Marriage and Economic Incentives
Evidence from a Welfare Experiment

Wei-Yin Hu

A B S T R A C T

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 benefit incentives arises from a randomized experiment carried out in California that allows me to measure responses beyond simple year-to-year changes in benefit levels. I find that a regime of lower benefits and stronger work incentives encourages married aid recipients to stay married, but has little effect on the probability that single-parent aid recipients marry. The effects on married recipients become larger over time, suggesting that long-run effects may exist.

I. Introduction

‘‘The decline of the American family’’ has been a catchphrase 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. As an example of the primacy of this question, a 2002 House of Representatives welfare reform bill included $300 million for policies to promote marriage.

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 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. Although 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. A further difficulty with within-state, over-time comparisons is that they generally can 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 or the nature of the treatment itself (Moffitt and Kehrer 1981; Moffitt 1992). 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; Hannan and Tuma 1990).

This study avoids the previously mentioned difficulties of previous empirical studies in two major ways: (1) the source of variation in welfare benefits is a social experiment with simple random assignment, and (2) repeated observations on the treatment and control groups allow me to distinguish between short-term and long-

---

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.

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 program changes 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 low-income women's marriage decisions. The evidence suggests that stronger work incentives (from a combination of lower welfare benefits and lower benefit reduction rates) significantly increase marital stability for poor two-parent families. These effects are larger the longer a woman is in such a benefit regime. There is no evidence that this treatment either encourages or discourages marriage among single-parent welfare recipients. The next section describes the incentives of the AFDC program, Section III describes the data and the social experiment in California, Section IV explains the empirical results, and Section V concludes.

II. Marriage and Cohabitation Incentives of the AFDC Program

The standard description of the AFDC[2] 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: (1) AFDC benefits are available to married couples, and (2) 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 the 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.[3] Before the 1996 welfare reform, AFDC-UP applied the following rules. Eligibility in the UP program is conditioned on the primary earner having a significant attachment to the labor force[4] 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

---

2. 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 prewelfare reform era.

3. 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.

4. Significant attachment is defined as having worked and earned at least $50 in at least six of the previous 13 calendar quarters, or having been eligible for unemployment compensation benefits during the previous year.

AFDC program,[5] in which an AFDC-UP family with two adults and two kids re-
ceives benefits applicable to an AFDC-Basic family with one parent and three 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 thus work in the
opposite direction.

   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. Further-
more, 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.[6] In the case of cohabiting, nonparent males, 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 recip-
ients are married—a proportion too large to be accounted for by AFDC-UP recipi-
ents. In the empirical analysis, I will distinguish between welfare's effects on mar-
riage 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 chil-
dren.

   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 bene-
fit levels corresponding to California:

   In California, the maximum monthly benefit is $607 for a family of three and
$723 for a family of four. Most important, Table 1 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 incen-
tives against marriage may be affected both by the absolute level of benefits (Cases
3 and 4) and by the relative size 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 poten-
tially 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 one cannot adequately

---

5. 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.
6. This rule applies in all but seven states. See Moffitt, Reville, and Winkler (1994).

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

define the potential set of spouses or cohabitors. In the following empirical work, I will attempt to determine whether marriage behavior responds to the differential treatment of parents and nonparents. 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.

Another potential policy lever that may affect marriage is the break-even level of income—that is, the level of earnings at which welfare benefits are reduced to zero. For AFDC-UP families facing low break-even income levels, the inability of the primary earner to keep earned income without making the family welfare-ineligible may be a strong disincentive for marriage or cohabitation. Viewed in this light, welfare programs can be structured to achieve two policy goals at the same time: promoting work effort in two-parent families and enhancing marital stability.

In the discussion above, I have ignored other income maintenance programs such as General Assistance (GA) and the Earned Income Tax Credit (EITC).[7] A low-income 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

---

7. 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).

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 data set allows me to isolate the effect of AFDC program changes holding other program parameters constant.

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 $2\frac{1}{2}$-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.[8]

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 intended to increase work incentives for the treatment group: maximum benefit levels were decreased, and, for those recipients in spells lasting longer than four months, the benefit reduction rate was reduced from 100 percent to 67 percent. In addition, the treatment extended the $30-per-month income disregard past the initial 12 months of AFDC receipt.[9] 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 versus two southern counties, and two counties with large urban centers versus two rural counties. The research design selected a large number of cases (about 15,000) from the baseline caseload as of December 1992,[10] 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. Cases that left AFDC and subsequently returned retained their original control-treatment status. The benefit levels under the experiment are shown 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.)

8. 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.

9. Prior to this change, the $30 disregard applied only for the first 12 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.

10. 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.

Table 2
Maximum Monthly AFDC Benefit Payments

Family Size	Control Group	Treatment Group December 1992–August 1993	Treatment Group September 1993–December 1996
1	326	307	299
2	535	504	490
3	663	624	607
4	788	743	723
5	899	847	824
6	1,010	952	926
7	1,109	1,045	1,017
8	1,209	1,139	1,108
9	1,306	1,230	1,197
10	1,403	1,322	1,286

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

Other changes in the AFDC program were instituted at the same time, 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 $1,500 to $4,500, allowable resources increased from $1,000 to $2,000, and savings accounts up to $5,000 for specialized purposes such as children's college education, downpayment 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 an aid check but continue to receive only Medicaid coverage and child care assistance. Effective May 1994.

- For treatment cases, the need standard[11] 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.

---

11. The need standard (NS) affects benefits in the following way. Benefits paid are equal to max[0, min{B, $NS - t \times$ earnings}], where B is the maximum monthly benefit and t is the benefit reduction rate. In many states, B and NS are identical; in California, $NS > B$.

The primary data in this analysis come from merging two data sets: longitudinal case histories of all 15,000 demonstration cases, dating from January 1988 through September 1997, and a computer-aided telephone survey of a smaller subgroup (2,214 cases) 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 appendix (available from the author) includes several supplemental analyses that indicate that attrition does not bias the measured effects of welfare benefits. (The administrative data on whether welfare was received is available even for families that attrited from the household survey.) The analysis sample includes 2,164 women respondents (out of 2,214 survey respondents, 49 men were dropped and one 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 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 righthand 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 deal with the following questions: (A) What was the effect of the California welfare experiment on marriage rates? (B) What explains differences between transitions into marriage and transitions out of marriage? And (C) Do marriage and cohabitation respond in predictable ways? An examination of whether nonrandom attrition biases the estimates is presented in the Appendix.

A. 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

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

	Wave 1			Wave 2		
	Control	Treatment	T-C	Control	Treatment	T-C
No covariates						
AFDC-Basic	0.1812	0.1836	0.0023	0.2124	0.2316	0.0192
	(10.7857)	(14.5714)	(0.1080)	(10.6734)	(14.9419)	(0.7442)
AFDC-UP	0.8850	0.8996	0.0146	0.7642	0.8398	0.0756*
	(43.3824)	(63.3521)	(0.5887)	(25.5585)	(43.0667)	(2.1787)
Covariates included						
AFDC-Basic			0.0023			0.0214
			(0.1095)			(0.8458)
AFDC-UP			0.0091			0.0761*
			(0.3669)			(2.2317)

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

and treatment groups. I also separate women according to whether they started the experiment in the AFDC-Basic program versus the AFDC-UP program. The labels ''AFDC-Basic'' and ''AFDC-UP'' in this and subsequent tables 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 are transitions out of marriage (among AFDC-UP women). This distinction has not been explored in the nonexperimental literature.

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 treatment effect after controlling for demographic variables (via ordinary least-squares (OLS) regression): Those results are the same as the raw differences.[12] 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.*

A further finding is that the AFDC-Basic estimates argue against a large effect on marriage in that population. With a point estimate of 0.0192 and a standard error

---

12. Full regression results are reported in Appendix Table 3.

of 0.0258, a treatment effect larger than 0.0616 can be ruled out with a one-tailed test. If all of this treatment effect were attributable to changes in the benefit level rather than the benefit reduction rate (or other components of the treatment), then this suggests that a $100 decrease in the benefit level for a family of four would have smaller than a 0.0948 effect on marriage.[13]

The tabulations are performed separately for Wave 1 and Wave 2 because the treatment 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 infrequent events such as marriage or divorce. In contrast, Wave 2 interviews take place between 29 and 41 months after the start of the experiment. The comparison of estimates from Wave 1 and Wave 2 in Table 3 shows that the welfare effects grow larger 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. As a result, 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 is explored in the next section.

B. Marital Formation versus Marital Dissolution

Why is the experimental 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 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.

It is natural to ask why the marriage effect grows stronger over time mainly for AFDC-UP women. Note first that the earnings of male partners of women initially on AFDC-UP are by definition low enough to qualify for benefits. Yet, as time passes, one might 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. If this effect is large enough, then AFDC-UP effectively ceases to become an option for many women over time, and the attraction of higher benefits (for control group women) in AFDC-Basic in turn causes a higher divorce rate in the control group.

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

---

13. This conversion uses the fact that the treatment-control difference in the maximum benefit for a family of four is $65 per month, so $0.0948 = 0.0616 * 100/65$.
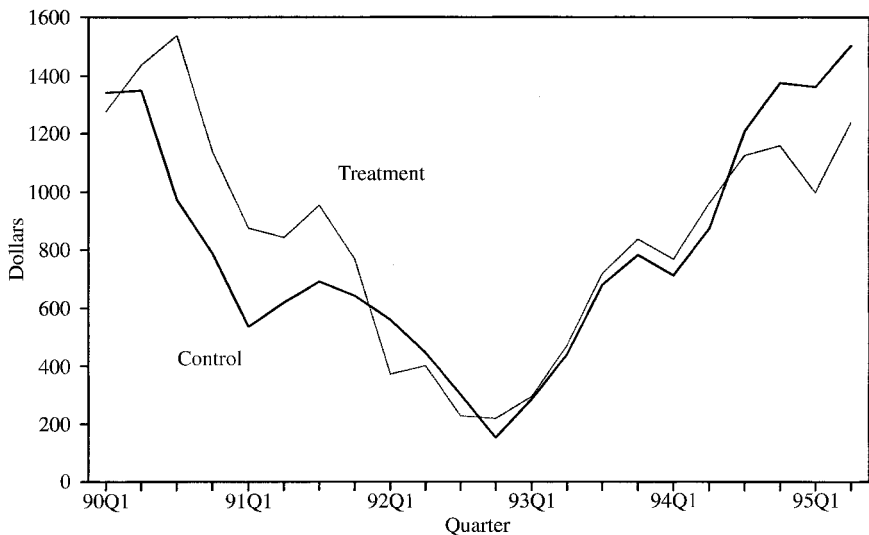
Figure 1
Quarterly Male Earnings: AFDC-UP Cases

the woman's welfare case at the time of sampling in 1992; these data provide infor-
mation 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.[14] 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.[15] For women initially on AFDC-
Basic, the 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.[16]

Finally, to provide a further test of the interpretations offered here, one can catego-

---

14. 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 women in the control and treatment groups in whether
they have EDD matches.
15. 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.
16. Unfortunately, I cannot test this hypothesis directly because I do not have data on potential male
partners in AFDC-Basic cases.

Table 4
Estimated Effects of Experimental Treatment on Marriage and Cohabitation

	Wave 1		Wave 2	
	Marriage	Cohabitation	Marriage	Cohabitation
AFDC-Basic	0.0085	−0.0061	0.0013	0.0200
	(0.482)	(0.366)	(0.213)	(1.149)
AFDC-UP	0.0044	0.0035	0.0617*	0.0101
	(0.314)	(0.309)	(2.163)	(1.437)

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. * indicates significance at 0.05 level.

rize 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 (ignoring the difficult interpretation when stratifying on a lagged endogenous variable). The treatment effect on marriage in Wave 2 is significant and positive 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 nearly identical to the effect reported in Table 3 for AFDC-UP women. (These results are available from the author.[17]) Thus, the experimental effects seem to reflect effects on marital dissolution, rather than effects for a peculiar population of AFDC-UP recipients.

C. 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 *might* 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 might ultimately have consequences for expenditures on children). Table 4 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.

Recall that the only significant effect from Table 3 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

17. 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 treatment effect (between Wave 1 and Wave 2) for these women would not reflect the treatment effect that should apply to *married AFDC recipients*.

Table 5
Multinomial Logit Estimated Effects of Experimental Treatment

	Wave 1 Probabilities			Wave 2 Probabilities		
	Actual	Predicted	*dP/dT*	Actual	Predicted	*dP/dT*
(i) Single	85.70	84.84	−0.0003	82.88	81.31	−0.0117
(ii) Cohabit, nonparent	3.12	3.30	−0.0012	3.07	3.45	−0.0095
(iii) Cohabit, parent	4.12	4.70	−0.0029	4.37	4.74	0.0348*
(iv) Marry, nonparent	0.55	0.71	−0.0014	0.59	1.03	0.0028
(v) Marry, parent	6.51	6.45	0.0057	9.09	9.47	−0.0165

Note: *dP/dT* is the mean derivative of the probability with respect to the experimental treatment. * indicates significance at the 0.05 level.

two separate probits with the dependent variables being binary indicators of marriage and cohabitation, respectively.) Thus, it appears that welfare incentives have more of an effect on longer-term commitments through marriage rather than on choices of living arrangements alone. Most of the welfare-induced transitions in the AFDC-UP population occur between marriage and female headship.[18]

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.

In order to determine whether the choice between marriage and cohabitation responds to these welfare incentives, I estimated a multinomial logit where the choices are (i) female headship, (ii) cohabit with a nonparent male, (iii) cohabit with a parent male, (iv) marry a nonparent male, and (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 nonparent male, so I restrict the sample for this logit model to those women initially from the AFDC-Basic population. In order to conserve degrees of freedom, the only regressor in this logit is the treatment dummy variable. Adding other regressors does not affect the coefficient, since the treatment was randomly assigned and hence orthogonal to other potential variables. Table 5 below reports actual and predicted probabilities and probability derivatives.

Women during Wave 2 are significantly more likely to cohabit with the parent as a result of the experimental treatment. This effect can be explained by two features of the treatment: a lower benefit level and a more generous allowance for earned income. The treatment's lower benefit level means that for women whose potential male partners' earnings are above the eligibility limit for AFDC-UP benefits, the loss of benefits is now a smaller disincentive to cohabitation. Such women would be more likely to cohabit than live alone. For women with partners whose earnings

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

are near the point of making them ineligible for AFDC, the treatment allows some couples to cohabit and maintain benefits. Thus, the one significant effect in Table 5 is consistent with benefit incentives.

V. Discussion and Conclusions

I have presented evidence that AFDC's incentives relating to marriage and cohabitation have large and statistically significant effects on the behavior of low-income women. 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; the inferences are not confounded by simultaneous changes across states in other policies or economic factors affecting marriage. The effects of changing benefit incentives 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. 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 United States, AFDC-UP cases represented 7 percent of the average monthly caseload and 10 percent of benefit expenditures in 1995. An important caveat is that these results do not necessarily measure the long-run, steady-state effect of the treatment on marriage rates. However, the fact that effects are empirically significant after $3\frac{1}{2}$ years suggests that long-run effects may exist—a finding that previous studies have not had the power to support or reject.

Another limitation of the current study is that the treatment was multifaceted. While one cannot isolate the effects of changes in maximum benefit levels,[19] the evidence that any package of incentives *does* affect marriage is significant.

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 more extensive 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-

---

19. See Hu (1998) for an extended discussion of results that are suggestive of the treatment effect being attributable to the benefit level change.

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 may have important marriage-encouraging effects. In this way, the 1996 welfare reform already may 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 stepfather 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. This study's finding of an effect of welfare on marriage bolsters the case for pursuing further research on marriage's effects on well-being.

Appendix Table 1
Means and Standard Errors of 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
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
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	0.3755	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	32.65	0.2559	32.22	0.2972
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.

Appendix Table 2A
Transition Matrix of Marital Status

	Wave 2 Status				
	Unmarried	Cohabiting	Married	Attrited	Total
AFDC-Basic					
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 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

	Wave 2 Status				
	Unmarried	Cohabiting	Married	Attrited	Total
Control					
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 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

	Wave 2 Status				
	Unmarried	Cohabiting	Married	Attrited	Total
Control					
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 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 3
Regression Coefficients Corresponding to Table 3

	Wave 1				Wave 2			
	AFDC-Basic		AFDC-UP		AFDC-Basic		AFDC-UP	
	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
Treatment group	0.0023	0.1097	0.0091	0.3685	0.0214	0.0253	0.0761	2.2341
Less than high school	0.0305	0.8542	−0.0077	−0.2048	0.0050	0.1103	0.0214	0.3950
High school dropout	0.0004	0.0153	−0.0263	−0.7965	−0.0371	−1.1590	−0.0174	−0.3856
Any college	−0.0059	−0.2237	−0.0433	−1.1872	0.0139	0.4486	0.0145	0.2972
Black	−0.1521	−5.5067	−0.0345	−0.7337	−0.1558	−4.6800	−0.0778	−1.2165
Hispanic	−0.1016	−3.5897	0.0416	1.2561	−0.0592	−1.7420	0.1168	2.6603
Asian	−0.1222	−1.4203	0.0331	0.4234	−0.0902	−0.9090	0.0951	0.8766
Other race	0.0561	0.7778	−0.0649	−0.8587	0.0997	1.0927	−0.0482	−0.4789
Age/10	−0.0029	−0.0552	0.1262	1.2086	−0.0715	−1.1190	0.2045	1.3894
Age^2/1000	0.0531	0.7822	−0.1288	−0.8554	0.1163	1.4348	−0.2049	−0.9564
Alameda	−0.0364	−1.1874	−0.0447	−1.0074	−0.0404	−1.1420	−0.0699	−1.1851
San Bernardino	0.0464	1.5272	−0.0035	−0.1020	0.0640	1.8288	−0.0795	−1.7227
San Joaquin	0.0384	1.2299	0.0358	0.9029	0.0683	1.8829	−0.0181	−0.3442
Interviewed 1/94–3/94	0.0471	1.8939	0.0398	1.2020				
Interviewed 4/94–6/94	−0.0071	−0.1989	−0.0037	−0.0836				
Interviewed 7/94–9/94	0.0279	0.6704	0.0030	0.0626				
Interviewed 8/95–10/95					−0.0336	−1.1320	−0.0878	−1.9246
Interviewed 11/95–1/96					0.0173	0.4212	0.0035	0.0679
Interviewed 2/96–5/96					−0.0095	−0.2330	−0.1084	−1.8358
Constant	0.1830	1.7614	0.6058	3.3461	0.3758	3.0104	0.3458	1.3664
N	1,471		693		1,163		559	

Note: Reference category is a white, younger than 25-year-old high school graduate in Los Angeles county interviewed between Oct–Dec 1993 (Wave 1) or between May–Jul 1995 (Wave 2).

References

Becerra, Rosina, Alisa Lewin, Michael Mitchell, and Hiromi Ono. 1996. ''California Work Pays Demonstration Project: Interim Report of First Thirty Months.'' School of Public Policy and Social Research, University of California, Los Angeles.

Cain, Glen, and Douglas Wissoker. 1990. ''A Reanalysis of Marital Stability in the Seattle-Denver Income-Maintenance Experiment.'' *American Journal of Sociology* 95(5): 1235–69.

California Work Pays Demonstration Project: County Welfare Administrative Database, Public Use Version 1. [machine-readable data file]. 1995. Berkeley, Calif.: Research Branch, California Department of Social Services and UC Data Archive & Technical Assistance, University of California [producers]. Berkeley, Calif.: UC Data Archive and Technical Assistance, University of California [distributor].

California Work Pays Demonstration Project Survey: English-Spanish Interviews, 1993– 1994 Public Use Version 1 [machine-readable data file]. 1995. Berkeley, Calif.: Survey Research Center, University of California [producer]. Berkeley, Calif.: UC Data Archive and Technical Assistance, University of California [distributor].

California Work Pays Demonstration Project Survey: English-Spanish Interviews, 1995– 1996 Preliminary Release [machine-readable data file]. 1996. Berkeley, Calif.: Survey Research Center, University of California [producer]. Berkeley, Calif.: UC Data Archive and Technical Assistance, University of California [distributor].

Groeneveld, Lyle, Nancy Tuma, and Michael Hannan. 1980. ''The Effects of Negative Income Tax Programs on Marital Dissolution.'' *Journal of Human Resources* 15(4): 654– 74.

Hannan, Michael, and Nancy Tuma. 1990. ''A Reassessment of the Effect of Income Maintenance on Marital Dissolution in the Seattle-Denver Experiment.'' *American Journal of Sociology* 95(5):1270–98.

Hu, Wei-Yin. 1998. ''Marriage and Economic Incentives: Evidence from a Welfare Experiment.'' Unpublished.

McLanahan, Sara, and Gary Sandefur. 1994. *Growing Up With A Single Parent: What Hurts, What Helps*. Cambridge, Mass.: Harvard University Press.

Michel, Richard. 1980. ''Participation Rates in the Aid to Families with Dependent Children Program, Part I.'' Urban Institute Working Paper 1387–02.

Moffitt, Robert. 1992. ''Evaluation Methods for Program Entry Effects.'' In *Evaluating Welfare and Training Programs*, ed. Charles Manski and Irwin Garfinkel. Cambridge, Mass.: Harvard University Press.

———. 1994. ''Welfare Effects on Female Headship with Area Effects.'' *Journal of Human Resources* 29(2):621–36.

———. 1997. ''The Effect of Welfare on Marriage and Fertility: What Do We Know and What Do We Need to Know?'' Institute for Research on Poverty Discussion Paper no. 1153–97.

Moffitt, Robert, and Kenneth Kehrer. 1981. ''The Effect of Tax and Transfer Programs on Labor Supply: The Evidence from the Income Maintenance Experiments.'' In *Research in Labor Economics* Volume 4, ed. Ronald Ehrenberg. Greenwich, Conn.: JAI Press.

Moffitt, Robert, Robert Reville, and Anne Winkler. 1994. ''State AFDC Rules Regarding the Treatment of Cohabitors: 1993.'' *Social Security Bulletin* 57(4):26–33.

———. 1995. ''Beyond Single Mothers: Cohabitation, Marriage, and the U.S. Welfare System.'' Institute for Research on Poverty Discussion Paper no. 1068–95.

Murray, Charles. 1984. *Losing Ground*. New York: Basic Books.

U.S. Department of Commerce. 1997a. *America's Children at Risk*. Census Brief
    CENBR97-2.
———. 1997b. *Poverty in the United States: 1996*. Current Population Reports P60–198.
U.S. House of Representatives. 1996. *1996 Green Book: Background Materials and Data
    on Programs Within the Jurisdiction of the Committee on Ways and Means*. Washing-
    ton, D.C.: GPO.