Marriage and Economic Incentives: Evidence from a Welfare Experiment

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Abstract

Can economic incentives be used to affect marriage behavior and slow the growth of single-parent families? This paper provides new evidence on the effects of welfare benefit levels on the marital decisions of poor women. Exogenous variation in welfare benefits arises from a randomized experiment carried out in California. Whereas previous studies have measured women's responses to year-to-year changes in welfare benefit changes, I am able to measure responses over longer periods of time. The analysis recognizes that married women can receive AFDC benefits under some circumstances, and distinguishes between transitions into marriage and transitions out of marriage. I find that higher benefits encourage aid recipients who are married to divorce, but have little effect on the probability that single-parent aid recipients marry. The effects on married recipients become larger over time and do not appear to be measuring responses to other, simultaneous changes in California's welfare system. For policy purposes, these results suggest that states can increase the incentive to stay married by increasing two-parent benefits *relative* to single-parent benefits, an option that only recently became allowed by federal welfare reform.

I. Introduction

"The decline of the American family" has been a catch-phrase applied to a variety of demographic trends in recent decades. The trend that is probably most responsible for this view is the increasing prevalence of families headed by unmarried women. The proportion of children living with only one parent increased from 12 percent in 1970 to 28 percent in 1996 (U.S. Department of Commerce 1997a). Female headship is of interest to economists because it is highly correlated with poverty: the poverty rate for female-headed families was 33 percent in 1996 compared to just 6 percent for married-couple families (U.S. Department of Commerce 1997b). Thus, two avenues that policymakers have taken to reduce poverty are to discourage women from having children out of wedlock and to encourage couples to stay married.[1] A central policy concern is whether economic incentives can be used effectively toward these ends.

Scholars and politicians alike have assumed that economic incentives matter, often blaming the welfare system for contributing to the rise in female headship. The main cash welfare program available to poor families with children—Aid to Families with Dependent Children (AFDC)—was explicitly created to give benefits to single parents with children under age 18. Because benefits are conditioned on marital status, opponents of the welfare system have long argued that the system discourages marriage and encourages divorce. Although there is no doubt about the existence of these incentives, there has been continued disagreement over the degree to which the incentives actually affect behavior of individuals in an adverse way. The comprehensive welfare reform of 1996 gave states much flexibility to customize their welfare programs to succeed the now-defunct AFDC program; these programs, now called Temporary Assistance to Needy Families (TANF), retain their emphasis on single-parent poverty. As individual states take greater advantage of their new freedom to change the welfare laws in the future, they will increasingly grapple with at least two questions. First, how can economic

[1] This motivation presumes that female headship is the *cause* of poverty, and that it is not other characteristics of women who become female heads that causes them to be poor.

incentives be used to change family structure in desirable ways? Second, will changes in the relative size of single-parent and two-parent welfare benefit entitlements have unintended consequences for family stability? This study aims to provide some basis for answering these policy questions.

Moffitt's (1997) review of the literature on welfare's effects on family structure indicates that there is not a uniform set of findings across studies. While there are more studies that find a significant effect of welfare benefits than there are that find no significant effect, no consensus about the size of the effect has emerged due to the problems of inference based on either cross-state comparisons or within-state, over-time comparisons. Cross-state comparisons, which essentially rely on comparing state marriage rates with state welfare benefit levels, suffer from the problem that a negative correlation between marriage and welfare benefits may be due to an unobserved third factor such as social norms (Ellwood and Bane 1985). Within-state, over-time comparisons (or "state fixed-effect" models) mitigate this problem by subtracting out influences of time-invariant unobserved state characteristics. The remaining problem is to ensure that an individual state's change in welfare benefits over time is not caused by something that would also have a direct effect on marriage behavior. An example of such an unobserved factor could be a change in local labor markets, which may affect both marriage markets and legislators' willingness to provide income support to the poor (Moffitt, Ribar, and Wilhelm 1998). A further difficulty with within-state, over-time comparisons is that they are generally able to identify only welfare effects that operate within one year, not longer-term responses (Moffitt 1994).

Given these problems, it is useful to draw upon evidence from social experiments when available. One important set of experiments was the Seattle and Denver Income Maintenance Experiments (SIME-DIME). These experiments provided a variety of benefit schemes applicable to married couples. A number of scholars have taken issue with the results of the experiments, on the basis of either the randomization design[2] or the nature of the treatment itself [3]. For these and many other reasons, scholars still disagree over what conclusions may be drawn from these experiments about the effects of income guarantees on marriage and divorce (see Cain and Wissoker (1990) and Hannan and Tuma (1990)).

This study avoids the above-mentioned difficulties of previous empirical studies in two major ways: (i) the source of variation in welfare benefits is a social experiment with simple random assignment and (ii) repeated observations on the treatment and control groups allows me to distinguish between short-term and long-term effects of changes in welfare benefits. In contrast to many prior studies, I also distinguish between welfare's effects on marriage *formation* versus marital *dissolution*. This distinction is achieved by analyzing the effects of benefit levels separately for women who began the study period in the single-parent AFDC-Basic program and for women initially in the two-parent AFDC-UP (Unemployed Parent) program.

The evidence provided in this paper shows that welfare program incentives do affect poor women's marriage decisions. The evidence suggests that higher welfare benefits significantly increase marital dissolution rates for poor two-parent families. These effects are larger the longer a woman is in a higher-benefit regime. There is no evidence that higher benefits either encourage or discourage marriage among single-parent welfare recipients. The next section describes the incentives of the AFDC program, section 3 describes the data and the social experiment in California, section 4 explains the empirical results, and the last section concludes.

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[2] The assignment to control and treatment groups was stratified based on pre-experiment income. Moffitt and Kehrer (1981) point out that serial correlation in, say, labor supply error terms may result in the treatment variable being correlated with unobserved heterogeneity and hence subject to simultaneity bias.

[3] Moffitt (1992) points out that the treatment group members were issued benefit checks automatically if they were eligible, thus resulting in a 100 percent takeup rate. In contrast, control group members had the choice of not applying for AFDC benefits. Michel (1980) estimates an AFDC takeup rate of only 69 percent in 1970. Treatment group members were also required to file income and family composition reports every month instead of every four months for the control group.

II. Marriage and Cohabitation Incentives of the AFDC Program

The standard description of the AFDC[4] program is the following: AFDC is primarily available only to single-parent families, therefore increases in AFDC benefit levels lead to a decrease in the likelihood of being married. Yet the true pattern of incentives is more complicated. In particular, two important considerations render even the direction of the benefit level's effect on marriage ambiguous: (i) AFDC benefits are available to married couples, and (ii) we cannot predict how couples allocate consumption or income between individuals. The first consideration means that a broad increase in the benefit level will increase income opportunities for women both in the married state and in the unmarried state. The second consideration implies that we cannot determine *a priori* the relative magnitude of marginal utility of AFDC benefits for married women versus for single women.

Most studies of the AFDC system's effects on marriage do not recognize that AFDC benefits are in fact available to two-parent families through the AFDC-UP program.[5] Before the 1996 welfare reform, AFDC-UP applied the following rules.[6] Eligibility in the UP program is conditioned on the primary earner having a significant attachment to the labor force[7] and working fewer than 100 hours per month. Total family income must meet the same income cutoffs as under the single parent "AFDC-Basic" program. Benefit levels are the same in both components of the AFDC program[8], where an AFDC-UP family with 2 adults and 2 kids receives benefits applicable to an AFDC-Basic family with

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[4] I will use the term AFDC here to also refer to TANF, since the incentives are basically unchanged and the data in this study come from the pre-welfare reform era.

[5] Studies that do recognize AFDC-UP typically add a dummy variable indicating whether AFDC-UP is available in a given state in a given year. This simple specification should not be expected to capture the true incentive effects discussed in this section.

[6] I have found no systematic survey of state changes to AFDC-UP. The pre-reform program is likely to serve as the benchmark for many states' new programs.

[7] Significant attachment is defined as having worked and earned at least $50 in at least six of the previous thirteen calendar quarters, or having been eligible for unemployment compensation benefits during the previous year.

[8] The 1996 welfare reform allowed states to establish different benefit schedules in the two programs, a policy option I will discuss more in the conclusion of the paper.

1 parent and 3 kids. In some cases, a poor couple may be eligible for more benefits if they marry (and receive AFDC-UP) than if they remain separate (and the woman receives AFDC-Basic). The supposed marriage-discouraging effect of AFDC may work in the opposite direction.

Recognizing the availability of AFDC-UP benefits, the determinants of a woman's decision to be married may be described by the following:

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\begin{aligned}
M^* &= V_1(A_1, W_h, W_w, X) - V_2(A_2, W_h, W_w, X) \\
A_1 &= \max[0,\ B - t(W_h H_h + W_w H_w)] \\
A_2 &= \max[0,\ B - t(W_w H_w)] \\
V_1(A_1, W_h, W_w, X) &= \max U_1(A_1, W_h H_h, W_w H_w, H_h, H_w, X) \\
V_2(A_2, W_h, W_w, X) &= \max U_2(A_2, W_h H_h, W_w H_w, H_h, H_w, X)
\end{aligned}
\qquad \textbf{(1)}
$$

where M^* measures the difference between the woman's maximum utility if married and the woman's maximum utility if single. The arguments of the underlying utility functions U_1 and U_2 are welfare income (A_1 or A_2), male and female earnings ($W_h H_h$ and $W_w H_w$), and male and female labor supply (H_h and H_w), and X is a vector of other variables affecting utility. B and t are the maximum benefit level and the benefit reduction rate, respectively. For the purposes of this argument, I assume that H_h and H_w are constant across the two choices and do not respond to changes in B. In addition, I do not explicitly examine the theoretical effects of changes in t, even though the welfare experiment I analyze empirically includes such changes.[9]

Several recent studies have provided evidence that the "common preference" model of household behavior is not appropriate for studying consumption and labor supply decisions.[10] That is, the neoclassical model in which households behave as if they have one utility function, depending on *total*

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[9] As we shall see, the predicted benefit effects are ambiguous, so assuming no hours responses and no changes in the benefit reduction rate is innocuous. Incorporating these additional features would also yield ambiguous effects. I also have ignored the fact that B is greater under marriage due to the larger family size.

[10] See the work of Schultz (1990), Thomas (1990), and Browning et al. (1994). A more complete review of the literature is provided by Strauss and Thomas (1995).

household income, may not apply because consumption allocations (and hence any individual's utility) may depend on relative incomes of household members rather than merely on total income. Early formalizations of this hypothesis are provided by the models of Manser and Brown (1980) and McElroy and Horney (1981), in which husbands and wives reach a Nash bargaining outcome and the "threat points" are affected by each partner's wage and non-labor income. The utility functions U_1 and U_2 are a very general way to allow income sharing within marriage to be systematically different from income sharing between unmarried partners. In particular, if relative male-female incomes affect income sharing, then the marginal utility of welfare benefits will also depend on relative male-female incomes and not just on total income.[11] As special cases, the common preference model would have U_1 take the form $U_1(A_1 + W_h H_h + W_w H_w, H_h, H_w, X)$, while a model that ignores any form of altruism between unmarried (non-cohabiting) partners might yield $U_2(A_2 + W_w H_w, H_w, X)$.

The effect of the welfare benefit level on the desirability of marriage is obtained by differentiating the expression for M^* with respect to B, yielding the following expression:

$$\frac{dM^*}{dB} = \frac{\partial V_1}{\partial A_1}\frac{\partial A_1}{\partial B} - \frac{\partial V_2}{\partial A_2}\frac{\partial A_2}{\partial B} \qquad \textbf{(2)}$$

An increase in benefits available in the unmarried or living independently state leads to an "independence" effect that discourages marriage ($-\partial V_2/\partial A_2$). However, there is a simultaneous increase in benefits available within marriage ($\partial V_1/\partial A_1$). Without any restrictions on the relative sizes of $\partial V_1/\partial A_1$ and $\partial V_2/\partial A_2$, the sign of the derivative dM^*/dB is ambiguous.

The prediction may be made unambiguous if further assumptions are made. For instance, since A_1 is paid to both husband and wife while A_2 is paid solely to the woman, we might expect that $\partial V_1/\partial A_1$

[11] This formulation is not completely general, however. For instance, I have not allowed AFDC benefits available as a single person to affect income sharing within marriage. The bargaining framework would recognize this effect as operating through the woman's threat point. Nevertheless, the somewhat simpler specification adopted in (1) is enough to demonstrate the ambiguity of AFDC's effects on marriage.

$\leq \partial V_2/\partial A_2$. Because married couples are more likely to be income-ineligible (meaning A_1=0 more often than A_2=0), we also have $0 \leq \partial A_1/\partial B \leq \partial A_2/\partial B \leq 1$; putting these two conditions together, we might expect $dM^*/dB \leq 0$. Alternatively, assuming no benefits available to married couples would yield simply $dM^*/dB = -\partial V_2/\partial A_2 \times \partial A_2/\partial B \leq 0$ as is normally supposed. However, suppose a couple considering marriage would have the same income sharing arrangement whether married or unmarried. Also suppose their incomes are low enough so that they would qualify for AFDC even if married, so that $\partial A_1/\partial B = \partial A_2/\partial B = 1$. For this couple, the additional marriage-related taxation of male earnings by AFDC means $A_1 < A_2$.[12] Since the marginal utility of consumption declines, we would have $\partial V_1/\partial A_1 > \partial V_2/\partial A_2$ and hence $dM^*/dB > 0$.

Thus, we have seen that the standard prediction that $dM^*/dB \leq 0$ requires assumptions (not usually specified by previous authors) about the availability of benefits to married couples or about the allocation of income within households. While this study does not aim to provide evidence on the form of income allocation rules within households, it is nevertheless important to recognize the theoretical ambiguity.

Understanding the incentives of AFDC becomes even more complicated because marriage is treated differently depending on whether the male partner is the father of the children, and because marriage is treated differently from cohabitation. An AFDC-Basic recipient is allowed to cohabit with a partner as long as the spouse/partner is not the parent of the woman's children. If a cohabiting male is the father, then the household may only receive AFDC under the AFDC-UP program. Furthermore, marriage rather than cohabitation is penalized if a woman marries a male who is not the parent of the children; in this case, a portion of the male's income is counted as part of household income and thus makes the household eligible for lower benefit payments.[13] In the case of cohabiting, non-parent males,

[12] This holds as long as the taxation of male earnings outweighs the fact that the larger family size yields a higher maximum benefit level B.

[13] This rule applies in all but seven states. See Moffitt, Reville, and Winkler (1994).

some states reduce AFDC benefits depending on the contribution of the male to shared expenses. In California, no benefit reduction is made regardless of shared expenses by cohabitors (see Moffitt, Reville, and Winkler's 1994 survey of state rules on cohabitors). As shown by Moffitt, Reville, and Winkler (1995), a substantial fraction of AFDC recipients are married—a proportion too large to be accounted for by AFDC-UP recipients. In the empirical analysis, I will distinguish between welfare's effects on marriage and effects on cohabitation, because the ultimate well-being of children may differ between these two types of living arrangements, either due to a differing level of commitment between spouses or due to different levels of expenditures on children.

The combination of these two lesser-known aspects of AFDC benefit rules can be illustrated with a more concrete example. Suppose a woman with two children and zero earnings is contemplating marrying or cohabiting with a male partner. Then the benefits available can be summarized according to the following table, with benefit levels corresponding to California:

Table 1
AFDC Incentives for Marital Status

	Earnings of male	Marital status	Program eligibility	Maximum benefit ($)
(1) Male is parent of children	zero	Married	AFDC-UP	723
		Cohabiting partner	AFDC-UP	723
		Living separately	AFDC-Basic	607
(2) Male is not parent	zero	Married	AFDC-Basic	607
		Cohabiting partner	AFDC-Basic	607
		Living separately	AFDC-Basic	607
(3) Male is parent of children	above eligibility limit	Married	none	NA
		Cohabiting partner	none	NA
		Living separately	AFDC-Basic	607
(4) Male is not parent	above eligibility limit	Married	none	NA
		Cohabiting partner	AFDC-Basic	607
		Living separately	AFDC-Basic	607

In California, the maximum monthly benefit is $607 for a family of three and $723 for a family of four. The table most importantly shows that the incentive to stay single is not invariant to the relationship of the male to the children and to the income of the male, as seen by comparing case (1) to either case (3) or case (4). *In some cases, welfare payments may actually increase due to marriage.* Note also that the incentives against marriage may be affected both by the absolute level of benefits (cases (3) and (4)) and by the relative level of benefits between the two parts of the AFDC program (case (1)).

Although the incentives seen in Table 1 are complicated, they also present a potentially rich set of testable implications with which to confront the data. As a practical matter, however, it is impossible to know with much confidence which of the four rows above pertain most to a particular woman's choice, since we cannot adequately define the potential set of spouses or cohabitors. In the empirical work to follow, I will attempt to determine whether marriage behavior responds to the differential treatment of parents and non-parents in the way we should expect. For the moment, it should be clear that the sign of the coefficient on welfare benefits in a marriage regression equation is *a priori* ambiguous and does not tell us the size of the effect of changing opportunities only in the unmarried state, as most researchers have assumed.

In the discussion above, I have also ignored other income maintenance programs such as General Assistance (GA) and the Earned Income Tax Credit (EITC).[14] A poor male who chooses to get married may lose benefits under GA (up to approximately $200 per month in California), thus increasing the incentive to stay single. On the other hand, the EITC may be a powerful incentive *for* marriage. If a male has low earnings and the female does not work, then the couple can qualify for EITC payments (up to $3,556 per year in 1996 for two-children families) only if the male claims the children as dependents. In this case, it may be income-maximizing to be married and collect both EITC and AFDC-UP benefits. In the empirical work to follow, I do not explicitly consider the interactions of AFDC with other programs; the experimental design of the dataset allows me to isolate the effect of AFDC program changes holding other program parameters constant.

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[14] Food Stamps and Supplemental Security Income do not have important marriage incentives, other than the incentive to be poor (in both programs) or to have a larger family size (for Food Stamps).

It is appropriate to ask whether there is in fact any overlap between the AFDC-Basic and the AFDC-UP populations to support the complicated discussion of incentives above. In the data I will describe in the next section, 3 percent of women who start out as AFDC-Basic cases eventually use AFDC-UP at some point within a 2½-year time frame, and 27 percent of initially AFDC-UP women eventually use AFDC-Basic in that time frame. Thus, it is not unreasonable to expect some women to respond to the incentives I have described, because they actually experience benefits under both programs.[15] Indeed, the fact that it is the two-parent recipients who change programs more frequently will be seen to be an important reason why significant marriage effects are found only for this population.

III. Data: The California Welfare Experiment

Beginning in December 1992, the state of California, under its waiver agreement with the federal government, began conducting a social experiment with its AFDC program. The main changes in the welfare system were twofold: maximum benefit levels were decreased and work incentives were increased. The work incentive changes included reducing the benefit reduction rate from 100 percent to 67 percent for those recipients in spells lasting longer than four months, and extending the $30 per month income disregard past the initial twelve months of AFDC receipt.[16] A welfare demonstration project, called the California Work Pays Demonstration Project (CWPDP), was established in four counties in California: Alameda, Los Angeles, San Bernardino, and San Joaquin. These were chosen to represent a broad spectrum of the welfare caseload, including two northern counties v. two southern

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[15] Responses to incentives also depend on the extent to which welfare recipients can misreport their marital status or living arrangements. Those who can engage in this kind of fraud costlessly should have no response to increases in benefit levels.

[16] Prior to this change, the $30 disregard applied only for the first twelve months of AFDC recipiency, and the 67 percent tax rate rose to 100 percent after four months of recipiency. Thus, the change meant that welfare benefit calculations did not change over the length of a spell.

counties, and two counties with large urban centers v. two rural counties. The research design selected a large number of cases (about 15,000) from the baseline caseload as of December 1992[17], and then randomly assigned one-third of these cases to a control group. The treatment cases were subject to the new benefit rules, whereas the control cases were subject to the pre-reform rules (higher benefits). Cases that left AFDC and subsequently returned retained their original control-treatment status. The benefit levels under the experiment are shown below in Table 2. (Although the treatment group received two separate benefit cuts, I have marital status data only for the period following the second cut.)

Table 2
Maximum Monthly AFDC Benefit Payments

Family size	Control Group	Treatment Group Dec. 92 - Aug. 93	Treatment Group Sep. 93 - Dec. 96
1	326	307	299
2	535	504	490
3	663	624	607
4	788	743	723
5	899	847	824
6	1010	952	926
7	1109	1045	1017
8	1209	1139	1108
9	1306	1230	1197
10	1403	1322	1286

Note: Beyond 10 persons, benefit is increased $14 per month per person.

Other changes in the AFDC program were instituted at the same time, but my analysis will show that these simultaneous changes do not confound the inferences about benefit effects. The other AFDC changes were as follows:

- Elimination of the 100-hour per month work limitation on AFDC-UP recipients. The 100-hour rule continues to apply for initial eligibility determination. Effective December 1992 for treatment cases.

- AFDC recipients may be exempt from participation in GAIN (Greater Avenues for Independence, California's welfare-to-work training program) if they have a child younger than three years old, but this exemption may only be used once. Applicable to treatments beginning April 1994.

- Changes in the asset limits for treatment cases: equity value of an automobile increased from $1500

[17] This sample thus is disproportionately composed of long-term AFDC recipients, a group that is more likely than the average person in the general population to respond to welfare benefit changes but perhaps less likely than the average AFDC *entrant* to respond to economic incentives.

to $4500, allowable resources increased from $1000 to $2000, and savings accounts up to $5000 for specialized purposes such as children's college education, down-payment on homes, or for starting a business. Effective April 1994. Old asset tests still apply at the time of eligibility determination.

- Treatment cases may elect to not receive grant check but continue to receive only Medicaid coverage and child care assistance. Effective May 1994.

- For treatment cases, the need standard[18] was increased July 1993 and July 1994. This tended to increase benefit payments, although payments for cases with zero income would receive only the maximum benefit.

The primary data in this analysis come from merging two datasets: longitudinal case histories of all demonstration cases, dating from January 1988 through June 1995, and a computer-aided telephone survey of a smaller subgroup conducted in English and Spanish. The case history data provide monthly information on type of aid received; amount of benefits paid; county of residence; and number of people in the case and their ages, gender, and race/ethnicity as long as the case was on aid and in the state of California. The survey data provide much more detailed information, including education; marital status or cohabitation; and income from earnings, welfare, and transfers. The telephone survey, conducted by UC Berkeley's Survey Research Center rather than the welfare agency, provides information for each welfare case at two points in time. The first wave of the survey was conducted between October 1993 and September 1994, and the second wave was conducted between May 1995 and May 1996. The average time elapsed between interviews was 18 months, and there was a 20 percent attrition rate between waves. The next section includes an analysis of the determinants of attrition and an assessment of whether attrition biases the measured effects of welfare benefits. The analysis sample includes 2164 women respondents (out of 2214 survey respondents, 49 men were dropped[19] and 1 woman was dropped due to missing marital status information). Appendix Table 1 presents means and standard errors of regression variables, and Appendix Table 2 describes the correlation between marital

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[18] The need standard (NS) affects benefits in the following way. Benefits paid are equal to max[0,min{B,NS-tWH}]. In many states, B and NS are identical; in California, NS>B.

[19] Almost all of these men were in the survey based on participation in the AFDC-Basic program. I omit them from the analysis because almost any covariate may have a different effect on men's choices than on women's choices.

status in the two survey waves. All statistics and regressions in this paper are weighted, using sample weights that weight the sample up to the caseload population in the four counties.

The fact that randomization was executed properly in this experiment is documented in Becerra *et al.* (1996). In addition, randomization applies to the subsample in the telephone survey: a probit regression of control/treatment status on all of the exogenous right-hand side variables used in my analysis shows no significant correlation, either for Wave 1 or Wave 2. (These results are available from the author.)

IV. Empirical Results

This section of the paper is divided into subsections that in turn deal with the following questions: (A) what was the effect of the California welfare experiment on marriage rates, (B) how do we translate these experimental effects into effects of benefit levels, (C) what explains differences between transitions into marriage and transitions out of marriage, (D) can we attribute all of the experimental impacts to the change in benefit levels, and (E) do marriage and cohabitation respond in predictable ways. An examination of whether non-random attrition biases the estimates is presented in the Appendix.

A. Raw experimental impacts on marriage rates

The simple experimental impacts on marital status can be measured by comparing rates of marriage/cohabitation between the control and treatment groups. Table 3 reports the proportion of women who are married or cohabiting at each survey date in the control and treatment groups, as well as the difference between the control and treatment groups. I also separate women according to whether they started the experiment in the AFDC-Basic program versus the AFDC-UP program. Thus, the labels "AFDC-Basic" and "AFDC-UP" in the table define a woman's status at the beginning of the experiment, not necessarily her status as of the survey waves. Thus, the fraction of AFDC-UP women married as of Wave 1 is not 1.0. Distinguishing these populations is important because transitions into marriage (among AFDC-Basic women) may be affected differently than transitions out of marriage (among AFDC-UP women). This distinction has not been explored in the non-experimental literature.

Table 3
Rates of Marriage/Cohabitation in the California Welfare Demonstration

	Wave 1			*Wave 2*		
	Control	Treatment	Difference	Control	Treatment	Difference
No Covariates:						
AFDC-Basic	0.1812	0.1836	-0.0023	0.2124	0.2316	-0.0192
	(0.0168)	(0.0126)	(0.0213)	(0.0199)	(0.0155)	(0.0258)
AFDC-UP	0.8850	0.8996	-0.0146	0.7642	0.8398	**-0.0756***
	(0.0204)	(0.0142)	(0.0248)	(0.0299)	(0.0195)	(0.0347)
Covariates Included:						
AFDC-Basic			-0.0011			-0.0204
			(0.0210)			(0.0254)
AFDC-UP			-0.0091			**-0.0771***
			(0.0248)			(0.0342)

Note: Standard errors in parentheses. All statistics are weighted. Additional covariates in bottom panel include age, education, race/ethnicity, county, and month of interview. * indicates difference between control and treatment groups is significant at the 0.05 level.

The tabulations in Table 3 show a statistically significant difference only in Wave 2, and only for women initially drawn from the AFDC-UP caseload. Women in the control group (higher benefits and higher benefit reduction rates) were less likely to be married or cohabiting than women in the treatment group. The bottom panel of the table shows the control-treatment difference after controlling for demographic variables (via ordinary least-squares (OLS) regression): those results are the same as the raw differences. In addition to being statistically significant, the effects are also large in economic terms: for AFDC-UP women in Wave 2, there was a control-treatment difference of more than 7 percentage points in marriage rates. *Thus, the welfare program incentives under the experiment had sizeable and statistically significant effects on marriage behavior.*

The tabulations are performed separately for Wave 1 and Wave 2 because the benefit effects may change with the length of the experiment. One might expect very little response in Wave 1 because this survey occurs between 10 and 21 months after the start of the experiment—a short time to measure differences in the occurrence of rare events such as marriage or divorce. In contrast, Wave 2 interviews take place between 29 and 41 months after the start of the experiment.[20] The comparison of estimates from Wave 1 and Wave 2 in Table 3 shows that the welfare effects grow more negative over time, particularly for women initially on AFDC-UP. The fact that the effects change at all between two waves of the survey may seem surprising. However, an examination of the transitions in Appendix Table 2 shows that the AFDC population experiences considerable change in marital status over a relatively short time span: nearly 20 percent of the women (among those who don't attrit from the sample) have a change in marital status. Thus, it is not surprising to find an effect between Wave 1 and Wave 2. The fact that the effect becomes larger over time may simply reflect that as time goes on, more women undergo marital transitions and hence understand the incentives. The distinction between marital formation among AFDC-Basic women and marital dissolution among AFDC-UP women will be explored later.

B. Interpreting the experiment's effects as benefit effects

The previous tabulations demonstrate that welfare program incentives do indeed affect marital decisions. Most of the literature has focused on the more specific question of whether benefit levels (rather than work incentives) affect marriage; thus, the rest of the analysis will interpret the control-treatment differences in this light. I will also provide evidence using additional data that supports the interpretation that the experiment's main effects operate through benefit levels rather than through work incentives.

Under the presumption (to be tested later) that the welfare experiment's effects operate solely through benefit levels, we can use the following regression model to measure the impact of welfare

[20] There are no significant differences between control and treatment groups in their dates of Wave 1 or Wave 2 interviews. The regression-adjusted differences also control for month of interview.

benefits on marriage and cohabitation behavior:

$$
\begin{aligned}
M_{it}^{*} &= \alpha_t BEN_{it} + X_{it}\beta_t + \varepsilon_{it} \\
M_{it} &= 0 \;\; if \;\; M_{it}^{*} < 0 \\
M_{it} &= 1 \;\; if \;\; M_{it}^{*} \geq 0
\end{aligned}
\qquad \textbf{(3)}
$$

where M_{it} equals one if woman *i* chooses to be married or cohabit with a male partner in period *t* and zero otherwise. BEN_{it} measures the maximum monthly AFDC benefit; I will use first a measure that varies with the woman's number of children and then a measure of benefits available for a family of four people. The latter measure is exogenous (and orthogonal to other regressors) because it is determined by random assignment.[21] X_{it} is a set of demographic variables indicating age, education, race, county of residence at the beginning of the experiment, and month of interview. Later specifications will add other variables, some of which will be interacted with the benefit variable to measure differential responses to incentives in different sub-populations. Note that the coefficients are allowed to vary over time, in order to measure the extent to which the response to welfare benefits changes with longer exposure to a given benefit regime. All regressions will also be estimated separately for women who are initially in AFDC-Basic cases versus women initially in AFDC-UP cases, again to distinguish between transition into marriage and transitions out of marriage.

Because a woman's child-bearing choices may respond to welfare benefits, using a measure of welfare benefits that varies with family size may be subject to simultaneous-equations bias. Most studies of the effect of welfare benefits have used a benefit measure that is specific to a family of four individuals. This latter measure may also be undesirable because it measures the AFDC opportunity for any given woman with error, resulting in an estimated coefficient biased toward zero. I am able to assess the extent to which these concerns are important by employing instrumental variables (IV) to equation

[21] This benefit measure merely results in a rescaling of the control-treatment difference, because it varies only between the control and treatment groups.

(3); the instruments include a dummy variable indicating whether a woman is in the control or treatment group as well as all variables in X_{it}. Results from this specification are shown in Table 4, for marriage/cohabitation behavior in each wave of the survey. Column 1 is an ordinary least-squares (OLS) estimate, where the benefit variable varies with number of children and column 2 presents IV estimates with the same benefit variable, instrumented by control/treatment status. The third column presents probit estimates (probability derivatives) with the benefit variable for a family of four, which is exogenous due to random assignment. As before, the labels "AFDC-Basic" and "AFDC-UP" refer to a woman's status at the beginning of the experiment, which is not necessarily the status at the time of the survey.

Table 4
Estimated Effects of $100 Benefit Increase on Marriage/Cohabitation

	Wave 1			*Wave 2*		
	OLS	IV	Probit	OLS	IV	Probit
AFDC-Basic	0.0110	-0.0022	-0.0030	0.0130	-0.0386	-0.0310
	(0.0063)	(0.0362)	(0.0318)	(0.0078)	(0.0504)	(0.0392)
AFDC-UP	-0.0000	-0.0113	-0.0139	0.0072	**-0.0965***	**-0.1062***
	(0.0081)	(0.0403)	(0.0351)	(0.0115)	(0.0452)	(0.0505)

Note: Standard errors in parentheses. See text for explanation of benefit variable used in each column. Full regression results reported in Appendix Table 3. * indicates significance at 0.05 level.

The differences between the OLS and IV estimates imply that there is an important, positive correlation between number of children and marriage, as one would expect. The IV and probit (with the benefit for a fixed family size) estimates are very similar. Thus, previous studies using this latter benefit variable probably do not lead to grossly inaccurate estimates of welfare benefit effects.[22] In further regressions, I will use this fixed-family-size benefit variable because it allows me to interact the benefit variable with other covariates without having to find more instrumental variables.[23] When the number

[22] Applying IV to the fixed family-size benefit variable results in identical estimates and standard errors, because the variation in the benefit variable occurs only over the instrument for control/treatment status.

[23] In practice, it was difficult to identify benefit effects that vary across race, age, or education groups because of small sample sizes. These inconclusive results are reported in Appendix Table 4.

of children is included as a regressor and as an instrumental variable, only the first OLS estimates change noticeably from those in Table 4. In all further regressions, number of children is excluded in order to be on the cautious side of the potential endogeneity of family size decisions.

C. Marital formation versus marital dissolution

Why is the benefit response so much stronger for AFDC-UP women than for AFDC-Basic women? First, note that 49 percent of AFDC-Basic women had never been married as of Wave 1, whereas all AFDC-UP women were by definition either married or cohabiting as of the beginning of the experiment. It is reasonable to suppose that women who have little option for marriage do not respond to benefit level incentives. AFDC-UP women may simply find it easier to move *out of* marriage than AFDC-Basic women can move *into* marriage, because beginning a marriage is a result of two people's decisions while marriage may be ended unilaterally. Second, the AFDC-UP effect is plausible only if these women actually move onto the AFDC-Basic program after breakup. We should expect the benefit effect to be negative only for those women who move from AFDC-UP to AFDC-Basic, while the benefit effect should be positive for other AFDC-UP women due to the income effect ($\partial V_1/\partial A_1 > 0$). This is indeed exactly what happens: a probit regression including an interaction between the benefit level and an indicator of whether the woman ever received AFDC-Basic benefits after the start of the experiment shows that the benefit effect is only negative for those who did switch from AFDC-UP to AFDC-Basic. For (initially) AFDC-UP women in Wave 2, the main benefit effect is an insignificant 0.0727 (t=1.01) while the interaction with ever receiving AFDC-Basic is -0.2824 (t=-2.78). Thus, it appears that marriage decisions are indeed responding to the amount of benefits available outside of marriage.[24]

It is natural to ask why it is that the benefit effect grows stronger over time mainly for AFDC-UP

[24] The insignificant but large positive coefficient for those with no AFDC-Basic experience is consistent with the marriage-preserving effect intended by the creators of AFDC-UP.

women.[25] One hypothesis relates back to equation (1), in which the marriage decision is a function of both available AFDC-Basic benefits (A_2) and available AFDC-UP benefits (A_1). The earnings of male partners of women initially on AFDC-UP are by definition low enough to qualify for benefits (A_1>0). Yet, as time passes, we may expect these male earnings to rise to the point where some fraction of these couples would become ineligible for benefits if they were to stay together (A_1=0 and $\partial A_1/\partial B$=0).

To explore this idea, I use matched data from California's Employment Development Department (EDD) on quarterly earnings for all individuals who were part of the woman's welfare case at the time of sampling in 1992; these data provide information only for jobs covered by unemployment or disability insurance and span the period January 1984 through June 1995. Figure 1 shows the time pattern of male earnings associated with AFDC-UP cases.[26] The last quarter of 1992 represents the low point of average earnings because this is the point at which all cases are on AFDC-UP. Figure 1 shows that AFDC-UP males do indeed experience significant earnings growth after the time of initial selection into the AFDC sample; thus, AFDC-UP becomes a less viable option over time.[27] The essence of this argument is that as male earnings increase, the benefit variable in the regression is likely capturing more the effect of changing AFDC-Basic benefits than the effect of changing AFDC-UP benefits (the first

[25] An alternative interpretation of the results is that for both AFDC-Basic and AFDC-UP women, the benefit effect grows by ten-fold between Wave 1 and Wave 2. Thus, there may not be a disparity that needs to be explained.

[26] Since the EDD data do not provide sufficiently accurate information to identify which male is the male spouse, I add earnings of all males in each AFDC case. No significant earnings differences between the control and experimental groups are found. It is important to note that this figure refers only to the 60 percent of AFDC-UP cases that have matches with EDD earnings records. Factors that significantly raise the likelihood of a case having no matching male earnings data include young age, low education, whether Hispanic, residence in Alameda or Los Angeles counties, and short durations on AFDC prior to the experiment. There is no difference between controls and experimentals in whether they have EDD matches.

[27] At the same time, women's earnings are also likely to grow over time and make them less likely to be eligible for AFDC-Basic benefits. Between waves of the survey, monthly female earnings grew an average of $146 while monthly earnings of their male spouses grew an average of $264, among those AFDC-UP cases that were married/cohabiting in both waves of the survey. It is an open question whether the earnings growth of males who stay married or cohabiting is different from earnings growth of males whose marriages or partnerships dissolve. There is some earnings difference between these populations in Wave 1. AFDC-UP males who are married in both waves have average earnings of $238, while AFDC-UP males who end marriages have average earnings of $188. Females' earnings in these two groups are $89 and $56, respectively. These averages are computed only over those who don't attrit from the sample, and neither of these differences is statistically significant.

term in equation (2) becomes zero). Thus, we would expect the effect dM^*/dB to become more negative because the AFDC-Basic benefit has an unambiguously negative effect on marriage desirability (by increasing the woman's independence). For women initially on AFDC-Basic, the benefit effect on marriage does not change much over time perhaps because their (potential) male partners need not have had low incomes when the women were selected into the sample (and hence these men experience little earnings growth, unlike the male partners of the initially AFDC-UP women).[28]

Finally, to provide a further test of the interpretations offered here, one can categorize women according to Wave 1 marital status, instead of by the AFDC program in which they were enrolled at the start of the experiment. These regressions yield similar patterns. The effect of benefits on marriage in Wave 2 is significant and negative for those who had a Wave 1 spouse, and insignificant and small for those without a Wave 1 spouse. Moreover, the magnitude of the probability derivative for those married in Wave 1 is -0.1042, which is nearly identical to the effect reported in Table 4 for AFDC-UP women. (These results are available from the author.[29]) Thus, the benefit effects seem to reflect effects on marital dissolution, rather than effects for a peculiar population of AFDC-UP recipients.

D. Is it benefit levels or other welfare changes?

The benefit variable used thus far varies only over control/treatment status, so the estimates presented so far are merely rescaled estimates of the total effect of the experiment. That is, multiplying the effects by 0.65 ((788-723)/100) yields the control-treatment difference in each regression. Our ability to identify these control-treatment differences as purely benefit effects depends on the assumption

[28] Unfortunately, I cannot test this hypothesis directly because I do not have data on potential male partners in AFDC-Basic cases.

[29] The regressions described are performed for those women still on some kind of AFDC as of Wave 1. This is done to ignore those women starting on AFDC-Basic at the start of the experiment who leave the program due to marriage by Wave 1. The benefit effect (between Wave 1 and Wave 2) for these women would not reflect the benefit effect that should apply to *married AFDC recipients*.

that the other differences in the AFDC program between control and treatment groups are unrelated to marital status. Since the California experiment imposed other changes besides the benefit levels, it is possible that these other changes are biasing the benefit coefficient away from the true effect of changing benefit levels alone. In this subsection, I will test whether the apparent benefit effects are biased by the possible effects of control-treatment differences in the benefit reduction rate, the 100-hour rule for AFDC-UP cases, and requirements for training program participation.

One likely candidate for an important omitted variable is the benefit reduction rate, which is 67 percent for the treatment group and varies between 67 and 100 percent for the control group. Because the benefit level is higher for the control group, the benefit effect in the regressions performed so far may be measuring the effect of higher implicit tax rates on earnings (in addition to the effect of higher benefit levels). Another work incentive provision differing across control and treatment groups is that the treatment group in AFDC-UP is no longer subject to the 100-hour rule. Both the tax rate effect and the 100-hour rule effect may be expected to increase earnings in the treatment group relative to the control group. In fact, no significant impacts on earnings were found in the preliminary evaluation of the full demonstration caseload of 15,000 cases (Becerra *et al.*, 1996), suggesting that the work incentives of the demonstration project are unlikely to be responsible for the estimated effect of welfare benefit levels on marriage.

Even though earnings did not change due to the experiment, the differential *taxation* of earnings means that AFDC benefits actually paid will differ between treatment and control groups. The higher benefit reduction rate for control cases means lower benefits for a woman (or couple) with positive earnings. Thus, higher benefit reduction rates are correlated with lower benefit payments *ceteris paribus*, and this mechanism may be biasing the measured effect of *maximum benefit levels* on marriage.[30] The potential bias introduced by the simultaneous changes can be explained by the fact that

[30] Referring back to equation (2), the regression may be measuring $\partial A_1/\partial t$ and $\partial A_2/\partial t$ rather than $\partial A_1/\partial B$ and $\partial A_2/\partial B$.

the regressions in Table 4 measure $\hat{\alpha}$*=(dM/dC)/(dB/dC)*, where *C* indicates control group status. However, if *C* also affects AFDC income through tax rates, then we can write the reduced form effect of *C* on *M* as:

$$\frac{dM}{dC} = \frac{\partial M}{\partial B}\frac{\partial B}{\partial C} + \frac{\partial M}{\partial t}\frac{\partial t}{\partial C} \tag{4}$$

Rearranging terms yields

$$\hat{\alpha} = \frac{dM}{dC} \div \frac{dB}{dC} = \frac{\partial M}{\partial B} + \frac{\partial M}{\partial t}\frac{\partial t}{\partial C} \div \frac{dB}{dC} \tag{5}$$

The second term measures the bias resulting from the simultaneous change in tax rates due to the experiment. Empirically, α is significantly negative for AFDC-UP women in Wave 2. Since *dt/dC* and *dB/dC* are both positive, equation (8) tells us that either $\partial M/\partial B$ or $\partial M/\partial t$ is negative. Indeed, since both *B* and *t* operate through the amount of AFDC income received, we can determine that $\partial M/\partial B$ and $\partial M/\partial t$ are of opposite sign. This implies that the regression estimate of $\partial M/\partial B$ is biased toward zero, thus understating the benefit effect.

Is it possible that our estimated effects are purely the effect of changing tax rates rather than changing benefit levels? To address this possibility, note that the sign of the effect of AFDC benefits *received* indicates the sign of $\partial M/\partial B$ and $\partial M/\partial t$. If AFDC *income* has a negative effect on marriage, then so does the benefit level, because the benefit level has a positive effect on benefits paid. Thus, a natural way to address this problem is to replace the benefit level with AFDC benefits *received* by each woman in regression equation (3), using control/treatment status as an instrument for benefits. If the effects in Table 4 are merely the effect of changing tax rates, then the effect of AFDC income should be positive, because the tax rate and the benefit level have opposite effects on AFDC income. These regressions are summarized in Table 5. Although the estimated coefficients are all statistically insignificant, they are large and negative in Wave 2. Indeed, the effects in Wave 2 are of much larger magnitude than the effects from Table 4. These numbers suggest that the effects in Table 4 are indeed capturing effects of changing benefit levels.[31]

Table 5
Estimated Effects of AFDC *Income* on Marriage/Cohabitation

	Wave 1	Wave 2
AFDC-Basic	0.0102	-0.0640
	(0.1159)	(0.0910)
AFDC-UP	-0.0093	-0.1734
	(0.0245)	(0.1365)

Note: Standard errors in parentheses. IV regressions include regressors as in Appendix Table 3, except for benefit level. AFDC income divided by 100.

Another potential explanation for the fact that the control group has higher rates of marital breakup in the AFDC-UP caseload relates to the 100-hour rule. Suppose that there is no effect of the experiment on earnings or hours of work. However, the male spouses in this group experience substantial gains in earnings and employment (because they were selected from a population of mostly unemployed males in AFDC-UP cases). Among the control group, those who experience increases in hours of work are likely to become ineligible for AFDC-UP due to the 100-hour rule, while this does not occur among the experimental group.[32] Some couples who become ineligible for AFDC-UP in the control group may break up, since the woman can receive AFDC-Basic benefits if single. Note that this hypothesis can also apply to the control-treatment difference in benefit reduction rates: higher taxation of earnings in the control group means more control cases lose AFDC-UP eligibility and have an incentive to switch to AFDC-Basic. If this is the main explanation for the apparent effect of benefits on

[31] Another piece of evidence supporting the interpretation of the benefit coefficient as a benefit effect rather than a tax effect is that the first-stage regression of AFDC income on the instrumental variables always yields a positive coefficient on the control group dummy variable. This coefficient would be negative if the tax rate dominated the benefit level in determining AFDC payments.

[32] Some males in both groups who increase their earnings will of course become income-ineligible for AFDC-UP.

marriage, then we would expect the effect of higher benefits (or control group status) to be more negative for those cases in which males experienced the highest gains in employment or earnings. The available data do not provide measures of hours of work for males who leave the household. However, the matched earnings data from the Employment Development Department (EDD) may provide some insight.

An indirect test of the hypothesis described in the previous paragraph is whether there is a negative interaction between the benefit level and earnings growth. The median growth in earnings is zero, and a substantial fraction have negative earnings growth between 1992 and later years. For this reason, I specify earnings growth as a spline with a break point at zero. Table 6 reports results from three specifications, all showing that those with larger positive earnings growth actually have a *smaller* benefit effect. The benefit effect for those with zero earnings growth is as large as it is in the base regression for Wave 2. For those with negative earnings changes, greater earnings losses mean a more positive benefit effect. A further piece of evidence arguing against the 100-hour rule explanation is that the negative benefit effect on marriage is substantially stronger for those without EDD earnings matches (-0.20 versus -0.07 for those with EDD matches). If we suppose that workers in uncovered employment are better able to understate hours of work to satisfy the 100-hour rule, then we would not expect a benefit effect to show as strongly in this population (if the 100-hour rule explanation were correct).[33]

[33] Of course, this difference between those who match and those who don't could also reflect that couples are more likely to experience benefit-induced breakup when the male has a worse (i.e., uncovered) job. This reduces the power of the test shown in Table 6.

Table 6
Interactions between Benefit Level and Earnings Changes

	Time horizon	Benefit	Benefit×Negative growth	Benefit×Positive growth
1.	1992Q4 - 1993	-0.1084	-1.55×10^{-5}	1.26×10^{-5}
		(0.0737)	(5.90×10^{-5})	(1.60×10^{-5})
2.	1992Q4 - 1994	-0.1857*	-1.11×10^{-4}	3.26×10^{-5}*
		(0.0739)	(1.14×10^{-4})	(1.39×10^{-5})
3.	1992Q4 - 1995Q1,2	-0.1418	-3.90×10^{-5}	1.34×10^{-5}
		(0.0763)	(6.74×10^{-5})	(1.05×10^{-5})

Note: Regressions performed on sample of Wave 2 AFDC-UP cases with earnings data (n=349). Other regressors include a spline in earnings growth, age, race, education, county, and month of interview. Earnings changes are expressed as changes in annual dollars. Main cell entries are probability derivatives from probit coefficients; standard errors are in parentheses. * indicates significance at 0.05 level.

Another potentially important feature of the experiment in California was that experimental cases were subject to broader requirements for participation in the GAIN program (California's welfare-to-work training program). To the extent that GAIN may have resulted in finding jobs or increased human capital for women who participated, the benefit variable (whose variation is provided by control-treatment status) may be biased by the correlation between control-treatment status and GAIN participation. The ideal way to test whether this is the case would be to add a dummy variable for GAIN participation to equation (3) and instrument this GAIN variable. However, we cannot use control-treatment status as an instrumental variable because it is already effectively an instrumental variable for the benefit level. Nevertheless, estimating this augmented probit equation ignoring the endogeneity of training may provide some insight. Table 7 below reports the main coefficients from these regressions. Since the survey does not measure GAIN participation per se, I define a dummy variable that equals one if a woman participated in vocational training or classes to help find a job within the past 12 months.[34] Other regressors (not shown) include those shown in Appendix Table 3.

[34] For Wave 2, the question refers to the period since the Wave 1 interview, which is typically 18 months. If I define the Wave 2 training dummy as participation in vocational training or job classes at any time during the period starting 12 months before the Wave 1 interview through the date of the Wave 2 interview, the coefficients on the benefit variable are unchanged.

Table 7
Estimated Effects of Training and Benefit Level on Marriage/Cohabitation

	Wave 1		*Wave 2*	
	Train	Benefit	Train	Benefit
AFDC-Basic	-0.0372	-0.0040	-0.0302	-0.0338
	(0.0240)	(0.0318)	(0.0288)	(0.0393)
AFDC-UP	-0.1250*	-0.0144	-0.1020*	-0.0919
	(0.0357)	(0.0345)	(0.0390)	(0.0504)

Note: Probability derivatives from probit regressions and standard errors in parentheses. Benefit variable is maximum benefit for a family of four, divided by 100. * indicates significance at 0.05 level.

The estimated effects of the benefit variable are only slightly changed from those in Table 4, so the correlation of training with control-treatment status does not significantly bias our estimated benefit effects. The effect of training on marital status is negative, consistent with the hypothesis that human capital investment increases a woman's independence (or that independent women are more likely to engage in human capital investment). It should be noted in addition that the effect of control-treatment status on training is not very strong: regressions of training on control-treatment status, education, race, age, county, and month of interview yield a significant (negative for controls) coefficient only for initially AFDC-Basic women in Wave 2.

To summarize this subsection, the additional analysis presented here shows that the control-treatment differences in marriage behavior do not seem to be due to differences other than in benefit levels. Differences in the taxation of earned income or in limitations on hours of work under the experiment are an unlikely explanation for the control-treatment differences. Thus, the estimates provided in this paper can be compared to the rest of the literature, which focuses on the effect of benefit levels on marriage.

E. Marriage versus cohabitation

The regressions reported so far combine marriage and cohabitation into one choice. We may be concerned about the distinction between these two alternatives to the extent that marriage may represent a deeper commitment and thus be better for the children's well-being in the long run, or to the extent that married couples share their economic resources differently from cohabiting couples (where this sharing may ultimately have consequences for expenditures on children). Table 8 reports results from multinomial logits in which the three choices are marriage, cohabitation, and female headship. Other regressors in these equations are identical to those reported in Appendix Table 3; their coefficients are not reported for brevity.

Table 8
Estimated Effects of $100 Benefit Increase on Marriage and Cohabitation

	Wave 1		*Wave 2*	
	Marriage	Cohabitation	Marriage	Cohabitation
AFDC-Basic	-0.0126	0.0086	-0.0040	-0.0273
	(0.469)	(0.334)	(0.262)	(1.046)
AFDC-UP	-0.0071	-0.0021	-0.0975*	-0.0130
	(0.245)	(0.210)	(2.190)	(1.409)

Note: Mean probability derivatives calculated from multinomial logit estimates, with the omitted category defined as female headship. Numbers in parentheses are absolute values of *t*-statistics of logit coefficients. Benefit variable is maximum monthly benefit for a family of four. * indicates significance at 0.05 level.

Recall that the only significant effect from Table 4 was for AFDC-UP women in Wave 2. In this case, the effect comes mostly through changes in marital status rather than changes in cohabitation relationships. (This result is the same if one estimates two separate probits with the dependent variables being binary indicators of marriage and cohabitation, respectively.) Thus, it appears that welfare benefits have more of an effect on longer-term commitments through marriage rather than on choices of living arrangements. Most of the benefit-induced transitions in the AFDC-UP population occur between marriage and female headship.[35]

Inspection of Table 1 demonstrates that a woman faces strong disincentives to marrying a male who is not the father of her children. If the male has significant earnings, then the AFDC payment may be reduced to zero under marriage; in contrast, the woman if she cohabits with the male would still be eligible for AFDC-Basic benefits. Does the disincentive to marry a non-parent increase with the AFDC benefit level? To address this, we can formulate the utility difference between marriage and cohabitation

[35] A similar conclusion can be reached by examining the transitions in Appendix Table 2C.

as

$$
\begin{aligned}
M^*_{-c} &= V_1(A_1, W_h, W_w, X) - V_3(A_3, W_h, W_w, X) \\
A_1 &= \max[0,\ B - t(W_h H_h + W_w H_w)] \\
A_3 &= \max[0,\ B - t(P \times W_h H_h + W_w H_w)]
\end{aligned} \tag{6}
$$

where the dummy variable P equals one if the male is the father of the children. Differentiating yields

$$
\frac{dM^*_{-c}}{dB} = \frac{\partial V_1}{\partial A_1}\frac{\partial A_1}{\partial B} - \frac{\partial V_3}{\partial A_3}\frac{\partial A_3}{\partial B} \tag{7}
$$

For the case P=0, $A_1 \leq A_3$ and $\partial V_1/\partial A_1 \geq \partial V_3/\partial A_3$ if marginal utility declines and income sharing is the same between marriage and cohabitation.[36] However, $\partial A_1/\partial B \leq \partial A_3/\partial B$ because the taxation of male earnings makes the married couple less likely to be eligible. Thus, we cannot determine the sign of the effect when P=0. If P=1, then A_1=A_3 and $\partial A_1/\partial B = \partial A_3/\partial B$. A change in AFDC benefits would affect the utility difference only if $\partial V_1/\partial A_1$ and $\partial V_3/\partial A_3$ differed due to income sharing differences between marriage and cohabitation. If we assume male earned income is shared more unequally under cohabitation, then AFDC benefits may affect the woman's utility more when cohabiting then when married (because marginal utility decreases with income). This yields $\partial V_1/\partial A_1 < \partial V_3/\partial A_3$, so we might expect an increase in the benefit level to discourage marriage relative to cohabitation when the male partner is the children's parent.

In order to determine if women choose between marriage and cohabitation according to the incentives described above, I estimated a multinomial logit where the choices are (i) female headship, (ii) cohabit with a non-parent male, (iii) cohabit with a parent male, (iv) marry a non-parent male, and

[36] This is true as long as the male has enough earnings to outweigh the fact that the maximum benefit level is higher in A_1 when P=0 (due to the larger AFDC case size). When P=1, there is no difference in the maximum benefit level (see Table 1).

(v) marry a parent male. Among women in the survey sample selected from the AFDC-UP population, only a handful ever chose to cohabit with or marry a non-parent male, so I restrict the sample for this logit model to those women initially from the AFDC-Basic population. (Since almost all AFDC-UP women's spouses were parents of the children, the previous Table 8 indicates that higher benefits do indeed discourage marriage relative to cohabitation for this group, as predicted.) In order to conserve degrees of freedom, the only regressor in this logit is the maximum benefit level for a family of four. Adding other regressors does not affect the benefit coefficient, since the variation in benefits comes from random assignment and is thus orthogonal to other potential regressors. Table 9 below reports actual and predicted probabilities and probability derivatives.

Table 9
Multinomial Logit Estimated Effects of Welfare Benefit Level

	Wave 1			*Wave 2*		
	Probabilities			Probabilities		
	Actual	Predicted	dP/dB	Actual	Predicted	dP/dB
(i) Single	85.70	84.38	0.0051	82.88	80.56	0.0260
(ii) Cohabit, non-parent	3.12	3.33	0.0011	3.07	3.55	0.0107
(iii) Cohabit, parent	4.12	4.92	0.0007	4.37	4.97	-0.0524
(iv) Marry, non-parent	0.55	0.74	0.0016	0.59	1.08	-0.0061
(v) Marry, parent	6.51	6.62	-0.0084	9.09	9.85	0.0218

Note: dP/dB is the mean derivative of the probability with respect to the benefit level. The benefit variable is the maximum benefit for family of four, divided by 100.

The first thing to note from Table 9 is that women are very unlikely to choose to marry males who are not parents of the women's children. This rarity is in itself consistent with the incentives I have described. For either parents or non-parents, there is no consistent effect of welfare benefit levels on the likelihood of marriage relative to cohabitation. (The only statistically significant effect is for choice (iii) in Wave 2. This effect for parents is the opposite of what is expected, relative to choice (v).) The fact that there is for the most part no response may not be surprising, since the disincentives even before a change in benefits are so strong. The extra disincentive caused by an increase in the maximum benefit level is only a small fraction of the total disincentive to be married or cohabit. Another reason the data do not match the theory precisely is that a woman may not accurately report cohabitation and parental status of the male to the welfare agency.

V. Discussion and Conclusions

I have presented evidence that AFDC's incentives relating to marriage and cohabitation have large and statistically significant effects on poor women's behavior. The variation in marriage incentives arises from a randomized social experiment rather than from policy decisions taken by different states at different times to change their benefit levels; thus, my inferences are not confounded by simultaneous changes across states in other policies or economic factors affecting marriage. The evidence also supports the interpretation that the California welfare experiment primarily operated through benefit levels rather than through other AFDC program changes. The effects of changing benefit levels are larger the longer a woman is exposed to a different benefit regime and primarily operate for initially married women in the AFDC-UP caseload. Further support for the interpretation given here is that AFDC-UP women's divorces (due to higher benefits) are accompanied by a movement onto the single-parent AFDC program. These women represent a small proportion of the overall AFDC caseload. In California, AFDC-UP cases comprised 18 percent of the average monthly caseload and 21 percent of total benefit payments in 1995 (U.S. House of Representatives, 1996). In the U.S., AFDC-UP cases represented 7 percent of the average monthly caseload and 10 percent of benefit expenditures in 1995. Extrapolating the Wave 2 estimated effects to the U.S., then, the overall effect on the probability of being married or cohabiting is approximately -0.0363 ($0.93\times(-0.0310)+0.07\times(-0.1062)$) for the average AFDC woman. Grogger and Bronars (1996) estimate that increasing benefits by 10 percent leads to a 2.4 percentage point decrease in the proportion of initially unwed mothers who marry, for white women 5 years after their first childbirth. For comparison, my estimates imply that a ten percent increase in benefits ($72) would decrease the proportion married by 2.6 percentage points.[37] An important caveat is that these results do not necessarily measure the long-run, steady-state effect of a higher benefit level on marriage rates. However, the fact that effects are empirically significant after 3½ years suggests that long-run effects may exist—a finding that previous studies have not had the power to support or reject.

The data used in this study allow us to address the question: If we change the benefit level, what happens to marriage and cohabitation rates for women on AFDC at the time of the change? A potentially important component of AFDC's total incentive effects on marriage that is not measured here is that higher benefits may lead women to become single mothers in order to get onto the caseload in the first place (see Moffitt (1992) for a fuller discussion of entry effects). The *existence* of the AFDC program may have bigger incentive effects than moderate changes in benefit levels (Murray 1984). This study's results do not necessarily indicate that AFDC-UP failed to encourage marriage in the low-income population. After all, much of the initial population of AFDC-UP couples may have been divorced had it not been for the availability of AFDC-UP benefits. Thus, AFDC-UP may have an *entry effect* that encourages marriage, but conditional on being in the program, marriage is discouraged if benefits in both AFDC programs are increased.

How should states use their new freedom to reform their welfare programs in light of these findings? The AFDC-UP program was originally mandated for all states partly due to a desire to reduce the marriage disincentive of AFDC-Basic. Until the 1996 welfare reform, benefit levels in the two programs were identical. It is not difficult to see that the incentive to be married could be increased by raising AFDC-UP benefit levels *relative* to AFDC-Basic benefit levels. We can also consider benefit levels as only one measure of welfare's "generosity" in a general sense. For example, tightening work requirements or imposing tighter time limits on single parents relative to two-parent recipient families

[37] The SIME-DIME results are difficult to compare to my estimates, due to the different structure of the negative income tax. Groeneveld, Tuma, and Hannan (1980) report that higher income guarantees actually *lowered* marital dissolution rates, even though the overall effect of the negative income tax was to discourage marriage.

may have important marriage-encouraging effects. In this way, the 1996 welfare reform may already have decreased the incentive to divorce, even without a change in benefit levels.

Policymakers also need to address the question of whether marriage is always a desirable outcome in the low-income population. Some studies (see McLanahan and Sandefur, 1994 for an example and references) suggest that children who grow up in single-parent families are likely to have worse outcomes in terms of school completion rates, teen childbearing, and “idleness” (neither being in school nor working). McLanahan and Sandefur (1994) also suggest that these outcomes are not due purely to a loss in income. Thus, a policy conundrum arises: how can the state provide income support to children in low-income families without greatly increasing the risk of losing a parent through divorce or delayed marriage? Of course, it is not *necessarily* the case that losing a parent will always harm the children: there are some cases in which a father or step-father may be a negative influence upon a child or may be abusive of the child. The prevalence of this situation among welfare recipient families is not well understood, nor is it known whether the negative effect of having a single parent is stronger or weaker at low levels of income. A plausible argument can be made that women who respond the most to benefit incentives against marriage are those women with the least attractive male partners, and that these male partners are not always good parents.

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Appendix

Attrition between Wave 1 and Wave 2 for AFDC-Basic and AFDC-UP women was 21 and 19 percent, respectively. I assess the importance of bias resulting from potential non-random attrition in two ways: (i) I estimate regression models explaining which women attrit, and (ii) I estimate a sample selection model. Appendix Table 5 shows the first set of regressions, where the dependent variable equals one if the woman attrits and zero otherwise. Significant predictors of attrition include education, age, and month of Wave 1 interview. Interestingly, Wave 1 marital status has no significant relationship to attrition. Thus, these regressions suggest that attrition based on unobservables related to marital status choice is unlikely to be important. (Attrition related to observable, exogenous characteristics such as age and education does not bias the previous regression results.)

Despite the lack of evidence for non-random attrition, it is useful to control for potential attrition bias in a more formal way. A standard method that accounts for attrition is a sample selection model, described by:

$$\begin{aligned} I_i^* &= X_{1i}\beta_1 + \varepsilon_{1i} \\ M_i^* &= X_{2i}\beta_2 + \varepsilon_{2i} \ \ if\ I_i^* \geq 0 \\ M_i &= 0 \ \ if\ M_i^* < 0 \\ M_i &= 1 \ \ if\ M_i^* \geq 0 \end{aligned} \qquad \textbf{(8)}$$

$$\begin{pmatrix} \varepsilon_{1i} \\ \varepsilon_{2i} \end{pmatrix} \sim N\left(0\ ,\begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}\right)$$

M_i indicates whether woman i is married in the Wave 2 data, a choice that is observed only if she remains in the survey ($I_i^* \geq 0$). Note that this differs from a standard Heckman model in that both equations have binary outcomes rather than continuous dependent variables. A common problem for selection models is that X_1 and X_2 typically include the same variables, so that identification relies on distributional assumptions. In my analysis, I rely on real exclusion restrictions for identification.

Specifically, I include indicators identifying survey interviewers in X_1 but not in X_2. Differences in interviewer skills may be correlated with respondents' decisions to remain in the survey for a second-round interview but are plausibly uncorrelated with Wave 2 marriage decisions and hence are excludable from X_2.

Full-information maximum-likelihood results for this selection model are presented in Appendix Table 6.[38] The predicted change in the probability of having a spouse or partner due to a $100 change in the benefit level is -0.0847 for AFDC-UP women and -0.0358 for AFDC-Basic women. Thus, attrition does not appear to bias the earlier estimates substantially. The Wald statistics for joint significance of the interviewer dummy variables are 9.93 for AFDC-UP women (3 constraints) and 8.48 for AFDC-Basic women (6 constraints), the first of which is significant at the 0.05 level; in addition, several variables are individually significant. Thus, the selection model is not identified purely by functional form assumptions.[39,40]

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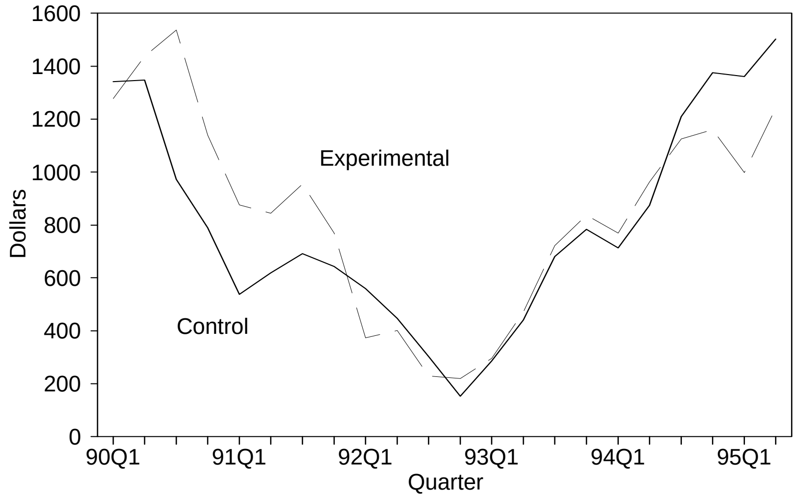
[38] The choice of interviewer dummies to include was determined as follows: First, I estimated an independent probit determining whether a woman stays in the sample, including all 19 interviewer dummies on the right-hand side. I then aggregated interviewers with similar coefficients and re-estimated the probit. I then dropped those variables that had estimated coefficients with *p*-values greater than 0.20 and used the remaining variables in the joint selection model. Thus, the variables included in the selection model are those indicating whether a woman's Wave 1 interviewer was significantly better or worse than average.

[39] I also estimated the selection model including the interviewer dummies in the marriage equation. These variables were jointly and individually insignificant in the marriage equation but retained their significance in the selection equation.

[40] The use of interviewer dummies may not be valid if particular interviewers were assigned to hard-to-interview cases. The design of the welfare survey was such that each respondent was initially assigned an interviewer at random; interviewers could be reassigned if a case proved to be difficult to reach. However, the dataset only indicates the interviewer who completed the interview; thus, there could be a correlation between interviewer and the respondent's desire to undergo an interview. In order to examine this possibility, I estimated probits of the selection equation only on the sample of respondents who completed the interview the first time they were contacted. For this sample, the estimated effects of the interviewer dummies on the probability of attrition were usually very similar to the estimated magnitudes in the full sample. (The exception was interviewer 4, which was therefore omitted from the full selection model.) Thus, while it is not ideal to include potentially endogenous interview dummies, there does not seem to be a significant problem with doing so.

Figure 1
Quarterly Male Earnings: AFDC-UP Cases



Appendix Table 1
Means and Standard Errors of Means for Regression Variables

	AFDC-Basic		*AFDC-UP*	
	Mean	S.E.	Mean	S.E.
Wave 1 Variables:				
Married/Cohabiting	0.1827	0.0101	0.8949	0.0117
Married	0.1006	0.0078	0.7184	0.0171
Divorced	0.2085	0.0106	0.0395	0.0074
Separated	0.1734	0.0099	0.0745	0.0100
Widowed	0.0241	0.0040	0.0077	0.0033
Cohabiting	0.0821	0.0072	0.1764	0.0145
AFDC Benefit 1	6.7464	0.0434	8.4814	0.0575
Wave 2 Variables:				
Married/Cohabiting*	0.2247	0.0122	0.8130	0.0165
Married*	0.1374	0.0101	0.7115	0.0192
Divorced*	0.2404	0.0125	0.0703	0.0108
Separated*	0.1473	0.0104	0.0924	0.0123
Widowed*	0.0332	0.0053	0.0124	0.0047
Cohabiting*	0.0874	0.0083	0.1015	0.0128
AFDC Benefit 1*	6.6286	0.0479	8.4381	0.0637
AFDC Benefit (family of four)	7.4621	0.0081	7.4631	0.0119
Control group	0.3571	0.0125	0.3587	0.0182
Less than high school	0.1428	0.0091	0.2930	0.0173
High school dropout	0.2796	0.0117	0.2771	0.0170
High school graduate	0.3253	0.0122	0.2493	0.0164
Any college	0.2522	0.0113	0.1805	0.0146
Black	0.3086	0.0120	0.0884	0.0108
Hispanic	0.3775	0.0126	0.5713	0.0188
Asian	0.0141	0.0031	0.0247	0.0059
Other race	0.0201	0.0037	0.0263	0.0061
Age <25	0.1542	0.0094	0.1270	0.0127
Age 25-34	0.4354	0.0129	0.4449	0.0189
Age 35-44	0.2814	0.0117	0.3420	0.0180
Age 45-54	0.0889	0.0074	0.0773	0.0102
Age 55+	0.0401	0.0051	0.0088	0.0035
Alameda County	0.1953	0.0103	0.1119	0.0120
Los Angeles County	0.4110	0.0128	0.4277	0.0188
San Bernardino County	0.2104	0.0106	0.3066	0.0175
San Joaquin County	0.1832	0.0101	0.1538	0.0137
Interviewed 10/93-12/93	0.4419	0.0130	0.3681	0.0183
Interviewed 1/94-3/94	0.3747	0.0126	0.3957	0.0186
Interviewed 4/94-6/94	0.0993	0.0078	0.1147	0.0121
Interviewed 7/94-9/94	0.0841	0.0072	0.1215	0.0124
Interviewed 5/95-7/95*	0.4152	0.0145	0.3507	0.0202
Interviewed 8/95-10/95*	0.3194	0.0137	0.2636	0.0187
Interviewed 11/95-1/96*	0.1302	0.0099	0.2121	0.0173
Interviewed 2/96-5/96*	0.1353	0.0100	0.1736	0.0160

Note: * indicates Wave 2 data. "AFDC Benefit 1" defines maximum benefit for each woman's family size. All benefit variables are divided by 100.

Appendix Table 2A
Transition Matrix of Marital Status

AFDC-Basic:

	Wave 2 status: Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	840	56	54	254	1204
	70	*5*	*4*	*21*	
Cohabiting	36	43	15	27	121
	30	*36*	*12*	*22*	
Married	25	4	90	27	146
	17	*3*	*62*	*18*	
Total	901	103	159	308	N=1471

AFDC-UP:

	Wave 2 status: Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	46	2	11	16	75
	61	*3*	*15*	*21*	
Cohabiting	19	47	23	33	122
	16	*39*	*19*	*27*	
Married	40	8	363	85	496
	8	*2*	*73*	*17*	
Total	105	57	397	134	N=693

Note: Unweighted row percentages are shown in italics.

Appendix Table 2B
Transition Matrix of Marital Status - AFDC-Basic

Control Group:

	Wave 2 status:				
	Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	312	19	18	84	433
	72	*4*	*4*	*19*	
Cohabiting	14	13	7	10	44
	32	*30*	*16*	*23*	
Married	9	0	32	8	49
	18	*0*	*65*	*16*	
Total	335	32	57	102	N=526

Treatment Group:

	Wave 2 status:				
	Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	528	37	36	170	771
	68	*5*	*5*	*22*	
Cohabiting	22	30	8	17	77
	29	*39*	*10*	*22*	
Married	16	4	58	19	97
	16	*4*	*60*	*20*	
Total	566	71	102	206	N=945

Note: Unweighted row percentages are shown in italics.

Appendix Table 2C
Transition Matrix of Marital Status - AFDC-UP

Control Group:

	Wave 2 status:				
	Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	16	1	4	5	26
	62	*4*	*15*	*19*	
Cohabiting	9	17	7	11	44
	20	*39*	*16*	*25*	
Married	22	2	125	27	176
	13	*1*	*71*	*15*	
Total	47	20	136	43	N=246

Treatment Group:

	Wave 2 status:				
	Unmarried	Cohabiting	Married	Attrited	Total
Wave 1 status:					
Unmarried	30	1	7	11	49
	61	*2*	*14*	*22*	
Cohabiting	10	30	16	22	78
	13	*38*	*21*	*28*	
Married	18	6	238	58	320
	6	*2*	*74*	*18*	
Total	58	37	261	91	N=447

Note: Unweighted row percentages are shown in italics.

Appendix Table 3A - Regression Coefficients corresponding to Table 4, Wave 1

	AFDC-Basic						*AFDC-UP*					
	OLS		IV		Probit		OLS		IV		Probit	
	Coef.	S.E.	Coef.	S.E.	dP/dX	S.E.	Coef.	S.E.	Coef.	S.E.	dP/dX	S.E.
AFDC Benefit ÷ 100	0.0110	0.0063	-0.0022	0.0362	-0.0030	0.0318	-0.0000	0.0081	-0.0113	0.0403	-0.0139	0.0351
Less than high school	0.0296	0.0358	0.0395	0.0448	0.0362	0.0374	0.0000	0.0378	0.0043	0.0407	0.0062	0.0374
High school dropout	0.0003	0.0260	0.0030	0.0270	0.0064	0.0263	-0.0267	0.0329	-0.0253	0.0333	-0.0280	0.0333
Any college	-0.0066	0.0264	-0.0089	0.0272	-0.0079	0.0264	-0.0428	0.0364	-0.0437	0.0366	-0.0400	0.0381
Black	-0.1520	0.0278	-0.1454	0.0331	-0.1336	0.0220	-0.0284	0.0470	-0.0244	0.0491	-0.0150	0.0418
Hispanic	-0.1061	0.0286	-0.0979	0.0363	-0.0845	0.0249	0.0393	0.0338	0.0483	0.0463	0.0426	0.0318
Asian	-0.1249	0.0859	-0.1139	0.0910	-0.0799	0.0573	0.0303	0.0783	0.0377	0.0826	0.0265	0.0584
Other race	0.0500	0.0722	0.0549	0.0735	0.0397	0.0713	-0.0571	0.0752	-0.0542	0.0761	-0.0468	0.0811
Age 25-34	0.0471	0.0297	0.0516	0.0321	0.0631	0.0331	0.0838	0.0373	0.0895	0.0423	0.0638	0.0295
Age 35-44	0.0492	0.0324	0.0551	0.0361	0.0697	0.0381	0.0874	0.0396	0.0968	0.0514	0.0649	0.0279
Age 45+	0.1378	0.0382	0.1337	0.0398	0.1632	0.0516	0.1257	0.0529	0.1231	0.0538	0.0810	0.0220
Alameda	-0.0338	0.0307	-0.0386	0.0334	-0.0481	0.0294	-0.0457	0.0444	-0.0481	0.0452	-0.0359	0.0489
San Bernardino	0.0482	0.0304	0.0493	0.0305	0.0482	0.0324	-0.0040	0.0343	-0.0022	0.0349	-0.0006	0.0341
San Joaquin	0.0441	0.0312	0.0424	0.0316	0.0450	0.0333	0.0375	0.0398	0.0381	0.0399	0.0353	0.0326
Interviewed 1/94-3/94	0.0473	0.0248	0.0464	0.0250	0.0492	0.0259	0.0417	0.0332	0.0372	0.0369	0.0417	0.0292
Interviewed 4/94-6/94	-0.0036	0.0361	-0.0083	0.0384	-0.0086	0.0374	0.0067	0.0437	0.0054	0.0440	0.0022	0.0377
Interviewed 7/94-9/94	0.0308	0.0415	0.0301	0.0416	0.0406	0.0462	0.0093	0.0487	0.0047	0.0514	0.0031	0.0457
Constant	0.1100	0.0565	0.1907	0.2254			0.7953	0.0799	0.8802	0.3080		
N	1471		1471		1471		693		693		693	

Note: Reference category is a white, younger than 25-year old high school graduate in Los Angeles county interviewed between Oct-Dec 1993.

Appendix Table 3B - Regression Coefficients corresponding to Table 4, Wave 2

	AFDC-Basic						*AFDC-UP*					
	OLS		IV		Probit		OLS		IV		Probit	
	Coef.	S.E.	Coef.	S.E.	dP/dX	S.E.	Coef.	S.E.	Coef.	S.E.	dP/dX	S.E.
AFDC Benefit ÷ 100	0.0130	0.0078	-0.0386	0.0504	-0.0310	0.0392	0.0072	0.0115	-0.0965	0.0452	-0.1062	0.0505
Less than high school	0.0078	0.0452	0.0423	0.0568	0.0185	0.0456	0.0252	0.0550	0.0461	0.0596	0.0401	0.0535
High school dropout	-0.0377	0.0321	-0.0269	0.0344	-0.0361	0.0315	-0.0147	0.0453	-0.0243	0.0488	-0.0232	0.0442
Any college	0.0122	0.0310	0.0128	0.0316	0.0143	0.0318	0.0157	0.0492	-0.0185	0.0546	0.0066	0.0450
Black	-0.1596	0.0336	-0.1305	0.0443	-0.1460	0.0284	-0.0653	0.0646	-0.0390	0.0702	-0.0540	0.0661
Hispanic	-0.0677	0.0344	-0.0330	0.0484	-0.0487	0.0316	0.1147	0.0452	0.1930	0.0586	0.1067	0.0435
Asian	-0.0907	0.0992	-0.0594	0.1055	-0.0622	0.0786	0.0843	0.1102	0.1916	0.1264	0.0687	0.0753
Other race	0.0897	0.0914	0.1081	0.0948	0.0807	0.0974	-0.0351	0.1016	-0.0141	0.1092	-0.0478	0.1069
Age 25-34	0.0019	0.0385	0.0372	0.0519	0.0096	0.0395	0.0940	0.0554	0.1665	0.0668	0.0783	0.0471
Age 35-44	-0.0034	0.0414	0.0291	0.0525	0.0039	0.0425	0.1313	0.0578	0.2160	0.0714	0.1025	0.0462
Age 45+	0.0507	0.0481	0.0276	0.0538	0.0522	0.0528	0.1861	0.0761	0.1452	0.0834	0.1342	0.0383
Alameda	-0.0367	0.0355	-0.0516	0.0390	-0.0499	0.0355	-0.0682	0.0595	-0.0812	0.0640	-0.0824	0.0707
San Bernardino	0.0662	0.0351	0.0685	0.0358	0.0653	0.0376	-0.0879	0.0469	-0.0606	0.0515	-0.0970	0.0533
San Joaquin	0.0743	0.0364	0.0731	0.0371	0.0713	0.0397	-0.0197	0.0533	0.0077	0.0582	-0.0321	0.0575
Interviewed 8/95-10/95	-0.0336	0.0296	-0.0410	0.0310	-0.0365	0.0292	-0.0874	0.0461	-0.0875	0.0494	-0.0844	0.0494
Interviewed 11/95-1/96	0.0144	0.0409	0.0004	0.0439	0.0099	0.0421	0.0080	0.0520	0.0025	0.0558	0.0339	0.0490
Interviewed 2/96-5/96	-0.0075	0.0407	-0.0154	0.0422	-0.0075	0.0404	-0.1109	0.0597	-0.1061	0.0640	-0.1308	0.0741
Constant	0.2013	0.0679	0.4996	0.2957			0.6633	0.1117	1.4232	0.3405		
N	1163		1163		1163		559		559		559	

Note: Reference category is a white, younger than 25-year old high school graduate in Los Angeles county interviewed between May-Jul 1995.

Appendix Table 4
Interactions of Benefit Level with Demographic Characteristics

	AFDC-Basic				*AFDC-UP*			
	Wave 1		Wave 2		Wave 1		Wave 2	
	dP/dB	S.E.	dP/dB	S.E.	dP/dB	S.E.	dP/dB	S.E.
Age <25	0.1210	0.0874	0.1977	0.1070	-0.0630	0.0713	-0.1655	0.1363
Age 25-34	-0.0027	0.0488	-0.0251	0.0600	-0.0212	0.0458	-0.0940	0.0737
Age 35-44	-0.0241	0.0588	-0.1112	0.0733	0.0090	0.0545	-0.1047	0.0874
Age 45+	-0.0683	0.0811	-0.0953	0.1019	0.9671*	0.1166	-0.0752	0.2182
Less than high school	0.0474	0.0753	0.0484	0.1009	-0.1247	0.0729	-0.1702	0.1156
High school dropout	0.0288	0.0614	0.0240	0.0809	0.0022	0.0639	-0.1335	0.0948
High school graduate	-0.0689	0.0568	-0.0937	0.0691	-0.0049	0.0671	-0.0942	0.0916
Any college	0.0119	0.0650	-0.0462	0.0730	0.0643	0.0745	-0.0301	0.1072
White	0.0264	0.0523	-0.0054	0.0645	0.0353	0.0550	-0.1689*	0.0772
Black	-0.0056	0.0662	0.0217	0.0782	0.0093	0.0977	-0.0398	0.1453
Hispanic	-0.0283	0.0504	-0.0889	0.0637	-0.0662	0.0501	-0.0717	0.0725
Asian & other	-0.0197	0.0671	-0.0428	0.0800	0.0221	0.0581	0.0397	0.2197
Alameda	-0.1144	0.0905	0.0022	0.0987	0.1394	0.1003	-0.0377	0.1375
Los Angeles	0.0201	0.0487	-0.0622	0.0640	-0.0985	0.0554	-0.1825*	0.0859
San Bernardino	0.0318	0.0656	0.0256	0.0802	0.0049	0.0585	-0.0915	0.0848
San Joaquin	-0.0193	0.0696	-0.0620	0.0834	0.0274	0.0940	-0.0351	0.1221
Time on aid: 1st quartile	0.1488*	0.0610	0.0355	0.0785	-0.1008	0.0754	-0.2182*	0.1102
Time on aid: 2nd quartile	-0.1832*	0.0684	-0.0776	0.0789	-0.0375	0.0748	-0.3494*	0.1076
Time on aid: 3rd quartile	-0.0060	0.0700	0.0158	0.0865	0.0315	0.0659	0.0037	0.0919
Time on aid: 4th quartile	0.0038	0.0584	-0.0777	0.0725	0.0099	0.0645	0.0318	0.0933

Note: "White" indicates non-Hispanic white women. * indicates significance at 0.05 level. Time on aid quartiles refer to months on aid during the five years prior to the start of the experiment. The anomalously large effect for AFDC-UP women aged 45 and older in Wave 1 is due to the fact that derivatives are evaluated at the mean of the data. The coefficient from a linear probability model is merely 0.0795.

Appendix Table 5
Probit Regressions Predicting Attrition between Survey Waves

	AFDC-Basic		*AFDC-UP*	
	dP/dX	S.E.	dP/dX	S.E.
Control group	-0.0255	0.0222	-0.0323	0.0305
Less than high school	0.0679	0.0398	0.1310*	0.0519
High school dropout	0.0100	0.0276	0.0182	0.0432
Any college	-0.0890*	0.0295	0.0029	0.0525
Black	-0.0235	0.0318	0.0456	0.0660
Hispanic	-0.0147	0.0313	-0.0137	0.0442
Asian	-0.0783	0.0948	0.0561	0.1038
Other race	0.0640	0.0840	-0.1063	0.1030
Age 25-34	-0.1306*	0.0314	-0.0781	0.0429
Age 35-44	-0.1642*	0.0349	-0.1954*	0.0450
Age 45+	-0.1258*	0.0422	-0.1675*	0.0413
Alameda	-0.0537	0.0318	-0.0632	0.0508
San Bernardino	-0.0189	0.0323	-0.0488	0.0422
San Joaquin	-0.0557	0.0322	-0.0162	0.0486
Interviewed 1/94-3/94	0.0197	0.0276	0.0599	0.0445
Interviewed 4/94-6/94	0.1667*	0.0420	0.1935*	0.0662
Interviewed 7/94-9/94	0.0727	0.0470	0.1747*	0.0724
Time on aid: 1st quartile	-0.0210	0.0324	-0.0765	0.0409
Time on aid: 2nd quartile	-0.0225	0.0303	-0.0597	0.0409
Time on aid: 3rd quartile	-0.0129	0.0303	-0.0790*	0.0379
Married	0.0422	0.0495	-0.0557	0.0774
Divorced	-0.0066	0.0334	-0.0473	0.0749
Separated	0.0483	0.0323	-0.0068	0.0757
Widowed	-0.0502	0.0728	-0.0558	0.1699
Cohabiting	0.0736	0.0546	-0.0029	0.0714
Number of children	0.0076	0.0078	0.0019	0.0111
Number of adults	-0.0020	0.0140	-0.0117	0.0240
In school	-0.0024	0.0296	-0.0642	0.0454
Working	-0.0563*	0.0287	0.0512	0.0460
Spouse in school	0.0108	0.1170	-0.0322	0.0482
Spouse working	-0.1033*	0.0442	0.0159	0.0360
Health poor	0.0616	0.0379	-0.0474	0.0510
Health limits work	-0.0333	0.0277	0.0809	0.0449
Has child living elsewhere	0.0504	0.0299	0.1582*	0.0488
Woman living with parents	-0.0242	0.0365	-0.0344	0.0720
Homeless in past year	0.2117*	0.0685	-0.0972	0.0921
N	1467		691	

Note: Probability derivatives from probit regressions are shown. * indicates significance at 0.05 level.

Appendix Table 6
Selection Model Maximum-Likelihood Estimates

	AFDC-Basic Coef.	S.E.		*AFDC-UP* Coef.	S.E.
Selection Equation:					
Constant	0.7838	0.4284	Constant	-0.1433	1.0431
Control group	0.0872	0.0838	Control group	0.1285	0.1407
Less than high school	-0.2643	0.1364	Less than high school	-0.4607*	0.2068
High school dropout	-0.1280	0.0975	High school dropout	-0.1413	0.1865
Any college	0.4158*	0.1167	Any college	0.1470	0.2270
Black	-0.0126	0.1088	Black	-0.2455	0.2549
Hispanic	0.0377	0.1135	Hispanic	0.1234	0.1958
Asian	0.1189	0.3785	Asian	-0.3002	0.4049
Other race	-0.3200	0.2789	Other race	0.1426	0.5002
Age/10	-0.0051	0.2169	Age/10	0.6828	0.5832
$Age^2/1000$	0.0845	0.2858	$Age^2/1000$	-0.7910	0.8229
Alameda	0.1981	0.1186	Alameda	0.2613	0.2170
San Bernardino	0.0105	0.1244	San Bernardino	0.0787	0.2161
San Joaquin	0.1893	0.1122	San Joaquin	0.0961	0.2088
Interviewed 1/94-3/94	-0.1030	0.0966	Interviewed 1/94-3/94	-0.4221*	0.2071
Interviewed 4/94-6/94	-0.5820*	0.1308	Interviewed 4/94-6/94	-0.7040*	0.2336
Interviewed 7/94-9/94	0.1139	0.2069	Interviewed 7/94-9/94	-0.5852*	0.2728
Interviewers 2,10,11	-0.1238	0.0989	Interviewer 11	-0.8992*	0.4296
Interviewers 3,18	-0.5060*	0.2261	Interviewer 15	0.3812	0.2546
Interviewer 8	-0.1192	0.1327	Interviewer 16	0.6267*	0.3065
Interviewer 9	-0.6708	0.3697	Density $\varphi(\overline{X}_1\beta_1)$	0.258	
Interviewer 12	-0.1879	0.1267			
Interviewer 19	-0.7487*	0.3758			
Density $\varphi(\overline{X}_1\beta_1)$	0.281				
Marriage Equation:					
Constant	1.1778	1.0467	Constant	2.1493	2.4193
AFDC Benefit	-0.1420	0.1257	AFDC Benefit	-0.3673	0.2455
Less than high school	0.1616	0.1465	Less than high school	0.0284	0.3146
High school dropout	-0.0520	0.1098	High school dropout	-0.0990	0.1954
Any college	-0.0938	0.1186	Any college	0.0448	0.2030
Black	-0.4701*	0.1310	Black	-0.2802	0.2406
Hispanic	-0.1499	0.1066	Hispanic	0.4324*	0.1879
Asian	-0.2515	0.3017	Asian	0.2355	0.4036
Other race	0.3251	0.2573	Other race	-0.1179	0.4668
Age/10	-0.2259	0.1960	Age/10	0.6327	0.7551
$Age^2/1000$	0.3197	0.2449	$Age^2/1000$	-0.5179	1.1646
Alameda	-0.1843	0.1210	Alameda	-0.2310	0.2359
San Bernardino	0.1734	0.1183	San Bernardino	-0.3056	0.2370
San Joaquin	0.1411	0.1198	San Joaquin	-0.0643	0.2131
Interviewed 8/95-10/95	-0.0633	0.0965	Interviewed 8/95-10/95	-0.3371	0.1913
Interviewed 11/95-1/96	0.1091	0.1254	Interviewed 11/95-1/96	0.0965	0.2585
Interviewed 2/96-5/96	0.0718	0.1323	Interviewed 2/96-5/96	-0.5441*	0.2729
Density $\varphi(\overline{X}_2\beta_2)$	0.380		Density $\varphi(\overline{X}_2\beta_2)$	0.279	
Rho	-0.7679	0.2080	Rho	0.4951	0.5515

Note: Asymptotic standard errors are presented. * indicates significance at 0.05 level.