & & p_{N_t t} \end{bmatrix}, \text{ and}$$

where p_{it} is equal to one minus the fraction of the period that had elapsed prior to individual i's entry into the state. In the second period for the woman in Figure 5-1, for example, p is 1 for the first spell of marriage, two-thirds for the period of singleness, and one-sixth for the second spell of marriage in the second six-month period.

All linear probability models that we estimated have the form of Equation (3).

The model defined in Equation (3) can be estimated for each of the observed six-month periods. However, given that we expect the background variables to have identical effects in each period, statistical efficiency is gained by estimating the following pooled model:

$$\underline{y} = P\ (\underline{ZA} + \underline{XB} + \underline{W\Gamma} + \underline{\varepsilon}) \tag{4}$$

where

$$\underline{y} = (\underline{y}_1, \ldots, \underline{y}_T)$$

$$\underline{P} = \begin{bmatrix} \underline{P}_1 & & & 0 \\ & \underline{P}_2 & & \\ & & \ddots & \\ 0 & & & \underline{P}_T \end{bmatrix}$$

$$\underline{Z} = (\underline{Z}_1, \ldots, \underline{Z}_T)^*$$

$$\underline{X} = (\underline{X}_1, \ldots, \underline{X}_T)$$

$$\underline{W} = (\underline{W}_1, \ldots, \underline{W}_T)$$

$$\underline{A} = (\underline{\alpha}_1, \ldots, \underline{\alpha}_T) \text{ (we assume } \alpha_1 = \alpha_2 = \cdots = \alpha_T\text{)}$$

$$\underline{B} = (\underline{\beta}_1, \ldots, \underline{\beta}_T)$$

$$\underline{\Gamma} = (\underline{\gamma}_1, \ldots, \underline{\gamma}_T)$$

$$\underline{\varepsilon} = (\underline{\varepsilon}_1, \ldots, \underline{\varepsilon}_T) \quad .$$

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*We allow the matrices $\underline{Z}_t$ to vary from period to period to allow for changes in variables such as age.

The model in Equation (4) is the pooled linear probability model that we employ in this document.

The main disadvantages of the model in Equation (4) are its less than optimal statistical properties. Two problems apply to linear probability models, and one problem applies to pooled models. First, predicted values of a dependent variable--which may be interpreted as the conditional probability of a marital status change during an experimental half--may be less than zero or greater than one. In SIME/DIME data the mean proportion of women changing marital status in an experimental half is relatively close to zero (it is usually between 0.05 and 0.10), therefore negative predicted probabilities are especially likely to occur. Thus, even though the signs and relative magnitudes of regression coefficients should be meaningful, interpretation of the predicted probabilities is difficult. Second, though ordinary least squares (OLS) estimators of partial regression coefficients in a linear probability model are consistent and unbiased (assuming proper specification of the model), models with dummy dependent variables have heteroscedastic disturbances. This means that OLS estimators are not efficient (i.e., do not have minimum variance).* In addition, in a heteroscedastic model, OLS estimators of the standard errors of the regression coefficients are biased. The direction of the bias is

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*Goldberger[24] has suggested a two-stage modified generalized least squares procedure to alleviate the problem of heteroscedasticity. There are several reasons for not using this procedure. The first stage of the procedure consists of using OLS to estimate the variance of the error term. However, negative variances are often obtained because of estimated probabilities greater than one or less than zero. Since negative variances are impossible in reality, it is not clear how to use the negative estimates of variances. Other reasons for not using the Goldberger procedure are based on our actual usage of the procedure in preliminary analyses: results were nonsensical and more than doubled the costs of analysis. Consequently, we have concluded that the practical problems associated with this procedure outweigh its theoretical advantages.

not clear, therefore this problem cannot be mitigated by altering the significance level used in interpreting the results. Third, pooling of observations over periods complicates the structure of the disturbances. If the disturbance term has components (e.g., omitted personal characteristics) that affect the probability of marital status change in each period and are relatively stable over time, disturbances will be correlated for the same individual from different periods. Therefore, the variance-covariance matrix of disturbances of Equation (4) will not be diagonal. This is another reason why ordinary least squares estimators will be inefficient. Also, the estimators of standard errors of slopes will tend to be biased towards zero.[25] Since we have not corrected for autocorrelation, we expect that our estimated standard errors in the linear probability models are lower than the true standard errors.

2. A Log-Linear Model of the Instantaneous Rate of a Marital Status Change

Most women change their marital status relatively infrequently. This suggests that the processes of marital formation and dissolution be approached mathematically in a way similar to other rare events.[26] One way to specify a stochastic model of a rare event is in terms of the instantaneous rate at which the event occurs at time t when the last event for the person occurred at time t': $f(t|t')$.* We specify separate models for dissolution and remarriage.

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*In our case both events are marital status changes. If the event at t' is a marital dissolution, then the event at t is a marital formation. On the other hand, if the event at t' is a marital formation, then the event at t is a marital dissolution. Thus we are assuming, and it is in practice true over 99% of the time, that women do not go immediately from marriage to one spouse to marriage to another spouse without an intervening period of singleness. We are also assuming that we are studying <u>re</u>marriage, and not a woman's first marriage, which is reasonable in our sample.

By definition $r(t|t')$ is the probability density of dissolution at time t, given that a dissolution has not occurred since t', the date of marriage. The following describes a model for dissolution: The remarriage model is analogous.

$$r(t|t') = \frac{\frac{dF(t|t')}{dt}}{[1 - F(t|t')]} \tag{5}$$

where

$$F(t|t') = \Pr(\text{a woman whose marriage began at time } t' \text{ dissolves her marriage before } t) \tag{6}$$

and

$$F(t|t') = 0 \text{ at } t \leq t'. \tag{7}$$

Essentially $F(t|t')$ is a continuous time analogue to the dependent variable y in our pooled linear probability model. Note, however, that the rate $r(t|t')$ cannot be negative because it is proportional to the ratio of a probability density to a probability, both of which are always positive. For a similar reason $r(t|t')$ <u>can</u> exceed one. Thus, $r(t|t')$ is not a probability.

Equation (5) is a differential equation with the following solution:

$$F(t|t') = 1 - \exp\left[- \int_{t'}^{t} r(u|t')du\right] \tag{8}$$

Equation (8) shows that specifying the rate r serves to specify the probability that a woman dissolves her marriage before time t, given that her marriage began at t'. In general, the rate may depend not only on time (including age, duration, and calendar time), but also on exogenous variables. It is necessary, however, to specify the functional form of the dependence of the rate on time and on exogenous variables.

The results in this report are based on a model in which the rate depends on exogenous variables but not on time.* The particular form of the relationship between the rate and exogenous variables that we have chosen to use is a log-linear one:

$$\ln r = A\ Z(\cdot) + B\ X(\cdot) + C\ W(\cdot) \tag{9}$$

or

$$r = \exp[A\ Z(\cdot) + B\ X(\cdot) + C\ W(\cdot)] \tag{10}$$

where $Z(\cdot)$, $X(\cdot)$, and $W(\cdot)$ still refer to background, experimental, and interaction variables, respectively.

This particular specification of the rate has three advantages. First, in contrast to a linear specification, it prevents the possibility of predicting negative rates, when in reality rates must be nonnegative. Second, it is consistent with arguments that variables may have multiplicative effects on the processes of making and breaking marital unions. Third, in preliminary work using both a linear and a log-linear rate model, we consistently obtained results indicating that a log-linear specification fits our data more satisfactorily than a linear one, given a particular set of explanatory variables.

Note that, as in the pooled linear probability model, the basic unit of analysis is a spell of either singleness or marriage. Each woman

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*A subsequent report will extend this to a more realistic model in which the rate also depends on age, duration, and experimental time. The time-independent rate model has two advantages for the present analysis. First, we are missing data on the duration of marriage or singleness at enrollment for many of the female heads. This loss of information is not crucial in a time-independent rate model, but necessitates dropping data on the first spell of these women in a time-dependent rate model. Second, t' has elements of endogeneity in the case of spells after the one existing at the time of enrollment. The impact study requires omission of endogenous variables. (For a further discussion see Appendix B.)

may have multiple spells of both types. Under certain assumptions, pooling of information on multiple spells should not affect inferences about effects of variables on the two processes. (See Appendix B for a discussion.)

a. Estimation and Test Statistics

To test the proposed model, one must be able to estimate the coefficients of variables expected to influence the rate of a marital status change. It is also desirable to have a test statistic that permits one to decide whether a coefficient is further from some value (e.g., zero) than would be predicted solely on the basis of chance.

We have estimated coefficients in the log-linear rate model by the method of maximum likelihood. The likelihood, L, is defined as the probability density of the joint set of observations; assuming independent observations, L is the product of the probability densities predicted by the model. The maximum likelihood estimates are those values of the coefficients that make the observations <u>in the data</u> "most probable," assuming the model is true. Maximum likelihood estimators are asymptotically consistent, efficient, and normally distributed under fairly weak regularity conditions on the distribution function specified by a model.[27] Consequently, given a sufficiently large number of observed spells, the method of maximum likelihood should give "good" estimates of the coefficients of the proposed model.

Furthermore, we can estimate the standard errors of the coefficients and test the hypothesis that individual coefficients are zero. This is possible because the inverse of the matrix of second derivatives of the natural logarithm of L with respect to the coefficients gives an estimate of the lower bound of the variance-covariance matrix of the coefficients.[27] In addition, the likelihood ratio statistic* is easily computed from L and can be used to test a model with a given number of variables against the same model with one or more constraints. The alternative model in which the coefficients of all experimental variables are constrained to be zero is of special interest.

When available data not only contain information on whether a marital status change has occurred by some time, τ, but also on the exact date of the change for those who had a change before time, τ, the form of the likelihood function is as follows:[28]

$$L = \prod_{j=1}^{N} \left[\frac{dF(t_j|t'_j,V_j)}{dt}\right]^{d_j} \left[1 - F(\tau_j|t'_j,V_j)\right]^{(1-d_j)} \tag{11}$$

where τ_j is the last date that the woman was observed; d_j equals one if the jth spell ended before τ_j and otherwise equals zero, t'_j and t_j are the starting and ending dates of spell j, respectively; V_j includes the values of Z(•), X(•), and W(•) for spell j; and N is the total number of spells of either marriage (in the dissolution study) or singleness (in the marriage study). A function that gives identical estimates of coefficients and is computationally easier to estimate is the logarithm of the likelihood function. For the time-independent rate model, the logarithm L reduces to:

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*Let L_1 represent the likelihood function for a model and L_0 represent the likelihood function for this model when there are k additional constraints (usually k coefficients constrained to be zero), respectively. The likehood ratio λ is defined as the maximum of L_0 divided by the maximum of L_1. It can be shown that $-2 \log_e \lambda$ has a chi-square distribution with k degrees of freedom.[26]

$$\ln L = \sum_{j=1}^{N} d_j \left\{ \ln r(V)_j + \ln \left[1 - F(t_j | V_j)\right]\right\}$$

$$+ (1 - d_j) \ln \left[1 - F(T_j | V_j\right] \quad (12)$$

Equation (12) has a particularly simple form that is easy to maximize in the case of the log-linear specification that we have used.

b. Attrition

Equation (11) permits us to use all available information on people who drop from the experiment. For women who have dropped from the experiment within the first 18 months and have not had a marital status change, τ is the length of time that we have information on them. This assumes, of course, that the processes are identical for marital formation and dissolution describing women who drop out and women who stay on the experiment; in Section VIII, we investigate the sensitivity of our results to several alternative assumptions about marital dissolution and formation among those who drop from the experiment.

c. Advantages and Disadvantages of the Instantaneous Rate Model

As Table 5-1 indicates, the instantaneous rate model has several desirable features: it uses information on all marital status changes and the time of these changes; it is multivariate, permitting a variety of background variables to be controlled in the same equation. The method of estimation we have used has desirable asymptotic statistical properties. Moreover, a small simulation study[29] indicates that with a sample size similar to ours, estimated coefficients almost always fall within one estimated standard deviation of the true coefficients. The coefficients are relatively unaffected (except for the constant term) by simple types of model misspecification, in particular, omission of a variable that affects the process but is uncorrelated with included variables. Since the experimental treatments are only slightly correlated with background variables (except for the assignment variables) because of the experimental design, the results of the simulation study give confidence in the effects of experimental treatments as estimated for the instantaneous rate model.

The primary disadvantage of the instantaneous rate model is that it requires specification of a particular probability distribution function. It is not known to what extent estimated effects of variables are sensitive to the particular probability distribution that is selected. In Keeley's simulation study, he found that estimated effects of variables were similar in sign and statistical significance when data fitting the log-linear rate model were estimated by log-linear and linear rate models and by a linear probability model. But this information is, at best, only suggestive that the rate model may be insensitive to the particular specification selected.

VI IMPACT ON MARITAL DISSOLUTION

In our previous work on income and marital dissolution, we noted two processes by which income maintenance could affect the likelihood of dissolution. First, the cash transfers involved in income maintenance will raise levels of family resources for many families (those not over the breakeven point). If increased resources (i.e., income) decreases dissolution, such transfers will lower the probability of marital dissolution. However, the income maintenance guarantee also applies outside of marriage. As a result, income maintenance also raises the level of potential resources outside of the marriage and thereby lowers the dependence of the members on the marriage. Such an independence effect will raise the probability of marital dissolution.

More systematically, the two broad hypotheses of interest can be stated as follows:

$$D = f(R, R_W)$$

$$\frac{\partial D}{\partial R} < 0$$

$$\frac{\partial D}{\partial R_W} > 0$$

where

D denotes the probability of a marital dissolution,

R denotes the level of family resources,

R_W denotes the level of resources available to the wife outside the marriage.

Note that a couple's resources are functions of wage income, E, nonwage income, Y^N, and other unobserved characteristics of the partners and of the marriage, Z (their nonmarket productivities, for instance:

$$R = g\ E, Y^N, Z) \quad .$$

A similar expression can be written for the wife's resources if the marital union ends--both her wage and nonwage income as an unmarried woman may differ from what they were in the marriage. Her wage income will differ both because she loses the fraction of her husband's wage income that she consumes in the marriage and because her labor supply as a single woman will usually differ from what it is as a married woman. Obviously, the unobserved characteristics summarized as Z will also change since the husband's characteristics no longer contribute to her resources after she leaves the marriage.

Before considering the effects of income maintenance, we will consider the effect of an increase in nonwage income to a couple from, say, jointly held assets (given a rule that each partner will receive a portion of the nonwage income if the marriage dissolves). The jointly held assets have both income and independence effects, as is shown by the following expression:

$$\frac{\partial D}{\partial Y^N} = \underset{(-)}{\frac{\partial D}{\partial R}}\ \underset{(+)}{\frac{\partial R}{\partial Y^N}} + \underset{(+)}{\frac{\partial D}{\partial R_w}}\ \underset{(+)}{\frac{\partial R_w}{\partial Y^N}}$$

We chose to examine the effect of nonwage income because income maintenance is a nonnegative supplement to nonwage income. For families on income maintenance, total nonwage income, $\tilde{Y}^N$, is the sum of their grant and any other nonwage income, Y^N. As outlined above, the income maintenance grant is defined by a support level, S, and a tax rate, T, on on other sources of income. Consequently, for families on the income maintenance treatment, total nonwage income is a function of existing sources of nonwage income, of the experimental parameters, and of all other sources of income (which affect the size of the grant through the tax function):

$$\tilde{Y}^N = g(Y^N, E, S, T) \quad .$$

We can express the total effect of the support and tax rate as follows:

$$\underset{}{\frac{\partial D}{\partial S}} = \underset{(-)}{\frac{\partial D}{\partial R}} \; \underset{(+)}{\frac{\partial R}{\partial \tilde{Y}^N}} \; \underset{(+)}{\frac{\partial \tilde{Y}^N}{\partial S}} + \underset{(+)}{\frac{\partial D}{\partial R_w}} \; \underset{(+)}{\frac{\partial R_w}{\partial \tilde{Y}^N}} \; \underset{(+)}{\frac{\partial \tilde{Y}^N}{\partial S}}$$

$$\underset{}{\frac{\partial D}{\partial T}} = \underset{(-)}{\frac{\partial D}{\partial R}} \; \underset{(+)}{\frac{\partial R}{\partial \tilde{Y}^N}} \; \underset{(-)}{\frac{\partial \tilde{Y}^N}{\partial T}} + \underset{(+)}{\frac{\partial D}{\partial R_w}} \; \underset{(+)}{\frac{\partial R_w}{\partial \tilde{Y}^N}} \; \underset{(-)}{\frac{\partial \tilde{Y}^N}{\partial T}}$$

Obviously the same sort of indeterminacy concerning the sign of the total effect noted for nonwage income holds for the parameters of the income maintenance treatment. The total effect of the support level, S, has two parts: an income effect with a negative sign and an independence effect with a positive sign. Similarly, there are two parts to the total effect of the tax rate; however, the signs of the parts are reversed. As a result, if the income effect dominates over that range of incomes for which the income effect dominates, the total effect of the supports will be to reduce the rate of dissolution. Alternatively, if the independence effect dominates, the rate of dissolution will increase.

The situation is even more complex. Not only is the sign of the total effect indeterminate without additional information, the pattern of effects of variations in support levels or tax rates is not monotonic. If there are nonlinearities in effects, such as threshold effects in either income or independence effects, the income effect may dominate for some support levels while the independence effect dominates at other support levels.

Our ultimate goal is to model such complex effects. In a reduced-form study, such as that reported here, it is not possible to separate the effects. We seek only to discover total effects of income maintenance. The purpose of raising the complexities introduced by the operation of income and independence effects is to warn the reader that the total effects ought to be interpreted with caution. In the following sections, we report the results of our linear probability model of marital dissolution. Where the results are available we also present the results of a log-linear rate model. As we noted in Section V, the two models differ in their strengths and weaknesses. Both are presented here to increase our confidence in the robustness of our findings.

A. Race-Ethnic Interactions

We have conducted an extensive search for race-ethnic differences in response to the experiment on the Denver sample. Our results consistently show that the entire marital dissolution process differs for the three samples with respect to both the background function and the response to the experiment. More concretely, separate linear probability models for each population fit the observations significantly better than do models with race-ethnic groups merged. The results for equations similar to those reported in Section VI.D using three-month rather than six-month observation periods are typical. The F-test on the joint hypothesis that all slopes are equal across race-ethnic groups is:

$$F = 1.66 \ (81,8118 \ df) \ p < 0.01.$$

Consequently we conduct all analyses separately for the three populations.

B. Site Interactions

We have no reason to expect differences between the two sites. Tests of merging data for the two sites, comparable to those just discussed for merging race-ethnic groups, support this view. The hypothesis that all slopes are equal for the two sites (but constants differ) cannot be rejected at conventional significance levels (e.g., 0.05). The results for the model containing the background function and the expanded list of experimental treatments are typical. The F-tests on the joint hypothesis that all slopes are identical across sites are:

Blacks: $F = 1.42$ (28,2485 df) $p > 0.05$

Whites: $F = 1.22$ (29,3657 df) $p > 0.05$

Given these results, we report only analyses that merge data for Blacks and Whites in the two sites.

C. Effects of Background Variables

The background function adjusts for the nonrandom experimental assignment discussed earlier. As such, it plays no substantive role in the reduced form analysis. It is informative, however, to see whether the pattern of background effects agrees with previous research. In particular, we want to know whether or not there is evidence for income and independence effects in estimates of background variables. The relevant effects (from linear probability models that also contain the additive experimental effects) are presented in Table 6-1.

Notice first the pattern of family normal earnings at enrollment. The excluded category is family normal earnings in the range of 0 to \$999. For Black, White, and Chicana samples, dissolution rates tend to decrease as normal earnings increase. For Blacks and Whites many of these differences are significantly different from the excluded, lowest category.

Table 6-1

EFFECTS OF BACKGROUND VARIABLES ON MARITAL DISSOLUTION: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Normal earnings level						
$ 1,000-$ 2,999	-0.013	(0.034)	-0.135***	(0.033)	0.112*	(0.051)
3,000- 4,999	-0.018	(0.030)	-0.142***	(0.031)	0.124**	(0.055)
5,000- 6,999	-0.053*	(0.030)	-0.155***	(0.031)	0.102*	(0.057)
7,000- 8,999	-0.056*	(0.030)	-0.157***	(0.032)	0.102*	(0.059)
9,000- 10,999	-0.025	(0.032)	-0.162***	(0.033)	0.095	(0.060)
11,000- 12,999	-0.060	(0.056)	-0.142***	(0.055)	0.068	(0.097)
Unclassified	-0.011	(0.045)	-0.096**	(0.046)	0.160*	(0.090)
Education	-0.088**	(0.003)	-0.001	(0.002)	-0.001	(0.004)
Wage rate	0.031***	(0.010)	0.020**	(0.009)	0.051	(0.035)
Age	-0.003***	(0.0006)	-0.002***	(0.0006)	-0.002	(0.001)
April-September (0,1)	-0.002	(0.010)	-0.005	(0.009)	0.008	(0.016)
Experimental months 7-12 (0,1)	0.018	(0.013)	-0.001	(0.011)	0.015	(0.019)
Experimental months 13-18 (0,1)	0.019	(0.013)	0.012	(0.011)	0.062***	(0.019)
Denver (0,1)	0.024**	(0.011)	0.006	(0.009)	--	--
AFDC (0,1)	0.026	(0.015)	0.007	(0.014)	0.049**	(0.021)
Children aged 5 or under (0,1)	-0.005	(0.015)	0.003	(0.014)	-0.027	(0.026)
Children aged 9 or under (0,1)	-0.023	(0.017)	0.0004	(0.015)	0.019	(0.028)
Constant	0.202	(0.055)	0.196***	(0.048)	-0.120	(0.100)
Mean of dependent variable	0.066		0.047		0.064	
R^2	0.036		0.025		0.051	
F-ratio for equation	2.79***		2.82***		2.56***	
N	2445		3572		1499	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include experimental variables from Table 6-2.

For Chicanas, this excluded, lowest category has a lower rate than every other normal earning category, so that the Chicana earning level coefficients are positive rather than negative, but the probability of dissolution still tends to decrease as earning level increases.

These income effects are the net of at least one measure of the status dimension of social class: female head's education (in years). The effect of education, which is to lower dissolution rates, is consistent with the broad social class hypothesis and with previous research.

The availability of a measure of female head's wage rates provides an opportunity to examine the independence hypothesis when both education and normal family earnings are controlled. Wage rates reflect generalized earnings capacities and thus indicate a woman's ability to sustain herself outside of a marriage.* In each sample, the wage has the larger positive effect on dissolution rates expected under the independence hypothesis. For Blacks and Whites, the estimated effects are many times larger than the standard errors. These results strongly support the previously hypothesized but understudied independence hypothesis.

Several remaining variables in the control function have systematic effects consistent with those reported by other researchers, but we do not discuss them because of their subsidiary role in the analysis.

The estimates of the background effects obtained with the log-linear rate model are consistent with those for the linear probability model. The estimates of the log-linear background effects are reported in Appendix C, Table C-1.

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*The role of female wage rates is potentially more complex than this. Since earnings by female heads in a marriage have both income and independence effects, the wage has both income and independence effects for married women who work. We are presuming that the independence effect is far more important for these women. For the 60% of married female heads who did not work in the preexperimental period, the wage has only independence effects.

Our brief review of results on the background function serves two purposes. First, the similarity of our results to those of previous researchers--many of whom used a more restrictive definition of marriage and of dissolution--suggests that our findings concerning experimental effects are not artifacts of our liberal definitions. Second, the evidence for the income and independence hypotheses prepares us for complex patterns of response to income discussed earlier.

D. Experimental Effects

Table 6-2 presents the results of the extended list of experimental manipulations on the probability of dissolution within six months using observations pooled over the first three six-month periods of the experiment. As we outlined in Section IV, the extended list of treatments consists of 11 combinations of support levels and tax rates, a three- or five-year treatment, and three manpower treatments.

Eight of the 33 financial treatment effects differ significantly from zero at the 0.05 level; all significant effects are positive. For Whites and Chicanas, the large effects are restricted to low support treatments. Black women, on the other hand, respond significantly to at least one treatment in each support level.

The dummy for the three-year treatment is negative in each equation but never differs significantly from zero.* Thus, the effects of the financial treatments are smaller when the period of support is three rather than five years.

The various manpower treatments show no clear pattern of effects. Only two of the treatments in one equation differ significantly from zero.

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*Allowing the three-year treatment dummy to interact with support levels does not significantly improve the fit of the models.

Table 6-2

EXPERIMENTAL IMPACTS ON MARITAL DISSOLUTION
USING EXPANDED LIST OF TREATMENTS:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF
DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

		Blacks		Whites		Chicanas	
Financial treatments							
Support	(Tax Rate)						
$3,800	(50%)	0.045*	(0.024)	0.062***	(0.021)	0.077**	(0.038)
3,800	(70%)	0.019	(0.032)	0.080***	(0.029)	0.197***	(0.041)
3,800	(70% declining)	0.038	(0.028)	0.057**	(0.026)	0.020	(0.038)
3,800	(80% declining)	0.010	(0.033)	0.008	(0.028)	-0.036	(0.041)
4,800	(50%)	0.079***	(0.026)	0.003	(0.022)	-0.003	(0.039)
4,800	(70%)	0.065***	(0.025)	0.012	(0.020)	0.013	(0.039)
4,800	(70% declining)	0.021	(0.027)	0.021	(0.023)	-0.024	(0.045)
4,800	(80% declining)	0.035	(0.026)	0.021	(0.024)	0.035	(0.037)
5,600	(50%)	-0.023	(0.035)	-0.026	(0.028)	-0.042	(0.051)
5,600	(70%)	0.077**	(0.031)	0.021	(0.024)	-0.0002	(0.043)
5,600	(80% declining)	0.022	(0.024)	0.020	(0.021)	0.0001	(0.036)
Manpower treatments							
M1		-0.007	(0.014)	0.024*	(0.013)	-0.002	(0.022)
M2		0.001	(0.013)	0.003	(0.011)	-0.022	(0.021)
M3		0.014	(0.016)	0.024*	(0.014)	-0.034	(0.025)
Three-year treatment		-0.022	(0.016)	-0.012	(0.014)	--	--
F-ratio for all experimental treatments		1.43		1.75**		2.64***	
F-ratio for financial and three-year treatments (manpower treatments excluded)		1.68*		1.71		3.09**	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Since the manpower treatments as a group do not substantially add to the fit of the model, we ignore them in the remainder of this section.

It is impractical to employ a long list of experimental treatments in exploring subtle effects, such as interactions of treatments with characteristics of individuals. We therefore explore the consequences of using only support level dummies and a dummy for the three-year treatment. The results are reported in Table 6-3. For Blacks and Whites, we cannot reject the more constrained model of Table 6-3, i.e., the null hypothesis that the effects of the manpower treatments, taxes, and all support-tax interactions are zero.

On the other hand for Chicanas the constrained hypothesis fails. The additive support specification reported in Table 6-3 fits significantly less well than the expanded list of financial treatments. Inspection of the pattern of effects in Table 6-2 suggests that the constrained hypothesis fails for Chicanas because of the peculiarly strong effects of the low support, 70% constant tax treatment.

Table 6-4 presents the results from the log-linear rate model that parallel the results reported in Table 6-3.* Because we did not estimate equations with the full representation of the financial treatments parallel to Table 6-2 we cannot test the hypothesis that the support-tax interactions are zero.

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*At this point it seems wise to note the differences in the coefficients in the two models. The coefficients of a linear probability model such as those of Table 6-3 give the additive increment in the probability of dissolution within a six-month period for each unit of an independent variable. The log-linear rate model predicts the natural logarithm of an instantaneous rate. The signs of the coefficients have the same meaning in both models: positive coefficients indicate an increase, negative coefficients a decrease in the probability. The magnitudes of the coefficients in the log-linear model are not additive increments to the probability but rather additive increments to the logarithm of the rate.

Table 6-3

SUPPORT LEVEL EFFECTS ON MARITAL DISSOLUTION:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY
OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Support level						
$3,800	0.031*	(0.018)	0.051***	(0.016)	0.064**	(0.027)
4,800	0.049***	(0.017)	0.010	(0.015)	0.009	(0.027)
5,600	0.028	(0.019)	0.009	(0.016)	-0.010	(0.029)
Three-year treatment	-0.021	(0.015)	-0.005	(0.013)	-0.014	(0.022)
R^2	0.031		0.021		0.032	
F-ratio for equation	3.64***		3.67***		2.45***	
F-ratio for support levels and three-year treatment	2.14*		3.27**		2.40**	
F-ratio for replacing financial and manpower treatment variables (Table 6-3) with support level variables	1.17		1.20		2.71***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables from Table 6-1.

Table 6-4

SUPPORT LEVEL EFFECTS ON MARITAL DISSOLUTION:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Support level						
\$3,800	0.672**	(0.265)	0.848***	(0.262)	0.656*	(0.342)
4,800	0.836***	(0.249)	0.602**	(0.266)	-0.007	(0.392)
5,600	0.523*	(0.298)	0.249	(0.344)	-0.317	(0.484)
Three-year treatment	-0.289	(0.211)	-0.208	(0.218)	-0.234	(0.295)
Likelihood ratio test statistic for support and three-year treatment effects	12.6*	(4 df)	16.2**	(4 df)	8.68	(4 df)
Likelihood ratio test statistic for equation	75.2***		72.84***		36.92***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Appendix C, Table C-1.

Since we want to retain comparability across race-ethnic groups, we use the specification containing additive support and year effects (as in Tables 6-3 and 6-4) for all three samples in subsequent analyses across three groups. Although this may introduce some error in our analyses of Chicanas, we doubt that the distortion is serious.

In both the linear probability and log-linear rate models the more constrained specification of financial treatments yields estimates of effects similar to those in Table 6-3. The significant support level effects are all positive, indicating high dissolution rates for financials relative to the controls. The low support effects are significant for all racial groups in both models. In addition, Blacks have a substantial medium support effect in both models and a large high support effect in the log-linear rate model. Whites have a significant medium support effect in the log-linear model. From the results in Tables 6-3 and 6-4, the impact of income maintenance on dissolution appears overwhelmingly positive and tends to be greater the lower the support.

The experimental effects just reported are static in that responses are constrained to be constant over experimental time. We can think of two ways in which the pattern of response might be more complex. First, there may be an explosive effect early in experimental time as the stock of very unstable marriages breaks up in response to the treatments. Alternatively, there may be a delay in response as recipients test the believability of the experiment. We construct reduced-form models that allow for such dynamic effects by interacting support level dummies with dummy variables denoting the three experimental half years. This gives us the nine experimental effects reported in Table 6-5. The addition of the dynamic terms does not improve the fit for any of the three groups, nor is there a consistent pattern of increases or decreases in experimental effects across support levels or across race-ethnic groups.

E. Predicted Payment Effects

For the Black and White samples we can investigate the effects of estimated transfer payments. Extensive study of the preexperimental financial position of each originally enrolled family enables us to calculate the grant the family would receive in the first experimental year if its financial picture remains unchanged (including no change in labor supply or in family composition). As we argued earlier, income maintenance payments will have both income and independence effects.

Table 6-5

SUPPORT LEVEL INTERACTED WITH EXPERIMENTAL HALF YEAR: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	-0.003	(0.027)	0.070***	(0.024)	0.012	(0.039)
3,800 support × 2nd exp half	0.038	(0.035)	-0.045	(0.030)	0.028	(0.050)
3,800 support × 3rd exp half	0.066*	(0.035)	-0.012	(0.030)	0.137***	(0.051)
4,800 support	0.046*	(0.025)	0.012	(0.022)	0.019	(0.039)
4,800 support × 2nd exp half	-0.001	(0.032)	-0.008	(0.028)	-0.057	(0.051)
4,800 support × 3rd exp half	0.012	(0.032)	0.003	(0.028)	0.029	(0.051)
5,600 support	0.045	(0.029)	0.022	(0.025)	-0.015	(0.044)
5,600 support × 2nd exp half	-0.029	(0.039)	-0.014	(0.033)	-0.021	(0.057)
5,600 support × 3rd exp half	-0.026	(0.039)	-0.027	(0.033)	0.039	(0.057)
Three-year treatment	-0.020	(0.015)	-0.005	(0.013)	-0.014	(0.022)
R^2	0.033		0.022		0.039	
F-ratio for equation	3.03**		2.97**		2.28**	
F-ratio for support level-experimental half year interactions	0.89		0.53		1.70	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Contrary to appearances, the estimated payment does not permit a direct test of the income effect hypothesis because the female head of married couples receiving large payments will also tend to receive large payments if she becomes single. It is likely that the perceived veracity of the experimental guarantees is greatest for those women receiving positive transfers. Moreover, the actual receipt of cash transfers presumably heightens the experience of a change in the opportunity structure relative to the preexperimental situation. Both factors should increase the strength of the independence effects of the income maintenance treatment. Therefore, the effects of estimated payments should reflect both income and independence effects.

Tables 6-6 and 6-7 show the effect of including estimated transfer payments in the models containing the background function, the support levels, and the duration of the experiment. In both models payment has a strongly positive effect for Whites, but a much smaller and insignificant effect for Blacks. The results for Whites conform to our arguments based on the veracity and disequilibrium-heightening effect of payments. The most important finding is that most of the significant support effects for Whites and for Blacks found in Tables 6-3 and 6-4, though somewhat reduced by the inclusion of payments in the equation, are still significant. In other words, the support levels affect the likelihood of marital dissolution even when we take into account the effects of the estimated payment, which is itself a function of support level.

F. Experimental-Background Interactions

It is reasonable to assume that women in less stable marriages might respond more to the experimental treatments than those in more stable marriages. Thus, characteristics of women that affect the rate of marital dissolution may *also* condition the impact of income maintenance on the marital dissolution rate.

Table 6-6

SUPPORT AND PAYMENT EFFECTS ON MARITAL DISSOLUTION: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites	
Support level				
\$3,800	0.026	(0.019)	0.039**	(0.017)
4,800	0.041**	(0.019)	-0.007	(0.017)
5,600	0.019	(0.021)	-0.014	(0.019)
Three-year treatment	-0.021	(0.015)	0.006	(0.013)
Payment (thousands of dollars)	0.006	(0.006)	0.012**	(0.005)
R^2	0.031		0.023	
F-ratio for equation	3.51***		3.77***	
F-ratio for support levels, three-year treatment and payment	2.37**		3.78***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Table 6-7

SUPPORT AND PAYMENT EFFECTS ON MARITAL DISSOLUTION:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites	
Support level				
\$3,800	0.600**	(0.278)	0.678**	(0.281)
4,800	0.724***	(0.281)	0.360	(0.299)
5,600	0.399	(0.330)	-0.083	(0.392)
Three-year treatment	-0.303	(0.212)	-0.236	(0.219)
Payment (thousands of dollars)	0.075	(0.084)	0.150*	(0.081)
Likelihood ratio test statistic for support and payment effects	13.36**	(5 df)	19.6***	(5 df)
Likelihood ratio test statistic for equation	75.97		76.21***	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations include background variables of Appendix C, Table C-1.

The existence of such experimental-background interactions would not mean that the experimental treatments have no effect on dissolution, only that the impact of the treatments is stronger among women with certain characteristics. Identification of such interactions improves the ability to predict the effects of an income maintenance program in another sample or in a national program.

Since all reported results are for a particular race-ethnic group, we have permitted race-ethnicity to condition the effects on dissolution of all other variables, including both experimental treatments and background variables. Undoubtedly race-ethnicity is associated with a variety of traits, norms, objective life situations, and other factors influencing marital dissolution. Still, other background variables also affect marital dissolution and may condition the response to income maintenance.

In this section, we report the results of interacting the support levels with normal earnings level, AFDC experience, a woman's wage and age, and the children in the family under six years of age. We begin with the results on the interactions between support levels and normal earnings levels.

1. Support-Normal Earnings Interactions

The assignment model of SIME/DIME (see Section III) had placed the largest proportion of families with low normal earnings in the treatments with the lower support levels. Of the families with two heads at enrollment, those with normal earnings in the $0 to $6,999 group form 93.7% of those on the low support level, 50.6% of those on the medium support level, 33.4% of those on the high support level, and 44.6% of those in the control group.

We have argued (see Section II) that families with greater resources have lower rates of marital dissolution, and our findings on the effects of normal earnings (see Tables 6-2 and C-1) are consistent with this argument. Thus, if we did not control for normal earnings level, the concentration of families with low normal earnings in the lowest support levels would lead to higher dissolution rates for families on these support levels. Since we do control for normal earnings level, the greater response to income maintenance in the lower support levels could occur because women with riskier marriages (i.e., those in families with low normal earnings) have a larger response.

One way to test the hypothesis that women in families with low normal earnings respond more strongly to income maintenance is to examine the effects of interacting support levels with normal earnings levels. It is unwieldy to interact the three support levels with each of the seven existing normal earnings categories used in assignment of families to treatments. Therefore, to study interactions of support levels and normal earnings levels we created a new, continuous normal earnings variable. A family's value on this variable is the midpoint of their normal earnings category in thousands of dollars. For example, a family in the $0 to $999 group has the value 0.5, one in the $1,000 to $2,999 group has the value 2, and so forth.

The results of including the interactions of support levels with the continuous normal earnings variable in the linear probability and log-linear rate models are reported in Tables 6-8 and 6-9, respectively. The test statistic for the inclusion of the set of interaction terms is not significant for any race-ethnic group in either table. This means that we cannot reject the null hypothesis that these interactions do not affect the rate of marital dissolution. Only one coefficient of any of the interactions of support levels with continuous normal earnings (that for Whites on the high support) achieves statistical significance. More importantly, the positive effect of the low support level on marital dissolution is still evident for Whites and Chicanas in both models and for Blacks in

Table 6-8

SUPPORT LEVEL INTERACTED WITH EARNINGS LEVEL:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF
DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	0.028	(0.042)	0.102***	(0.037)	0.140**	(0.056)
3,800 support × normal earnings	0.001	(0.008)	-0.010	(0.007)	-0.016	(0.010)
4,800 support	0.053	(0.041)	0.037	(0.034)	0.016	(0.060)
4,800 support × normal earnings	-0.004	(0.005)	-0.004	(0.005)	-0.001	(0.008)
5,600 support	0.089	(0.062)	0.094*	(0.052)	-0.025	(0.074)
5,600 support × normal earnings	-0.008	(0.008)	-0.012*	(0.007)	0.002	(0.010)
Three-year treatment	-0.021	(0.015)	-0.005	(0.013)	-0.015	(0.022)
R^2	0.031		0.023		0.034	
F-ratio for equation	3.23**		3.41**		2.25**	
F-ratio for support-earnings level interactions	0.39		1.55		0.93	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Table 6-9

SUPPORT LEVEL INTERACTED WITH NORMAL EARNINGS:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	0.879*	(0.519)	0.980*	(0.556)	1.47**	(0.739)
3,800 support × normal earnings	-0.030	(0.097)	-0.014	(0.104)	-0.162	(0.135)
4,800 support	0.989*	(0.524)	1.03*	(0.559)	0.684	(0.946)
4,800 support × normal earnings	-0.019	(0.069)	-0.067	(0.085)	-0.110	(0.145)
5,600 support	1.61**	(0.799)	1.43	(0.974)	0.220	(1.29)
5,600 support × normal earnings	-0.142	(0.101)	-0.182	(0.145)	-0.084	(0.192)
Three-year treatment	-0.319	(0.212)	-0.232	(0.216)	-0.286	(0.295)
Likelihood ratio test statistic for support-normal earnings interactions	1.88	(3 df)	2.94	(3 df)	0.38	(3 df)
Likelihood ratio test statistic for equation	77.08***		69.89***		36.56***	

* $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$

† All equations include background variables of Appendix C, Table C-1.

the log-linear model. Although the pattern of medium and high support effects for Blacks and Whites previously found in Tables 6-1 and 6-4 no longer appears in the linear probability model, it still occurs in the log-linear rate model.

We conclude that (a) the greater impact of the low support treatment on marital dissolution is not an artifact of the assignment of families with low normal earnings to the low support treatment, and (b) there is no evidence of women from families with low normal earnings responding to the support levels differently than those from families with high normal earnings.

2. Support-AFDC Interactions

We have suggested that women with less stable marriages may respond more to income maintenance than those with more stable marriages. There are several reasons for thinking that women with recent AFDC experience have less stable marriages. In most cases, recent AFDC experience indicates that a woman was recently single (and is newly married), that the male head is not the father of children in the family and does not support them, or that the male head is unemployed. Each possibility would lead us to expect that the woman's marriage is less stable than the marriage of an otherwise comparable woman. The main effect of AFDC (see Tables 6-1 and C-1) is to increase dissolution (significantly for Chicanas in both models and for Whites in the log-linear rate model), which is consistent with the argument above that <u>at all support levels</u> dissolution rates are higher for women with recent AFDC experience than for those without.

Thus, if women with less stable marriages have a larger response to the experimental treatments, we might expect positive effects of interactions between support levels and recent AFDC experience. We give the results of including the interactions between support levels and AFDC in the linear probability and log-linear rate models in Tables 6-10 and 6-11, respectively. No consistent pattern of results appears. The test statistics for inclusion of the set of interactions are significant only for Chicanas and Whites in the linear probability model. In both models, the only significant coefficients for individual interaction terms occur for Chicanas on the low support and Whites on the high support. While we tentatively conclude that recent AFDC experience does not condition a woman's response to income maintenance (except perhaps for low support Chicanas and high support Whites), we note that controlling for recent AFDC experience diminishes but does not remove the significant low and medium support effects for Blacks and Whites (cf. Tables 6-3 and 6-4).

3. Support-Wage Interactions

As we noted previously, a wage rate summarizes a great deal of information about an individual's general earnings capacity and employment history. Moreover, we have argued that for female heads of families, higher wage rates lead to less dependence on marriage, and consequently a higher rate of marital dissolution. The positive and significant effects of wage on dissolution rates of Black and White women reported in Tables 6-1 and C-1 support this argument.

In the introduction to this section we argued that the experimental impact might be greatest for women with the least stable marriages. Above we proposed that the highest wage women have relatively unstable marriages. On the other hand, it can also be argued that the experimental impact might be greatest for the most dependent women, for whom the experimental guarantees provide the most change in opportunities outside of marriage. Obviously there is a continuum of realistic possibilities among these alternative arguments.

Table 6-10

SUPPORT-AFDC INTERACTION EFFECTS ON MARITAL DISSOLUTION: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	0.038*	(0.021)	0.062***	(0.018)	0.007	(0.032)
3,800 support × AFDC	-0.021	(0.035)	0.018	(0.032)	0.150***	(0.044)
4,800 support	0.052***	(0.018)	0.012	(0.016)	-0.003	(0.029)
4,800 support × AFDC	-0.016	(0.036)	0.008	(0.032)	0.047	(0.048)
5,600 support	0.021	(0.020)	0.0001	(0.017)	-0.011	(0.032)
5,600 support × AFDC	0.075	(0.054)	0.097**	(0.043)	-0.006	(C.059)
Three-year treatment	-0.021	(0.015)	-0.008	(0.013)	-0.015	(0.022)
R^2	0.032		0.023		0.041	
F-ratio for equation	3.32***		3.50***		2.71***	
F-ratio for support level-AFDC interactions	1.07		2.31*		4.38***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Table 6-11

SUPPORT-AFDC INTERACTION EFFECTS ON MARITAL DISSOLUTION:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	0.776**	(0.311)	0.922***	(0.297)	0.247	(0.425)
3,800 support × AFDC	-0.157	(0.490)	-0.073	(0.499)	0.975*	(0.564)
4,800 support	0.840***	(0.267)	0.574**	(0.286)	-0.253	(0.476)
4,800 support × AFDC	0.053	(0.499)	0.211	(0.523)	0.813	(0.678)
5,600 support	0.337	(0.332)	-0.040	(0.399)	-0.390	(0.555)
5,600 support × AFDC	1.00	(0.627)	1.22*	(0.691)	0.324	(0.952)
Three-year treatment	-0.307	(0.211)	-0.228	(0.220)	-0.274	(0.295)
Likelihood ratio test statistic for support-wage interaction effects	3.12	(3 df)	2.23	(3 df)	1.38	(3 df)
Likelihood ratio test statistic for equation	78.32***		75.04***		38.29***	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations include background variables of Appendix C, Table C-1.

To investigate the hypothesis of a differential experimental impact based on a woman's wage, we employed interactions of support levels with three different specifications of a woman's wage: (1) the wage term itself, (2) a quadratic wage (i.e., the wage and the wage squared), and (3) a three-piece linear spline of the wage. In each instance, the background function included the specification of the wage corresponding to that in the interactions.

Including interactions of support levels with either a quadratic wage expression or with the three-piece linear spline of the wage did not significantly improve the results obtained by interacting support and wage. For this reason the tables giving these results for the quadratic and linear spline interactions are omitted from this report.

Tables 6-12 and 6-13 report the results for the support levels interacted with the wage rate of the female head.* The test statistics for the inclusion of the set of interactions are significant for Blacks in both models and for Whites in the linear probability model. Five of the six significant coefficients for support-wage interactions are positive, indicating that the impact of support tends to increase as wage increases. This suggests that income maintenance response is conditioned by individual characteristics, particularly with those having destabilizing effects on marriage.

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*The coefficients for the support levels differ greatly in both magnitude and significance from those in the tables without the support-wage interactions (cf. Tables 6-3 and 6-4). The coefficients for support levels in the tables without the support-wage interactions essentially indicate the response of a woman with an average wage, which is about $2.00 per hour in the SIME/DIME sample. On the other hand, the coefficients for support levels in Tables 6-12 and 6-13 indicate the response of a woman with a zero wage rate. The predicted response to a support level for a woman with an average wage is very close to the coefficients of the support in Tables 6-3 and 6-4.

Table 6-12

SUPPORT-WAGE INTERACTION EFFECTS ON MARITAL DISSOLUTION: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF DISSOLUTION WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
$3,800 support	0.114*	(0.066)	-0.043	(0.054)	-0.098	(0.172)
3,800 support × wage	0.073**	(0.031)	0.046*	(0.025)	0.085	(0.088)
4,800 support	0.118**	(0.057)	0.040	(0.045)	-0.384**	(0.162)
4,800 support × wage	-0.031	(0.025)	-0.014	(0.020)	0.204**	(0.083)
5,600 support	0.052	(0.069)	0.098	(0.052)	-0.109	(0.174)
5,600 support × wage	-0.010	(0.030)	-0.043*	(0.023)	0.052	(0.089)
Three-year treatment	-0.023	(0.015)	-0.004	(0.013)	-0.016	(0.022)
R^2	0.034		0.024		0.036	
F-ratio for equation	3.56**		3.59		2.40	
F-ratio for support level-wage interaction	2.94**		2.96**		2.06	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table 6-1.

Table 6-13

SUPPORT-WAGE INTERACTION EFFECTS ON MARITAL DISSOLUTION:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
$3,800 support	-1.33	(0.939)	0.016	(0.752)	-1.67	(2.10)
3,800 support × wage	0.944**	(0.409)	0.378	(0.327)	1.23	(1.08)
4,800 support	1.52*	(0.867)	0.193	(0.776)	-6.16**	(2.61)
4,800 support × wage	-0.312	(0.385)	0.184	(0.338)	3.15**	(1.29)
5,600 support	0.483	(1.21)	1.34	(1.16)	-2.56	(3.01)
5,600 support × wage	0.014	(0.517)	-0.546	(0.558)	1.20	(1.54)
Three-year treatment	-0.281	(0.212)	-0.199	(0.218)	-0.290	(0.296)
Likelihood ratio test statistics for support-wage interactions	7.96**	(3 df)	1.64	(3 df)	3.88	(3 df)
Likelihood ratio test statistic for equation	83.15***		74.47***		40.81***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include background variables of Table C-1, Appendix C.

4. Other Support Interactions

The remaining support interactions examined (those with a woman's age and a dummy variable indicating the presence in the family of children under six years of age) show no clear pattern of effects. Consequently the tables reporting these results are omitted.

The test statistic for the inclusion of the set of age-support level interactions is significant (at the 0.10 level) only for Chicanas in the linear probability model. Only the low support-age interaction is significant (0.05 level) in the linear probability model for Chicanas. This coefficient is negative, indicating that the response to the low support declines as a Chicana's age increases. Although the test statistics for the inclusion of the set of age-support interactions are not significant for either Blacks or Whites in either model, the medium support-age interaction is significant at the 0.05 level for Blacks in the linear probability model.

In both the linear probability and log-linear rate models, the set of interactions of support with the presence of children under six is significant for Blacks at the 0.10 level and insignificant for both Whites and Chicanas. The only significant coefficient for one of these interactions is that for Blacks on the medium support. This coefficient is positive, implying that Blacks with children under six have a stronger response to the medium support than do those without children in this age group.

In summary, our analysis indicates that income maintenance increases the rate at which marriages break up. The effects tend to be larger the lower the support level, and are clearest and strongest for Blacks and Whites. Chicanas appear to respond differently to income maintenance than do Blacks and Whites. Except for Chicanas, the effects of financial treatment are satisfactorily captured by the effects of the support levels. The effects of tax rates showed no clear pattern. The response to the experimental treatments was consistently stronger among the five-year treatment group than among the three-year group for all three race-ethnic groups. Though the difference between the three- and five-year groups' response to income maintenance was never significant, it is similar in magnitude for all three race-ethnic groups in every equation.

Our analysis of the effects of interactions of support levels with background characteristics, particularly with the female head's wage, suggests that responses to income maintenance are conditioned by individual characteristics. To the extent that this occurs, responses tend to be greater among women whose characteristics are associated with high rates of marital dissolution.

Finally we note that the results of the linear probability and log-linear rate models usually agreed in substance. This provides evidence that our conclusions concerning the impact of income maintenance on marital dissolution are not artifacts of the particular method of analysis used.

VII IMPACT ON REMARRIAGE

In this section we describe the impact of the income maintenance treatments on the probability of remarriage. Since most women in the sample who were not married at enrollment were previously married (and all have children), we refer to the outcome as remarriage, rather than marriage. In Section II, paralleling our arguments concerning dissolution effects, we hypothesized that income maintenance would have opposing independence and dowry effects on remarriage. Therefore, we cannot form an a priori hypothesis concerning the total effect of income maintenance on the probability of remarriage. If the independence effect dominates, rates of remarriage will be lower in the experimental population than in the control group. On the other hand, if the income or dowry effect dominates, rates of remarriage will be higher in the experimental group. And, as was the case with dissolution, nonlinearities such as threshold effects in either independence or dowry effects, may cause nonmonotonic patterns of treatment (e.g., support level) effects.

The structure of this section follows exactly that used in the previous section on dissolution. The background, experimental, and interaction variables examined in this section are identical to those used in models of dissolution. We present results from both the linear probability and the log-linear rate models.

A. Race-Ethnic Interactions

Tests of differences in slopes across the three race-ethnic groups in the Denver sample produced the same conclusion reached in the dissolution analysis: the entire remarriage process, both background and experimental functions, differs for the three race-ethnic groups. The results for an equation similar to those reported in Table 7-4 but using three-month rather than six-month observation periods are typical. The F-ratio on the joint hypothesis that all slopes are equal across race-ethnic groups is:

$$F = 1.51 \ (79,8118 \ df) \ p > 0.01 \quad .$$

Thus, we analyze the three populations separately in all that follows.

B. Site Interactions

Tests of homogeneity of slopes across the two sites also produced the same conclusion as in the dissolution process: the sites do not differ systematically in either the control or the experimental portions of the remarriage process. The test statistics on homogeneity of slopes across sites for the model containing the background function and the expanded list of experimental treatments are as follows:

$$\text{Blacks:} \quad F = 0.78 \ (28,2609 \ df) \ p > 0.10$$

$$\text{Whites:} \quad F = 0.94 \ (28,2157 \ df) \ p > 0.10$$

On the basis of these findings, we analyze the merged site data on Blacks and Whites.

C. Effects of Background Variables

The estimates of effects of background variables (from the model containing the expanded list of experimental treatments) are presented in Table 7-1. The absence of systematic effects of background variables among race-ethnic groups is striking relative to the systematic background effects observed for dissolution. Neither education nor wage rates have

Table 7-1

EFFECTS OF BACKGROUND VARIABLES ON REMARRIAGE:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF
REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Normal earnings levels						
$ 1,000-$2,999	-0.034*	(0.019)	0.004	(0.027)	0.001	(0.057)
3,000- 4,999	-0.027	(0.019)	0.045*	(0.027)	0.037	(0.060)
5,000- 6,999	-0.007	(0.022)	0.017	(0.030)	0.101	(0.064)
7,000- 8,999	-0.014	(0.025)	-0.015	(0.034)	-0.00002	(0.084)
9,000-10,999	0.091**	(0.036)	0.101*	(0.057)	0.208*	(0.124)
11,000-12,999	-0.075	(0.197)	--	--	--	--
Unclassified	-0.019	(0.034)	0.053	(0.049)	-0.034	(0.098)
Education	-0.005	(0.003)	-0.0007	(0.004)	-0.007	(0.008)
Wage rate	-0.008	(0.010)	0.019	(0.013)	0.056	(0.063)
Age	-0.002***	(0.0007)	-0.005***	(0.001)	-0.003	(0.002)
AFDC (0,1)	-0.023*	(0.013)	-0.003	(0.118)	0.003	(0.040)
April-September (0,1)	0.015	(0.011)	-0.030**	(0.015)	0.024	(0.034)
Experimental months 7-12 (0,1)	-0.006	(0.013)	-0.007	(0.019)	-0.018	(0.041)
Experimental months 13-18 (0,1)	0.002	(0.013)	-0.020	(0.019)	-0.032	(0.040)
Children aged 5 and under (0,1)	-0.010	(0.015)	-0.032	(0.022)	-0.028	(0.050)
Children aged 9 and under (0,1)	0.007	(0.017)	-0.006	(0.023)	0.002	(0.053)
Denver (0,1)	0.009	(0.012)	-0.004	(0.016)	--	--
Constant	0.198	(0.056)	0.201	(0.077)	0.175	(0.169)
Dependent variable mean	0.045					
R^2	0.021		0.029		0.029	
F-ratio for equation	1.69***		2.03***		0.86	
N	2581		2129		901	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include the experimental treatment variables of Table 7-2.

effects significantly different from zero in any equation. The absence of background effects is not peculiar to this specification. The effects are hardly changed by the inclusion of a variety of interaction terms and other specifications of experimental effects.* There is a slight suggestion of a normal earnings effect for two groups. But, the effect is negative for Blacks and positive for Whites. The significant coefficients for the $9,000 to $10,999 normal earnings level should be interpreted with caution since only women originally enrolled while married may be at this level (this is also true for the $11,000 to $12,999 level). This may be an indication that women at the higher earnings levels are more likely to reconcile than other women. It is not obvious that the effects are strong enough to rule out sampling error, however. The only variable that behaves as expected is age. For every group, the probability of remarriage declines with age (although the effect is not significant for Chicanas).

The estimates of background variables for the log-linear analyses, presented in Appendix C, are similar to the linear probability estimates.

D. Experimental Effects

Estimates of effects of the expanded list of experimental treatments are presented in Table 7-2. For Blacks and Whites, there are significant financial treatment effects. For Blacks each of the 11 financial treatment dummies is positive and three are significantly different from zero. For Chicanas, on the other hand, all but one effect is negative, although

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*One might argue that the inclusion in this analysis of a large number of reconciliations might obscure these effects. For example, if the effect of normal earnings on the probability of reconciliation differs in sign from that on other remarriages, combining both types of outcomes would tend to yield no systematic effect. Our analysis of reconciliation in Appendix A suggests that this is not the case.

Table 7-2

EXPERIMENTAL IMPACTS ON REMARRIAGE USING EXPANDED LIST OF TREATMENTS: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

		Blacks		Whites		Chicanas	
Financial treatments							
Support	(Tax Rate)						
$3,800	(50%)	0.042*	(0.023)	0.051*	(0.030)	-0.067	(0.067)
3,800	(70%)	0.015	(0.027)	-0.011	(0.038)	-0.061	(0.071)
3,800	(70% declining)	0.026	(0.028)	0.075**	(0.038)	0.137	(0.085)
3,800	(80% declining)	0.012	(0.031)	-0.018	(0.041)	-0.045	(0.086)
4,800	(50%)	0.005	(0.029)	-0.013	(0.040)	-0.079	(0.098)
4,800	(70%)	0.047**	(0.024)	0.002	(0.033)	-0.100	(0.072)
4,800	(70% declining)	0.056*	(0.030)	0.011	(0.038)	-0.032	(0.123)
4,800	(80% declining)	0.027	(0.028)	0.003	(0.038)	-0.059	(0.077)
5,600	(50%)	0.087	(0.056)	0.028	(0.089)	-0.078	(0.138)
5,600	(70%)	0.056*	(0.032)	-0.021	(0.042)	-0.036	(0.092)
5,600	(80% declining)	0.018	(0.030)	0.042	(0.043)	-0.125	(0.090)
Manpower treatments							
M1		0.018	(0.016)	0.0001	(0.021)	-0.037	(0.047)
M2		0.020	(0.014)	0.001	(0.020)	-0.043	(0.043)
M3		0.008	(0.016)	0.008	(0.024)	-0.041	(0.054)
Three-year treatment		-0.020	(0.017)	0.018	(0.023)	0.010	(0.048)
R^2		0.021		0.029		0.029	
F-ratio on all treatments		0.92		0.95		0.86	
F-ratio on financial and three-year treatments (manpower excluded)		0.95		1.18		0.96	
F-ratio for equation		1.69***		2.03***		0.860	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain the background variables of Table 7-1.

none is significantly different from zero. The results are mixed for Whites. While we cannot be confident about conclusions based on statistically insignificant coefficients, it appears that income maintenance raises the rate of remarriage for Blacks, decreases it for Chicanas, and has no systematic effect for Whites. Neither duration of the experiment nor any of the manpower treatments significantly affect the rate of remarriage. The experimental effects are concentrated on the financial treatments.

Given the low levels of explained variance in remarriage regressions (and the consequent large standard errors), it is particularly important to focus on representations of treatment effects with fewer parameters. As in the dissolution analysis we first estimated a model containing support level, tax rate, and three-year dummies and then a model that excluded the tax rates. On the whole, we find that we lose very little information with the simpler representations. That is, the simpler models reported in Table 7-3 fit about as well as those containing more experimental parameters. Therefore, we focus on the effects of support levels and the three-year treatment. For Blacks we find significant positive experimental effects for both the $4,800 and the $5,600 support levels. No effects are significant for Whites or Chicanas. The effects for Chicanas are all negative and greater in magnitude than for either Blacks or Whites.

Table 7-4 contains the support level effects estimated by the log-linear rate model. The effects for Blacks and Whites are similar to those from the pooled linear probability model in Table 7-3. For Chicanas the log-linear rate model has significant coefficients for the $4,800 and $5,600 support level that were not found in the linear probability model. These significant negative effects are enormous, indicating that the remarriage rate for Chicanas on the $4,800 support is 34% of the rate for the controls. The remarriage rate for the $5,600 support level Chicanas

Table 7-3

SUPPORT LEVEL EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF
REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks	Whites	Chicanas
Support level			
$3,800	0.029 (0.019)	0.029 (0.025)	-0.034 (0.053)
4,800	0.036* (0.019)	-0.001 (0.025)	-0.086 (0.056)
5,600	0.044* (0.023)	0.008 (0.032)	-0.095 (0.068)
Three-year treatment	-0.018 (0.016)	0.022 (0.022)	0.015 (0.046)
R^2	0.017	0.025	0.018
F-ratio for equation	2.16***	2.74***	0.920
F-ratio for replacing financial and manpower treatment variables with support level variables	0.80	0.74	0.83
F-ratio for support level and three-year treatment	1.26	1.54	0.93

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations contain the background variables of Table 7-1.

Table 7-4

SUPPORT LEVEL EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF REMARRIAGE†
(Standard Errors in Parentheses)

	Blacks	Whites	Chicanas
Support level			
$3,800	0.406 (0.358)	0.036 (0.312)	-0.360 (0.502)
4,800	0.766** (0.345)	0.072 (0.316)	-1.08* (0.580)
5,600	0.767* (0.407)	-0.007 (0.039)	-1.77** (0.847)
Three-year treatment	-0.167 (0.284)	0.347 (0.260)	0.437 (0.473)
Likelihood ratio test statistic for support and three-year treatment effects	7.00* (4 df)	4.22 (4 df)	8.82* (4 df)
Likelihood ratio test statistic for equation	41.34***	68.68***	34.81***

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations contain the background variables of Appendix C, Table C-2.

is only 17% of the control rate. At this point we can only note the magnitude and direction of these effects. An explanation of these findings must await further analyses.

Just as in the case for dissolution impacts, we can imagine several ways in which the response to the experimental treatments will depend on time. For example, those who are enrolled as single women but who are contemplating entering a marital relationship could hasten their marriages after enrollment. Such a speedup should produce an explosion of marriages early in experimental time. On the other hand, the response may be delayed for individuals who need to search for a desirable mate.

To examine these possibilities empirically, we use the same technique employed in the previous section. We estimate a support level effect for each semiannual experimental period (see Table 7-5). We see, first, that the dynamic representation discloses a low support effect for Whites that were hidden in the static representation. There is a significant positive effect of the low support level in the first six months and a significant negative effect of the low support level in the second six months. These results are consistent with the hypothesis that the introduction of the experiment causes those already contemplating remarriage to enter marital unions sooner than they would have otherwise. However, since the coefficients for the $4,800 and $5,600 support levels do not exhibit the same pattern, we cannot take these results as conclusive. The only other significant coefficient in Table 7-5 is for high support Blacks in the first six months. Overall, Table 7-5 indicates that there are no dynamic effects on remarriage.

E. Predicted Payment Effects

As in Section VI, we can investigate the effects of estimated transfer payments for Blacks and Whites. Table 7-6 shows that including the

Table 7-5

SUPPORT-EXPERIMENTAL HALF-YEAR INTERACTION EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF REMARRIAGE
WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

Support	Experimental Period	Blacks		Whites		Chicanas	
$3,800	Months 1- 6	-0.0002	(0.027)	0.096***	(0.036)	-0.081	(0.079)
3,800	Months 7-12	0.049	(0.033)	-0.127***	(0.047)	0.019	(0.100)
3,800	Months 13-18	0.039	(0.033)	-0.075	(0.046)	0.120	(0.099)
4,800	Months 1- 6	0.029	(0.028)	0.008	(0.037)	-0.115	(0.086)
4,800	Months 7-12	0.0002	(0.035)	-0.018	(0.047)	-0.015	(0.113)
4,800	Months 13-18	0.022	(0.034)	-0.008	(0.047)	0.102	(0.110)
5,600	Months 1- 6	0.086**	(0.037)	-0.014	(0.051)	-0.130	(0.106)
5,600	Months 7-12	-0.055	(0.048)	0.106	(0.067)	-0.009	(0.143)
5,600	Months 13-18	-0.069	(0.048)	-0.043	(0.069)	0.108	(0.139)
Three-year treatment		-0.019	(0.016)	0.022	(0.022)	0.016	(0.046)
R^2		0.020		0.033		0.021	
F-ratio for equation		1.95***		2.75***		0.759	
F-ratio for support-experimental half-year interactions		1.21		2.70**		0.397	

*p < 0.10 **p < 0.05 **p < 0.01

†All equations contain the background variables of Table 7-1.

Table 7-6

SUPPORT AND PAYMENT EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF
REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS,
USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks	Whites
Support level		
$3,800	0.048** (0.021)	0.042 (0.027)
4,800	0.063*** (0.023)	0.018 (0.030)
5,600	0.073*** (0.027)	0.028 (0.037)
Payment (thousands of dollars)	-0.012** (0.005)	-0.008 (0.007)
Three-year treatment	-0.015 (0.016)	0.025 (0.022)
R^2	0.019	0.026
F-ratio for equation	2.27	2.68
F-ratio for support level, three-year treatment, and payment	2.38**	1.49

*p < 0.10   **p < 0.05   ***p < 0.01

†All equations contain the background variables of Table 7-1.

payment variable generally increases the support level effects (compare Table 7-3). The support levels for Blacks are significant and positive, with the higher support levels having the greater effects. The effects are also positive for Whites, although no support effects are significant. Payment has a negative effect for both races, but is significant only for Blacks. Thus, when considered together, support level and estimated payment have opposite effects: support level increases the probability of remarriage, and payment decreases it. Estimated payment represents income available to the woman as long as she remains single; the support level is a measure of resources available whether she is single or married. While for single women the estimated payment has a primarily independence effect, support level still includes both independence and dowry effects. In the log-linear rate model estimates of support and payment effects in Table 7-7, we fail to find the significant low support effect for Blacks that was present in the linear probability model. In both models payment has a negative effect but is not significant.

Table 7-7

SUPPORT AND PAYMENT EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF REMARRIAGE†
(Standard Errors in Parentheses)

	Blacks		Whites	
Support level				
$3,800	0.604	(0.385)	0.195	(0.338)
4,800	1.05***	(0.399)	0.304	(0.374)
5,600	1.03**	(0.465)	0.242	(0.437)
Payment (thousands of dollars)	-0.137	(0.097)	-0.087	(0.084)
Three-year treatment	-0.122	(0.284)	0.355	(0.261)
Likelihood ratio test statistic for support, three-year and payment effects	5.62	(5 df)	4.68	(5 df)
Likelihood ratio test statistic for equation	39.97***		69.14***	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain the background variables of Appendix C, Table C-2.

F. Experimental-Background Interactions

In analyzing the experimental impacts on remarriage, we investigated the same interactions as were used in the analysis of dissolution. With few exceptions, these interactions failed to improve the fit of our models. We therefore discuss the nonadditive effects only briefly.

We interacted the support levels with the midpoint of the normal earnings categories to test the impact of the nonrandom assignment method on our experimental results. The results for the pooled linear probability and log-linear rate models are reported in Tables 7-8 and 7-9, respectively. None of the joint tests for the interactions is significant and only one interaction coefficient, the low support interaction for Blacks, in the log-linear rate model, is significant. The medium and high support effects that were significant for Blacks in both models and for Chicanas in the log-linear rate model are not significant here. A comparison of the coefficients in Tables 7-8 and 7-9 to the coefficients from the equations without the normal earnings interactions (Tables 7-3 and 7-4) shows that while the levels of significance have changed, the magnitudes of the coefficients have not changed substantially. The exception is the coefficient for Blacks on the $5,600 support treatment, which is much smaller in the equation with the interactions. On the whole then, given the lack of significance of the joint tests for the interactions and the relative stability of the support level coefficients, it does not appear that the experimental impacts noted in Tables 7-3 and 7-4 are due to the nonrandom assignment of families to experimental treatments.

We first tested the interactions of support level with the AFDC variable that indicates whether or not the woman had received AFDC payments during the two calendar years before enrollment. The results of the linear probability model are presented in Table 7-10; the results of the log-linear rate model are presented in Table 7-11. The support level AFDC interaction

Table 7-8

SUPPORT-EARNINGS LEVEL INTERACTION EFFECTS ON REMARRIAGE: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks	Whites	Chicanas
$3,800 support	0.051* (0.029)	0.018 (0.041)	0.064 (0.085)
3,800 support × normal earnings	-0.007 (0.006)	0.003 (0.009)	-0.028 (0.019)
4,800 support	0.017 (0.031)	0.008 (0.042)	-0.002 (0.102)
4,800 support × normal earnings	0.004 (0.005)	-0.003 (0.008)	-0.022 (0.022)
5,600 support	0.013 (0.046)	-0.027 (0.066)	-0.040 (0.123)
5,600 support × normal earnings	0.006 (0.008)	0.007 (0.011)	-0.014 (0.026)
Three-year treatment	-0.017 (0.016)	0.024 (0.022)	0.021 (0.046)
R^2	0.019	0.026	0.021
F-ratio for equation	2.04***	2.42***	0.867
F-ratio for support-earnings level interactions	1.15	0.249	0.805

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain the background variables of Table 7-1.

Table 7-9

SUPPORT-NORMAL EARNINGS INTERACTION EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF REMARRIAGE†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
$3,800 support	1.29**	(0.553)	0.219	(0.560)	0.393	(0.788)
3,800 support × normal earnings	-0.305**	(0.135)	-0.035	(0.111)	-0.211	(0.165)
4,800 support	0.687	(0.580)	0.371	(0.524)	-1.20	(1.15)
4,800 support × normal earnings	0.010	(0.089)	-0.061	(0.091)	0.021	(0.217)
5,600 support	0.196	(0.867)	0.187	(0.796)	-1.71	(2.07)
5,600 support × normal earnings	0.084	(0.132)	-0.032	(0.136)	-0.020	(0.461)
Three-year treatment	-0.159	(0.284)	0.331	(0.262)	0.483	(0.476)
Likelihood ratio test statistic for support-normal earnings interactions	3.78	(3 df)	0.16	(3 df)	1.96	(3 df)
Likelihood ratio test statistic for equation	45.13***		68.52***		36.78***	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain the background variables of Appendix C, Table C-2.

Table 7-10

SUPPORT-AFDC INTERACTION EFFECTS ON REMARRIAGE: ESTIMATES FOR A LINEAR MODEL OF THE PROBABILITY OF REMARRIAGE WITHIN SIX-MONTH EXPERIMENTAL PERIODS, USING OBSERVATIONS POOLED OVER THE FIRST THREE PERIODS†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
$3,800 support	0.041	(0.026)	0.058*	(0.030)	-0.114	(0.074)
3,800 support × AFDC	-0.023	(0.029)	-0.058	(0.039)	0.137	(0.085)
4,800 support	0.043*	(0.023)	-0.026	(0.030)	-0.143*	(0.080)
4,800 support × AFDC	-0.016	(0.028)	0.057	(0.040)	0.104	(0.097)
5,600 support	0.067**	(0.029)	-0.017	(0.038)	-0.146	(0.096)
5,600 support × AFDC	-0.053	((0.040)	0.062	(0.058)	0.100	(0.119)
Three-year treatment	-0.018	(0.016)	0.025	(0.022)	0.014	(0.046)
R^2	0.018		0.029		0.022	
F-ratio for equation	1.98***		2.77***		0.885	
F-ratio for addition of support-AFDC interactions	0.66		2.85**		0.941	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations contain the background variables of Table 7.1.

Table 7-11

SUPPORT-AFDC INTERACTION EFFECTS ON REMARRIAGE:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF REMARRIAGE†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
\$3,800 support	0.637	(0.521)	0.103	(0.358)	-0.047	(0.648)
3,800 support × AFDC	-0.519	(0.595)	0.031	(0.508)	0.171	(0.702)
4,800 support	1.10***	(0.423)	-0.330	(0.382)	-1.09	(0.781)
4,800 support × AFDC	-0.791	(0.551)	0.994**	(0.505)	0.030	(0.930)
5,600 support	1.19**	(0.493)	-0.754	(0.576)	-1.54	(1.13)
5,600 support × AFDC	-1.24	(0.778)	1.56**	(0.716)	-0.410	(1.51)
Three-year treatment	-0.138	(0.283)	0.356	(0.260)	0.430	(0.475)
Likelihood ratio test statistic for support-AFDC interactions	0.18	(3 df)	8.59**	(3 df)	0.16	(3 df)
Likelihood ratio test statistic for equation	41.53***		77.26***		34.99***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations contain the background variables of Appendix C, Table C-2.

coefficients are significant only for Whites in both models. White women with recent experience on AFDC who are receiving medium or high support are more likely to remarry than those who had not been on AFDC. Since this result holds for only one race-ethnic group, we are reluctant to place much emphasis on the result.

In investigating the effects of female's wage rate of remarriage, we examined the same functional forms as we did for dissolution. None of the support-wage interactions (linear, linear splines, or quadratic) significantly improved the fit of the model. From our analysis, it does not appear that the female's wage rate significantly affects any experimental impact on remarriage. Likewise, interacting age with support level does not affect the support level impacts on remarriage. In general, older women are less likely to remarry than younger women, but the relationship between age and remarrying does not differ by support level. Having young children does not affect the probability of remarrying for Blacks or Whites. In the log-linear model, Chicanas on the $4,800 support level were significantly less likely to remarry if they did not have children under six years old and significantly more likely to remarry if they had young children. The coefficients were not significant in the linear probability model.

There are two difficulties in summarizing our analysis of impacts on remarriage. First, the phenomenon itself appears less systematic in our sample than does dissolution. Our equations fit less well and our estimated coefficients generally have large standard errors. This makes it very difficult to draw inferences from models that contain many experimental parameters. Second, those impacts that appear strongest show great differences by race-ethnicity.

In general, it appears that income maintenance increases the rate of remarriage for Blacks, decreases it for Chicanas, and does not systematically affect it for Whites.

VIII THE SENSITIVITY OF THE EXPERIMENTAL RESULTS TO ATTRITION

As we noted in Section V, some women originally enrolled in the experiment did not participate for the entire 18 months. In this section, we investigate the potential bias in our results that may arise if attrition from the experiment is related both to our dependent variable (marital status change) and the independent variables (experimental treatments). First, we describe the amount of attrition and the relationship of attrition to the experimental treatments. Second, we discuss the possible consequences for our results if attrition is related to both experimental treatments and changes in marital status. Third, we present an analysis designed to test the sensitivity of our results to several hypotheses about the associations among attrition, marital status change, and experimental treatment.

A. Attrition from the SIME/DIME Sample

We define attrition as termination of a female head's periodic interview sequence.* Tables 8-1 and 8-2 give the percentages of originally enrolled female heads of household who dropped out of the experiment during the first 18 months by support level, race, and last-observed marital status for Seattle and Denver, respectively. The attrition levels are remarkably low compared to those experienced in other panel studies. Overall 6.9% of the eligible female heads of household failed to remain

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*We do not treat death of heads of households as an attrition. When the male head dies, we end the record for the marriage without recording a dissolution and place the female head in the single state. When the female head dies, the entire observation is ended.

Table 8-1

PERCENT OF ORIGINALLY ENROLLED FEMALE HEADS
OF HOUSEHOLD IN SEATTLE DROPPING OUT
OF THE EXPERIMENT WITHIN THE FIRST 18 MONTHS:
BY SUPPORT LEVEL, RACE, AND LAST-OBSERVED MARITAL STATUS
(Number of Cases in Parentheses)

	Blacks	Whites	Total
	Married at Last Observation		
Controls	13.9 (201)	4.6 (345)	8.1 (546)
$3,800 support	9.3 (86)	2.6 (116)	5.4 (202)
$4,800 support	5.3 (94)	2.0 (153)	3.2 (247)
$5,600 support	7.3 (55)	1.2 (82)	3.6 (137)
Total	10.3 (436)	3.3 (696)	6.0 (1132)
	Single at Last Observation		
Controls	6.4 (187)	7.7 (182)	7.0 (369)
$3,800 support	3.7 (108)	3.6 (110)	3.7 (218)
$4,800 support	1.7 (120)	2.7 (111)	2.2 (231)
$5,600 support	0 (35)	0 (28)	0 (63)
Total	4.0 (450)	4.9 (431)	4.4 (881)

in the experiment for the full 18 months. The New Jersey experiment, in contrast, experienced 11.2% attrition during the first one and one-half years.*

The impact of attrition on our analyses is even smaller than indicated in Tables 8-1 and 8-2 since we do use the observed portion of marital histories of women who drop from the experiment.

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*This does not count as attritions families who missed one or more interviews during the first six quarters but completed subsequent interviews. See Peck,[30] Table 1.

Table 8-2

PERCENT OF ORIGINALLY ENROLLED FEMALE HEADS
OF HOUSEHOLD IN DENVER DROPPING OUT
OF THE EXPERIMENT WITHIN THE FIRST 18 MONTHS:
BY SUPPORT LEVEL, RACE, AND LAST-OBSERVED MARITAL STATUS
(Number of Cases in Parentheses)

	Blacks	Whites	Chicanas	Total
	Married at Last Observation			
Controls	8.4 (202)	6.4 (251)	11.6 (190)	8.6 (643)
$3,800 support	11.0 (73)	10.5 (95)	4.7 (107)	8.4 (275)
$4,800 support	12.2 (90)	8.0 (137)	9.6 (115)	9.6 (342)
$5,600 support	6.9 (58)	2.1 (95)	7.4 (81)	5.1 (234)
Total	9.5 (423)	6.7 (578)	8.9 (493)	8.2 (1494)
	Single at Last Observation			
Controls	8.5 (199)	10.2 (128)	9.3 (118)	9.2 (445)
$3,800 support	6.6 (152)	9.2 (87)	9.6 (114)	8.2 (353)
$4,800 support	8.1 (111)	4.9 (81)	5.3 (76)	6.3 (268)
$5,600 support	5.7 (53)	7.7 (39)	4.9 (41)	6.0 (133)
Total	7.6 (515)	8.4 (335)	8.0 (349)	7.9 (1199)

A comparison of Table 8-1 with Table 8-2 indicates that attrition is greater in Denver than in Seattle. In Seattle, 5.3% of the women dropped out before the end of the 18th month compared with 8.1% in Denver.

The differences between Seattle and Denver reflect differences in the status of the preparation of data for analysis. The data files for Seattle have been updated more recently. The next updating of the Denver file will likely further lower the attrition rates in that city.

The control subsample shows that attrition is higher in Seattle than in Denver. For every race and marital status combination in Seattle, the controls are more likely than the experimentals to drop from the program.

The association between treatment and attrition is less clear in Denver. In three of the six race and marital status combinations, attrition is higher for at least one of the support levels than for the controls. While the relationship is less clear than in Seattle, controls in Denver as a whole are more likely than experimentals to drop from the program. We therefore conclude that for both sites, controls are more likely to leave the experiment before the end of the 18-month period than experimentals. Race is a factor in attrition. Whites are less likely than Blacks or Chicanas to drop from the program. There is no clear pattern of association of attrition and marital status at last observation.

There are reasons to suspect that attrition is affected by the outcomes that we study--marital status changes. Reports from our interviewers indicate that refusals to continue in the study are often associated with a recent marital status change. That is, female heads who marry and couples that separate may resist our attempts to complete interviews more than others. Also, marital status changes may lead to residential moving that makes it difficult to locate the individuals involved. Unfortunately, we cannot observe the effects of marital status change on attrition. Precisely those marital status changes that lead to attrition are unobserved by definition. We can investigate attrition empirically only in terms of the independent variables measured at the enrollment; i.e., those variables not subject to missing data due to attrition.

To measure the impact of various independent variables on attrition, we estimated linear probability models separately by race and by site. Our basic model contains all background variables used in analysis of marital status change as well as the experimental variables. None of the background variables (other than race) displays systematic effects on the probability of attrition. In fact, the best fitting equation (in Seattle--no equation is significant in Denver) contains only the support levels.

The results for this equation are presented in Table 8-3. None of the support level coefficients is significant for Denver. Four of the six coefficients for Seattle are significant and all are negative. For Blacks in Seattle, the medium support level has the strongest effect on attrition; for Whites in Seattle, the high support level has the strongest effect. In Seattle those women enrolled on the financial treatments are clearly less likely than the controls to drop out. While the coefficients are not significant, the results from Denver suggest the same conclusion. The fact that other variables do not systematically affect attrition suggests that net of financial treatment, race, site, and perhaps marital status change, attrition is random. We consider the consequences of associations among attrition, marital status change, and treatment in Section VIII B.

B. <u>The Problem of Attrition</u>

Whenever attrition is related to both independent and dependent variables, analysis of the surviving sample gives misleading results. Consider the hypothetical (intentionally exaggerated) example in Table 8-4. The two samples--experimental and control--have identical rates of marital status change of 10%. However, attrition is related additively to both experimental status (controls have higher rates) and to marital status change (those who change have higher rates). Attrition is also related nonadditively to the same two variables. (It is disproportionately high for controls who change their marital status.) Although the true rates of marital status change are the same in the two populations, the rates observed in the remaining samples differ substantially. The observed rate for the controls is 30% less than that for experimentals. Even in less extreme cases, the differences between controls and experimentals can be substantial. If everything were the same as the hypothetical example in Table 8-4, except the controls dropped from the program when their marital

Table 8-3

SUPPORT LEVEL EFFECTS ON ATTRITION DURING THE FIRST 18 MONTHS: ESTIMATES FROM LINEAR PROBABILITY MODEL

	Seattle		Denver		
	Black	White	Black	White	Chicana
Support level					
$3,800	-0.031*	-0.024*	-0.008	0.019	-0.029
$4,800	-0.039**	-0.019	0.011	-0.014	-0.029
$5,600	-0.025	-0.043**	-0.018	-0.038	-0.041
Constant	0.059***	0.043***	0.086***	0.077***	0.109***
R^2	0.01	0.01	0.001	0.005	0.003
F-ratio for equation	2.17*	2.49**	0.30	1.37	0.86
N	834	1061	899	882	820

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

status changed at a rate of 30% instead of 40%, the observed rate of marital status change for controls would be 0.08. If the rate of leaving the program for controls who change their marital status were only 20%--twice that of the nonchanging controls--the observed rate would be 0.09 instead of the true rate of 0.10.

Any nonrandom attrition of this type produces upward biases in estimates of experimental impacts on rates of marital status change. If, as we suspect, the relationship between marital status change and attrition in our sample is similar to this example, we have underestimated the true rate of marital status change among the controls. Thus, the positive experimental impacts reported in Section VI may be due to underestimating the rates for controls because of attrition rather than a true experimental

Table 8-4

BIAS DUE TO NONRANDOM ATTRITION:
HYPOTHETICAL EXAMPLE

	No Marital Status Change		Marital Status Change		
	Remain	Dropout	Remain	Dropout	Total
Experimental	855	45	95	5	1,000
Control	810	90	60	40	1,000
Total	1,665	135	155	45	2,000

	True rate of marital status change	Observed rate of marital status change
Experimental	100/1,000 = 0.10	95/950 = 0.10
Controls	100/1,000 = 0.10	60/870 = 0.07

impact on marital status change. That is, interactive associations between treatment, marital status change and attrition may have caused us to overestimate the experimental response.

C. Sensitivity Analysis

We conducted an analysis to determine the sensitivity of the results reported in Sections VI and VII to attrition. We formulated several hypotheses about the relationship between marital status change and attrition. We reestimated the experimental impacts using the log-linear rate model for each hypothesis.

Recall from Section V that the log-linear rate model analyzes spells of marriage or singleness. A woman enters a spell at enrollment or after a marital status change and remains there until she changes her marital status, drops out of the experiment, or the observation period ends.

Three pieces of information about each spell are used in the analysis: the time it begins, the time it ends, and whether or not it ends in a marital status change. The beginning time poses no problem for spells ending in attrition since it is observed in the same way for those who leave the experiment and those who remain. The end time for spells ending in attrition is the date of the last completed interview. Spells ending in attrition are coded as not ending in a marital status change indicating that during the time she was observed, the woman did not experience a marital status change.

We considered several assumptions about how the spells that ended in attrition would have ended if attrition had not occurred. For experimentals, we assumed that the proportion of spells ending in a marital status change for each combination of race, support level, and site was the same for those who dropped from the experiment as for those who stayed in the experiment. For controls, we assumed that the proportion of spells ending in a marital status change was greater for those who dropped from the experiment than for those who remained.

Specifically, we reconstructed our data under four hypotheses:

H1: The proportion of control attrition spells that would have ended in a marital status change is <u>twice</u> the proportion of control nonattrition spells that ended in a marital status change.

H2: The proportion of control attrition spells that would have ended in a marital status change is <u>three times</u> the proportion of control nonattrition spells that ended in a marital status change.

H3: The proportion of control attrition spells that would have ended in a marital status change is <u>five times</u> the proportion of control nonattrition spells that ended in a marital status change.

H4: <u>All</u> control spells ending in attrition would have ended in a marital status change.

Our reconstruction of the data changed a randomly selected subset of the experimental attritions to marital status changes so that for experimentals the proportion of spells ending in a marital status change for each combination of race, site, support level, and marital status was the same for those who dropped from the experiment as for those who stayed. This operationalized our assumption that attrition and marital status change are not associated within the experimental sample. Similarly, for each combination of site, race, and marital status, we changed randomly selected subsets of the control attritions to marital status change in accord with each hypothesis. We did not change data on spells not ending in attrition.*

We also assigned a new date to the attrition spells. Over 100 attritions occurred between enrollment and the first interview after enrollment so that the attrition date (date of last completed interview) was the first day of enrollment. A marital status change occurring on the first day of observation would unduly increase the estimated rates in the log-linear rate model [see Equation (16) of Section V]. Attrition <u>actually</u> occurred sometime between the last completed interview and the next scheduled interview, which usually occurred about four months later. To estimate the actual date of attrition, we selected a random number between 1 and 120 for each attrition, divided it by 365, and added that number to the date of the last completed interview to get a new end date (time is measured in years). For those attritions that were changed to marital status changes, we assumed that the new end time was the time of marital status change. With our new marital status change variables and our new

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*In this sensitivity analysis we were not able to distinguish spells ended by the death of a spouse from spells ended by attrition. This does not confound our analysis since deaths are infrequent and we have no evidence that there is any relationship between deaths and experimental treatment. This means that we have slightly overestimated attrition in this analysis.

end times, we then reestimated the equation with background, support level, and three-year treatment variables for each race and marital status.

D. Results of the Sensitivity Analysis

The results of our sensitivity analysis for dissolution are reported in Table 8-5 and for remarriage in Table 8-6. The columns labeled "original results" give the effects of the experimental treatments from Sections VI (Table 6-4) and VII (Table 7-4), respectively. The hypotheses about attrition increase the frequency of marital status change among the controls relative to the experimentals. Therefore, for each racial ethnic group the coefficients for each support level decrease from one hypothesis to the next.*

In Table 8-5 we see that even under the unrealistic hypothesis that <u>all</u> controls who leave the experiment have changed their marital status (H4), there are significant positive experimental impacts on dissolution. The coefficients for Blacks on the $4,800 support and for Whites on the $3,800 support are significant and positive even under this extreme hypothesis. Under the more realistic hypotheses that marital status change is two (H1) or three (H2) times more likely for controls who drop from the experiment as for other controls, the coefficients for both the $3,800 and $4,800 support levels are significantly positive for both Blacks and Whites. The results for Chicanas do not appear as robust. Under the more extreme hypotheses (H3 and H4), there are significant negative experimental impacts on dissolution for Chicanas.

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*For Blacks with the $4,800 support level the coefficient for H1 is greater than the original coefficient for both dissolution and remarriage. This indicates that the effect of changing some of the $4,800 support level attritions to marital status changes was greater than the effect of changing some of the controls.

Table 8-5

EXPERIMENTAL IMPACTS ON DISSOLUTION USING FOUR HYPOTHESES ABOUT ATTRITION: ESTIMATES USING LOG-LINEAR MODEL OF RATE OF DISSOLUTION†

	Support Level	Original Results	H1	H2	H3	H4
Black	$3,800	0.672**	0.626**	0.568**	0.423*	0.306
	4,800	0.836***	0.838***	0.774***	0.656***	0.506**
	5,600	0.523*	0.420	0.360	0.237	0.094
White	3,800	0.848***	0.792***	0.767***	0.718***	0.485**
	4,800	0.602**	0.504*	0.461*	0.401	0.157
	5,600	0.249	0.107	0.069	0.002	-0.273
Chicana	3,800	0.656*	0.522	0.392	0.225	0.145
	4,800	-0.007	-0.182	-0.250	-0.434	-0.570*
	5,600	-0.317	-0.494	-0.539	-0.742*	-0.873**

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain background variables and the three-year treatment variable as in Table 6-4.

In Section VII, we found that the experimental impacts on remarriage were generally less dramatic than those on dissolution. This is reflected in Table 8-6. However, the significant positive coefficients for Blacks on the $4,800 and $5,600 support levels appear to be fairly insensitive to attrition, retaining significance except for the more extreme hypotheses. The experimental effects on Whites become more negative but remain insignificant. The initially negative effects of supports on Chicana's remarriage rates become increasingly negative under more extreme hypotheses.

Table 8-6

EXPERIMENTAL IMPACTS ON REMARRIAGE USING FOUR HYPOTHESES ABOUT ATTRITION: ESTIMATES FOR THE LOG-LINEAR MODEL OF RATE OF REMARRIAGE†

	Support Level	Original Results	H1	H2	H3	H4
Black	$3,800	0.406	0.422	0.377	0.292	-0.165
	4,800	0.766**	0.773**	0.730**	0.654**	0.183
	5,600	0.767*	0.725*	0.676*	0.590	0.092
White	3,800	0.036	0.030	-0.030	-0.110	-0.275
	4,800	0.072	0.048	-0.025	-0.112	-0.287
	5,600	-0.007	-0.064	-0.128	-0.219	-0.373
Chicana	3,800	-0.360	-0.460	-0.450	-0.621	-0.753
	4,800	-1.08 *	-1.27 **	-1.23 **	-1.39 **	-1.51 ***
	5,600	-1.77 **	-1.95 **	-1.92 **	-2.10 **	-2.18 ***

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations contain background variables and the three-year treatment variable as in Table 7-4.

Our sensitivity analysis gives us greater confidence in the results reported in Sections VI and VII. For dissolution among Blacks and Whites and remarriage among Blacks, it appears that the positive experimental impacts cannot be accounted for by attrition. If the interaction between attrition and marital status change among the controls has led us to underestimate the incidence of marital status change among the controls (and we do believe that it has to some degree), the underestimate is not great enough to account for the positive experimental impacts observed.

IX SUMMARY AND CONCLUSIONS

This report addresses the question of whether or not income maintenance alters the rates of movement in and out of marital unions. The impact of income maintenance on the rate of marital dissolution and the rate of remarriage is of interest in itself. In this section, we present preliminary findings on these questions from the first 18 months of the SIME/DIME programs.

The pertinent theoretical and empirical literatures indicate that income maintenance may have complex effects on dissolution and remarriage For each of these outcomes, the literature suggests both positive and negative effects. If these are so, the direction of the total impact depends on which of the competing effects dominates. Consequently, we cannot specify a priori even the sign of the total effect.

In our analyses, we considered only the overall impact of income maintenance on dissolution and remarriage. We made no attempt to construct and test behavioral models of the processes that underlie the total impacts. Consequently, we use analytic designs that rely heavily on the experimental nature of the study. That is, we relate marital status changes during the first 18 months to experimental treatments and to variables that affect assignment to experimental treatment. These changes are, however, exogenous with respect to behavior during the experimental period. Our sample includes all women enrolled as either heads in dual-headed families or female heads of familes. Unlike the research conducted on the other income maintenance experiments (see Section V), we analyzed all events of marital status change that occurred to these women during the experimental period.

We used two different procedures for assessing impacts of income maintenance. The first procedure employs a linear probability model with observations from each six-month period pooled into a single equation. The alternative procedure employs a stochastic model of rare events in which the rate at which an event occurs is assumed to depend log-linearly on the set of exogenous variables. This model is estimated by maximum likelihood. We report the results from each procedure in an effort to determine the robustness of our findings. Surprisingly, we find high levels of agreement between the two sets of results.

Preliminary analysis indicated that we could pool observations from the two sites (that is, the set of site interactions with other exogenous variables were insignificant) but there were significant race-ethnic differences in response. Consequently, we report results separately by race but merge observations from Seattle and Denver.

We analyzed marital dissolution and remarriage separately. In this section, we first summarize the main results using these analyses. We then discuss the impact of the two sets of findings on the prevalence of female-headed families in the target population.

We find that income maintenance has powerful impacts on both dissolution and remarriage. For dissolution, the pattern of impacts is consistent among the three race-ethnic populations. For remarriage, the pattern of impact varies substantially, even the direction of the response, among the three race-ethnic populations.

A. Impact on Marital Dissolution

The overall impact of income maintenance is to increase the rate of marital dissolution. Controlling for variables that affect experimental assignment and for other background variables, women on the financial treatments have rates of dissolution that are significantly higher than the controls. This result holds for each of the three race-ethnic populations studied.

Most of our analysis concentrates on the impacts of the three levels of support. That is, we estimate the differences between women on each of the support levels and the controls. We find a striking nonmonotonic pattern of effects: the income maintenance impact is strongest and most systematic for the support levels closest to the control condition. Concretely, only for the lowest level of support ($3,800) do we find statistically significant impacts in each of the three populations. For Blacks (and for Whites in the log-linear analysis only) there is a significant medium support ($4,800) impact. Finally, the only evidence of a positive impact on the high support ($5,600) treatment is for Blacks in the log-linear analysis.

This paradoxical pattern of effects is found, regardless of the types of analysis employed or of the type of interactions effects estimated. Moreover, it is insensitive to the attrition problem. Even when we assume that all controls who left the experiment had an unobserved marital dissolution, the nonmonotonic pattern remains.

In Section VIII, we examined the sensitivity of these findings to nonrandom attrition from the experiment. In particular, we considered the hypothesis that we underestimate the rate of marital dissolution for the controls because those who leave the experiment have a higher, unobserved rate of dissolution than the controls that remain. We considered a variety of situations increasing in severity from the realistic hypothesis that all controls who left had an unobserved dissolution. The remarkable result is that the pattern of effects just summarized remains substantially unaltered under each of these hypotheses. For example, the low support effect is still statistically significantly positive even under the most extreme hypothesis. As a result, we are confident that our findings are not caused by nonrandom attrition.

Although this report is intended to establish the existence of income maintenance impacts rather than to explain observed impacts, we feel obliged to comment on the strong and persistent pattern of impacts on the rate of dissolution. Our discussion of the competing income and independence effects should have prepared the reader for the possibility of a nonmonotonic pattern of impacts. Since the hypothesized income and independence effects differ in sign, the experimental impact will change sign over a range of support levels whenever one effect dominates over one part of the range and the other effect dominates over other parts. In particular, if the independence effect dominates over lower levels of support and the income effect takes over at higher levels of support, the pattern of experimental impacts will be as we have observed.

In a sense the problem remains. The low support treatment does not differ substantially in financial terms from the combination of AFDC and food stamps.[31] Why, then, should there be a strong independence effect for low support income maintenance treatments? To answer this we must consider the nonpecuniary differences between the financial treatments and control (AFDC and food stamp) situations. There are several salient differences:

- Income maintenance presumably involves much less stigma than welfare (e.g., AFDC and food stamps). Women who would refuse to enroll on welfare because they object to adopting the role of the "disreputable poor," are unlikely to object to income maintenance. For such women, the addition of income maintenance to the control environment constitutes an important change in their dependence on existing marriages.

- Income maintenance guarantees are explained to all families on the experiment. Welfare programs are not announced to all those eligible for benefits. Presumably many women with no welfare experience are unaware that they would be eligible either for welfare were their marriage to end or of the levels of support available. We take great pains to explain that income maintenance guarantees apply outside of marriage. Therefore, while the two programs might differ little under full and perfect information, the introduction of income maintenance treatments changes the environment for all those women with less than full information about their welfare rights.

- The information content of income maintenance may have another effect--that of introducing a shock to the preexperimental equilibrium. The literature on marriage indicates that many unhappy and unfulfilling marriages are stable for long periods of time because the partners reach some kind of accommodation. The introduction of an income maintenance program into such a situation may focus attention on the problems in the marriage. That is, when we explain to heads of households that the guarantee applies outside the existing marriage, we may focus their attention on the marriage market and heighten their sense of dissatisfaction with the existing marriage. Of course, the sudden and obtrusive announcement to the family that AFDC has the same properties would have the same shock effect. We doubt that many families received such announcements during the period we studied, however.

- Income maintenance entails lower levels of transaction costs than does AFDC and other welfare programs. Compared with the welfare situation, each maintenance makes minimal demands on participants.

  Each of these differences increases the independence of women on an experimental treatment financially similar to welfare. At least one important difference between the two programs may not have such an effect.

- Benefits of income maintenance and of welfare may differ in certainty. Enrolled women may not believe (and therefore not discount) income maintenance guarantees. Because of the reimbursement of the positive tax, most families on financial treatments receive some cash transfers from income maintenance. This should increase the credibility of income maintenance. Nonetheless, some women may not believe that their benefits will continue if they leave their marriages. One can as well argue that the income maintenance program is more credible than welfare. Images of high levels of administrative discretion and arbitrariness in the application of eligibility rules may lead women without experience in welfare to discount welfare benefits more than income maintenance benefits.

There are differences between welfare and even the low support income maintenance treatment. To the extent that these differences bear on the independence of women from marriage, income maintenance should convey greater independence at the same support levels.

If stigma and information considerations are important, much of the independence effect of income maintenance may be relatively constant across support levels. In other words, the availability of a known, nonstigmatizing alternative to marriage may be critical in the sense that the program effect dominates the guarantee effect on independence. That is, the differences in independence between a woman on the low support treatment and one on the high support treatment may be small relative to the difference between the independence of a woman on a support treatment and a woman in the control situation. Under these circumstances, even a linear income effect could produce a nonmonotonic pattern of experimental impacts.

A simple model helps clarify these ideas. In Section VI we proposed that the probability of marital dissolution (D) is a function of family resources (R), and the resources available to the wife outside the marriage (R_w). Suppose that the relationship is approximately linear, i.e., for controls:

$$D = \alpha + \beta R_w - \gamma R \qquad \alpha, \beta, \gamma > 0.$$

Assume further that the low support treatment (S1) does not differ financially from the prevailing control situation (i.e., that R and R_w are both unchanged). These considerations suggest a set of nonpecuniary differences between the low support treatment and the control situation. To simplify, we represent this set of differences as a shift parameter that indicates the independence-producing potential of these nonpecuniary differences:

$$D(S1) = \alpha + \beta R_w - \gamma R + \delta \qquad \delta > 0 \quad .$$

Assume, finally, that both the medium (S2) and high (S3) support levels increase both R and R_w so that:

$$D(S2) = \alpha + \beta(R_w + \Delta_2 R_w) - \gamma(R + \Delta_2 R) + \delta$$

$$D(S3) = \alpha + \beta(R_w + \Delta_3 R_w) - \gamma(R + \Delta_3 R) + \delta$$

where $\Delta_2 R_w$ and $\Delta_2 R$ (both nonnegative) denote the changes in R_w and R from the control condition under the S2 treatment. The terms $\Delta_3 R_w$ and $\Delta_3 R$ are defined similarly with respect to the control condition.

Under these assumptions, the low support effect is always positive:

$$D(S1) - D = \delta > 0 \quad .$$

But, what about the difference between the low support and the other two financial treatments? We see that:

$$D(S2) - D(S1) = \beta \, \Delta_2 R_w - \gamma \, \Delta_2 R$$

$$D(S3) - D(S1) = \beta \, \Delta_3 R_w - \gamma \, \Delta_3 R$$

The first term on the right-hand side of each of the above expressions is the pecuniary independence effect of the treatment (relative to both control and S1 situations); the second term is the income effect. The nonmonotonic pattern of differences we observe, namely

$$D_{S3} - D_{S1} < 0 \qquad \text{(given that } D_{S1} - D > 0\text{)}$$

will arise whenever the income effect of the high support treatment dominates the pecuniary independence effect; that is, when:

$$|\beta \, \Delta_3 R_w| < |\gamma \, \Delta_3 R| \quad .$$

Thus, the combination of nonpecuniary income maintenance effects on independence that are relatively constant across treatments with strong income effects will produce the nonmonotonic pattern of impacts. Other more complex models that entail nonlinearities, such as thresholds in the income effects, have similar empirical consequences. We are not yet in the position to evaluate the various other models with data. Our future work will concentrate on modeling income effects and pecuniary and nonpecuniary independence effects to try to explain our findings.

B. Impact on Remarriage

The impacts on remarriage vary considerably across populations. For Black women income maintenance increases significantly the probability of remarriage and the impact increases monotonically with the level of support. For Chicanas the effects, which again are significant, are in the opposite direction. The overall effect on this group is to lower the rate of remarriage. The magnitude of this negative impact increases with the level of support. There is no discernible impact of income maintenance on remarriage rates of White women. Our findings on remarriage appear more sensitive to attrition than those on marital dissolution. But the pattern just discussed holds under our sensitivity analysis.

Appendix A

RECONCILIATIONS

Appendix A

RECONCILIATIONS

As we discussed in Section IV, approximately 10% of the marital dissolutions we observe were reconciled within six months. Rates of reconciliation over longer periods of time are of course higher. In this appendix we first consider possible alternative ways of handling reconciliations and then estimate the impact of our decision on the results reported in the body of this report.

A. Alternative Procedures

The most natural alternative procedure would be to impose a time limit on separations before we recorded them as dissolutions. For example, we might require an uninterrupted separation of six months. This altered definition of a dissolution would imply a dependent variable that is a compound or joint outcome in terms of the events we have defined: a dissolution at time t would be defined as either (1) a separation at t followed by remarriage to someone else, or (2) separation at t and no remarriage prior to t + 6 months.

This solution has several important flaws. The choice of waiting period is completely arbitrary. Should it be three months, six months, a year? While three months might be a very long separation to one person, a year might not be to another. We have no confidence in our ability to make interpersonal comparisons of time intensities. Lacking any better criterion, we are content to rely on our respondents' reports of permanence (however long that may be in real time).

The alternative also results in a loss of observations in two ways. First, the observation period must be shortened from 18 months to 18 months less the waiting period. For example, if we were to choose a six-month period, we could analyze only those separations that occurred during the first 12 months (to see whether any reconciliation occurred).* Second, we would lose the observations on all women who change their marital status and then leave the sample before the end of the waiting period.

The alternative would produce serious complications in the analysis of remarriage. With our procedure, a woman becomes eligible ("at risk") of becoming remarried the day that her marriage is dissolved. But, with a waiting time, how would we determine which women are at risk of becoming remarried? If we assume that all separated women (within the waiting period) as well as divorced women can remarry at any instant, then we have an inconsistency with our definition of separation: How can a woman remarry if she has not yet dissolved her previous marriage? On the other hand, to assume that only those women who actually remarried during the waiting period were at risk of remarriage would completely confound the issue. A worse case is women who actually remarried within the waiting period would be included in the remarriage analysis while those who did not would not. This procedure would be ruinous to statistical inference.

Such a procedure would lead to underestimates of the costs of an income maintenance program. When computing payments, it would be impossible to treat women who will eventually reconcile differently from those who will not. A woman would receive the same support while single

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*Note that we would lose even those events that occurred after 18 months less the waiting period that were followed by remarriage to some other person (where reconciliation is by definition impossible). To include those events in the sample would amount to selecting the sample by an endogenous variable (remarriage); selection by endogenous information ordinarily leads to biased estimation.

(assuming that the separation from her husband is intended to be permanent) whether or not she will eventually reconcile. Thus from a policy perspective, we do not want to exclude those who reconcile from the analysis.

For the foregoing reasons, we are convinced that it is preferable not to impose any requirement on separations other than that those involved declare them to be permanent. Moreover, we believe that we can achieve what is intended by the alternative just discussed by methods that do not produce such complications. What we propose is to investigate the determinants of reconciliation <u>conditional on dissolution</u>. To do this, we analyze the women who have dissolved a marriage during the experimental period. Within this limited sample, we can investigate how experimental treatments and other variables affect the likelihood of reconciliation. Having done so, we can obtain preliminary estimates of the permanent effects of the treatments by combining the unconditional estimates of the effects of treatments on dissolution with the estimates of the conditional (given dissolution) effects of the same treatments on reconciliation.

B. Estimated Impact of Reconciliation on Rates of Marital Status Changes

If reconciliations are more common under income maintenance, our estimates of impacts on both marital dissolution and remarriage rates are somewhat inflated relative to those that would be calculated using more restrictive definitions of marital status changes. To obtain an indication of how much difference a change in definition would produce, we conduct a linear probability analysis of income maintenance effects on reconciliation.

To transform our estimates to those that could be produced under a rule that did not count dissolutions followed by reconciliations, we do the following. Let

$$D^* = D(1 - R|D)$$

where

- D is the probability of dissolution we report for some groups.
- D^* is the probability for the same group disregarding the dissolutions that were reconciled within the observation period.
- $R|D$ is the probability of reconciling conditional on dissolution.

That is, our rate should be discounted by one minus the conditional probability of dissolution. If for example, the probability of dissolution in some treatment is 0.20 (higher than the controls) and the probability of reconciling is also 0.20, the revised estimate of the effect is 0.20 (0.80) = 0.16.

There is no completely satisfactory way to estimate income maintenance impacts on reconciliation. We discussed a number of problems in the previous section. In addition, the sample of women that experienced a marital dissolution during the observation period is dependent on the treatments. If the treatment effects interact with unobserved variables causing differences in probabilities of marital status change, analysis of the endogenously determined sample will give biased estimates of impacts on reconciliation. In addition, analysis of the "dissolution sample" gives us very small sample sizes.

Despite all these difficulties, it is worthwhile to explore the qualitative patterns of effects. To do so, we define our sample to include all women who have a dissolution of marriage within the first 18 months and whom we observe for at least six months following the dissolution. The dependent variable is equal to one if the couple reconciles at any time within our observation record without an intervening new marriage and zero otherwise. To enable us to consider the extreme case, we do not place any time limit on the separation period, using our entire observation record which, for many women, extends beyond 30 months. Consequently, the probability of reconciliation is considerably higher than we estimated in Section IV, using six- and 12-month "waiting periods."

Linear probability estimates of experimental impacts are presented in Table A-1. Due, no doubt, to the small sample sizes, none of the equations is significant. Yet, there appear to be income maintenance effects. For Blacks and Whites, there are large (though not significant) positive effects of the support levels (with the exception of the high support for Blacks). For Chicanas (who have the highest mean probability of reconciling) the support effects are negative. At the same time, there are positive tax effects (relative to the excluded 50% tax) for this group; and, the 70% declining tax, which had a significant positive effect on dissolution, has a similar effect on reconciliation. In general, we find substantial positive effects on reconciliation for those treatments that exhibit positive effects on dissolution. In only one combination of support level and tax rates (Chicana low support--70% declining tax) does the financial treatment effect on the probability of reconciliation exceed 0.33. Modal effects are closer to 0.20.

Given these findings, we see that eliminating dissolutions followed by reconciliations will lead one to discount our earlier estimates of impacts by 20% to 30% in most cases. Thus, even under the most restrictive definition of marital status changes, our earlier qualitative conclusions would remain unaltered.

Table A-1

INCOME MAINTENANCE EFFECTS ON RECONCILIATION
LINEAR PROBABILITY ESTIMATES†
(Standard Errors in Parentheses)

	Blacks	Whites	Chicanas
Support level			
\$3,800	0.051 (0.141)	0.201 (0.126)	-0.027 (0.222)
4,800	0.059 (0.138)	0.126 (0.152)	-0.260 (0.247)
5,600	-0.031 (0.173)	0.269 (0.208)	-0.228 (0.352)
Tax rate			
70%	0.131 (0.135)	-0.135 (0.128)	0.310 (0.191)
70% declining	0.184 (0.151)	-0.058 (0.125)	0.572** (0.252)
80% declining	0.016 (0.139)	-0.148 (0.144)	0.308 (0.289)
Three-year treatment	0.047 (0.105)	-0.133 (0.096)	-0.190 (0.165)
R^2	0.124	0.141	0.292
F-ratio for equation	0.75	0.77	1.12
N	135	115	68
Mean of the dependent variable	0.22	0.15	0.31

$^{*}p < 0.10$ $^{**}p < 0.05$ $^{***}p < 0.01$

†All equations include the background variables of Table 6-2.

Appendix B

CONTINUOUS TIME ANALYSIS OF THE TIMING OF MARITAL STATUS CHANGE

Appendix B

CONTINUOUS TIME ANALYSIS OF THE TIMING OF MARITAL STATUS CHANGE

In this appendix we outline a general framework for continuous time analysis of the timing of marital status changes when probabilities of a change in marital status may depend both on exogenous conditions (including experimental treatments) and on time. First, we describe a general stochastic process generating sequences of spells of marriage and nonmarriage and indicate how such a process can be analyzed by applying the method of maximum likelihood estimation to observations on dates of marital status changes. We then explain why limitations on available information on women's marital histories necessitate several restrictions on assumptions about the stochastic process governing the making and breaking of marital unions. Finally, we describe an additional assumption that simplifies analytic procedures.

The aspects of a woman's marital history considered in this document are the sequence of points in time at which her marital status changes. Let s represent marital status and take the values 0 = not married and 1 = married.* In addition, let j(s) represent the number of previous spells in which the woman has had marital status s, where j(s) takes nonnegative integer values. We define t_0 as a woman's birthdate and assume that at t_0 every female is not married. We also assume that a spell of nonmarriage always follows the termination of a woman's union with a particular man. Then $k = s + 2j(s) + 1$ equals the number of all

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*See definition of marriage in Section IV.

spells (both married and not married) that a woman has had. We define t_k as the date that the woman leaves spell k and enters spell (k+1). Because we assume that a woman begins life not married and alternates between nonmarriage and marriage, t's with odd-valued subscripts refer to termination dates of spells of nonmarriage and to starting dates of spells of marriage, while t's with even-valued subscripts refer to termination dates of spells of marriage and to starting dates of spells of nonmarriage. Thus the timing of a woman's marital status changes can be represented by the strictly ordered sequence $\{t_k\} = \{t_1, t_2, \ldots, t_M\}$, where $t_0 < t_1 < t_2 < \ldots < t_M$ and M is the maximum number of marital status changes examined.

While our ultimate objective is to estimate the experimental impact on the timing of marital status changes occurring within the SIME/DIME experimental period, problems presented by the experimental analysis can be understood more clearly if first preceded by a discussion of general problems involved in estimating the effects of a vector of exogenous variables V on the timing of marital status changes.

We begin with the following assumptions: (1) the date at which a woman terminates the kth spell in her marital history is a random variable T_k for $k = 1,2,\ldots,M$; (2) conditional on variables V, the sequence of random variables $\{T_k\} = \{T_1, T_2, \ldots, T_M\}$ is independently and identically distributed for all women in the population being studied.

We define the conditional, joint probability distribution of the sequence of random variables $\{T_k\}$ as follows:

$$F_{T_1,T_2,\ldots,T_M|V}\,(t_1,t_2,\ldots,t_M|V) = \Pr(T_1 \leqq t_1, T_2 \leqq t_2, \ldots, T_M \leqq t_M|V) \tag{1}$$

where $t_0 < t_1 < t_2 < \ldots < t_M$ and $t_0 \in V$. The conditional, joint probability density function, which is assumed to exist, is defined as follows:

$$f_{T_1,T_2,\ldots,T_M|V}(t_1,t_2,\ldots,t_M|V) = \frac{\partial^M F_{T_1,T_2,\ldots,T_M|V}(t_1,t_2,\ldots,t_M|V)}{\partial t_1 \partial t_2 \cdots \partial t_M} \quad . \tag{2}$$

A definition often used on our subsequent discussion is that

$$\begin{aligned}
&f_{T_1,T_2,\ldots,T_M|V}(t_1,t_2,\ldots,t_M|V) \\
&= f_{T_M|V,T_1,\ldots,T_{M-1}}(t_M|V,t_1,\ldots,t_{M-1}) \cdot f_{T_1,\ldots,T_{M-1}|V}(t_1,\ldots,t_{M-1}|V) \\
&= f_{T_M|V,T_1,\ldots,T_{M-1}}(t_M|V,t_1,\ldots,t_{M-1}) \cdot f_{T_{M-1}|V,T_1,\ldots,T_{M-2}}(t_{M-1}|V,t_1,\ldots,t_{M-2}) \\
&\quad \cdots f_{T_2|V,T_1}(t_2|V,t_1) \cdot f_{T_1|V}(t_1|V) \\
&= f_{T_2,\ldots,T_M|V,T_1}(t_2,\ldots,t_M|V,t_1) \cdot f_{T_1|V}(t_1|V) \quad .
\end{aligned} \tag{3}$$

If sample observations are available and we know the form of the conditional, joint probability density [any of the equivalent expressions given in Equation (3)], then we can assess the effects of variables in V on the stochastic process describing the timing of marital status changes by the method of maximum likelihood (ML) estimation. Under quite unrestrictive conditions (see e.g., Dhrymes, 1970) this method yields estimators of parameters that are sufficient, consistent, and asymptotically normally distributed.

ML estimators are defined to be those parameter values that maximize the likelihood function L, where L is the joint probability density of all sample observations. If, as assumed above, observations on the $i = 1,2,\ldots,N$ individuals in the sample are independent and identically distributed, then L is the product of the probability densities of observations on each person in the sample. Thus, in the present problem,

$$L = \prod_{i=1}^{N} f_{T_1,T_2,\ldots,T_M|V}(t_{1i},t_{2i},\ldots,t_{Mi}|V_i) \quad . \tag{4}$$

Equation (4) assumes that the entire sequence $\{t_{1i},t_{2i},\ldots,t_{Mi}\}$ is observed for all individuals in the sample, $i = 1,2,\ldots,N$. But a problem in our research (as in many other panel studies) is for certain observations in this sequence to be missing for some persons in the sample. We are particularly concerned with information loss arising because only marital status changes occurring between $t_{\alpha i}$ and $t_{\omega i}$, $t_{\alpha i} < t_{\omega i}$, can be observed. One can think of $t_{\alpha i}$ and $t_{\omega i}$ as the first and last dates, respectively, at which observations are recorded for individual i.

Information loss occurring because $t_{\omega i} < t_{wi}$ for some i and w is termed right-hand side censoring of observations and can readily be handled within the context of maximum likelihood estimation. First, suppose that $w = M$, i.e., $t_{1i} < t_{2i} < \ldots < t_{M-1,i} < t_{\omega i}$ but $t_{\omega i} < t_{Mi}$. In this case the probability density of the ith woman's observations (and her contribution to the likelihood equation) is simply the product of the probability density of the sequence $\{t_{1i},\ldots,t_{M-1,i}\}$ conditional on V_i and the probability that the Mth spell has not ended by t_{wi}, given V_i and the sequence $\{t_{1i},\ldots,t_{M-1,i}\}$:

$$\left[1 - F_{T_M|V,T_1,\ldots,T_{M-1}}(t_{\omega i}|V_i,t_{1i},\ldots,t_{M-1,i})\right] \cdot$$

$$f_{T_1,\ldots,T_{M-1}|V}(t_{1i},\ldots,t_{M-1,i}|V_i) \quad . \tag{5}$$

Next, suppose that we observe $t_{1i} < t_{2i} < \ldots < t_{w-1,i} < t_{\omega i}$ but that $t_{\omega i} < t_{wi} < \ldots < t_{Mi}$. Note that

$$\Pr\left[T_{w+h} > t_{\omega i}|T_w > t_{\omega i}\right] = 1 \qquad \text{for } h = 1,2,\ldots,M-w \quad . \tag{6}$$

This is true because we assumed that the sequence of t_k's is strictly ordered. Hence the probability density of the ith woman's observations (and her contribution to the likelihood equation) when $t_{\omega i} < t_{wi}$ is the product of the joint density of the sequence $\{t_{1i},\ldots,t_{w-1,i}\}$ conditional on V_i and the probability that the wth spell has not ended by $t_{\omega i}$, given V_i and the sequence $\{t_{1i},\ldots,t_{w-1,i}\}$:

$$\left[1 - F_{T_w|V,T_1,\ldots,T_{w-1}}(t_{\omega i}|V_i,t_{1i},\ldots,t_{w-1,i})\right] \cdot$$

$$f_{T_1,\ldots,T_{w-1}|V}(t_{1i},\ldots,t_{w-1,i}|V_i) \quad . \qquad (7)$$

To handle right-hand side censoring in this way, we must at least assume that conditional on V, the same probability distribution function describes both censored and noncensored observations. This assumption is reasonable in our research.

Information loss occurring because values of t_{ai} are unknown for some i and a when $t_{ai} < t_{\alpha i}$ is termed left-hand side censoring of observations and is generally much more serious than right-hand side censoring. This is because we commonly assume that while the past may cause the future, the future does not cause or affect the past. If we assume that the timing of <u>any</u> marital status changes prior to $t_{\alpha i}$ affect the timing of <u>any</u> marital status changes after $t_{\alpha i}$, then left-hand side censoring of observations fatally damages our ability to estimate effects of variables in V on the stochastic process describing the timing of marital status changes. This can readily be seen by noting in Equations (4) through (7) that in general the conditional probability density of $t_{a+h,i}$, where h takes positive integer values and $t_{ai} < t_{\alpha i} \leqq t_{a+1,i}$, depends not only on V_i but also on the unobserved sequence $\{t_{1i},\ldots,t_{ai}\}$, and furthermore that this unobserved sequence depends also on V_i. Consequently, to estimate the effect of variables in V_i on the timing of marital status changes occurring after $t_{\alpha i}$, it is necessary to make assumptions that are more restrictive than those made before this point.

There is an assumption that permits us to assess the influence of variables in V on the stochastic process that describes the timing of marital status changes, even when the timing of marital status changes are unknown for $t_a < t_\alpha$. This assumption--a type of first-order Markov assumption--is that

$$f_{T_{k+1}|V,T_1,\ldots,T_k}(t_{k+1}|V,t_1,\ldots,t_k) = f_{T_{k+1}|V,T_k}(t_{k+1}|V,t_k) \tag{8}$$

where $k > a$. When Equation (8) holds and both right- and left-hand side censoring of observations occur, the likelihood function has the following form:

$$L = \prod_{i=1}^{N}\left[\prod_{k=a+1,i}^{w-1,i} f_{T_k|V,T_{k-1}}(t_{ki}|V_i,t_{k-1,i})\right]\cdot$$

$$\left[1 - F_{T_\omega}|V,T_{w-1,i}(t_{\omega i}|V_i,t_{w-1,i})\right] \quad . \tag{9}$$

In the experimental analysis both right- and left-hand side censoring is present. We assume that $t_{\alpha i}$ is the date of the last marital status change before t_{ei}, the date of enrollment on the experiment, and $t_{\omega i}$ is either the end of the experimental period of study (e.g., 18 months after t_{ei}) or the last observation date, whichever occurs first.

The experimental analysis presents still another problem, requiring still another restrictive assumption. While we know the marital status at t_e of every female head enrolled in SIME/DIME, which we denote by s_e, we do not know the number of times that she has held this status, which we denote by $j_e = j(s_e)$. Consequently, it is necessary to assume that the stochastic process governing transitions from marriage to nonmarriage, and vice versa, depends only on marital status s and not on the number of times j(s) that the status has previously been held. More formally, we assume:

$$f_{T_{s+2j(s)+2}|V,T_{s+2j(s)+1}}(t_{s+2j(s)+2}|V,t_{s+2j(s)+1})$$

$$= f_{T_{s+2}|V,T_{s+1}}(t_{s+2}|V,t_{s+1}) \quad \text{for } j = 0,1,\ldots,J \text{ and } s = 0,1 \ . \qquad (10)$$

This assumption implies that the likelihood equation has the following form:

$$L = \prod_{i=1}^{N} \left\{ \prod_{k=a+2,i}^{w-1,i} \left[f_{T_m|V,T_n}(t_{ki}|V_i,t_{k-1,i}) \right]^{s_k} \left[f_{T_n|V,T_m}(t_{ki}|V_i,t_{k-1,i}) \right]^{1-s_k} \right\} \cdot$$

$$\left[1 - F_{T_m|V,T_n}(t_{\omega i}|V_i,t_{w-1,i}) \right]^{s_{wi}} \left[1 - F_{T_n|V,T_m}(t_{\omega i}|V_i,t_{w-1,i}) \right]^{1-s_{wi}}$$

(11)

where T_n and T_m are random variables describing the dates at which periods of singleness and marriage respectively are terminated and s_k is 1 if the kth spell of individual i is a marriage (k is even) and 0 otherwise (k is odd).

Because conditional probability densities in the above equation depend not only on exogenous variables V but sometimes on endogenous information--namely, the dates of marital spells begun during the experimental period--it may seem that this equation should not be used in an impact study such as our present one. But, given our previous assumptions, the joint probability density of the timing of all marital status changes occurring during the experimental period identically equals the product of the conditional probability densities appearing in Equation (11).

All restrictive assumptions leading to Equation (11) have been necessitated by limitations on the data available from SIME/DIME. An additional assumption that is not essential, but simplifies the analysis, is that the conditional probability densities of terminating spells of marriage and of nonmarriage do not depend on one another. This assumption, in combination with previous assumptions, implies that the likelihood function in Equation (11) can be partitioned into the product of two functions that can be maximized separately to give the same ML estimators as would be obtained by maximizing L in Equation (11). One function L_n is the product of conditional probability densities of forming a marital union, while the other L_m is the product of conditional probabilities densities of breaking a marital union. The two equations are as follows:

$$L_m = \prod_{i=1}^{N} \left\{ \prod_{k=a+2,i}^{w-1,i} \left[f_{T_m|V,T_n}(t_{ki}|V_i,t_{k-1,i}) \right]^{s_k} \right\} \cdot \left[1 - F_{T_m|V,T_n}(t_{\omega i}|V_i,t) \right]^{s_{wi}} \tag{12}$$

$$L_n = \prod_{i=1}^{N} \left\{ \prod_{k=a+2,i}^{w-1,i} \left[f_{T_n|V,T_m}(t_{ki}|V_i,t_{k-1,i}) \right]^{1-s_k} \right\} \cdot \left[1 - F_{T_n|V,T_m}(t_{\omega i}|V_i,t_{w-1,i}) \right]^{1-s_{wi}} \tag{13}$$

These equations are the basis of the continuous time analysis of the timing of marital status changes in this report; further details of our procedure are given in Section C-2 of Section V.

Appendix C

EFFECTS OF BACKGROUND VARIABLES ON MARITAL DISSOLUTION AND REMARRIAGE ESTIMATED USING A LOG-LINEAR RATE MODEL

Table C-1

EFFECTS OF BACKGROUND VARIABLES ON MARITAL DISSOLUTION:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF DISSOLUTION†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Normal earnings level						
$1,000-$2,999	-0.029	(0.409)	-0.514	(0.398)	1.26	(1.05)
3,000- 4,999	-0.261	(0.357)	-0.569	(0.371)	1.48	(1.03)
5,000- 6,999	-0.695*	(0.364)	-0.664*	(0.365)	1.57	(1.03)
7,000- 8,999	-0.626*	(0.370)	-1.11	(0.405)	1.45	(1.07)
9,000-10,999	-0.249	(0.381)	-1.28**	(0.466)	1.02	(1.13)
Unclassified	-0.065	(0.519)	0.127	(0.541)	2.06*	(1.24)
Education	-0.120**	(0.049)	-0.012	(0.045)	-0.081	(0.066)
Wage rate	0.430	(0.158)	0.326**	(0.150)	0.634	(0.501)
Age	-0.058***	(0.011)	-0.044***	(0.012)	-0.042**	(0.018)
AFDC (0,1)	0.345	(0.210)	0.380*	(0.207)	0.670***	(0.253)
Children aged 5 and under (0,1)	-0.022	(0.269)	0.074	(0.299)	-0.413	(0.381)
Children aged 9 and under (0,1)	-0.356	(0.289)	-0.158	(0.330)	0.412	(0.447)
Denver (0,1)	0.391**	(0.176)	-0.178	(0.173)	--	--
Constant	0.255	(0.794)	-1.24*	(0.747)	-3.23**	(1.55)
N	944		1342		571	
Likelihood ratio test statistic for equation	75.20***		72.84***		36.92***	

*p < 0.10 **p < 0.05 ***p < 0.01

†All equations include the experimental variables of Table 6-4.

Table C-2

EFFECTS OF BACKGROUND VARIABLES ON REMARRIAGE:
ESTIMATES FOR A LOG-LINEAR MODEL OF THE RATE OF REMARRIAGE†
(Standard Errors in Parentheses)

	Blacks		Whites		Chicanas	
Normal earnings level						
$1,000-$2,999	-0.650*	(0.354)	0.277	(0.359)	-0.023	(0.543)
3,000- 4,999	-0.523	(0.343)	0.598	(0.352)	0.557	(0.518)
5,000- 6,999	-0.615	(0.426)	0.432	(0.388)	0.612	(0.549)
7,000- 8,999	-0.166	(0.433)	-0.197	(0.490)	-0.393	(0.885)
9,000-10,999	0.804*	(0.482)	1.22**	(0.549)	1.46	(0.823)
Unclassified	-0.106	(0.502)	0.793*	(0.471)	-0.879	(1.11)
Education	-0.095	(0.066)	0.015	(0.055)	-0.0001	(0.074)
Wage rate	0.031	(0.209)	0.323**	(0.155)	0.403	(0.491)
Age	-0.053***	(0.016)	-0.074***	(0.014)	-0.087***	(0.023)
AFDC (0,1)	-0.023	(0.026)	0.144	(0.212)	-0.043	(0.359)
Children aged 5 and under (0,1)	-0.114	(0.304)	-0.051	(0.263)	-0.635	(0.421)
Children aged 9 and under (0,1)	0.099	(0.354)	-0.186	(0.311)	0.468	(0.496)
Denver (0,1)	0.132	(0.023)	0.018	(0.190)	--	--
N	980		833		368	
Likelihood ratio for equation	41.34***		68.68***		34.81***	

*$p < 0.10$ **$p < 0.05$ ***$p < 0.01$

†All equations include the experimental variables of Table 7-4.

Appendix D

PERCENTAGE DISTRIBUTION OF SIME/DIME FEMALE HEADS OF HOUSEHOLD OVER SELECTED BACKGROUND CHARACTERISTICS AND EXPERIMENTAL TREATMENTS

BACKGROUND CHARACTERISTICS

	Blacks	Whites	Chicanas
Number of cases	1,824	2,040	842
Percent married at enrollment	51.3%	63.7%	63.3%
Normal earnings level			
Less than $1,000	3.8%	2.3%	3.4%
$1,000-$2,999	9.0	7.2	6.7
3,000- 4,999	13.5	12.4	16.5
5,000- 6,999	20.5	19.1	23.3
7,000- 8,999	21.9	24.9	24.5
9,000-10,999	19.6	22.7	15.1
11,000-12,999	10.9	10.9	9.9
Unclassified	0.7	0.6	0.7
Education (years)			
0-8	8.3	8.2	24.8
9-11	31.3	27.5	46.2
12	42.3	44.2	24.8
13 and over	13.4	17.8	2.4
Missing	4.7	4.1	1.8
Age at enrollment			
Less than 18	0.4	0.7	1.1
18-24	21.8	27.6	31.1
25-34	36.7	37.2	35.0
35-44	21.5	18.1	17.9
45 and over	15.9	14.1	12.4
Missing	3.8	2.3	2.5
Wage rate (dollars per hour)			
Less than $1.75	16.0	22.2	22.7
$1.75-$2.00	20.3	23.2	35.0
2.01- 2.25	22.2	20.4	28.9
2.26- 2.50	14.8	13.9	7.8
2.51 and over	21.3	18.2	1.9
Missing	5.4	3.0	3.3
Number of children			
0	6.8	7.0	2.6
1	21.6	26.6	17.8
2	26.9	29.5	30.0
3	22.4	20.3	24.7
4 or more	22.3	16.6	24.9
Missing	0.3	0.3	0.4
Percent who were on AFDC prior to enrollment	34.0	24.8	39.3

EXPERIMENTAL TREATMENTS

		Blacks	Whites	Chicanas
Financial treatment				
Support Level	Tax Rate			
Controls		43.3%	44.4%	36.6%
$3,800	50%	9.2	8.1	8.6
$3,800	70%	4.8	3.7	6.4
3,800	70% (declining)	5.3	4.4	6.1
3,800	80% (declining)	3.7	3.8	5.2
$4,800	50%	4.7	5.8	5.2
4,800	70%	7.3	7.3	6.7
4,800	70% (declining)	5.6	5.9	4.2
4,800	80% (declining)	5.2	4.7	6.7
5,600	50%	1.9	2.2	2.7
5,600	70%	3.6	4.3	5.0
5,600	80% (declining)	5.5	5.2	6.8
Manpower treatment				
Control		41.1	42.3	38.2
M_1		18.2	19.0	20.5
M_2		24.8	23.6	26.1
M_3		14.9	15.1	15.1
Percent on three-year program		80.4%	80.4%	82.2%

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SEATTLE AND DENVER INCOME MAINTENANCE EXPERIMENTS RESEARCH MEMORANDA*

The following SIME/DIME Research Memoranda are available upon written request to:

Center for the Study of Welfare Policy
Stanford Research Institute
200 Middlefield Road
Menlo Park, California 94025

Research Memorandum Number	Title and Authors
15	*The Assignment Model of the Seattle and Denver Income Maintenance Experiments,* J. Conlisk and M. Kurz, July 1972.
18	*The Design of the Seattle and Denver Income Maintenance Experiments,* M. Kurz and R. G. Spiegelman, May 1972.
19	*The Payment System for the Seattle and Denver Income Maintenance Experiments,* M. Kurz, R. G. Spiegelman, and J. A. Brewster, June 1973.
21	*The Experimental Horizon and the Rate of Time Preference for the Seattle and Denver Income Maintenance Experiments: A Preliminary Study,* M. Kurz, R. G. Spiegelman, and R. W. West, November I973.
22	*Social Experimentation: A New Tool in Economic and Policy Research,* M. Kurz and R. G. Spiegelman, November I973.
23	*Measurement of Unobservable Variables Describing Families,* N. B. Tuma, R. Cronkite, D. K. Miller, and M. Hannan, May I974.
24	*A Cross Sectional Estimation of Labor Supply for Families in Denver 1970,* M. Kurz, P. Robins, R. G. Spiegelman, R. W. West, and H. Halsey, November 1974.
25	*Job Search: An Empirical Analysis of the Search Behavior of Low Income Workers,* H. E. Felder, May 1975.
26	*Measurement Errors in the Estimation of Home Value,* P. Robins and R. W. West, June 1975.
27	*A Study of the Demand for Child Care by Working Mothers,* M. Kurz, P. Robins, and R. G. Spiegelman, August 1975.
28	*The Impact of Income Maintenance on the Making and Breaking of Marital Unions: Interim Report,* M. Hannan, N. B. Tuma, and L. P. Groeneveld, June 1976.

*Research Memoranda 1 through 14, 16, 17, and 20 are obsolete and are not available for distribution.