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**Staying Single:
The Effects of Welfare Reform Policies on
Marriage and Cohabitation**

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The authors welcome comments and discussion.

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Introduction

Marriage has become a hotly debated subject among the public and policymakers, particularly as the formation and maintenance of two-parent families has gained prominence as a goal of welfare reform. While there is some debate over government's role in this area, there is clear value in at least understanding how new policies affect children's likelihood of living in healthy two-parent families, given evidence that children growing up in single-parent families are more likely to be poor and to have worse outcomes than children who grow up in two-parent families (McLanahan and Sandefur, 1994). Understanding the role played by current policies is an important first step in considering the role interventions specifically designed to promote marriage might play in the future.

A long-standing criticism of means-tested public assistance is that it may contribute to the decline in marriage rates and increase in out-of-wedlock childbearing in low-income populations, by providing low-income mothers with a way to support themselves and their children outside of marriage. In fact, the results of empirical research examining the effect of cash assistance on the formation of female-headed households have been mixed. Some studies find a positive correlation between female headship and welfare participation, or, conversely, a negative correlation between marriage and welfare (Bitler, Gelbach and Hoynes, 2002; Ellwood, 2000; Hoynes, 1997; Lichter and McLaughlin, 1997; Moffitt, 1990, 1992; Schultz, 1994). Other studies show that the incidence of female-headed households has increased even when welfare benefit levels have remained stagnant (Moffitt, 1998).

But what has been the effect of recent changes in the welfare system on marital behavior?¹ Do policies that promote work among single parents, including time limits on welfare, make-work-pay strategies, and work requirements, influence the likelihood that single parents will either cohabit or marry? Such programs could either increase or decrease the likelihood the single parents will marry — by eliminating marriage disincentives that have historically been built into welfare eligibility rules, and by changing single parents' income or employment.

So far, recent random assignment studies of welfare and work programs have produced inconsistent findings about the relationship between recent welfare policies and marriage rates for single parents (for a brief review, see Fein, London, and Mauldon, 2002). Most programs show no overall effect on marriage, although a few programs have shown a smattering of intriguing subgroup effects (Bloom et al., 2000, 2002; Gennetian and Miller, 2000; Hamilton, et al., 2002;

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Harknett and Gennetian, 2002). For example, Minnesota's pilot welfare reform program, which mixed financial incentives to work with mandates to work for long-term recipients, increased marriage for long-term recipients but not for new applicants. A Canadian welfare program, which provided generous financial incentives to work, increased marriage by a small amount in one province and reduced it in another. In addition, a study of a welfare program in Delaware found increases in marriage for younger recipients and those with less education (Fein, 1999). In contrast, two other studies showed reductions in marriage, with a program in Connecticut reducing marriage among mothers of very young children and a program in Iowa reducing marriage among new applicants to welfare (Fuller et al., 2002; Fraker et al., 2002). Interestingly, some of the most notable marriage effects from social policy interventions have been clustered among two-parent families — increases in marital dissolution that occurred in the Negative Income Tax experiments of the 1970's (Cain and Wissoker 1990; Groeneveld et. al., 1980; Hannan, Tuma, and Groeneveld, 1977) and more recently, substantial increases in marital stability among two-parent recipient families in the pilot Minnesota welfare program (Knox et al., 2000).

In short, while these studies have provided some broad lessons about how to improve the employment or economic well-being of families receiving welfare, the scattered nature of marriage impacts across studies has left policymakers with little guidance about whether these new policies toward welfare and work are discouraging or promoting marriage. The mixed findings have raised questions about the robustness of the effects that have been found in individual studies to date.

This paper provides a systematic appraisal of how welfare reform policies have affected marital behavior, comparing the effects on marriage and cohabitation among single parent families in 14 different U.S. welfare programs. The paper addresses the following questions:

- **What are the average effects of recent welfare and employment programs on marital behavior?**
- **How do these effects differ depending upon the characteristics of parents or families — including prior economic or welfare status; experience with marriage; age, race and ethnicity; and family composition at the time of program entry?**
- **How can these effects be characterized across a range of welfare policies, including expanded earned income disregards, mandatory employment services, and time limits on welfare?**

This paper makes several important contributions to our understanding of welfare policy and marital behavior. First, it draws on the power of meta-analysis to summarize what we have learned from numerous rigorous random assignment studies of welfare and employment

programs. Second, the paper provides a systematic examination of experimental effects on marital behavior across several subgroups of the welfare population. While some results have emerged within studies about the effects of welfare programs on marriage on particular subgroups of families, no prior work has established whether or not these effects are robust across studies. Third, effects on marital behavior are examined for three separate policy approaches, to determine whether there is any evidence that different strategies for reforming the welfare system produce different effects on marital behavior. Finally, the analyses consider cohabitation as well as marriage as an outcome. Some of the recent decrease in the proportion of children living in married families has been offset by increasing rates of cohabitation. And though relatively little is known about the effects of cohabitation on poverty and on children's well-being, the rising incidence of cohabitation may have implications for family stability and for the extent of financial and other resources available to a family (Bumpass and Sweet, 1991; Waite, 1995).

The Policy Context and the Studies

The 1996 federal welfare reform legislation introduced sweeping changes to the nation's system for supporting low-income families with children. During the prior six decades, Aid to Families with Dependent Children (AFDC) had guaranteed aid for economically deprived families with children. The new law eliminated AFDC, which was funded as an open-ended entitlement, and replaced it with Temporary Assistance for Needy Families (TANF), which provided block grants to states, introduced time limits on cash assistance, and imposed work requirements on recipients. The law made other substantial changes affecting child care, the Food Stamp Program, Supplemental Security Income (SSI) for children, and the Child Support Enforcement program, giving states increased flexibility.

Appendix A provides a detailed description of each study in this analysis, including a review of the program models and effects on employment, income and marriage that have been documented to date. Though studies were not specifically implemented to examine all of the policies as a result of the 1996 federal welfare reform legislation, the 14 programs examined in this synthesis provide information about the effects of the following policies:

Expanded earned income disregards One of the most unheralded changes in state welfare policies was the institution of policies designed to make work more financially rewarding. Most states accomplished this by increasing the earnings disregard (the amount of earnings that is not counted as income in calculating the amount of a family's welfare benefit) so that families could keep more of their welfare dollars when they went to work. Six of the implemented programs examined in this paper were designed to "make work pay" in the form of earned income disregards. Such financial incentives not only can increase income but are also intended to increase the incentive to make the transition from welfare to employment.

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Equalized eligibility for two-parent families. Recognizing that any restriction of eligibility for welfare benefits specifically tied to two-parent families may serve as a disincentive to become or remain married, many states since 1996 have streamlined eligibility rules between single and two-parent families. Examples of streamlined eligibility include elimination of the 100-hour rule that deemed a two-parent family ineligible for welfare once the primary earner in a household worked 100 hours or more per month; elimination of a work history requirement for the primary earner; and expansions of the stepparent earned income disregard. All six of the programs in this study that had an expanded earned income disregard also streamlined eligibility for two-parent families, in particular by eliminating the “100 hour rule” for two parent families.

Mandatory employment services. One of the most common policies used to increase employment is mandatory employment services, or requirements that recipients participate in employment-related activities as a condition of receiving their welfare benefits. Such policies have been used to varying degrees since the 1970s, applying to parents with ever-younger children and increasing the rigor of enforcement with each revision of welfare policy. The primary tool used to enforce participation mandates is sanctioning, whereby a recipient’s welfare grant is reduced if she or he does not comply with program requirements. Today, virtually all states are using such mandates in their attempt to reduce welfare use and increase parents’ self-sufficiency. In many cases, however, the mandates are more stringent (with respect to the number of hours of work required or the size of the sanction) than the mandates in the studies examined here. Nearly all of the programs included in this paper required recipients to participate in employment or employment-related activities in some way in order to receive cash assistance but only seven were designed to specifically test the effects of mandatory services alone.

Time-limited welfare. The federal welfare law of 1996 sets a lifetime limit² of five years on federally-funded welfare benefits, but states may shorten or extend the limit by using state funds. Once a family reaches the time limit, federally funded cash benefits are terminated, but the family normally remains eligible for Food Stamps, Medicaid, low-income child care assistance, and (where available) state-supported cash assistance. Time limits are intended to reduce welfare dependence, encouraging parents to work in order to support their families. Currently, 40 states have time-limit policies that result in benefit termination. Of those, 23 have time limits of 60 months, while 17 have time limits of fewer than 60 months, like two of the programs evaluated in this analysis.³

²Lifetime limits restrict the number of months in the recipient’s lifetime that she or he can receive welfare benefits. Fixed-period time limits, in contrast, restrict the number of months of benefits over a shorter, specified period — for example, 24 months in any 60-month period.

³The other 10 states and the District of Columbia either do not have time limits or have time limits that reduce or modify the grant, rather than resulting in a termination of benefits. This latter group of states comprises half the national caseload of welfare recipients (Bloom, Farrel, and Fink, 2002).

These policies are mixed and matched in various ways in the studies examined here, as they are in many current welfare programs. In the analysis that follows, we examine four combinations of policies: Programs that tested mandatory employment services alone; programs that implemented an enhanced earnings disregard; and two subsets of the programs with enhanced disregards — those that combined the disregard with time limits on welfare use, and those that did not. Several of the programs in the studies examined in this paper also bundled these policies with a number of other policies such as expanding supportive services for employment, including lengthening the eligibility period for Medicaid or child care assistance.

The 14 programs included in the present analysis were evaluated in studies of the Minnesota Family Investment Program (MFIP); Florida’s Family Transition Program (FTP); Vermont’s Welfare Restructuring Project (WRP); the National Evaluation of Welfare-to-Work Strategies (NEWWS); Connecticut’s Jobs First program (CT Jobs First); and Los Angeles Jobs First/GAIN. These studies were included because they are all recent studies of reforms that took place within the welfare system. While some random assignment studies have examined “make work pay” programs that were implemented outside the welfare system, the scope of the present paper is limited to the U.S. welfare system. In addition to meeting this criterion, data from these studies were readily available to run re-analyses to derive estimates for comparable subgroups across studies.

There are a number of additional experimental studies of welfare, employment or anti-poverty programs whose results can provide important insights into the findings from these six studies, and thus are incorporated, as appropriate, into the analysis or the discussion below. Four recent welfare programs were excluded because their data were not (yet) readily accessible for re-analysis — California’s Work Pays Demonstration, Delaware’s A Better Chance program, Iowa’s Family Investment Program, and Indiana’s Welfare Reform Evaluation.⁴ When estimates of the impacts of these programs on marriage and cohabitation for subgroups of interest were available from published reports, the estimates are included in sensitivity analyses of the meta-analytic estimates described below.

Two additional programs, Milwaukee’s New Hope Program and the Canadian Self-Sufficiency Program, provided earnings supplements that make work pay for low-income or welfare parents who work at least 30 hours per week. In addition, the Negative Income Tax experiments implemented in the late 1970s provided an income supplement to low-income families in a number of sites across the country and in Canada. Because the data were available, re-

⁴See Hu, 1998, for a discussion of the effects of California’s Work Pays Demonstration, Fein, 1999 for a discussion of the effects of Delaware’s ABC program on marriage; Fraker et al., 2002, for a discussion of the effects of Iowa’s Family Investment Program on marriage and Beecroft, et al., 2002, for a discussion of the effects of Indiana’s Welfare Reform Evaluation.

sults from the former two studies are selectively incorporated into the discussion below — and, in the meta-analytic estimates — when relevant.

Each of the programs mentioned above, including those whose results are not re-analyzed in this paper, produced changes in either the employment or the income levels of welfare recipients or low-income families, but few affected marital behavior. As mentioned earlier, those effects that were found were inconsistent. The approach of this paper — to estimate effects on marital behavior across comparable subgroups and across the four types of programs identified above — is motivated by an interest in understanding whether this inconsistency in results can be explained by two sources of variation across studies. The first source of variation is that each program served different populations of low-income families. Relatedly, when the initial evaluations of these programs did include estimates for specific subpopulations, the subgroups were not always defined in comparable ways. For example, the MFIP study focused primarily on single-parent families who were long-term recipients (defined as on welfare for 24 of the previous 36 months) whereas other studies represented a mix of single-parent families who were long-term recipients and more recent applicants. The second source of variation is that the reviewed experimental studies pursued different objectives and policy approaches, leading to different effects on employment and income. This variation in economic effects may have contributed to the variation in effects on marital behavior. A third possibility, more difficult to test using these data, is that the relationship between marriage and welfare policies varies depending upon other unobserved factors, including family characteristics that are associated with the geographic or cultural context, or characteristics of the “marriage market.” While the random assignment design of each study ensures that estimates within any one program are not confounded by local social, economic or cultural factors, these kinds of difficult-to-measure characteristics could create differences in program effects between sites with different cultural contexts, if the program treatment interacts with a site’s cultural context to create the program’s effect on marriage.

Conceptual Framework and Hypotheses: How are Changes in the Welfare System Expected to Affect Marriage and Cohabitation?

Economic theory proposes that the decision to get marry or cohabit with someone or stay single fits into a general “utility maximization” framework (Becker, 1973, 1974). Under this framework, individuals will weigh the costs and benefits of marriage and will marry if their expected utility from marriage exceeds their expected utility from remaining single. In its purest form, this framework predicts that the availability of welfare benefits or women’s own earnings will reduce marriage by increasing women’s economic independence, thus reducing the gains that women would achieve by marrying relative to remaining single (Moffitt, 1992). However, a more complex model of human behavior suggests that the effect of women’s economic inde-

pendence is more ambiguous. Women may delay marriage as a result of increased economic opportunities without necessarily decreasing rates of ever marrying (Oppenheimer, 1988). Alternatively, women's employment could even have a positive effect on marriage by alleviating financial stress in a relationship (an "income effect"); exposing women to new social networks through work; or increasing their appeal to prospective spouses.⁵

This general framework — as well as the results of recent empirical quantitative and qualitative research — leads to several hypotheses about how welfare programs that aim to increase employment and/or reduce poverty may affect marriage (e.g., see Bancroft and Vernon, 1995 and Edin, 1999). The overarching goal of all of the welfare programs examined in this paper was not to change marriage rates per se, but rather to increase employment. Some programs had further goals of reducing the use of welfare benefits or increasing income, and some programs eliminated restrictions on welfare use that may have discouraged marriage or partnerships. The relationship between the programs, their economic effects and their potential effects on marriage are depicted in Figure 1.

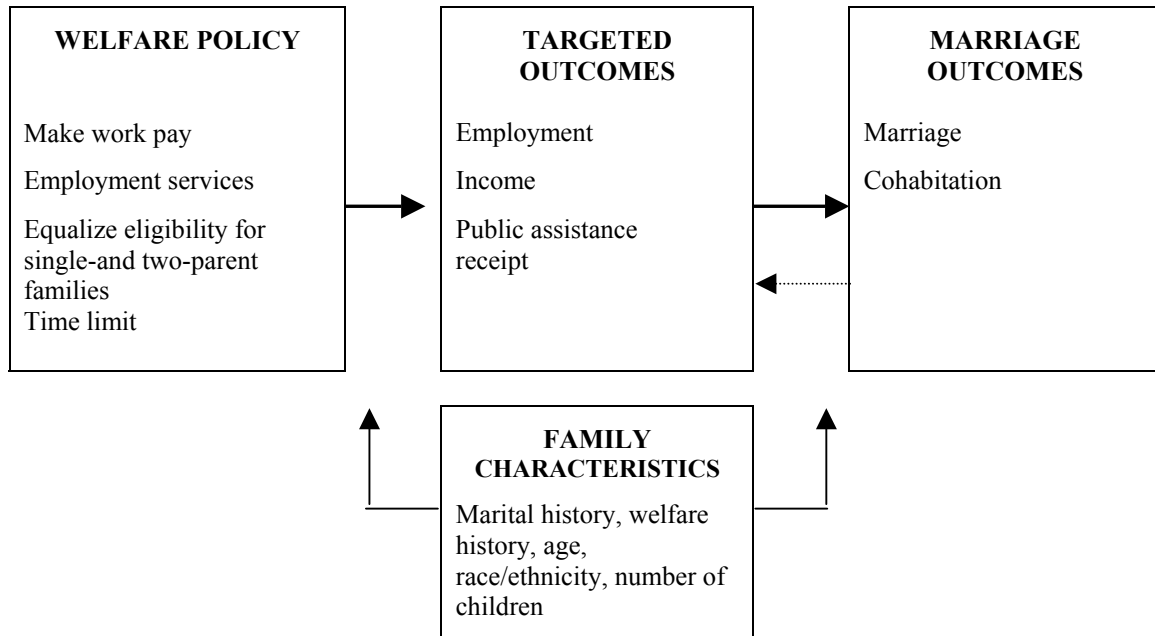
By equalizing eligibility requirements between coupled or two-parent families and single-parent families, welfare and anti-poverty policies may increase marriage, cohabitation or marital stability. Some of the new policies tested in these studies allowed more two-parent families to qualify for public assistance. For example, Jobs-First, FTP, MFIP and WRP eliminated work restrictions (e.g., the "100-hour rule") for two-parent families while other programs did not change the eligibility rules for two-parent families.

By increasing employment, welfare policies may either increase or decrease marriage and cohabitation. Employment may increase a single parent's likelihood of marrying by increasing her self-esteem, confidence, or attractiveness to a partner — or by increasing the pool of potential partners via social networks introduced through the work place. A number of studies find that women with higher levels of earnings, employment, and education are more likely to marry than those with lower levels, an association that is particularly strong for low-income women (Goldschneider and Waite, 1986; McLaughlin and Lichter, 1997; Lichter, McLaughlin, Kephart, and Landry, 1992; Sassler and Schoes, 1999). On the other hand, increased employment may decrease marriage by increasing the woman's financial independence, or by reducing the cost of time available to search for a partner or to socialize with a prospective partner.

⁵A more nuanced view is that marriage is a partnership, with a number of factors potentially contributing gains to the partnership (for a review, see Weiss, 1997). These include complementarities of partners' time in household production generated by specialization; joint consumption of household goods such as food, housing, and children; risk-sharing and pooling; and non-pecuniary reasons, such as love. Realizing the gains to marriage will also depend on the allocation of resources within the marriage. The gains to marriage will depend not only on the gains to the actual partnership under consideration but also on the range of potential matches or partners available (i.e., the marriage market).

Figure 1

**Conceptual Model: How Welfare Reform Policies
May Affect Marital Behavior**



It is difficult to predict how increases or decreases in income — through changes in generosity of monthly benefits or through time limits on welfare use — will affect marriage and cohabitation. On the one hand, if a single parent values autonomy, then increased income may make her less likely to marry or cohabit, while decreases in income may restrict her choices, leading her to maintain relationships that she otherwise would not. Similarly, increased income may allow single parents to leave abusive relationships while decreases in income may inhibit their ability to exit the situation. (There is some empirical support for the notion that the decision to live alone is a reflection of the demand for privacy or autonomy and that income level is positively related to the propensity to live alone. See Michael, 1980.) However, it is also possible that increased income could instead facilitate marriage by decreasing financial strain, by increasing a woman’s attractiveness to a potential partner; or by making women feel financially empowered to take the step of marriage (Edin, 1999).

If local opportunity structures, “marriage markets”, and cultural factors underlie rates of marriage and cohabitation, they may also influence the effects of welfare reform programs on

marriage. For example, if men's deteriorating economic position is a major explanation for the decline in marriage among low-income women as some argue (Wilson and Neckerman, 1987; Oppenheimer, 1988; Wilson, 1987), then changes in single women's employment or income may not lead to much change in marriage rates.⁶ Other factors such as community norms, culture or the local social policy context, including the dominant religion of the community and the stigma or acceptability of being a single parent, may also shape the influence of any change in women's economic circumstances on marital behavior. If these important influences vary systematically for different identifiable subgroups of families, they may cause different effects on marriage for different identifiable subgroups, as suggested below. However, because we do not fully understand how local context, economic opportunity structures, cultural norms and other factors affect marital decisions, these influences may also lead to variations in impacts across families, sites, or studies that are difficult to explain.

Effects of welfare reform programs on marriage and cohabitation are likely to vary for different subgroups of single parents. As shown at the bottom of Figure 1, characteristics of the individuals or families who enter these new programs will also affect program impacts on marriage (or on other types of household structure). For example, an increase in income may affect new applicants to welfare and long-term recipients quite differently. Because new applicants are more likely to have experienced a recent divorce or separation, they may be more likely to respond to increased income by remaining single, at least in the short run, rather than by marrying. Increased income will also affect single- and two-parent families differently, having the potential to affect entry into marriage for the former but marital stability for the latter. Different racial and ethnic groups may have different cultural norms about marriage or may face different prospects for marriage (i.e., the availability of "marriageable men") that may result in different effects on marital behavior (Wilson, 1996; Edin, 1999). In short, research on the impact of welfare and employment programs on the aggregate recipient population may be masking important effects on marriage among subgroups. Below we briefly describe the subgroups of the welfare population that we plan to examine in depth in the empirical analysis.

Prior welfare and work experience. If employment and earnings are indeed key factors in influencing marital behavior, then the groups of welfare recipients who experience the most pronounced changes in employment or earnings are likely to be the same groups who experi-

⁶The empirical evidence here is mixed. Although several studies have shown that employment and higher levels of earnings among men are associated with the likelihood of marriage, the magnitude of the effects have been too small to account for most of the decline in marriage in disadvantaged communities (Ellwood and Crane, 1990; Olsen and Farkas, 1990; Raly, 1996). When low-income women are interviewed, they do reveal that they view men's employment in jobs with earnings levels sufficient to support a family as a necessary precondition for marriage, and aggregate-level studies find lower rates of marriage in areas with greater economic opportunities for women (Edin, 1999; Edin, 2000; Lichter, 1991; McLanahan and Casper, 1995).

ence changes in marital behavior. Prior evidence on the effects of welfare and employment programs on employment and earnings for a variety of subgroups of the welfare population find that the largest increases in employment and earnings tended to occur among the most disadvantaged groups of the welfare population (Michalopoulos and Schwartz, 2001). In the current analysis, disadvantage is defined as a combination of low education, low prior work experience and/or high prior welfare experience.

Race/ethnicity. Nationally, marriage rates clearly differ across various racial or ethnic groups with white, non-Hispanics having a higher probability of first marriage by age 30 as compared to Hispanics or non-Hispanic Blacks. In 1995, 54 percent of white, non-Hispanic women aged 15 to 44 were married as compared to 47 percent of Hispanic women and 25 percent of Black women (Bramlett and Mosher, 2002). The underlying causes of these differences — whether they are related to men’s and women’s economic opportunities, cultural norms, or values — have important implications for whether single mothers are likely to respond to welfare and employment interventions by marrying. For example, if, as mentioned earlier, declining rates of marriage among black single mothers are caused by increasing neighborhood joblessness and subsequent decreases in the number of employed black males (Wilson, 1987), changes in the economic security of the mothers (rather than the fathers) may not lead to large changes in marriage rates because the underlying cause of low marriage rates has not changed. In fact, to the extent that these programs increase the “marriageability” of women relative to men, they could worsen the “mismatch in marriageability” and lead to further declines in marriage.

In contrast, values supporting the importance of marriage in low-income Mexican-American families are reportedly stronger than in low-income white or black families (Oropesa, 1996; Taub, 1991), which could mean that single Mexican-American mothers are particularly likely to respond to changes in economic circumstances by marrying. The fact that marriage rates across racial and ethnic groups vary so widely — with different motivations coming into play for different groups — lends support to the notion that economic opportunity structures, values, or cultural norms could play an important role in determining the marital responses of other subgroups as well.

Age of mother. Young mothers may face very different marriage prospects than older mothers for a number of reasons. For example, they may feel they have a longer time horizon to reap the benefits of marriage; they may be less jaded about the value of marriage; or they may face a greater availability of “high quality” partners given that more women marry older men than vice versa. Two of the experimental studies reviewed earlier (the California Work Pays Demonstration and Delaware’s ABC Demonstration) suggest that effects on marriage for single parents may be most pronounced for young single parents.

Age and number of children. Research finds that the age and number of a woman's children can affect the likelihood that she marries. Bumpass, Sweet, and Martin (1990), for example, studied remarriage rates and found that 81 percent of women with no children at the time of separation remarried compared with 57 percent of women with more than three children. This finding suggests that men may be reluctant to take on the additional financial and child-rearing responsibilities that come with marrying a woman who already has children, particularly if these children are not biologically related to him. If having children is indeed an obstacle to getting married for low-income women, one would expect an unmarried mother's chances of marrying would decline as the number of children she has increases.

Prior marital status. As mentioned earlier, the marital behavior of never married mothers may be affected differently by changes in income or employment than the marital behavior of mothers who have recently divorced. For example, programs that increase income may encourage never married mothers to marry because they have been waiting to marry the father of their children until they feel financially secure. In contrast, single mothers who just divorced may take advantage of increased financial security to delay remarriage.

To sum up, because it is difficult to make an *a priori* theoretical prediction about whether, or how, a particular set of welfare policies will affect marital behavior, the effect of welfare policies on marital behavior remains an empirical question. The availability of experiments that created exogenous changes in welfare policy and the resulting impacts on marital behavior provides an important opportunity to shed new light on this question of long-standing interest. Since these programs were primarily targeted at parental employment and family income, and had remarkably consistent effects in these areas, understanding their effects on marital behavior could also shed light on the long-standing question of how employment status or family income affect marriage decisions for low-income families, a question that has been difficult to answer using non-experimental data.

Sample, Data, Outcomes, and Empirical Methods

Sample

The analysis sample for each of the studies is comprised of single parents (primarily single mothers), who entered the studies upon applying for or being recertified for public assistance. Participants in each of the six studies were randomly assigned either to a program group that was eligible for the benefits and subject to the requirements of the new welfare program, or to a control group, that had access to the usual benefits available to low-income or welfare families in the site where the study took place. Because individuals were assigned to these groups at random, the two groups begin the study with very similar demographic characteristics and simi-

lar local and economic contexts. Any differences that emerge during the follow-up in average outcomes for program group members and control group members — considered program “impacts” — can be attributed to differences in the policies faced by the two groups. Thus, with an experimental design, the measured effects of these programs on marriage and cohabitation will not be biased by differences in families’ characteristics, or by changes in the labor markets or other public policies over time.

Data

Fortunately, the data collection process for the six studies examined in this paper was designed to be comparable to facilitate cross-study analysis. Each study collected demographic and socio-economic information about the participants at the start of the study, information on employment and welfare receipt from administrative records sources and conducted at least one follow-up survey that, in addition to a number of questions regarding employment, education, and child care, asked recipients about their marital status and the composition of their families.

Data for the survey were collected during in-person and/or telephone interviews and response rates ranged from 80 to 90 percent. The follow-up periods for the studies ranged from 18 to 48 months, and the study periods stretched from 1991 to 2001. MDRC has analyzed the survey data to assess its representativeness of the total research sample and to assess the validity of program impacts. This is done by reviewing response rates, by comparing respondents and non-respondents (since baseline information is collected for both groups), and by comparing respondents across research groups. Analyses of data for each study show that any significant differences in outcomes between research groups can be reliably attributed as program impacts and that the outcomes and impacts are representative of the total sample that entered the study (see cites to final reports for each of the studies in Appendix A).

Outcomes

The two key outcomes of interest for the analysis are whether or not a respondent reports being married, and whether or not a respondent reports cohabiting, at the time of the survey follow-up. Because this captures program and control group levels of marriage and cohabitation at a particular point in time, the absolute rates of marriage and cohabitation reported here are lower than would have been the case if the outcome were whether the respondent had ever been married or cohabited.

Empirical methods

The first step of the empirical analysis is to estimate the effect of each program on the likelihood of being married or cohabiting at the survey follow-up point, for subgroups of the

samples that are comparable across studies. To increase the precision of the estimated experimental-control group differences, all of these impacts are estimated using ordinary least squares regressions to adjust for a variety of baseline characteristics, including the parent's age, education level, and employment history. Even with binary outcomes, the estimates from ordinary least squares (OLS) models are similar to estimates derived from nonlinear techniques, and, thus, for simplicity we present the OLS-based estimates (Harknett and Gennetian, 2000).

All the impacts were tested for statistical significance, as were the important differences between subgroup impacts. An impact is considered statistically significant if $p < .10$, based on a two-tailed t test. Unless otherwise noted, all the impacts discussed in the text are statistically significant. The test of significance across subgroup impacts (the H-statistic), rather than the isolated test of significance on the impact within a subgroup, is used to determine whether a subgroup impact is meaningful, i.e., whether the impacts for one subpopulation are different in a statistical sense from the impacts for the comparison subpopulation. (This is different than the hypothesis tested for any individual impact estimate, i.e., whether or not it is significantly different from zero). The findings from the impact tests across subgroups will be mentioned throughout the paper to identify subgroup impacts that are noteworthy.

Once the impacts are estimated within each study for a comparable subgroup, the average effects across studies are estimated. The average effects presented in this paper result from a meta-analysis conducted using techniques outlined in Lipsey and Wilson (1996).⁷ The individual study impact findings lend themselves to meta-analysis because, despite the relatively small numbers of impact estimates, they are of comparably high methodological quality (all used random assignment and achieved high survey response rates); included commensurable measures; and applied similar statistical tests to detect effects. Effect sizes form the centerpiece of a meta-analysis, serving as a standardized measure of impacts that ensures comparability across studies. An effect size converts each program impact into standard deviation units, thus adjusting for any outcome differences arising study specific characteristics such as survey measurement (for instance, variations in the period of time that a measure covers, which should affect both the average size of an outcome and the standard deviation of that outcome). Assuming that samples from different studies are drawn from the same underlying population, effect sizes can be used as indicators of the underlying impacts on outcomes of interest. An average effect size across all the programs — hereafter referred to as the overall average effect size — was calculated for each outcome analyzed in this

⁷An approach to discerning patterns across studies traditionally used by researchers in psychology and medicine, a meta-analysis is a systematic review of a population of studies investigating a similar process or a similar type of outcome that can provide the statistical power to estimate a policy's overall effect. Unlike vote-counting methods, meta-analytic techniques produce weighted average effect sizes that take into account the different levels of confidence in the studies' findings arising from differences in sample size or in sampling error across studies.

synthesis, as were average effect sizes for the subgroups and the four programmatic approaches described earlier. For ease of presentation, once estimated, the overall average effect sizes are converted back into the original metric of percentage point impacts.

One important consideration for interpreting the results of a meta-analysis is whether the effects of the individual programs can be considered similar in size. The techniques followed here assume that the studies all drew samples of single parents from a single population with one “true” effect size. A test — the homogeneity test — was applied to all meta-analytic estimates to determine whether variability in observed effects was greater than expected by chance in a set of estimates based on samples from the same underlying population.⁸ The results of this test are mentioned as appropriate. One challenge for the application of the meta-analytic techniques used here is that some of the studies tested more than one program model in one site. In these studies, the two program groups shared a control group, which means that the estimated effect sizes in each of these sites are correlated, thus violating the independence assumption of the meta-analytic estimates. However, the overall average effect sizes were also calculated using pooled within-study estimates (e.g., pooling the impact estimate within the NEWWS programs and then using this one pooled estimate in the meta-analytic average) to partly assess the influence of shared control groups, and the results did not differ.

Findings

- **For the overall sample of single parents, these welfare reform programs did not affect marriage or cohabitation.**

Table 1 presents the results of the meta-analytic analysis, for single parents overall and by each of the subgroups that were previously described.⁹ Columns 1 to 4 present sample size,

⁸The Q statistic, on which the homogeneity test is based, follows a chi-square distribution with $k - 1$ degrees of freedom, where k is the number of effect sizes. The effect sizes for a given outcome were considered heterogenous if the statistical significance level was less than .10. However, the homogeneity test has relatively low statistical power when applied to a small number of estimates (most meta-analyses draw on more estimates than were available here). When the Q statistic reveals a statistically significant level of heterogeneity, researchers may decide that there is systematic variation among the estimates and adopt alternative models to account for this systematic variation, e.g., “random” or “mixed effects” models.

⁹To decrease within study correlation that could bias the meta-analytic averages, only impact estimates from the 2-year follow-up survey for NEWWS are included in the meta-analysis, even though findings are also available from a 5-year follow-up. The primary reason for using the 2-year follow-up is that this time period is most comparable to the follow-up period for the other programs. When the meta-analyses were re-estimated substituting the 5-year NEWWS findings for the 2-year findings, we did find that some of the prior nonstatistically significant results became statistically significant. Further analyses of long-term effects on marriage will be possible as more long-term data become available from other studies.

the mean of the program group, the mean of the control group and the impact estimate for the marriage outcome and the second four columns present comparable information for impacts on the cohabitation outcome. The impacts of each individual program that were used to estimate the meta-analytic averages are presented in Appendix Table 1.¹⁰

Because these data represent some of the little data that are available on the marital behavior of very low-income populations of single parents, the control group rates of marriage and cohabitation are, in themselves, of interest. The meta-analytic averages reveal that close to 80 percent of the population of single parents in these studies were not married or cohabiting at the time of survey follow-up two to four years later (i.e., at the survey follow-up point, approximately 10 percent on average reported being married and approximately 10 percent on average reported cohabiting). Recall that because the marriage outcome examined here captures one point in time, this estimate is a lower bound of the general rates of marriage or cohabitation that occur over time.

A second striking aspect of these results is the wide variation in rates of marriage and cohabitation across sites, as shown in Appendix Table 1. Marriage rates at the 2- to 4-year follow-up point vary from a low of 4 percent (in NEWS Atlanta) to a high of 16 percent (in WRP). Rates of cohabitation similarly vary from a low of 4 percent (in NEWS Atlanta) to a high of 23 percent (in Vermont's WRP program). Some of this wide variation could be a result of variation in the length of the follow-up time period. For example, in the NEWS sites, rates of marriage nearly doubled between the 2-year follow-up point (shown in Appendix Table 1) and the 5-year follow-up point (shown in Appendix Table 2). However, not all of the variation is explained by the length of the time period and could instead reflect a number of local cultural, economic or political factors. For instance, rates of cohabitation in FTP (with a 48 month follow-up point) are much lower than rates of cohabitation in WRP (with a 42 month follow-up point). Moreover, rates of marriage in some of the NEWS sites, e.g., in NEWS Grand Rapids at the 2-year follow-up point, are similar to rates of marriage in CT Jobs First or MFIP, both evaluated at a 3-year follow-up point.

As shown in Table 1, with 10.6% of the program group married by the time of the follow-up surveys and 10.9% of the control group, there was no statistically significant difference in marriage rates. Neither was there a difference in the proportion who were cohabiting by the time of the survey (10.9% of the program group vs. 10.8% of the control group).

¹⁰Appendix Table 1 shows that only one program, WRP Incentives Only, increased marriage by 5 percentage points among single parents overall. And, two programs, NEWS Portland (at the 2-year and 5-year point) and NEWS Riverside LFA increased cohabitation among single parents overall.

- **The welfare reform programs rarely had effects on marriage or cohabitation for specific subgroups of families.**

Control group rates of marriage generally vary across subpopulations as expected: Higher marriage rates by the time of follow-up were found for younger parents than older parents; for parents with younger children as compared to parents with older children; for parents who had ever been married when they entered the study as compared to those who had never been married; white as compared to black parents; and, among the least disadvantaged as compared to the most disadvantaged. Perhaps surprisingly, rates of marriage are quite similar for single parents with 1 or 2 children as compared to single parents with 3 or more children. The general pattern for rates of cohabitation across these subpopulations is quite similar to the patterns on rates of marriage with one exception: Rates of cohabitation are similar regardless of whether these single mothers had been married previously or not.

Table 1 shows that these welfare programs rarely had statistically significant impacts on marriage or on cohabitation across subgroups. Of the 20 meta-analytic estimates calculated on the marriage outcome, only one statistically significant impact emerged: Single parents with three or more children in the program group were, on average, 2.3 percentage points (16 percent) less likely to get married compared to their control group counterparts. Because one would have had a 10% likelihood of finding two statistically significant impacts simply by chance when estimating impacts for 20 subgroups, we conclude that there is no variation in impacts by subgroup. The meta-analytic estimates across studies make an important contribution. With the large sample sizes available for these analyses, any meaningful impacts that were arising across studies should have been detected. These conclusions remain after including some subgroup estimates that were publicly available from Delaware's ABC program, Indiana's welfare program and Iowa's Family Investment Program (shown in Appendix Table 3).

The scattered nature and small number of impacts for the individual studies that generally exist across subgroups in Appendix Table 1 further underscore the finding that these programs have very little effect on marriage. Blacks and Whites, for example, showed the largest number of statistically significant differences in impacts across subgroups, but the pattern of effects for each racial or ethnic group was equally split between negative and positive effects, providing little basis from which to predict how future welfare reform programs might affect marriage. These results suggest that the weak response of single parents' marital behavior to these programs may operate somewhat differently across different racial groups, but not with any consistency.

- **The full group of programs with expanded earnings disregards (and equalized eligibility for two-parent families) had no effect on marriage**

or cohabitation for the overall sample nor for any subgroups of single parent families.

As described earlier, the welfare programs examined in this paper tested three key policy approaches — earned income disregards, mandatory employment services and time limits — that play a prominent role in current welfare policy. To examine whether or not effects on marriage or cohabitation varied in any systematic way depending upon the policies being implemented, meta-analytic averages were calculated for subsets of programs that shared a common approach to welfare reform.

Table 2 presents the meta-analytic results for all the programs that had expanded earned income disregards (six estimates, derived from Connecticut Jobs First, FTP, MFIP, and VT WRP) — whether or not these disregards were combined with time limits on welfare use. This table shows that programs with expanded earned income disregards had no statistically significant effect on marriage and produced only one statistically significant effect on cohabitation for one subgroup. As mentioned above, when subgroups impacts are this rare, they could easily have occurred by chance.

- **The two programs that combined expanded earnings disregards with time limits on welfare use — Jobs First and FTP — had no overall effect on marriage and cohabitation. There was a slight trend toward reductions in marriage and increases in cohabitation for some subgroups of single parents in these programs.**

Table 3 presents the meta-analytic results for programs whose package of policy changes included not only an expanded earned income disregard but also a time limit on welfare use. These results are averages of impact estimates from two programs: CT Jobs First and FTP. (None of the random assignment studies of welfare programs tested time limits alone.) These time limited welfare programs did not affect marriage or cohabitation rates for their overall combined samples.

The impacts on marriage and cohabitation across subgroups were somewhat more consistent for programs that combined disregards with time limits than for all the programs that expanded earnings disregards. For example, nearly all of the impacts on marriage were in a negative direction, although they were statistically significant for only four of the twenty subgroups — single parents with a preschool-aged child, single parents aged 25 or less, moderately disadvantaged single parents and long-term welfare recipients. Similarly, the effects on cohabitation were positive for seventeen of the twenty subgroups, although only three — single parents with one child, single parent who had never been married and Black single parents — were statistically significant. The magnitude of the effects on marriage and cohabitation that reach

statistical significance range from 2 to 3 percentage points, or 20 to 30 percent decreases or increases, respectively.

While these patterns of effects for particular subgroups are of some interest, it is never the case that the subgroup effects are statistically significantly different from each other, i.e., the decreases in marriage occurring among single mothers aged 25 or less are not significantly different from the effects on marriage occurring among single mothers older than age 25.

- **Programs that expanded earnings disregards but did not place time limits on welfare use — two versions of the MFIP program, and two versions of Vermont’s Welfare Restructuring Project — showed a small positive effect on marriage and no effect on cohabitation.**

As shown in Table 4, 13.3 percent of control group families were married in these programs as compared to 15.7 percent of program group families, creating a small significant increase in marriage of 2.4 percentage points (or, 18 percent) at the time of the survey follow-up. The pattern of impacts by subgroup is also consistently positive.¹¹

These programs increased marriage among single parents who are at least age 25 (by 2.4 percentage points, or 19 percent); among those who were never married when they entered the programs (by 3.2 percentage points, or 35 percent); and among four groups of single parents who were disadvantaged in various ways (including those who were classified as most disadvantaged or moderately disadvantaged; those with no prior work experience, and long-term recipients).

Furthermore, significance tests indicate that many of these positive effects are significantly different from the effects of programs that combined expanded earnings disregards with time limits. The overall effect on marriage of programs that had an expanded earnings disregard without time limited welfare was significantly different from the overall effect on marriage of programs that had an expanded earnings disregard with time limited welfare. In addition, significant differences in impacts between expanded earnings disregard programs that placed time limits on welfare and those that did not occurred within several subgroups — those who were not previously married, for younger and older single mothers, Whites, for those with pre-school aged children and the most disadvantaged single parents (including long term recipients). A similar, but opposite, pattern occurs with cohabitation: For a number of subgroups (never married, long term recipients, Blacks and Hispanics), the negative effects on cohabitation for pro-

¹¹Unlike most of the other programs, VT WRP produced several statistically significant increases in marriage (see Appendix Table 1). More specifically, VT WRP Incentives Only increased marriage overall from 17 percent to 22 percent, an effect that also emerged among Whites, the moderately disadvantaged, long term welfare recipients and those with no prior work experience. The full program, VT WRP, also produced several statistically significant increases in marriage among single parents aged less than 25, those with a preschool aged child, and the most disadvantaged.

grams with expanded earnings disregards but no time limits was significantly different than the positive effects on cohabitation among programs with enhanced earned income disregards and welfare time limits. Thus, it is possible that time limits could be somehow reducing the potential positive effects of expanded earnings disregards on marriage.

These positive effects on marriage are intriguing and raise a question about whether or not earnings supplements defined more broadly also produce positive effects on marriage. Fortunately, data are available from New Hope and the Canadian Self-Sufficiency program, both of which provided an earnings supplement outside of the welfare system to welfare and low-income families who worked 30 or more hours per week, and both of which were found to increase employment as well as income (Bos et al., 1999; Michalopoulos et al., 2000). Does the positive average effect on marriage of MFIP and WRP hold up to the inclusion of estimates from New Hope and SSP? When the average effects of SSP and New Hope are added into the meta-analytic averages presented in Table 4, the meta-analytic impact estimate shrinks to 0.03 percentage points and is no longer statistically significant. (New Hope's overall effect on marriage was — 3.2 percentage points at the two-year follow-up; SSP's overall effect was 0.3 percentage points, and neither within-study impact estimate was statistically significant.)

It is not clear why the MFIP and WRP programs would have had a marriage effect, when neither the programs that added time limits on welfare to enhanced earnings disregards nor the two additional earnings supplement programs had any effect on marriage. One possibility is that MFIP and WRP did not place time limits on welfare, and could have led to greater economic security in the form of greater income gains, making single parents more likely to marry. But if this were the case, one would have expected the SSP and New Hope programs — both of which increased income — to show a similar pattern. Moreover, the program in this analysis that had the largest effect on marriage was WRP's Incentives Only program, and this program did not produce any increases in family income, because the earned income disregard was only enhanced by a small amount. Thus, any link between family income and the small effect on marriage for these programs is not consistent.

One could also argue that New Hope and SSP are quite different from MFIP and WRP, because their policies did not include elimination of the 100 hour rule, as was the case for the earned income disregard programs. However, if the change in the 100 hour rule underlies any positive effects on marriage, then a similar effect should have emerged for Jobs First and FTP. Moreover, SSP also removed the financial disincentive to marry, by disregarding all of the earnings of a spouse or common-law partner.

It is clear that any conclusion drawn about the effects of enhanced or expanded earned disregards should be very tentative given that the small positive effect found in MFIP and WRP is sensitive to either site variation or variation in the structure of these programs as well as in

similar kinds of “make-work-pay” programs. This is particularly important because many states are currently combining expansions in the earned income disregard with time limits on welfare, a combination that did not produce effects on marriage in the programs studied here.

- **Programs with only mandatory services did not affect marriage or cohabitation for the overall sample. The twenty subgroups examined showed a trend toward negative effects, but only one subgroup had a statistically significant effect.**

The effects of programs with mandatory employment services are presented in Table 5. These programs — six programs from the NEWWS evaluation and one program in the Jobs First GAIN evaluation — all tested mandatory employment services alone (without time limits or expanded earned income disregards for program group members). On average, these programs had no effect on rates of marriage or cohabitation. Of the twenty subgroups for whom impacts were estimated, seventeen showed a pattern of effects that were in a negative direction, with only one being statistically significant. On average, these programs significantly reduced marriage by 2.6 percentage points, or 24 percent, among single parents with 3 or more children. This effect is significantly different from the effect of programs with mandatory employment services on single parents with fewer than 3 children.

Programs with mandatory employment services produced mixed effects on cohabitation across subgroups, decreasing cohabitation among single parents with children aged 6 or older and blacks but increasing cohabitation among whites. Moreover, the differences in effects on cohabitation across corresponding subgroups were not statistically significant.

Discussion and Conclusion

This paper systematically examined the effects of 14 U.S. welfare programs on rates of marriage and cohabitation among single parents. One goal of the paper was to estimate the overall, average effect on marriage and cohabitation of these 14 programs for which data were available for re-analysis. A more important goal, given scattered subgroup findings that appeared in some individual studies, was to search for similar, consistent and significant effects for particular subgroups, to test the hypothesis that a lack of overall effects on marriage and cohabitation may mask important effects for specific subgroups.

We found that the vast majority (close to 80 percent) of those who entered these studies as single parents were neither married nor cohabiting at the time of the survey follow-up two to four years later. The meta-analytic results further show that these programs generally did not affect marriage or cohabitation overall or for most subpopulations characterized by the parent’s

age; prior marital status; race or ethnicity; prior welfare and work experience; and the age and number of her children.

When programs were clustered by policy approach, the only statistically significant effect to emerge was a small effect for programs that expanded the earned income disregard (and equalized eligibility for two-parent families) without time limiting welfare use. These programs tended to especially increase marriage among young single mothers, those who were not previously married, and those who were most disadvantaged in terms of their prior welfare or work experience. However, this one effect on marriage did not hold up when related programs, including programs that added a time limit on welfare use or programs that provided earnings supplements outside of the welfare system, were included in the average estimate. The small positive finding for programs with expanded earned disregards is somewhat similar to some of the prior findings from individual studies of earnings supplement programs. One example is from the Canadian Self-sufficiency Project, in which large increases in employment and income were accompanied by increases in marriage in one province and decreases in marriage in another province (Harknett and Gennetian, forthcoming). Another example is MFIP, which produced a small positive effect on marriage among single-parent long-term welfare recipients (Knox et al., 2000).

In general, the results presented here for subgroups of single parent families suggest that the findings that have emerged occasionally from individual studies are not part of a larger pattern of consistent effects for any one subgroup. Specifically, we found no patterns replicating increases in marriage for younger recipients and those with less education, as found in Delaware's evaluation, or reductions in marriage for mothers of very young children in Jobs First or new applicants to welfare in Iowa. These effects may have been idiosyncratic to the particular programs, populations, or sites that were being studied, rather than part of a consistent story of effects on marriage for a broad set of programs.

Thus, despite the scattered effects on marriage that have been reported in earlier studies, we conclude based on the evidence presented here that these welfare and work programs — at least in the short run — had few effects on marriage and cohabitation for single parents, whether for all the programs combined, for particular program types, or for subgroups. This conclusion is in line with economic and sociological theory, which provides no clear prediction about the effects of changes in economic circumstances on marital behavior, and with the past and emerging non-experimental literature on marriage, which has failed to find any consistent effects.¹² Unfortunately data from other studies were insufficient to investigate the effects of these and other welfare reform policies on the stability of marriage among two-parent families or on mar-

¹² For example, see many of the cited articles in this paper including Bitler, Gelbach, and Hoynes, 2002; Ellwood, 2000; Fitzgerald and Ribar, 2001; Hoynes, 1997, Moffitt, 1992, 1994, 1998, 2000.

riage among low-income single parents who were not on welfare at the time of study entry. Evidence on two-parent families from one study — MFIP — suggest that expanded earned disregards and streamlined eligibility rules for two-parent families could be important for helping low-income two-parent families stay together (Knox et al., 2000).

There are a number of possible reasons why these welfare programs did not produce any consistent effects on marriage and cohabitation. The most obvious explanation is that incremental differences in welfare and employment policies are simply not the primary factor in low-income single parents' marital decision-making. None of the programs studied here were specifically designed to increase or change marital behavior. Rather, the programs were designed to affect employment, income and receipt of public assistance; perhaps effects on economic outcomes of the size produced by these programs are just not enough to change marital behavior.

Another possibility is that these programs did affect marriage and cohabitation, but not in ways that the available data could capture. First, as mentioned earlier, few of these studies included two-parent families, leaving open the possibility that employment and economic security play a larger role in maintaining the stability of a two-parent family than in the formation of a two-parent family. Second, other welfare policies that were not tested here, particularly those that lead to significant reductions in family income, may affect marital behavior differently. Third, the marriage and cohabitation outcomes examined in this paper capture family structure at one point in time. Marital behavior, especially cohabiting relationships, is quite fluid and dynamic. It may be that welfare policies affect aspects of stability in relationships that are not captured in the point-in-time survey reports examined here. Over the next year, longer-term follow-up information will become available for some of the studies included in this meta-analysis, allowing us to more formally examine this hypothesis.

Finally, marriage effects for subgroups of the population may have been understated if there are complicated interactions between local context — including community norms or culture and the local availability of marriageable partners — and how welfare policies affect marital behavior. While some aspects of community or culture may be captured through the subgroup analyses conducted here, it may also be the case that “context” is not neatly tied to a particular subpopulation. In other words, perhaps people do take their economic circumstances into account when making decisions about marriage and cohabitation, but in such varied ways that the average effect is zero and that it is difficult to define subgroups that respond consistently in one direction or the other. The variation in control group rates of marriage and cohabitation across sites is consistent with this idea, as is the inconsistency of effects for subgroups across individual studies.¹³ Further subgroup analyses, in which subgroups are defined by multiple

¹³The potential role of local context is explored in depth in an attempt to understand the divergent findings by province on marriage and cohabitation in the Canadian Self-Sufficiency Project (Harknett and Gennettian, 2001).

characteristics, perhaps by “predicted likelihood of getting married or cohabiting in the absence of the program” might reveal more consistent effects across studies than was possible using conventional subgroup analysis methods. More sophisticated analyses conducted with pooled individual-level data, which could control for a variety of local characteristics including religiosity, male/female population ratios and other “marriage market” kinds of characteristics, could also reveal new insights into how single parents alter their marriage and cohabitation decisions in response to welfare and work policies.

In conclusion, while it is possible that continued analysis could find some subtle effects of changes in welfare policy on marriage and cohabitation among single parents, the analyses here, that provide some of the best evidence available, indicate that there is little or no effect from this range of welfare reform policies that states have typically adopted. Thus, policymakers whose goal is to increase the rate of healthy marriages will need to seek policies beyond those examined here. Given the tantalizing hints of increased marriage among some of the earned disregard programs, combining earnings supplements or other work supports with counseling or other services aimed at promoting healthy marriages may be a promising place to start. With the support of the federal government states are beginning to explore and evaluate new policies including counseling, information sharing and related interventions designed to foster and support healthy marriage.¹⁴

¹⁴Under contract to the Administration for Children and Families, U.S. Department of Health and Human Services, Building Strong Families is an initiative to develop and evaluate programs designed to help unwed parents achieve their aspirations for healthy marriage. The project is conducted by Mathematica Policy Research, Inc., with subcontractors MDRC, Public Strategies, Inc., the Urban Institute and Decision Information Resource, Inc. For more information, see www.buildingstrongfamilies.info.

Appendix A

Description of the Studies Included in the Meta-analysis

Connecticut Jobs First

Jobs First, Connecticut's welfare reform initiative, was implemented in 1996 in New Haven and Manchester Connecticut. The two key components of Jobs First were a very short welfare time limit (recipients were limited to 21 cumulative months of cash assistance unless they were granted an extension or exemption from the time limit) and a 100 percent earned income disregard (all earned income was disregarded, up to the federal poverty level). Jobs First also eliminated the 100-hour rule and the work history requirement for two-parent families. Jobs First was found to increase employment, earnings, and income, and though no effects on marital status were found for the overall population, Jobs First reduced marriage among a small sample of mothers with very young children (Bloom, Scrivener, Michalopoulos, Morris, et al., 2002; Fuller et al., 2002).

Florida's Family Transition Plan (FTP)

In 1994, Florida implemented a time-limited pilot welfare reform initiative in Escambia County called the Family Transition Program that operated until late 1999. Under the program, welfare benefits were limited to 24 months within any 60-month period in most cases, and 36 months in any 72-month period for those families deemed the least job-ready. FTP provided participants with enhanced support services (such as assistance with health, child care, and transportation services), a somewhat enhanced earned income disregard, eliminated the 100-hour rule and the work history requirement for two-parent families and income of a stepparent was disregarded for up to 6 months. FTP increased employment during the first two years of the follow-up, had a modest effect on income and no effect on marital status (Bloom et al., 2000).

Los Angeles Jobs First/GAIN

In January of 1995 Los Angeles County transformed its Greater Avenues for Independence program (GAIN), which encouraged welfare recipients to attend education courses to learn basic skills, into a strongly employment-focused welfare-to-work program called Jobs-First GAIN. The new program operated as a pilot study until March 1998. The key features of Jobs-First GAIN were: a strong message to recipients that they should move as rapidly as possible into employment; unusually intensive program orientation sessions; high quality job search assistance through job clubs and positive motivational messages; access to job developers in each Job-First GAIN office who specialized in cultivating relationships with local employers and

identifying job openings; and a strong enforcement orientation that emphasized compliance with program rules. Jobs First GAIN increased employment and earnings, and had no effect on marital status (Freedman et al., 2000).

Minnesota Family Investment Program

In 1994, the Minnesota Family Investment Program (MFIP) was implemented as a pilot in three urban counties (Hennepin, Anoka, and Dakota), and four rural counties (Mille Lacs, Morrison, Sherburne, and Todd) in Minnesota. MFIP implemented two complementary components: financial incentives via an enhanced income disregard and mandatory participation in employment-focused services for long-term welfare recipients.¹⁵ Families participating in MFIP remained eligible for assistance until their income reached 140% of the poverty line. In addition, MFIP simplified eligibility rules by consolidating AFDC assistance payment, Food Stamps, and a state-funded cash assistance program into one monthly payment. MFIP also eliminated the 100-hour rule that limited the number of hours a primary wage earner in a two-parent family could work and remain eligible for welfare and the work history requirement, and expanded the earned income disregard for stepparents. All welfare recipients in the study were randomly assigned to an MFIP group or an AFDC group. Both rural and urban counties had “Full Service” MFIP groups, while urban counties additionally could assign families to a second MFIP group termed “Incentives Only.” These families received MFIP’s financial provisions but were not mandated to participate in employment-related services. For single-parent long-term recipients of welfare, MFIP increased employment by 35 percent and increased total family income from earnings and welfare benefits by 15 percent. MFIP also increased rates of marriage among single parent long-term recipients (Knox et al., 2000; Miller et al., 2000). Similar effects were not found among single-parent recent applicants.

National Evaluation of Welfare to Work Strategies (NEWWS)

The National Evaluation of Welfare-to-Work Strategies was a multi-year MDRC study of 11 welfare-to-work programs in 7 cities that were implemented after the passage of the Family Support Act in 1988. The evaluation included a survey of 4 sites at a two year follow-up point and a five-year follow-up point. Three of these sites (Atlanta, Grand Rapids and Riverside) had side-by-side tests, via a three-group research design. Welfare recipients were randomly assigned either

¹⁵ First, MFIP included an enhanced earnings disregard, paid child care costs directly to providers, and provided Food Stamps benefits in the form of cash. Second, MFIP required single parents who had received public assistance for 24 of the past 36 months (and one earner in two-parent families who had receive public assistance for 6 of the past 12 months) to work at least 30 hours per week or participate in employment and training activities in order to continue receiving their full grants. For more details see Miller et al., 2000.

to a labor-force attachment or “work-first” program, a human capital development or education-first program or to a control group. The fourth site (Portland) implemented a mixed strategy in which recipients randomly assigned to the program group were either mandated to enter employment or an employment related activity that could include education. In the 2-year follow-up evaluation all of the programs increased participation in activities aimed at increasing employment. The employment focused programs had larger positive effects on employment and earnings than most of the education-focused programs. At the 5-year follow-up point, most of the employment and education focused programs increased employment and earnings. No consistent effects on marital status were found during the two or five year follow-up point, though cohabitation significantly increased in some sites (Hamilton et al., 2001).

Vermont’s Welfare Restructuring Project (WRP)

Vermont’s Welfare Restructuring Project was a statewide pilot that began in 1994. Like MFIP, WRP was implemented using a three-group research design. Welfare recipients randomly assigned to the WRP “Full Services” group were subject to a “work trigger” that required single-parent recipients to work in wage-paying jobs once they had received cash assistance for 30 cumulative months (the limit was 15 months for two-parent families with an able-bodied primary wage earner), and could keep more of their welfare benefits as their earnings increased, via an enhanced income disregard. Welfare families randomly assigned to the WRP “Incentives Only” group received all of the financial benefits of WRP but were not subject to the work trigger. WRP also discontinued the 100-hour rule that automatically disqualified low-income two-parent families from receiving ANFC cash assistance if the primary earner worked 100 hours or more, and WRP expanded transitional Medicaid coverage and child care assistance. WRP had modest effects on employment and earnings, and no consistent effects on marital status (Bloom et al., 2002).

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The Effects of Welfare Programs on Marriage and Cohabitation

Table 1

Average Effects Across Programs Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
All single parents	16,138	10.6	10.9	-0.3	10.9	10.8	0.1
By youngest child's age:							
Younger than 3	3,682	15.2	14.9	0.3	15.1	14.8	0.2
3 to 5	5,486	9.4	9.6	-0.2	10.0	9.8	0.2
6 or older	6,625	9.9	10.7	-0.8	8.2	8.9	-0.6
By number of children:							
1 child	6,259	10.2	10.6	-0.4	11.7	11.4	0.3
2 children	5,243	11.1	10.2	0.9	10.2	10.7	-0.5
3 or more children	4,358	9.8	12.2	-2.3 ***	9.6	9.5	0.2
By age:							
Younger than 25	3,657	12.9	13.4	-0.5	15.5	15.1	0.4
25 or Older	12,481	10.0	10.5	-0.4	9.6	9.5	0.1
By prior marital status:							
Ever married	7,508	14.9	15.9	-1.0	10.3	9.9	0.4
Never married	8,518	6.3	6.6	-0.2	11.3	11.5	-0.2

(continued)

Table 1 (continued)

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
By race:							
Black	6,986	5.3	6.0	-0.7	5.0	5.5	-0.5
White	6,599	15.9	16.1	-0.2	17.5	16.7	0.8
Hispanic	1,841	12.4	11.4	1.0	8.3	8.4	-0.1
By levels of disadvantage^c:							
Most disadvantaged	2,137	5.8	6.8	-0.9	9.5	8.2	1.3
Moderately disadvantaged	10,416	10.2	10.8	-0.5	11.0	11.3	-0.3
Least disadvantaged	3,397	13.1	13.0	0.2	11.1	11.0	0.1
By prior work experience^d:							
Has prior work experience	7,439	11.7	11.8	-0.1	11.2	11.4	-0.2
Has no prior work experience	8,699	9.5	10.1	-0.6	10.6	10.3	0.3
By length of welfare receipt^e:							
Long term recipient	5,911	6.9	7.3	-0.4	9.6	9.8	-0.1
Short term recipient	10,227	12.6	12.9	-0.3	11.6	11.3	0.3

(continued)

Table 1 (continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: FTP, Jobs First, Los Angeles Jobs-First GAIN, MFIP, NEWWS, and WRP (representing 14 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, within study or meta-analytic averages.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

^aThis is the sample size for both the marriage and cohabitation outcomes.

^bThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.

^cRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^dRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

^eRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

The Effects of Welfare Programs on Marriage and Cohabitation

Table 2

**Average Effects for All Programs with Any Expanded Earnings Disregard, Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics**

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
All single parents	7,753	13.9	13.7	0.2	14.2	13.7	0.5
By youngest child's age:							
Younger than 3	2,963	15.0	14.5	0.5	15.1	15.4	-0.3
3 to 5	1,998	14.5	15.0	-0.4	14.7	14.5	0.2
6 or older	2,521	12.1	12.2	-0.1	12.6	10.8	1.8
By number of children:							
1 child	3,129	13.9	14.2	-0.3	16.1	13.4	2.6 **
2 children	2,357	15.0	13.6	1.3	12.8	14.2	-1.3
3 or more children	1,998	12.3	14.0	-1.7	12.3	12.7	-0.3
By age:							
Younger than 25	2,295	15.9	16.8	-1.0	16.8	16.7	0.0
25 or Older	5,458	13.5	13.0	0.5	13.1	12.3	0.8
By prior marital status:							
Ever married	3,236	19.5	19.7	-0.2	13.3	12.8	0.5
Never married	4,425	9.7	9.5	0.2	15.0	14.3	0.6

(continued)

Table 2 (continued)

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
By race:							
Black	2,687	8.6	8.8	-0.2	9.0	7.6	1.3
White	4,132	17.9	17.3	0.6	17.8	18.2	-0.4
Hispanic	543	9.3	11.0	-1.7	9.1	6.1	2.9
By levels of disadvantage^c:							
Most disadvantaged	846	7.2	8.3	-1.1	14.0	12.0	2.0
Moderately disadvantaged	5,027	13.0	13.1	-0.2	13.4	14.1	-0.7
Least disadvantaged	1,773	18.8	17.0	1.9	16.2	13.9	2.3
By prior work experience^d:							
Has prior work experience	3,990	15.3	15.1	0.2	14.5	14.1	0.5
Has no prior work experience	3,763	12.3	12.1	0.2	13.9	13.5	0.4
By length of welfare receipt^e:							
Long term recipient	2,996	9.2	9.6	-0.4	12.7	13.0	-0.3
Short term recipient	4,757	16.6	16.4	0.2	15.2	14.0	1.1

(continued)

Table 2 (continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: FTP, Jobs First, MFIP, and WRP (representing 6 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, within study or meta-analytic averages.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

^aThis is the sample size for both the marriage and cohabitation outcomes.

^bThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.

^cRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^dRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

^eRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

The Effects of Welfare Programs on Marriage and Cohabitation

Table 3

**Average Effects for Programs with Expanded Earnings Disregard and a Welfare Time Limit, Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics**

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
All single parents	4,145	12.5	14.2	-1.8	10.6	9.4	1.2
By youngest child's age:							
Younger than 3	1,591	13.9	14.5	-0.6	12.1	11.2	0.9
3 to 5	1,048	12.4	16.2	-3.7 *	11.9	10.2	1.7
6 or older	1,367	10.7	12.4	-1.7	9.1	7.2	2.0
By number of children:							
1 child	1,650	12.3	13.9	-1.6	13.4	10.0	3.5 **
2 children	1,219	14.3	14.5	-0.2	8.5	10.0	-1.5
3 or more children	1,137	11.2	14.0	-2.8	9.1	7.9	1.2
By age:							
Younger than 25	1,228	12.8	16.3	-3.4 *	13.1	13.1	-0.1
25 or Older	2,917	12.3	13.4	-1.1	9.7	7.9	1.7
By prior marital status:							
Ever married	1,566	19.4	20.7	-1.3	9.8	9.4	0.4
Never married	2,520	8.1	10.0	-1.9	11.4	9.3	2.1 *

(continued)

Table 3 (continued)

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
By race:							
Black	1,914	9.0	9.9	-0.8	7.1	4.6	2.5 **
White	1,605	17.9	20.1	-2.1	15.7	17.0	-1.3
Hispanic	505	7.8	9.4	-1.7	7.9	4.3	3.7
By levels of disadvantage^c:							
Most disadvantaged	569	7.4	11.0	-3.6	8.6	6.6	1.9
Moderately disadvantaged	2,791	11.6	13.7	-2.1 *	10.5	10.2	0.3
Least disadvantaged	720	19.6	18.0	1.6	11.3	8.9	2.4
By prior work experience^d:							
Has prior work experience	2,049	13.7	15.3	-1.6	11.6	10.5	1.1
Has no prior work experience	2,096	11.3	13.3	-2.0	9.7	8.4	1.3
By length of welfare receipt^e:							
Long term recipient	1,579	6.8	10.0	-3.2 **	8.9	7.3	1.6
Short term recipient	2,566	15.9	16.9	-1.0	11.8	10.6	1.2

(continued)

Table 3 (continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: FTP and Jobs First (representing 2 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, within study or meta-analytic averages.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

^aThis is the sample size for both the marriage and cohabitation outcomes.

^bThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.

^cRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^dRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

^eRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

The Effects of Welfare Programs on Marriage and Cohabitation

Table 4

**Average Effects for Programs with Enhanced Earnings Disregard and No Welfare Time Limit Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics**

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
All single parents	3,608	15.7	13.3	2.4 **	17.0	17.4	-0.4
By youngest child's age:							
Younger than 3	1,372	16.2	14.4	1.8	17.4	19.0	-1.7
3 to 5	950	17.6	14.0	3.6	16.5	18.0	-1.5
6 or older	1,154	13.9	12.0	1.9	15.4	14.0	1.4
By number of children:							
1 child	1,479	15.6	14.5	1.1	17.9	16.3	1.7
2 children	1,138	15.9	12.9	3.1	16.4	17.5	-1.1
3 or more children	861	14.1	14.1	0.0	14.6	17.4	-2.8
By age:							
Younger than 25	1,067	19.2	17.3	1.8	19.9	19.8	0.2
25 or Older	2,541	15.1	12.7	2.4 *	15.6	16.1	-0.5
By prior marital status:							
Ever married	1,670	19.7	19.0	0.7	15.8	15.2	0.6
Never married	1,905	12.3	9.1	3.2 **	17.9	19.3	-1.4

(continued)

Table 4 (continued)

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
By race:							
Black	773	8.2	6.8	1.4	11.1	13.1	-2.0
White	2,527	18.1	15.9	2.1	18.9	18.8	0.1
Hispanic	38	27.1	32.5	-5.3	1.7	31.8	-30.1 *
By levels of disadvantage^c:							
Most disadvantaged	277	10.6	4.0	6.6 **	22.3	20.6	1.8
Moderately disadvantaged	2,236	14.9	12.6	2.2 *	15.8	17.8	-2.0
Least disadvantaged	1,053	18.5	16.5	2.1	18.6	16.4	2.2
By prior work experience^d:							
Has prior work experience	1,941	17.0	15.0	2.0	16.6	16.8	-0.2
Has no prior work experience	1,667	14.1	11.1	3.0 **	17.4	18.4	-0.9
By length of welfare receipt^e:							
Long term recipient	1,417	12.7	9.2	3.5 **	15.2	17.9	-2.6
Short term recipient	2,191	17.5	16.0	1.5	18.0	17.0	1.0

(continued)

Table 4 (continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: MFIP and WRP (representing 4 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, within study or meta-analytic averages.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

^aThis is the sample size for both the marriage and cohabitation outcomes.

^bThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.

^cRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^dRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

^eRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

The Effects of Welfare Programs on Marriage and Cohabitation

Table 5

Average Effects for Programs with Mandatory Employment Services Only, Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
All single parents	8,385	8.0	8.5	-0.5	8.2	8.3	-0.1
By youngest child's age:							
Younger than 3	719	16.4	16.6	-0.2	14.1	12.4	1.7
3 to 5	3,488	6.8	6.8	-0.1	7.5	7.3	0.2
6 or older	4,104	8.8	9.8	-1.0	6.5	7.9	-1.3 *
By number of children:							
1 child	3,130	6.9	7.3	-0.4	8.6	9.5	-0.9
2 children	2,886	8.5	7.7	0.7	8.1	8.2	-0.1
3 or more children	2,360	8.1	10.8	-2.6 **	7.5	7.1	0.4
By age:							
Younger than 25	1,362	8.2	8.2	0.0	13.5	12.7	0.8
25 or Older	7,023	7.9	8.6	-0.8	7.3	7.5	-0.2
By prior marital status:							
Ever married	4,272	11.9	13.2	-1.3	8.3	7.9	0.4
Never married	4,093	3.3	3.7	-0.4	8.1	8.8	-0.7

(continued)

Table 5 (continued)

Group	Sample Size ^a	Married at Time of Survey			Cohabiting at Time of Survey		
		Program Groups	Control Groups	Impact ^b	Program Groups	Control Groups	Impact ^b
By race:							
Black	4,299	3.6	4.5	-0.8	3.4	4.4	-1.0 *
White	2,467	13.3	14.3	-1.0	16.5	14.3	2.3 *
Hispanic	1,298	13.3	11.2	2.1	8.2	9.2	-1.0
By levels of disadvantage^c:							
Most disadvantaged	1,291	5.0	5.9	-0.9	6.9	5.9	1.0
Moderately disadvantaged	5,389	8.1	8.8	-0.7	8.9	8.9	0.0
Least disadvantaged	1,624	7.8	8.7	-0.9	8.7	9.7	-1.0
By prior work experience^d:							
Has prior work experience	3,449	8.0	8.3	-0.3	7.9	8.6	-0.7
Has no prior work experience	4,936	7.8	8.6	-0.9	8.4	8.1	0.3
By length of welfare receipt^e:							
Long term recipient	2,915	4.8	5.2	-0.4	6.7	6.8	0.0
Short term recipient	5,470	9.6	10.2	-0.6	9.0	9.1	-0.1

(continued)

Table 5 (continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: Los Angeles Jobs-First GAIN and NEWWS (representing 8 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, within study or meta-analytic averages.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

^aThis is the sample size for both the marriage and cohabitation outcomes.

^bThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.

^cRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^dRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

^eRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

The Effects of Welfare Programs on Marriage and Cohabitation

Appendix Table 1
Effects on Marriage and Cohabitation in Individual Studies Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics

	Overall Sample								
		Married at Time of Survey			Variation	Cohabiting at Time of Survey			Variation
	Sample	Program	Control	Impact	Across	Program	Control	Impact	Across
	Size	Group	Group		Subgroups	Group	Group		Subgroups
Jobs First	2,419	9.1	10.8	-1.6	N/A	9.6	9.2	0.4	N/A
FTP	1,726	17.2	19.1	-1.9	N/A	12.2	9.8	2.4	N/A
Los Angeles Jobs-First Gain	741	9.1	6.9	2.2	N/A	7.4	8.5	-1.1	N/A
MFIP									
Full Services	1,752	13.0	11.5	1.6	N/A	15.8	14.6	1.2	N/A
Incentives Only	1,451	12.7	11.5	1.3	N/A	13.3	14.6	-1.3	N/A
NEWWS									
Atlanta HCD	2,199	2.8	4.0	-1.2	N/A	3.4	4.4	-1.0	N/A
Atlanta LFA	1,890	3.8	4.0	-0.3	N/A	3.4	4.4	-1.0	N/A
Grand Rapids HCD	1,158	12.1	11.8	0.3	N/A	11.8	11.0	0.8	N/A
Grand Rapids LFA	1,158	13.1	11.8	1.3	N/A	11.2	11.0	0.3	N/A
Riverside HCD	1,350	12.5	10.9	1.6	N/A	9.7	10.1	-0.4	N/A
Riverside LFA	1,678	10.6	13.4	-2.7 *	N/A	12.0	11.1	0.9	N/A
Portland	610	8.7	9.0	-0.2	N/A	18.1	12.9	5.2 *	N/A
WRP									
Full Services	836	19.8	16.9	2.9	N/A	20.9	22.8	-1.9	N/A
Incentives Only	830	22.2	16.9	5.3 *	N/A	22.2	22.8	-0.6	N/A

(continued)

Appendix Table 1 (Continued)

Younger Than 25

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	668	9.0	11.6	-2.6		11.9	14.3	-2.4	
FTP	560	17.4	21.9	-4.4		14.3	11.7	2.6	
Los Angeles Jobs-First Gain	148	9.5	3.3	6.2		9.9	15.1	-5.2	
MFIP									
Full Services	564	12.6	13.1	-0.6		18.7	16.6	2.2	
Incentives Only	441	12.9	13.1	-0.3		18.2	16.6	1.6	
NEWWS									
Atlanta HCD	284	2.5	3.7	-1.2		3.2	4.1	-0.9	
Atlanta LFA	224	3.6	3.7	-0.1		5.8	4.1	1.7	
Grand Rapids HCD	314	11.8	8.8	3.0		19.8	14.0	5.8	
Grand Rapids LFA	311	11.3	8.8	2.6		13.5	14.0	-0.5	
Riverside HCD	179	10.7	10.2	0.5		13.1	19.7	-6.6	
Riverside LFA	195	10.6	15.3	-4.7		20.2	18.1	2.1	
Portland	121	12.1	16.2	-4.1		24.5	20.5	4.0	
WRP									
Full Services	228	28.9	18.4	10.4 *		18.5	26.9	-8.4	
Incentives Only	225	24.4	18.4	6.0		28.1	26.9	1.2	

(continued)

Appendix Table 1 (Continued)

25 or Older

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,751	9.1	10.5	-1.4		8.6	7.4	1.2	
FTP	1,166	17.2	17.8	-0.6		11.3	8.8	2.6	
Los Angeles Jobs-First Gain	593	9.0	7.5	1.5		7.5	6.0	1.4	
MFIP									
Full Services	1,188	13.2	10.6	2.6		14.4	13.4	0.9	
Incentives Only	1,010	12.8	10.6	2.2		10.6	13.4	-2.9	
NEWS									
Atlanta HCD	1,915	2.9	4.1	-1.2		3.3	4.5	-1.2	
Atlanta LFA	1,666	3.8	4.1	-0.3		3.2	4.5	-1.3	
Grand Rapids HCD	844	12.4	12.9	-0.5		8.7	10.1	-1.4	
Grand Rapids LFA	847	13.5	12.9	0.6		10.6	10.1	0.5	
Riverside HCD	1,171	12.4	11.4	1.0		9.2	8.6	0.6	
Riverside LFA	1,483	10.4	13.5	-3.1 *		10.9	10.1	0.8	
Portland	489	7.5	7.6	-0.1		16.3	11.1	5.2	
WRP									
Full Services	608	17.4	16.5	0.9		21.9	21.1	0.9	
Incentives Only	605	20.2	16.5	3.7		20.0	21.1	-1.0	

(continued)

Appendix Table 1 (Continued)

Youngest Child Younger Than 3 Years Old

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	883	10.3	12.2	-1.9		11.8	12.4	-0.6	
FTP	708	18.7	17.5	1.2		12.3	9.7	2.6	
Los Angeles Jobs-First Gain		--	--	--		--	--	--	
MFIP									
Full Services	758	13.6	12.8	0.8		15.8	16.7	-0.9	
Incentives Only	603	14.4	12.8	1.6		15.6	16.7	-1.2	
NEWS									
Atlanta HCD		--	--	--		--	--	--	
Atlanta LFA		--	--	--		--	--	--	
Grand Rapids HCD	344	11.9	11.1	0.8		14.0	11.4	2.6	
Grand Rapids LFA	326	9.4	11.1	-1.7		10.6	11.4	-0.8	
Riverside HCD		--	--	--		--	--	--	
Riverside LFA		--	--	--		--	--	--	
Portland	226	10.0	10.0	0.1		18.7	15.2	3.5	
WRP									
Full Services	252	21.5	18.9	2.6		20.1	25.3	-5.2	
Incentives Only	250	23.3	18.9	4.4		23.8	25.3	-1.5	

(continued)

Appendix Table 1 (Continued)

Youngest Child 3 to 5 Years Old

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	599	8.0	10.4	-2.3		10.1	9.6	0.5	
FTP	449	18.4	23.9	-5.6		14.4	11.0	3.4	
Los Angeles Jobs-First Gain	355	7.2	8.5	-1.3		5.8	8.1	-2.3	
MFIP									
Full Services	469	13.4	13.3	0.1		17.2	13.7	3.4	
Incentives Only	415	13.4	13.3	0.0		15.0	13.7	1.3	
NEWWS									
Atlanta HCD	1,082	3.4	3.3	0.1		4.7	5.1	-0.4	
Atlanta LFA	949	3.9	3.3	0.6		6.1	5.1	1.0	**
Grand Rapids HCD	300	8.1	12.4	-4.3		15.0	16.5	-1.5	
Grand Rapids LFA	312	12.7	12.4	0.3		10.2	16.5	-6.2	
Riverside HCD	618	10.5	7.4	3.1		10.5	11.3	-0.8	
Riverside LFA	751	9.1	10.9	-1.8		14.6	13.0	1.6	
Portland	156	7.1	6.4	0.8		23.0	10.8	12.3 *	
WRP									
Full Services	203	27.6	15.3	12.2 **		20.5	28.6	-8.1	
Incentives Only	190	25.2	15.3	9.8		21.1	28.6	-7.5	

(continued)

Appendix Table 1 (Continued)

Youngest Child 6 Years Old or Older

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	874	8.6	10.5	-1.9		7.7	6.5	1.2	
FTP	493	14.6	15.8	-1.2		12.0	8.3	3.6	
Los Angeles Jobs-First Gain	319	8.2	7.4	0.9		6.1	7.7	-1.7	
MFIP									
Full Services	478	11.7	8.7	3.0		14.6	10.1	4.4	
Incentives Only	394	11.3	8.7	2.5		7.1	10.1	-3.0	
NEWS									
Atlanta HCD	1,117	2.5	4.6	-2.1 *		2.2	4.1	-1.9 *	
Atlanta LFA	941	3.6	4.6	-1.0		1.5	4.1	-2.6 **	**
Grand Rapids HCD	514	14.3	12.4	2.0		8.2	8.5	-0.3	
Grand Rapids LFA	520	14.9	12.4	2.5		12.1	8.5	3.6	
Riverside HCD	732	14.4	15.0	-0.6		8.6	8.7	-0.2	
Riverside LFA	927	12.5	15.9	-3.4		9.5	9.3	0.3	
Portland	221	9.7	7.6	2.1		11.7	12.5	-0.8	
WRP									
Full Services	326	14.9	16.2	-1.3		18.8	19.0	-0.2	
Incentives Only	342	19.0	16.2	2.7		22.4	19.0	3.4	

(continued)

Appendix Table 1 (Continued)

With 1 Child

	Married at Time of Survey					Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups	
Jobs First	987	8.8	10.7	-2.0		11.4	9.7	1.6		
FTP	663	17.9	18.6	-0.7		17.2	10.3	6.9 ***	*	
Los Angeles Jobs-First Gain	297	4.9	5.2	-0.3		7.3	8.9	-1.6		
MFIP										
Full Services	757	14.7	12.9	1.8		17.2	13.7	3.5		
Incentives Only	583	12.7	12.9	-0.2		12.2	13.7	-1.5		
NEWS										
Atlanta HCD	744	2.8	3.2	-0.4	*	1.7	4.5	-2.8 **		
Atlanta LFA	663	2.0	3.2	-1.1		2.7	4.5	-1.9		
Grand Rapids HCD	451	15.1	10.3	4.8		11.5	12.4	-0.9		
Grand Rapids LFA	445	11.3	10.3	1.0		15.2	12.4	2.8		
Riverside HCD	501	8.9	9.2	-0.3		12.7	14.0	-1.2		
Riverside LFA	646	11.8	12.2	-0.4		12.7	12.9	-0.3		
Portland	236	6.6	10.2	-3.5		21.4	10.0	11.3 **		
WRP										
Full Services	327	17.0	17.5	-0.5		24.5	21.4	3.2		
Incentives Only	356	20.4	17.5	2.9		21.9	21.4	0.5		

(continued)

Appendix Table 1 (Continued)

With 2 Children

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	719	11.7	10.6	1.1		7.8	9.3	-1.4	
FTP	500	17.8	20.2	-2.4		9.4	11.0	-1.5	*
Los Angeles Jobs-First Gain	228	13.2	7.0	6.2		7.5	8.4	-0.9	
MFIP									
Full Services	520	8.1	10.6	-2.4		14.5	14.9	-0.4	
Incentives Only	462	14.8	10.6	4.2		15.2	14.9	0.3	
NEWS									
Atlanta HCD	748	3.2	2.6	0.6	*	4.3	4.3	0.0	
Atlanta LFA	640	4.4	2.6	1.8		2.7	4.3	-1.6	
Grand Rapids HCD	456	11.6	11.4	0.2		12.4	10.1	2.3	
Grand Rapids LFA	457	14.4	11.4	3.1		9.5	10.1	-0.6	
Riverside HCD	449	12.9	9.7	3.3		7.7	8.2	-0.5	
Riverside LFA	562	9.6	12.9	-3.3		12.5	10.0	2.5	
Portland	207	7.8	9.0	-1.2		17.9	20.9	-3.0	
WRP									
Full Services	279	24.3	17.1	7.2		19.1	22.2	-3.1	
Incentives Only	269	25.5	17.1	8.3		19.8	22.2	-2.3	

(continued)

Appendix Table 1 (Continued)

With 3 or More Children

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	650	7.5	11.6	-4.2 *		8.4	8.2	0.2	
FTP	487	16.5	17.2	-0.6		10.1	7.6	2.6	*
Los Angeles Jobs-First Gain	214	8.9	11.0	-2.1		9.0	7.1	1.8	
MFIP									
Full Services	428	14.3	12.3	2.0		12.5	15.5	-3.1	
Incentives Only	367	10.4	12.3	-1.9		10.1	15.5	-5.4	
NEWS									
Atlanta HCD	707	2.7	6.8	-4.1 **	*	4.1	4.4	-0.3	
Atlanta LFA	587	4.7	6.8	-2.1		5.5	4.4	1.0	
Grand Rapids HCD	251	8.4	14.6	-6.2		10.5	10.8	-0.3	
Grand Rapids LFA	256	13.8	14.6	-0.8		6.7	10.8	-4.1	
Riverside HCD	400	15.7	14.2	1.5		8.4	7.4	1.0	
Riverside LFA	470	11.2	15.4	-4.3		10.8	10.0	0.8	
Portland	160	12.2	7.6	4.5		10.8	7.2	3.6	
WRP									
Full Services	177	16.5	18.4	-1.9		19.1	21.9	-2.8	
Incentives Only	158	19.3	18.4	1.0		27.0	21.9	5.1	

(continued)

Appendix Table 1 (Continued)

Ever Been Married

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	765	14.8	15.1	-0.4		6.4	8.1	-1.6	
FTP	801	23.7	26.0	-2.2		13.8	10.7	3.2	
Los Angeles Jobs-First Gain	357	11.8	9.7	2.1		7.8	5.8	2.0	
MFIP									
Full Services	714	18.8	17.7	1.1		13.7	10.4	3.3	
Incentives Only	577	14.9	17.7	-2.8		9.1	10.4	-1.4	
NEWWS									
Atlanta HCD	822	6.3	7.4	-1.0		2.9	4.2	-1.4	
Atlanta LFA	684	7.8	7.4	0.4		2.5	4.2	-1.8	
Grand Rapids HCD	584	16.3	16.8	-0.6		11.8	10.5	1.3	
Grand Rapids LFA	607	20.5	16.8	3.6		10.9	10.5	0.4	
Riverside HCD	889	14.9	14.5	0.5		9.7	7.1	2.6	*
Riverside LFA	1,169	11.9	17.3	-5.4 ***	**	10.8	9.6	1.2	
Portland	340	10.2	13.0	-2.8		17.4	13.2	4.3	
WRP									
Full Services	471	21.8	20.7	1.0		20.6	21.8	-1.2	
Incentives Only	473	24.2	20.7	3.5		22.1	21.8	0.3	

(continued)

Appendix Table 1 (Continued)

Never Been Married

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,640	6.4	8.6	-2.2 *		11.3	9.6	1.7	
FTP	880	11.4	12.5	-1.1		11.5	8.7	2.9	
Los Angeles Jobs-First Gain	378	6.5	4.5	2.0		7.3	11.5	-4.2	
MFIP									
Full Services	1,015	9.3	7.8	1.5		17.1	17.3	-0.2	
Incentives Only	849	11.2	7.8	3.4		15.5	17.3	-1.8	
NEWWS									
Atlanta HCD	1,377	0.7	1.7	-1.0 *		3.5	4.7	-1.1	
Atlanta LFA	1,206	1.0	1.7	-0.7		4.0	4.7	-0.7	
Grand Rapids HCD	573	7.4	7.0	0.5		11.9	11.3	0.7	
Grand Rapids LFA	550	4.5	7.0	-2.5		11.5	11.3	0.3	
Riverside HCD	460	8.3	4.4	3.9		10.4	15.6	-5.2	*
Riverside LFA	505	7.1	5.4	1.8	**	14.9	14.0	0.9	
Portland	262	6.9	3.4	3.5		17.6	13.0	4.5	
WRP									
Full Services	365	17.7	12.5	5.3		20.4	24.3	-4.0	
Incentives Only	357	18.3	12.5	5.9		22.9	24.3	-1.4	

(continued)

Appendix Table 1 (Continued)

Black

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	991	6.4	10.0	-3.5 **		7.6	4.9	2.7 *	**
FTP	923	12.6	9.7	2.9	**	6.6	4.3	2.3	
Los Angeles Jobs-First Gain	237	2.9	6.6	-3.6	**	4.9	3.5	1.3	
MFIP									
Full Services	556	8.6	6.8	1.8		11.5	13.1	-1.6	
Incentives Only	500	7.7	6.8	0.9		10.7	13.1	-2.4	
NEWWS									
Atlanta HCD	2098	2.5	3.9	-1.3 *		3.2	4.1	-0.9	
Atlanta LFA	1803	3.4	3.9	-0.4		3.5	4.1	-0.5	
Grand Rapids HCD	434	3.6	5.3	-1.7		5.0	6.7	-1.7	
Grand Rapids LFA	437	5.5	5.3	0.2		4.1	6.7	-2.6	
Riverside HCD	226	7.9	5.2	2.6		4.6	6.0	-1.4	
Riverside LFA	297	6.6	7.7	-1.1		3.0	5.7	-2.7	
Portland	138	8.9	5.5	3.4		5.3	4.8	0.6	
WRP									
Full Services		--	--	--		--	--	--	
Incentives Only		--	--	--		--	--	--	

(continued)

Appendix Table 1 (Continued)

White

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	875	13.0	12.9	0.1		12.9	17.1	-4.2 *	**
FTP	730	23.4	28.7	-5.3	**	19.8	17.0	2.8	
Los Angeles Jobs-First Gain		--	--	--		--	--	--	
MFIP									
Full Services	962	15.0	15.0	0.0		16.8	14.9	1.8	
Incentives Only	775	15.2	15.0	0.1		16.0	14.9	1.1	
NEWS									
Atlanta HCD	77	13.0	5.5	7.5		7.8	13.4	-5.6	
Atlanta LFA	64	12.8	5.5	7.3		2.6	13.4	-10.8	
Grand Rapids HCD	608	18.0	16.4	1.6		16.3	14.5	1.8	
Grand Rapids LFA	599	18.2	16.4	1.8		15.8	14.5	1.3	
Riverside HCD	516	12.5	12.0	0.5		12.3	14.1	-1.8	
Riverside LFA	834	11.0	16.1	-5.1 **		18.3	14.7	3.7	
Portland	403	9.0	9.8	-0.7		20.3	13.2	7.1 *	
WRP									
Full Services	836	19.8	16.9	2.9		20.9	22.8	-1.9	
Incentives Only	830	22.2	16.9	5.3 *		22.2	22.8	-0.6	

(continued)

Appendix Table 1 (Continued)

Hispanic

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	505	7.8	9.4	-1.7		7.9	4.3	3.7 *	**
FTP		--	--	--		--	--	--	
Los Angeles Jobs-First Gain	377	13.9	6.2	7.7 **	**	7.4	11.4	-4.1	
MFIP									
Full Services	38	27.1	32.5	-5.3		1.7	31.8	-30.1 *	
Incentives Only		--	--	--		--	--	--	
NEWS									
Atlanta HCD		--	--	--		--	--	--	
Atlanta LFA		--	--	--		--	--	--	
Grand Rapids HCD	93	13.6	11.6	2.1		13.5	9.4	4.1	
Grand Rapids LFA	91	16.7	11.6	5.2		9.7	9.4	0.3	
Riverside HCD	570	14.1	12.6	1.6		9.0	8.1	0.9	
Riverside LFA	495	12.5	13.2	-0.7		7.2	8.7	-1.5	
Portland		--	--	--		--	--	--	
WRP									
Full Services		--	--	--		--	--	--	
Incentives Only		--	--	--		--	--	--	

(continued)

Appendix Table 1 (Continued)

Most Disadvantaged^a

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	398	5.9	8.6	-2.7		7.8	8.4	-0.6	
FTP	171	10.4	16.4	-6.0		13.9	2.5	11.4 ***	*
Los Angeles Jobs-First Gain	176	12.7	7.3	5.4		8.4	8.5	-0.1	
MFIP									
Full Services	114	8.6	4.2	4.4		15.9	20.5	-4.5	
Incentives Only	107	7.3	4.2	3.2		20.1	20.5	-0.4	
NEWWS									
Atlanta HCD	502	2.0	4.0	-2.0		4.1	2.4	1.6	
Atlanta LFA	403	2.2	4.0	-1.8	**	4.6	2.4	2.2	
Grand Rapids HCD	121	7.2	8.1	-0.9		9.1	7.7	1.4	
Grand Rapids LFA	109	8.8	8.1	0.7		14.7	7.7	7.0	
Riverside HCD	218	14.5	10.9	3.6		9.6	14.8	-5.1	
Riverside LFA	--	--	--	--		--	--	--	
Portland	58	6.1	2.1	4.1		11.0	13.2	-2.2	
WRP									
Full Services	69	19.7	3.8	16.0 **		27.9	20.8	7.1	
Incentives Only	69	10.8	3.8	7.0		31.4	20.8	10.6	

(continued)

Appendix Table 1 (Continued)

Moderately Disadvantaged^a

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,730	9.2	10.4	-1.3		8.9	9.7	-0.8	
FTP	1,061	15.5	19.0	-3.6		13.4	11.1	2.4	*
Los Angeles Jobs-First Gain	464	8.9	6.1	2.8		8.2	8.2	0.0	
MFIP									
Full Services	1055	11.8	10.9	0.8		14.6	15.3	-0.7	
Incentives Only	933	12.0	10.9	1.1		13.1	15.3	-2.2	
NEWWS									
Atlanta HCD	1330	3.0	4.6	-1.6		3.5	4.6	-1.1	
Atlanta LFA	1151	3.0	4.6	-1.5	**	3.1	4.6	-1.5	
Grand Rapids HCD	703	12.2	11.7	0.5		13.0	13.3	-0.3	
Grand Rapids LFA	739	11.7	11.7	0.0		10.4	13.3	-2.9	
Riverside HCD	1004	12.1	11.5	0.6		9.8	9.6	0.2	
Riverside LFA	1167	12.9	12.4	0.5	***	13.8	11.4	2.4	
Portland	402	8.6	10.2	-1.6		19.8	11.5	8.3 **	
WRP									
Full Services	521	17.9	16.0	1.9		18.1	22.5	-4.4	
Incentives Only	500	23.4	16.0	7.5 **		21.0	22.5	-1.5	

(continued)

Appendix Table 1 (Continued)

Least Disadvantaged^a

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	266	13.6	16.0	-2.3		15.1	8.2	7.0 *	
FTP	454	23.6	19.2	4.4		9.5	9.4	0.1	*
Los Angeles Jobs-First Gain	101	3.1	11.4	-8.4		2.4	9.9	-7.5	
MFIP									
Full Services	557	16.5	13.7	2.8		17.5	13.0	4.4	
Incentives Only	389	14.3	13.7	0.7		12.0	13.0	-1.0	
NEWS									
Atlanta HCD	366	2.9	2.1	0.8		2.6	5.6	-3.0	
Atlanta LFA	333	7.4	2.1	5.4 **	**	3.0	5.6	-2.6	
Grand Rapids HCD	334	13.8	12.5	1.3		10.3	7.4	2.9	
Grand Rapids LFA	310	17.5	12.5	5.0		11.6	7.4	4.2	
Riverside HCD	112	8.1	6.2	1.8		7.0	7.8	-0.7	
Riverside LFA	345	6.1	17.2	-11.1 ***	***	9.0	9.8	-0.8	
Portland	142	11.1	6.7	4.4		16.6	14.4	2.2	
WRP									
Full Services	241	26.2	21.9	4.3		24.3	22.8	1.5	
Incentives Only	256	21.5	21.9	-0.3		24.0	22.8	1.2	

(continued)

Appendix Table 1 (Continued)

Long-Term Welfare Recipient^b

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,133	6.1	8.6	-2.5		8.8	8.7	0.1	
FTP	446	8.4	13.5	-5.1 *		9.7	3.8	6.0 **	
Los Angeles Jobs-First Gain	385	9.1	5.8	3.3		7.5	7.6	-0.1	
MFIP									
Full Services	598	8.6	8.2	0.4		13.4	16.0	-2.6	*
Incentives Only	605	11.3	8.2	3.1		11.2	16.0	-4.8 *	*
NEWWS									
Atlanta HCD	985	1.8	2.6	-0.9		2.5	2.9	-0.3	
Atlanta LFA	823	2.1	2.6	-0.5		4.6	2.9	1.7	***
Grand Rapids HCD	340	6.5	8.7	-2.2		10.1	10.0	0.1	
Grand Rapids LFA	342	8.5	8.7	-0.2		8.8	10.0	-1.2	
Riverside HCD	357	12.6	9.0	3.6		9.0	10.5	-1.4	
Riverside LFA	355	5.7	7.5	-1.8		10.7	13.8	-3.1	
Portland	154	5.7	4.9	0.8		12.9	11.8	1.1	
WRP									
Full Services	343	15.5	11.1	4.4		20.3	21.2	-0.8	
Incentives Only	335	19.2	11.1	8.1 **		20.9	21.2	-0.3	

(continued)

Appendix Table 1 (Continued)

Short-Term Welfare Recipient or No Prior Welfare Receipt^b

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,286	11.6	12.6	-1.0		10.3	9.4	0.9	
FTP	1,280	20.2	21.2	-1.0		13.2	11.8	1.4	
Los Angeles Jobs-First Gain	356	8.7	8.7	-0.1		8.7	8.2	0.5	
MFIP									
Full Services	1154	15.6	13.7	2.0		16.8	13.4	3.4 *	*
Incentives Only	846	12.6	13.7	-1.1		15.5	13.4	2.2	*
NEWWS									
Atlanta HCD	1214	3.6	4.9	-1.3		3.9	5.4	-1.6	
Atlanta LFA	1067	4.9	4.9	0.0		2.7	5.4	-2.7 **	***
Grand Rapids HCD	818	14.8	13.0	1.9		12.6	11.3	1.4	
Grand Rapids LFA	816	14.6	13.0	1.6		12.2	11.3	0.9	
Riverside HCD	993	12.6	11.8	0.8		10.0	10.4	-0.3	
Riverside LFA	1323	11.8	15.2	-3.4 *		12.2	10.4	1.8	
Portland	456	10.3	9.7	0.5		19.8	13.5	6.3 *	
WRP									
Full Services	493	22.8	20.7	2.1		20.8	24.4	-3.5	
Incentives Only	495	24.3	20.7	3.7		23.1	24.4	-1.3	

(continued)

Appendix Table 1 (Continued)

Prior Work Experience^c

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,235	10.4	12.4	-1.9		11.5	10.4	1.1	
FTP	814	18.8	19.7	-0.9		11.7	10.6	1.1	
Los Angeles Jobs-First Gain	280	7.1	9.3	-2.2		5.3	10.4	-5.1	
MFIP									
Full Services	1037	14.2	12.7	1.5		15.4	14.0	1.4	
Incentives Only	827	15.1	12.7	2.4		11.9	14.0	-2.1	
NEWS									
Atlanta HCD	829	2.8	2.4	0.4		3.9	5.7	-1.8	
Atlanta LFA	706	4.7	2.4	2.3 *	**	2.4	5.7	-3.3 **	**
Grand Rapids HCD	652	13.0	10.6	2.4		12.2	9.7	2.5	
Grand Rapids LFA	624	13.5	10.6	2.9		13.0	9.7	3.3	*
Riverside HCD	460	11.5	11.5	0.0		10.1	9.3	0.9	
Riverside LFA	631	7.8	14.6	-6.8 ***	**	10.5	10.3	0.1	
Portland	249	7.5	10.2	-2.7		16.3	12.8	3.4	
WRP									
Full Services	395	23.4	20.4	3.0		21.6	23.5	-1.9	
Incentives Only	399	21.9	20.4	1.5		24.2	23.5	0.7	

(continued)

Appendix Table 1 (Continued)

No Prior Work Experience^c

	Sample Size	Married at Time of Survey				Cohabiting at Time of Survey			
		Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Jobs First	1,184	7.8	8.9	-1.1		7.7	7.7	0.0	
FTP	912	15.5	18.9	-3.3		12.5	9.3	3.2	
Los Angeles Jobs-First Gain	461	9.9	5.7	4.3 *		8.2	7.8	0.4	
MFIP									
Full Services	715	11.1	9.4	1.7		16.6	16.0	0.6	
Incentives Only	624	9.7	9.4	0.3		14.4	16.0	-1.6	
NEWWS									
Atlanta HCD	1370	2.8	5.1	-2.3 **		3.0	3.6	-0.5	
Atlanta LFA	1184	3.3	5.1	-1.8 *	**	4.1	3.6	0.5	**
Grand Rapids HCD	506	10.9	13.2	-2.3		12.0	12.0	-0.1	
Grand Rapids LFA	534	12.6	13.2	-0.6		8.7	12.0	-3.3	*
Riverside HCD	890	12.8	10.8	2.1		9.7	10.6	-0.8	
Riverside LFA	1047	12.5	12.5	0.0	**	12.8	11.7	1.1	
Portland	361	9.8	7.8	1.9		19.5	12.7	6.8 *	
WRP									
Full Services	441	17.3	13.6	3.7		20.0	22.0	-2.0	
Incentives Only	431	21.9	13.6	8.3 **		20.8	22.0	-1.2	

(continued)

Appendix Table 1 (Continued)

SOURCES: MDRC calculations based on 2 to 4 year follow-up survey data from the following studies: FTP, Jobs First, Los Angeles Jobs-First GAIN, MFIP, NEWS, and WRP (representing 14 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Chi-square tests were applied to differences between the impacts of the three subgroups. For the subgroups with only two categories, two-tailed t-tests were applied to differences between impacts. Statistical significance levels are indicated in the Variation Across Subgroups column as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

The sample size shown is for both the marriage and cohabitation outcomes.

"--" indicates the sample of the subgroup was zero or too small for analysis.

^aRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^bRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

^cRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

The Effects of Welfare Programs on Marriage and Cohabitation

Appendix Table 2
Effects on Marriage and Cohabitation , for NEWWS sites at the 5 year follow-up, Among Single Parents at Study Entry,
Overall and by Selected Baseline Characteristics

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Overall									
Atlanta HCD	1,146	6.9	8.4	-1.5	N/A	6.7	6.7	0.1	N/A
Atlanta LFA	1,071	9.8	8.4	1.3	N/A	7.1	6.7	0.4	N/A
Grand Rapids HCD	1,102	20.3	20.5	-0.2	N/A	16.2	15.9	0.3	N/A
Grand Rapids LFA	1,090	22.7	20.5	2.2	N/A	17.4	15.9	1.5	N/A
Riverside HCD	773	21.9	18.1	3.7	N/A	13.7	11.1	2.6	N/A
Riverside LFA	1,213	20.6	22.0	-1.4	N/A	15.3	10.7	4.6 **	N/A
Portland	501	17.4	23.6	-6.2	N/A	19.9	13.9	6.0 *	N/A
Younger Than 25									
Atlanta HCD	170	5.9	8.0	-2.2		13.5	9.1	4.4	
Atlanta LFA	140	16.4	8.0	8.4 *		12.5	9.1	3.4	
Grand Rapids HCD	305	28.8	21.6	7.2	*	19.4	21.4	-2.0	
Grand Rapids LFA	308	22.5	21.6	0.9		20.7	21.4	-0.7	
Riverside HCD	145	16.9	28.9	-12.0	**	16.6	18.9	-2.3	
Riverside LFA	169	35.8	33.4	2.4		27.4	13.2	14.2 **	
Portland	112	20.6	40.1	-19.5 *		23.3	7.5	15.8 *	
25 or Older									
Atlanta HCD	965	6.6	8.9	-2.4		5.9	6.4	-0.5	
Atlanta LFA	920	9.0	8.9	0.1		6.6	6.4	0.2	
Grand Rapids HCD	797	17.5	20.4	-2.9	*	15.1	14.0	1.1	
Grand Rapids LFA	782	22.3	20.4	1.9		15.9	14.0	1.9	
Riverside HCD	628	22.5	16.0	6.5 **	**	13.1	9.5	3.6	
Riverside LFA	1,044	18.1	20.2	-2.1		13.7	10.3	3.4 *	
Portland	389	16.6	18.2	-1.7		19.1	15.4	3.7	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Youngest Child Younger Than 3 Years Old									
Atlanta HCD	--	--	--	--		--	--	--	
Atlanta LFA	--	--	--	--		--	--	--	
Grand Rapids HCD	335	23.3	24.2	-0.9		20.0	20.0	0.0	
Grand Rapids LFA	322	20.9	24.2	-3.3		17.5	20.0	-2.5	
Riverside HCD	--	--	--	--		--	--	--	
Riverside LFA	--	--	--	--		--	--	--	
Portland	190	20.2	32.9	-12.7 *		25.1	13.2	11.9 *	**
Youngest Child 3 to 5 Years Old									
Atlanta HCD	720	8.5	7.6	0.9		10.4	8.6	1.8	
Atlanta LFA	633	10.9	7.6	3.2		7.8	8.6	-0.8	
Grand Rapids HCD	301	23.8	22.2	1.6		17.9	18.9	-1.1	
Grand Rapids LFA	307	25.3	22.2	3.1		17.5	18.9	-1.4	
Riverside HCD	501	20.1	20.3	-0.2		15.3	12.7	2.6	
Riverside LFA	644	25.2	23.1	2.1		17.0	11.6	5.4 **	
Portland	129	19.3	19.4	-0.1		22.0	10.6	11.5	**
Youngest Child 6 Years Old or Older									
Atlanta HCD	415	5.8	9.2	-3.3		4.3	5.6	-1.3	
Atlanta LFA	427	8.7	9.2	-0.4		6.3	5.6	0.7	
Grand Rapids HCD	466	16.4	17.0	-0.6		12.6	11.9	0.7	
Grand Rapids LFA	461	22.8	17.0	5.8		17.0	11.9	5.1	
Riverside HCD	272	24.0	15.7	8.3 *		11.8	8.0	3.8	
Riverside LFA	569	14.8	20.6	-5.8 *		13.1	9.6	3.5	
Portland	175	14.0	13.0	1.0		11.4	19.9	-8.6	**

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
With 1 Child									
Atlanta HCD	374	6.4	4.4	2.0		4.8	5.2	-0.5	
Atlanta LFA	384	5.5	4.4	1.0	**	6.7	5.2	1.5	
Grand Rapids HCD	416	19.3	18.5	0.8		17.2	15.8	1.4	
Grand Rapids LFA	409	22.8	18.5	4.3		17.9	15.8	2.1	
Riverside HCD	271	16.8	18.9	-2.0		14.7	13.8	0.8	
Riverside LFA	450	21.2	24.1	-2.9		16.1	13.2	2.9	
Portland	189	17.2	31.4	-14.2 **	*	22.8	13.9	8.9	
With 2 Children									
Atlanta HCD	386	6.4	7.2	-0.8		10.8	6.9	4.0	
Atlanta LFA	360	14.4	7.2	7.3 **	**	7.0	6.9	0.2	
Grand Rapids HCD	443	22.7	20.1	2.6		16.1	17.2	-1.1	
Grand Rapids LFA	442	24.5	20.1	4.5		16.7	17.2	-0.5	
Riverside HCD	258	20.5	14.6	5.9		14.5	12.1	2.3	
Riverside LFA	411	20.9	18.1	2.7		16.7	11.9	4.8	
Portland	170	15.2	9.7	5.5	*	21.5	15.5	6.0	
With 3 or More Children									
Atlanta HCD	375	9.0	15.1	-6.1 *		4.6	8.9	-4.3	
Atlanta LFA	316	8.8	15.1	-6.3 *	**	6.9	8.9	-2.0	
Grand Rapids HCD	243	17.8	24.6	-6.8		13.1	14.1	-1.0	
Grand Rapids LFA	239	19.8	24.6	-4.8		18.4	14.1	4.3	
Riverside HCD	244	28.7	22.3	6.4		12.2	7.3	4.9	
Riverside LFA	352	20.0	24.3	-4.2		12.4	6.5	5.9 *	
Portland	135	20.5	27.6	-7.1	*	13.7	12.6	1.1	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Ever Been Married									
Atlanta HCD	400	13.1	10.0	3.1	*	4.9	5.0	-0.2	
Atlanta LFA	358	16.1	10.0	6.0 *	*	7.9	5.0	2.9	
Grand Rapids HCD	546	27.0	25.3	1.6		16.1	14.1	2.0	
Grand Rapids LFA	542	28.0	25.3	2.7		17.5	14.1	3.3	
Riverside HCD	462	26.5	20.2	6.3		12.0	7.9	4.1	
Riverside LFA	821	22.5	25.6	-3.1		14.9	8.8	6.2 ***	
Portland	275	18.9	22.8	-4.0		17.8	14.5	3.3	
Never Been Married									
Atlanta HCD	735	3.3	7.0	-3.6 **	*	8.0	7.7	0.2	
Atlanta LFA	702	5.8	7.0	-1.1	*	6.4	7.7	-1.3	
Grand Rapids HCD	555	13.9	15.6	-1.7		15.7	18.2	-2.4	
Grand Rapids LFA	547	16.8	15.6	1.2		17.5	18.2	-0.7	
Riverside HCD	310	13.7	15.5	-1.7		17.8	15.8	2.1	
Riverside LFA	390	15.1	16.0	-0.9 **		16.1	13.9	2.1	
Portland	217	15.8	22.9	-7.1		23.0	13.7	9.4	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Black									
Atlanta HCD	1086	6.7	7.3	-0.6		6.7	6.3	0.4	
Atlanta LFA	1012	9.4	7.3	2.1		6.5	6.3	0.2	
Grand Rapids HCD	405	6.6	10.1	-3.5		9.3	11.7	-2.5	
Grand Rapids LFA	413	11.7	10.1	1.6		9.6	11.7	-2.2	
Riverside HCD	123	9.9	12.0	-2.1	**	8.4	3.1	5.3	
Riverside LFA	226	12.4	11.9	0.5		5.5	5.2	0.3	
Portland	124	18.4	10.0	8.5	**	8.6	7.8	0.8	
White									
Atlanta HCD	38	15.5	27.6	-12.1		13.7	19.8	-6.2	
Atlanta LFA	37	27.2	27.6	-0.4		10.6	19.8	-9.2	
Grand Rapids HCD	582	28.8	29.5	-0.7		21.2	19.3	1.9	
Grand Rapids LFA	564	29.2	29.5	-0.3		22.5	19.3	3.2	
Riverside HCD	304	19.3	25.6	-6.3	**	17.0	17.4	-0.4	
Riverside LFA	602	25.5	27.6	-2.1		21.1	15.3	5.8 *	
Portland	328	16.9	27.1	-10.2 **	**	25.1	15.0	10.0 **	
Hispanic									
Atlanta HCD	--	--	--	--		--	--	--	
Atlanta LFA	--	--	--	--		--	--	--	
Grand Rapids HCD	91	26.2	15.4	10.8		5.5	11.8	-6.3	
Grand Rapids LFA	84	27.9	15.4	12.5		25.9	11.8	14.1 *	
Riverside HCD	322	27.2	15.8	11.3 **	**	12.3	8.7	3.6	
Riverside LFA	343	19.0	19.0	0.0		12.5	7.2	5.3	
Portland	--	--	--	--		--	--	--	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey					Cohabiting at Time of Survey			
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Most Disadvantaged^a									
Atlanta HCD	227	1.0	9.8	-8.8 ***	**	7.3	7.3	0.0	**
Atlanta LFA	205	6.4	9.8	-3.4		4.5	7.3	-2.8	
Grand Rapids HCD	117	16.6	15.0	1.6		19.8	17.1	2.7	
Grand Rapids LFA	107	16.6	15.0	1.7		19.1	17.1	2.0	
Riverside HCD	144	24.1	24.1	0.0	*	14.8	11.5	3.3	
Riverside LFA	--	--	--	--		--	--	--	
Portland	50	26.5	4.8	21.7		14.5	16.6	-2.1	
Moderately Disadvantaged^a									
Atlanta HCD	714	7.8	9.0	-1.2	**	5.8	7.6	-1.9	**
Atlanta LFA	666	10.0	9.0	1.0		7.5	7.6	-0.1	
Grand Rapids HCD	687	18.1	19.7	-1.6		18.2	16.3	1.9	
Grand Rapids LFA	701	20.7	19.7	1.0		18.8	16.3	2.5	
Riverside HCD	561	21.0	17.1	3.9	*	13.5	12.8	0.7	
Riverside LFA	827	22.0	21.1	0.9		14.5	12.1	2.4	
Portland	330	17.1	24.1	-7.0		21.0	14.7	6.2	
Least Disadvantaged^a									
Atlanta HCD	194	8.5	5.6	2.9	**	10.7	1.4	9.3 **	**
Atlanta LFA	188	13.3	5.6	7.7 *		8.5	1.4	7.1 *	
Grand Rapids HCD	298	26.8	23.3	3.4		10.9	15.3	-4.4	
Grand Rapids LFA	281	28.9	23.3	5.6		13.1	15.3	-2.2	
Riverside HCD	60	24.4	17.7	6.7	*	12.6	5.7	6.9	
Riverside LFA	267	19.8	24.2	-4.4		15.0	8.8	6.3	
Portland	113	16.2	25.1	-8.9		21.6	9.8	11.8	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Long-Term Welfare Recipient^b									
Atlanta HCD	584	5.9	9.2	-3.2		5.8	8.0	-2.2	
Atlanta LFA	557	7.7	9.2	-1.5		3.5	8.0	-4.5 **	***
Grand Rapids HCD	334	16.5	17.1	-0.5		17.9	11.9	6.0	*
Grand Rapids LFA	336	14.4	17.1	-2.7		18.2	11.9	6.3	
Riverside HCD	226	23.4	19.2	4.2		15.0	9.2	5.8	
Riverside LFA	267	14.9	20.8	-6.0		12.3	9.1	3.2	
Portland	129	22.3	18.3	4.0		17.7	7.0	10.7	
Short-Term Welfare Recipient or No Prior Welfare Receipt^b									
Atlanta HCD	551	7.6	7.9	-0.2		7.9	5.1	2.7	
Atlanta LFA	503	11.5	7.9	3.6		10.2	5.1	5.1 **	***
Grand Rapids HCD	768	22.1	22.1	0.0		15.6	17.9	-2.4	*
Grand Rapids LFA	754	25.9	22.1	3.8		16.6	17.9	-1.3	
Riverside HCD	547	21.3	17.9	3.4		13.5	12.0	1.5	
Riverside LFA	946	22.1	22.4	-0.3		15.8	11.5	4.3 **	
Portland	372	15.7	25.4	-9.7 **		21.0	15.9	5.1	

(continued)

Appendix Table 2 (Continued)

	Married at Time of Survey				Cohabiting at Time of Survey				
	Sample Size	Program Group	Control Group	Impact	Variation Across Subgroups	Program Group	Control Group	Impact	Variation Across Subgroups
Prior Work Experience^c									
Atlanta HCD	409	6.5	8.3	-1.8		10.1	4.0	6.0 **	***
Atlanta LFA	387	11.0	8.3	2.7		7.8	4.0	3.7	
Grand Rapids HCD	604	23.0	20.2	2.8		13.9	17.1	-3.2	
Grand Rapids LFA	580	24.8	20.2	4.6		17.2	17.1	0.0	
Riverside HCD	248	23.4	14.8	8.7 *		12.4	10.1	2.3	
Riverside LFA	458	20.2	20.5	-0.3		15.2	10.6	4.6	
Portland	199	17.6	29.1	-11.5 *		23.4	7.0	16.4 ***	*
No Prior Work Experience^c									
Atlanta HCD	726	6.4	8.9	-2.5		5.1	8.0	-2.9	***
Atlanta LFA	673	9.3	8.9	0.4		6.5	8.0	-1.5	
Grand Rapids HCD	498	17.2	20.9	-3.7		18.7	14.5	4.1	
Grand Rapids LFA	510	20.0	20.9	-1.0		17.8	14.5	3.2	
Riverside HCD	525	20.9	19.5	1.4		13.8	12.4	1.4	
Riverside LFA	755	21.3	22.5	-1.2		14.9	11.5	3.4	
Portland	302	17.3	20.0	-2.6		18.6	16.7	1.9	*

(continued)

Appendix Table 2 (Continued)

SOURCES: MDRC calculations based on 5 year follow-up survey data from the following study: NEWWS (representing 7 programs).

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Chi-square tests were applied to differences between the impacts of the three subgroups. For the subgroups with only two categories, two-tailed t-tests were applied to differences between impacts. Statistical significance levels are indicated in the Variation Across Subgroups column as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

The sample size shown is for both the marriage and cohabitation outcomes.

"-" indicates the sample of the subgroup was zero or too small for analysis.

^aRespondents were classified as most disadvantaged if they: had received welfare payments in 23 or more of the 24 months prior to random assignment (because of data limitations in MFIP, this includes whether there was any receipt of welfare in 11 or more of the 12 months prior to random assignment), were not employed in any of the four quarters prior to random assignment, did not have a high school degree or GED, and their highest school grade completed was less than 12th. Respondents satisfying at least one but not all of these conditions were classified as moderately disadvantaged. Respondents satisfying none of these conditions were classified as least disadvantaged.

^bRespondents were classified as long term recipients if they had received welfare payments in each of the 24 months prior to random assignment and classified as short term recipients otherwise. In MFIP, welfare payment data is available for only 12 months prior to random assignment, therefore, whether they had received welfare payments in each of the 12 months prior to random assignment was used to classify respondents length of welfare receipt.

^cRespondents were classified as having prior work experience if they were employed in at least one of the four quarters prior to random assignment, and classified as having no work experience if not.

The Effects of Welfare Programs on Marriage and Cohabitation

Appendix Table 3

**Average Effects on Marriage Across Programs,
Including Published Estimates from Additional Studies**

Group	Sample Size	Program Groups	Control Groups	Impact ^a	p-value
<i>All single parents</i>	<i>20,196</i>	<i>13.48</i>	<i>13.29</i>	<i>0.19</i>	<i>0.60</i>
By age:					
<i>Younger than 25</i>	<i>4,255</i>	<i>13.62</i>	<i>12.50</i>	<i>1.12</i>	<i>0.14</i>
<i>25 or Older</i>	<i>12,481</i>	<i>10.04</i>	<i>10.45</i>	<i>-0.41</i>	<i>0.39</i>
By prior marital status:					
<i>Ever married</i>	<i>7,508</i>	<i>14.90</i>	<i>15.87</i>	<i>-0.98</i>	<i>0.20</i>
<i>Never married</i>	<i>10,123</i>	<i>7.45</i>	<i>7.63</i>	<i>-0.17</i>	<i>0.70</i>

SOURCES: MDRC calculations based on follow-up survey data from the following studies: Delaware, FTP, Indiana, Iowa, Jobs First, Los Angeles Jobs-First GAIN, MFIP, NEWWS, and WRP.

NOTES: Two-tailed t-tests were applied to differences between the program and control group outcomes, averaged across programs.

Statistical significance levels are indicated as: * = 10 percent; ** = 5 percent; *** = 1 percent.

Rounding may cause slight discrepancies in sums and differences.

Italicized rows indicate results include non MDRC estimates in calculations.

For all single parents, results from Delaware, Indiana, and Iowa were included in calculations.

For the Less than 25 subgroup Delaware results were included in calculations. For the 25 or Older group only MDRC results were used; the Delaware report only displays subgroup results for the 25 to 34 and 35 plus age groups.

For the never married subgroup, Delaware and Iowa results were included in calculations. For the ever married group only MDRC results were used; the Delaware report only displays subgroup results for the currently married and formerly married subgroups; no corresponding subgroup results were available in the Iowa report.

For Delaware, standard deviation was not available so a maximum estimate was used.

For Indiana, standard error was not available so it was approximated using knowledge of the impact estimate and its statistical significance.

^aThe percentage point impact estimates shown here are calculated from the meta-analytic effect size estimates.