

The Impact of Welfare Reform on Female Headship Decisions*

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Abstract

While much of the focus of recent welfare reforms has been on moving recipients from welfare to work, many reforms were also directed at decisions regarding living arrangements, pregnancy, marriage and cohabitation. This study assesses the impact of welfare reform waivers and Temporary Assistance for Needy Families (TANF) programs on women's decisions to become unmarried heads of families, controlling for confounding influences from local economic and social conditions. The study pools the 1990, 1992, 1993, and 1996 panels of the Survey of Income and Program Participation, which span the period when many states began adopting welfare waivers and implementing TANF. It estimates logit models of the incidence of female headship and state-stratified, Cox proportional hazard models of the rates of entry into and exit from headship. The study finds little consistent evidence that waivers affected female headship of families.

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INTRODUCTION

In the 1990s, states launched dramatic modifications in the Aid to Families with Dependent Children (AFDC) program, the largest cash welfare program in the U.S. These reforms were initially accomplished by states obtaining waivers of standing federal welfare policies through Section 1115 of the Social Security Act. Later in the decade, Congress and the President enacted the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), a major reform bill that replaced AFDC with the Temporary Aid to Needy Families (TANF) program. While many of the waiver and TANF reforms focused on work as a route out of welfare dependence and toward economic self-sufficiency, policymakers were also concerned about demographic outcomes including living arrangements, pregnancy, marriage and cohabitation. Indeed, the stated goals of the PRWORA included promoting marriage, reducing non-marital pregnancies, and encouraging and stabilizing two parent families (Maynard et al. 1998). Goals related to marriage and family formation have become an even more central feature of the current administration's plans for reauthorizing welfare.¹

A sizeable literature has developed examining the effects of welfare reform on program participation and economic well-being. More recently, analysts have also begun to investigate the implications of reform for demographic behavior. Some of the analyses have been based on experimental evaluations of state demonstrations. While marriage and other effects were

¹<<http://www.whitehouse.gov/news/releases/2002/02/welfare-reform-announcement-book.pdf>>.

initially reported in some of these studies (e.g., Knox et al. 2000), a recent meta-analysis by Gennetian and Knox (2003) concluded that there were no consistent effects of the demonstrations. The use of random assignment in these experiments should have provided convincing evidence regarding program effects. However, there were limitations that may have contributed to the weak demographic findings or reduced the experiments' usefulness. First, few of the policies were specifically aimed at demographic outcomes. Second, the subjects were all welfare recipients; so, some types of demographic outcomes, such as the initial transition to parenthood, could not be examined. Third, the demonstrations were of limited duration. Fourth, they usually involved bundles of initiatives, making it difficult to isolate the effects of particular policies. Finally, the demonstrations were limited to specific states or communities, making it difficult to generalize outcomes beyond the local context.

Other findings—also mixed—have come from non-experimental, observational studies. While it is more difficult to address problems such as policy endogeneity with observational data, the data can be used to examine broader portions of the population under wider sets of circumstances. Studies by Bitler, Gelbach, Hoynes and Zavodny (2002), Horvath-Rose and Peters (2001) and Schoeni and Blank (2000) analyzed aggregated data, and a study by Bitler, Gelbach and Hoynes (2002) analyzed individual-level repeated cross-section data. To our knowledge, however, no previous observational study of the demographic effects of welfare reform has used nationally representative, individual-level, longitudinal data. This study does this, examining national samples from the 1990, 1992, 1993, and 1996 panels of the Survey of Income and Program Participation (SIPP).

The specific aims of this investigation are to model female headship decisions (unmarried motherhood) and assess the impact of welfare reform provisions on those decisions while controlling for local economic and social contextual conditions. In particular, the study

examines whether the provisions initiated as state waivers—and in some cases incorporated into subsequent TANF programs—deterred women from becoming and remaining unmarried mothers. It examines female headship in the context of a rational-choice model in which changes in program rules affect not only the direct incentives to marry and bear children but also the indirect incentives that arise through job and marriage opportunities. In addition to the longitudinal, individual-level data on family outcomes and personal characteristics, the investigation develops and uses annual, county-level information on skill-specific wage and employment opportunities and on marriage market conditions. It also employs event history methods that control for other unobserved features that may have contributed to differences in headship transitions across states.

Many key provisions of welfare reform were tested via waivers from 1992-1996, and many of these waiver provisions were continued by the states under the PRWORA. This study considers features of the states' waiver experiments and TANF programs. It allows the policies to have different impacts pre- and post-TANF. If the reform provisions have significant effects, it should be able to detect them using data spanning 1989-2000.

BACKGROUND AND SIGNIFICANCE

In recent years, policy makers have paid increased attention to the role of the welfare system in influencing family structure. The percentage of female-headed families (families composed of an unmarried woman with own children) has risen dramatically over the past three decades from 12 percent of all families with children in 1970 to 26 percent in 2000 (Fields and Casper 2001). This trend reflects an increase in out-of-wedlock births as well as a retreat from marriage, although the rate of out-of-wedlock births stabilized in the 1990s (Blank 2002; Elwood

and Jencks 2001). The high rate of female headship is alarming because single parenthood is associated with a host of adverse outcomes. Poverty rates and welfare dependence are much higher on average for single-parent families than for two-parent families (Lerman 1996); schooling and other developmental outcomes for children in single-parent families are also typically worse than in two-parent families (Haveman and Wolfe 1994; McLanahan and Sandefur 1994).²

Some analysts, such as Murray (1984), blame public assistance programs for the rise in single headship. These analysts argue that the eligibility criteria for the earlier AFDC program effectively subsidized single parenthood. Reforms in the 1988 Family Support Act that required states to extend welfare eligibility to poor married families with an unemployed primary earner (through AFDC-UP programs) were intended address marriage disincentives. Some elements of the PRWORA, specifically its emphasis on reducing teenage births and encouraging two-parent families, also reflect these concerns.

The magnitude of welfare's marriage and fertility incentives has been the subject of much work. The large literatures on the flow components of female headship—marriage, cohabitation, divorce, childbearing—have been surveyed elsewhere (Acs 1995; Hoynes 1997b; Moffitt 1995,

² The literature here, however, is far from unanimous. Many studies of the impact of family structure on child well-being report insignificant associations (see, e.g., Amato's 1993 review). Studies also occasionally report that particular types of non-marital arrangements, such as living with a single mother in a multi-generational household (Deleire and Kalil 2002), are more beneficial than living with two parents. Studies have also found that marriage has negative associations for children whose parents fight and argue often (Amato et al. 1995, Jekielek 1998, Morrison and Coiro 1999) and children in blended families (Hofferth and Anderson 2003).

2001). Moffitt (1998) reviewed over 60 papers on the impact of welfare benefit levels on marriage and fertility and concluded that effects were in the expected direction, though likely small in magnitude.

Schultz (1994), Moffitt (1995, 1998) and others have cautioned that evidence that regarding the demographic effects of welfare generosity is sensitive to the methods used to control for differences in the economic, social, and institutional environment across states. Since welfare reform has taken place at the same time as significant changes in the labor market, disentangling the impact of reform from changes in employment conditions requires careful measurement of these contextual characteristics. Our study develops improved local measures of skill-specific employment opportunities, incorporates controls for marriage market conditions, and accounts for other unobserved differences across states to address concerns regarding confounding environmental influences. Past research by Grossbard-Shechtman (2003), Lichter et al. (1991, 1997, 2002), Matthews et al. (1997), Ribar (1998), Schultz (1994), and Wood (1995) has shown the importance of controlling these types of circumstances in modeling family formation.

Although numerous studies have examined the demographic effects of welfare policies, nearly all of the research conducted prior to 2000 focused on benefit levels and implicit tax rates. More recently, however, studies have begun to examine other provisions, such as whether a state undertook a major reform or experimented with specific types of reforms. Blank (2002) described many of these studies in her recent review.

Several analyses have examined results from social experiments and demonstrations. Groeneveld et al. 1983 investigated marriage and cohabitation outcomes among participants in the Seattle and Denver Income Maintenance Experiments. Treatments in these experiments eliminated the explicit financial disincentives to marriage by extending benefits to couples. The

treatments also changed transfer guarantees and benefit reduction rates and in some cases provided job training assistance and employment counseling. On balance, the treatment appeared to be substantially more generous than the existing combination of AFDC and food stamps, providing more assistance both inside and outside of unions. After three years of exposure, dissolution rates among blacks and whites in the experimental program were roughly 40 percent higher than among those in the control group. These results suggest that the removal of disincentives was dominated by an independence effect from higher overall benefits. However, Cain (1986) has cautioned that the findings are sensitive to alternative statistical controls and that the disincentives may be small.

Knox et al. (2000) examined marriage behavior among families in the Minnesota Family Investment Program (MFIP), which relaxed some provisions in the state's AFDC-UP program, imposed work requirements, and increased earnings disregards. They found that assignment to the MFIP treatment had positive effects on marriage. Specifically, the program led to higher rates of marriage among welfare recipients who were initially single and to lower rates of dissolution among recipients who were initially married. The Minnesota findings, however, appear to be an isolated result. Reports from several other state demonstrations either show no effects or negative effects on marriage (see, e.g., Table 9.C in Blank's 2002 summary).

Gennetian and Knox (2003) re-analyzed individual-level data gathered from 14 separate experimental treatments to examine the impact of welfare reform on marriage. The experimental programs included combinations of policies that equalized treatment for married and unmarried families, provided more generous earnings disregards, required employment services, and imposed time limits. The researchers examined the programs individually, in groups according to policies, and all together. They also examined the impacts for different types of people. They found no consistent effects—positive or negative—of the programs on marriage. An analysis by

Harknett and Gennetian (2003) of the impacts of experimental earnings supplements on union formation in two Canadian provinces similarly yielded inconclusive results—the supplements were associated with higher rates of marriage in one province but lower rates in the other.

Several observational studies have also investigated these issues. Schoeni and Blank (2000) used 1977-1999 data from the Annual Demographic Files of the Current Population Survey (CPS) aggregated by state, year, educational attainment, and age. They found that welfare reforms led to higher rates of marriage and lower rates of headship; the results were especially strong among less-educated women. Horvath-Rose and Peters (2001) examined state-level data on the proportion of births that occurred out-of-wedlock. They found that nonmarital birth rates were lower overall in states with welfare waivers. However, when they examined particular types of waivers they found that family caps were associated with fewer nonmarital births while teen co-residence requirements were associated with more nonmarital births.

Bitler, Gelbach, Hoynes and Zavodny (2002) analyzed state-level vital statistics data on marriages and divorces. The authors argued that these data might be more sensitive to policy changes because they represented flows into and out of marriage. However, the data also had some shortcomings, such as being measured on the basis of state of occurrence rather than state of residence. Bitler et al. found that welfare reform was associated with lower rates of both marriage and divorce.

A separate study by the Bitler, Gelbach and Hoynes (2002) used 1989-2000 individual-level data from the CPS to examine living arrangements. They found that TANF implementation had impacts on the black and Hispanic population, but not on all groups combined. For example, TANF reduced the fraction of black central-city children who lived with an unmarried parent, but increased the fraction living with neither parent. The researchers did not find consistent patterns for detailed measures of specific waivers, raising questions about the mechanism by

which TANF operates.

None of the observational studies on the demographic effects of welfare reform has examined individual-level, longitudinal data. The studies by Bitler, Gelbach and Hoynes (2002) and Schoeni and Blank (2000) focused on current living arrangements and not transitions between those arrangements. The findings of Bitler, Gelbach, Hoynes and Zavodny (2002) as well as other research on female headship by Moffitt and Rendall (1995) and welfare caseloads by Blank (1999), Blank and Ruggles (1996), Fitzgerald (1995), Gittleman (2001), Klerman and Haider (2000), and Ribar (2002) indicates that it is important to consider transitions.

The observational studies by Bitler, Gelbach, Hoynes and Zavodny (2002) and Horvath-Rose and Peters (2001) that have considered family structure transitions did so using aggregate data. Because of this, the studies were limited in their ability to control for relevant individual characteristics such as age, ethnicity and educational attainment. They were also unable to account for duration effects.

Finally, all of the observational studies on the demographic effects of welfare reform have used relatively crude, state-level controls such as general unemployment and economic growth rates to describe local economic opportunities and social conditions. Ribar (2003) found that county-level conditions are more appropriate descriptors of people's economic opportunities. He also found that local opportunities differed substantially for men and women with different educational attainments and that less-skilled individuals were less likely to commute out of their counties of residence. Fitzgerald (1995) and Ribar (2002) have further shown that county-level economic conditions are important determinants of welfare participation.

CONCEPTUAL FRAMEWORK

Headship outcomes result from several different family processes including marriage, childbearing and childrearing. A woman could become a single parent by bearing and raising a child without marrying, by bearing a child within marriage then divorcing, or through a more complicated series of transitions. Similarly, she could avoid single parenthood by remaining childless or by marrying before becoming a mother and then remaining married. A woman could transition out of single parenthood by marrying, giving up custody of her children, or having her children grow up and move out of her household. Welfare programs could affect any or all of these individual processes.

This investigation frames its analysis of headship in terms of an economic, rational-choice model (Becker 1981). Hoynes (1997b), Peters et al. (2003) and Gennetian and Knox (2003) have already undertaken detailed conceptual analyses of the implications of welfare reform for marriage and childbearing using this approach. Accordingly, this section only sketches the theoretical implications (readers who are interested in more thorough treatments of these issues are encouraged to go to the aforementioned sources). In the rational choice framework, people compare the perceived value of each alternative outcome and choose the outcome that they feel is best for them. In deciding whether to marry or remain married, a person would compare the personal, economic and social benefits associated with being married with the benefits of being single. The decision to become a parent would likewise hinge on a comparison of the net benefits of bearing and raising a child with those of remaining childless. The rational approach not only involves current considerations but future considerations as well. Thus, a woman might delay marriage or childbearing at some present cost if she believes that these actions would result in better outcomes for her at some later time.

Within this framework, cash assistance programs and policies directly and indirectly

change the incentives for childbearing and marriage. Unambiguous direct incentives for fertility arise because eligibility for the programs is conditioned on the presence and number of children. Mothers are potentially eligible (depending on their incomes and other circumstances), while non-mothers are categorically ineligible.³ Thus, AFDC and TANF potentially subsidize parenthood outcomes but not other outcomes.

The direct incentives of AFDC and TANF for marriage in a long-run rational choice model are less clear, though still likely to be negative. If we focus only on the immediate implications, the stringent eligibility requirements for married parents under the AFDC-UP and many successor TANF programs discourage marriage. From a long-term perspective, the programs may also create disincentives by subsidizing delays in marriage associated with additional search, that is, by leading women to be more selective regarding whom they marry. However, there may be offsetting long-term incentives to marry because the programs reduce the risk of destitution in the event of divorce or widowhood.

Indirect incentives may also arise, especially to the extent that program rules, eligibility and benefits interact with work status and earnings. Consider, for example, a decrease in benefits that causes mothers to work more. Greater earnings may increase mothers' independent resources, possibly lowering their rates of marriage and increasing their rates of subsequent fertility. Thus, the changes could contribute to higher rates of female headship. Alternatively, higher levels of employment might increase women's exposure to marriageable men, and the reduction in the time available to care for children might reduce fertility. These changes would lead to lower rates of headship.

³ An exception would be a teenager who is covered under either her parent's grant or a child-only grant.

The reforms that states have implemented through waivers and then TANF have encompassed a variety of changes (see the reviews by Blank 2002, Harvey et al. 2000 and the U.S. Department of Health and Human Services 1997). Some changes have been explicitly targeted at removing marriage disincentives, such as eliminating restrictions associated with the AFDC-UP program, and reducing fertility incentives, such as creating family cap policies. These are expected to reduce female headship rates. Some have reduced the generosity of welfare programs, such as by lowering benefits, or made benefits harder to obtain, such as by enacting time limits, setting work requirements or restricting eligibility among teenagers. These are also expected to have reduced female headship—though, as the foregoing discussion indicates, other effects are possible. Finally, a subset of changes, such as increases in earnings disregards and decreases in benefit reduction rates, may have made welfare more attractive and increased headship rates. Because of the ambiguity associated with the combined effects of welfare reform and the effects of some specific provisions, it is important to examine the policies empirically.

To motivate our empirical specifications, we follow Hoynes (1997a) and Moffitt (1994) and consider a simple two-state model in which a woman becomes or remains a single household head if the indirect utility associated with that outcome exceeds the utility associated with not being a single head. Specifically, let $V_F(Y_F, B_F, X)$ be the expected lifetime utility of choosing female headship, and let $V_N(Y_N, B_N, X)$ be the expected lifetime utility of not choosing headship. The arguments of the indirect utility functions include personal characteristics, environmental characteristics, and public policy parameters. In particular, Y_F and Y_N denote vectors representing the private economic resources available in each type of living arrangement (e.g., through own earnings, potential partner contributions, unearned income, etc.). B_F and B_N represent vectors of welfare benefits and policies under each arrangement. X represents a vector

of personal characteristics, taste parameters, and other environmental characteristics. A woman selects female headship at a given point in time if

$$F^* = V_F(Y_F, B_F, X) - V_N(Y_N, B_N, X) > 0$$

and selects another living arrangement if this quantity is zero or negative.

While the two-state specification provides a useful summary of the variables that influence headship outcomes, it plainly abstracts from the underlying childbearing and marriage decisions. In the empirical analysis, we focus on the associations that changes in welfare policies, economic conditions and other factors have with transitions into and out of headship. However, we also relax this restriction and examine the associations with the component childbearing and marriage processes in some sensitivity analyses.

DATA CONSTRUCTION

For our analysis we need individual longitudinal data on headship and personal characteristics linked with detailed measures of economic, social, and policy conditions.

Individual data from the SIPP

The impact of PWRORA on women's decisions can best be estimated by using longitudinal data from the 1990s. We use the Survey of Income and Program Participation, a logical data set in terms of both its content and time coverage.⁴ The SIPP includes detailed

⁴ The Panel Study of Income Dynamics has a smaller sample size than the SIPP and has recently changed to interviewing every other year. The National Longitudinal Surveys are limited to specific birth cohorts. Earlier cohorts were too "old" (members of the 1979 youth

information on monthly living arrangements, government program use, and other personal and family characteristics. The panels vary in length from 32 to 48 months. We pool the 1990, 1992, 1993, and 1996 panels, which together span the interval October 1989 to February 2000. The SIPP is a national survey with approximately 20,000-40000 households per panel. The survey over-samples low-income households and some other groups; when weights (supplied with the survey) are used, estimates are nationally representative. Respondents are interviewed every four months about their monthly activities in that period. Each four-month interview period is called a “wave.” Even though SIPP panels are relatively short (the longest covers four years), the large sample sizes give us an appreciable number of transitions.

In this study we undertake two types of analyses. We first look at levels of female headship and then at transitions into and out of female headship. In each analysis, we examine women aged 15 to 55 and define female heads as women who are unmarried and living with related children aged 17 or less.⁵ This definition includes unmarried mothers in subfamilies. For the analysis of headship levels, the headship indicator and the covariates are taken from the fourth month of each wave (the month immediately preceding each interview).

For the transition analyses, we examine spells, which we define as consecutive months of headship or consecutive months of non-headship. In most cases, the starting and ending dates of the spells are defined in terms of the observed transitions to or from female headship. In addition, we allow non-headship spells to begin at age 15 (i.e., assume that women enter the risk

cohort were at least 30 years old in the mid-1990s), while the latest cohort, was too young (aged 12-17 in 1997).

⁵ Women who report their status as “married spouse absent” were counted as married.

set for becoming a female head at age 15). Our analysis excludes left-censored spells (spells that were ongoing at the beginning of the panel) and uses only the complete and right-censored spells that began during the panel. This leads to a considerable loss of data and some loss of representativeness. In particular, the resulting sample is representative of new spells but not all existing spells.⁶ We allow the analysis sample to include multiple spells per person and adjust standard errors in our models accordingly. To reduce the number of observations in the data set, the covariates in the hazard models are taken only from the fourth month of each wave.⁷

Along with the marriage and childbearing information used to construct the headship

⁶ We also estimated models using retrospective marriage and fertility information collected in the SIPP. The retrospective information allowed us to extend each woman's headship history back to age 15. Unfortunately, the SIPP lacks retrospective information on some covariates such as children's residence; so, we had to make strong assumptions to impute these values. Hazard models estimated using the augmented data produced results that were similar to those reported here.

⁷“Seam” problems occur in longitudinal surveys that ask retrospective questions about conditions between interviews. Respondents sometimes incorrectly report that they have experienced a condition continuously since the last interview, when in fact the condition began sometime afterward. As a result of this misreporting, transitions appear to occur more frequently at interview dates (seams) than between those dates. Our results did not change substantially when we re-estimated our models by including a dummy variable for interview dates or by using wave-by-wave, rather than monthly, transitions.

variables, the SIPP also contains other data on individual characteristics. From these data, the study includes measures of the woman's age, race, educational attainment, and urban residence as covariates in its multivariate analyses.

To provide contextual and policy information, the study matches the time-varying individual-level data from the SIPP with state- and county-level measures gathered from other sources. The public use versions of the SIPP do not identify the county of residence for respondents. They also mask metropolitan area identifiers for some respondents and group some states with only a few respondents. All of these steps are taken to preserve the confidentiality of respondents. By special arrangement with the U.S. Census Bureau and the Boston Research Data Center, we gained access to SIPP files with detailed geographic information. This allowed us to perform the necessary matches in the data.⁸

Welfare Program and Policy Parameters

One important indicator of welfare policy is the generosity of benefits. We measure this as the maximum AFDC benefit available to a family of three in a state with no other income and adjust for inflation using the CPI-U. Using the value for a fixed family size avoids the potential endogeneity of allowing the benefit to depend on living arrangements and number of children.

The other indicators of welfare policies come from descriptors of the waiver and TANF provisions in states. Many waivers of federal welfare policy were tried in states prior to 1996. Since states adopted different provisions and at different times, there is variation across states

⁸The results in this study have been screened to ensure that identifying information is not revealed and approved for release by the U.S. Census Bureau.

and time that allows us to estimate impacts. We rely mainly on the data that were assembled by the U.S. Department of Health Human Services (DHHS) and Crouse (1999) and used by the Council of Economic Advisors in its 1997 and 1999 reports. These data were compared with print sources such as the *Green Book* and electronic sources such as the Urban Institute's Welfare Rules Database (WRD). The indicators include whether a state set a total time limit on benefits (Termination or Term Limit), adopted a family cap provision that limited or eliminated benefits for additional children while on welfare (Family Cap), adopted a time limit after which work was mandated (Work Requirement Time Limit), changed requirements of its Job Opportunity and Basic Skills (JOBS) program (JOBS waivers), and expanded the earnings disregards in calculating benefits (Earnings Disregard). In addition, we gathered information on whether a state required teenage recipients to live with their parents (Teen Coresidence Requirement) or relaxed the rules under which married couples could receive benefits under the Unemployed Parent program (AFDC-UP).⁹ The source data for the waivers include the month and year that a particular provision was adopted statewide.¹⁰ The indicators in our analysis take on values of zero prior to a state adopting a waiver and one afterwards.

For most of the analysis we group waivers into two indicators: a family/term reform

⁹ The AFDC-UP provisions included the 100-hour limit on work, the work history requirement, the waiting period for benefits, and the definition of the principal earner (see the U.S. Department of Health and Human Services 1997).

¹⁰ Data regarding teenage coresidence requirements came from the WRD; these data were measured on a yearly basis. There were some inconsistencies between the old and new versions of this database. In cases of conflict, we used the most recent information.

indicator for whether a state adopted a termination limit, imposed a family cap, required teenage mothers to coreside with their parents or modified its AFDC-UP provisions, and a work reform indicator for whether the state imposed a work requirement, changed the earnings disregard, or modified its JOBS program. We conducted sensitivity analyses in which we used (a) indicators for all of the individual waivers, (b) an omnibus indicator for receiving any waiver at all, and (c) an indicator for the number of waiver types. None of these changes led to appreciable changes in the results. We also considered alternative ways to set the dates on the policy changes. In one analysis, we lagged the adoption dates by 9 months (12 months for the teenage coresidence since that waiver is measured yearly) to allow for a delayed response of marriage and childbirth. In the other, we used the implementation date of the waiver when available. It is not obvious whether the adoption or implementation date is most meaningful. Women's behavior could be affected by the announcement of policy changes (i.e. the adoption date or even publicity prior to the adoption date), or could be more affected by actual experience with the waivers (implementation dates or lagged dates). As a practical matter, our results were not very sensitive to the way that we dated the waivers; we therefore just report results based on adoption dates.

We also include an indicator for TANF implementation using data from Crouse (1999). Since TANF implementation varied by state, albeit within a 14-month time window, its impact can potentially be identified even when including state and year fixed effects.

Besides the welfare policy variables, we also control for the generosity of the Earned Income Tax Credit (EITC). The EITC is a wage subsidy for low-income earners. As earnings rise, the EITC has a phase-in range where the subsidy initially increases, a flat range where the subsidy is at its maximum, and a phase-out range where the subsidy is finally reduced. Following Dickert-Conlin and Houser (1999) we used the maximum EITC benefit available to a

family with two children.¹¹ In addition to the federal EITC, ten states have adopted EITCs into their own tax schedules, which leads to both cross-section and time-series variation in the maximum benefits.

Local Labor and Marriage Markets

For its descriptors of local labor market conditions, the study extends work by Ribar (2003). In his analysis, Ribar constructed indirect annual measures for all counties from 1989-1997 by combining skill-specific information on earnings and employment from the Sample Edited Detail File (SEDF) of the 1990 Decennial Census and the 1990-1998 Annual Demographic files of the CPS with annual industry-specific information from the Regional Economic Information System (REIS). He used special versions of the SEDF and CPS files that identified counties of residence and work. Ribar regressed the low-skill employment and wage data from the SEDF and CPS files on a set of personal variables from those files and local employment and earnings measures from the REIS. The wage regressions accounted for county-specific effects and general time effects and were corrected for selectivity from the employment decision. For the present study, coefficients from the regressions were combined with the employment and earnings data from the REIS to impute wages for low-skill women workers across counties for the period 1989-2000. Earnings were adjusted for inflation using the CPI-U.

The analyses also include a measure of spouse availability. To describe marriage market conditions, we constructed race-specific sex (male/female) ratios for people aged 15-39 by county of residence for each year from 1989-2000 using decennial census and annual county

¹¹ In a model of welfare transitions, Grogger (2003) finds that using the maximum EITC benefits and using the EITC credit rate produce similar results.

population estimates data. Counties with ratios that exceeded 5 or were less than .2 were trimmed to those values.

In addition to the time varying covariates mentioned above, the study's multivariate models also include a set of calendar time dummy variables to control for common national economic and policy effects and a set of panel dummy variables to control for sample design effects. Table 1 shows the means of the variables used in the headship level and event history analyses. The sample sizes are shown in person waves.

RESULTS

Trends in Female Headship

We first examine data from the SIPP to determine whether there were different trends in headship between states that did and did not adopt pre-TANF welfare waivers. The general trends in the SIPP data, which indicate that headship rose through the mid-1990s and then gradually declined thereafter, are consistent with those reported from other sources. Three-quarters of the states adopted major waivers by 1996. The states that eventually adopted waivers had a higher level of headship in 1990 (12.3 percent) than those that did not (11.7 percent). These initial differences in headship levels illustrate why it is important to account carefully for state attributes.

In adopting states, female headship rose less rapidly through 1995 and fell less sharply afterwards (rose to 13.1 percent in 1995 and fell to 12.4 percent in 2000) than in non-adopting states (13.3 percent in 1995 and 11.8 percent in 2000). These trends can be read in two ways. On the one hand, evidence that the levels of headship increased by small and identical percentages in adopting and non-adopting states over the ten-year period 1990-2000 suggests

that waivers had no discernable impact on headship. On the other hand, the differences in the trends for the two five-year subperiods are consistent with first waivers and then TANF slowing the growth in female headship as policymakers intended. A less equivocal result requires that we consider other things that may have contributed to these trends.

Levels of Female Headship

We begin our multivariate analysis by estimating logit models of headship levels. Estimation results are reported in Table 2. Unlike the simple trends, the models condition on other variables including demographic characteristics of individuals, local labor and marriage market conditions, and policy parameters. They also include time-varying waiver indicators that turn on when the state adopts a waiver provision. The table lists results for two types of waiver specifications: the first groups waivers into the family/term and work categories, while the second uses the seven separate component waivers. In addition, each of the specifications includes indicators for TANF implementation and interactions of the TANF and waiver variables. The interactions allow the waivers to have different impacts pre- and post-TANF (initial tests indicated that the interactions were jointly significant in some specifications). Besides the variables listed in the table, all of the specifications control for a general set of calendar year effects, panel-specific effects, and age effects. To account for unobserved attributes of the states, we estimate specifications that include state fixed effects. Likelihood ratio tests confirm the joint significance of these controls. For purposes of comparison, results from models without state controls are reported in the first two columns of Table 2. The discussion in this section will focus, however, on the results from the last two columns that come from models that include state controls.

The coefficients for the demographic characteristics are statistically significant and

similar across all four specifications. They indicate that younger women, black women, less-educated women, and rural women are more likely than other women to be female heads, findings that accord with expectations and previous studies. The coefficients for the two measures of local economic opportunities are also significant in each of the specifications. The imputed wage rate appears in all of the specifications with a positive coefficient, while the imputed employment probability appears with a negative coefficient. The opposing signs on these coefficients are hard to reconcile. Taken alone, the positive association between wages and headship would be consistent with an income effect for fertility or an independence effect for marriage dominating opportunity cost and other effects. However, the negative coefficient on the employment measure seems to contradict this. One possible explanation is that wage rates primarily affect marriage (through the independence effect) while employment rates primarily affect fertility (through the time constraint). An alternative explanation is that greater employment opportunities increase women's exposure to marriageable men.

The EITC benefit is significantly negatively related to headship in the models that omit state effects, but the association becomes weaker and insignificant in the models with state effects. Though negative, the insignificant coefficients differ from the strong findings reported by Dickert-Conlin and Houser (1999), who also examined headship data from the SIPP. The race-specific county sex ratio is also estimated to be negative but insignificant. This contrasts with stronger findings reported by Lichter et al. (1997, 2002) and others.

Among the welfare policy variables, the maximum benefit level has the expected positive association with female headship; however, the coefficients are only significant in the models without state fixed effects. In models with state effects, the coefficients increase in magnitude but their precision falls off. The loss of significance is consistent with the findings of several

other headship studies, such as Dickert-Conlin and Houser (1999), Hoynes (1997a) and Moffitt (1994), that have included individual or area fixed effects. However, unlike some of those studies, the loss of precision in our fixed effects estimate does not allow us to reject the initial positive coefficient.

The simple and interacted indicators for waiver and TANF policies are also largely insignificant when state fixed effects are included in the models. All of the indicators are insignificant in the model with grouped waiver variables in the third column of Table 2. And only two of the indicators—TANF interacted with an AFDC-UP change and TANF interacted with a work requirement—are significant in the model with individual component waivers in the fourth column. The results from the final column suggest that states that made it easier to qualify for AFDC-UP experienced a reduction in female headship after TANF was put into place. At the same time, the results also unexpectedly suggest that more stringent work requirements contributed to higher rates of female headship after TANF was implemented. However, the lack of confirmation from either the grouped specification or the un-interacted variables leads us to view these results cautiously.

A potential shortcoming of our analysis sample is that it includes women who either are not eligible for welfare or unlikely to consider participating. As a specification check we re-estimated our models using a restricted sample of women with less than 12 years of education. The impact of waivers and other welfare policy variables might be stronger for this group because they are more likely to need or consider receiving welfare. With this restricted sample, we find that the coefficient on the indicator for implementing any type of work waiver is significantly positive in a model that accounts for state fixed effects (specification corresponds to the third column of Table 2). However, when we re-estimate the model with the separate component waivers (specification corresponds to the fourth column of Table 2), none of the

component waivers that we categorize as being work-related is individually significant. The only policy variable that is significant in this specification is the un-interacted indicator for relaxing the AFDC-UP requirements, which appears with a negative coefficient. Thus, the significant coefficients from Table 2 do not appear to be robust. It also does not appear that less-educated women are more sensitive to welfare policy changes than other women.

In additional sensitivity analyses, we tried other specifications of the waiver variable including a single indicator for whether any waiver had been adopted, a count of the number of waivers adopted, and respecified indicators for waivers and TANF that lagged the adoption date by 9-12 months. As with the results in Table 2, the coefficients on these alternative measures were generally insignificant. We also estimated models with different types of political variables to address concerns regarding policy endogeneity. We experimented with indicators for whether the state had a majority Republican house, a majority Republican senate, or a Republican governor. The political variables were not significant, and their inclusion did not qualitatively change the other coefficients.

Our conclusions from the logit analyses of headship levels are that local wage and employment conditions are important determinants of female headship but that waivers have little effect in models that control for demographic and other contextual conditions. One potential explanation for these results is that waivers only gradually affect the “stock” or level of female headship. More immediate impacts might appear in the flows into or out of headship. The analysis now turns to examine this possibility.

Exit Rates from Female Headship

A woman exits female headship in one of two ways: an unmarried woman with children marries or all children of an unmarried mother age beyond 17 or move out. In this section, we

group these routes together and look at all exits from headship. In the sequel, we discuss the separate routes out of headship.

To examine the determinants of leaving headship, we estimate proportional hazard models using the Cox partial likelihood approach. The Cox approach does not specify a parametric form for the underlying hazard but rather treats it as a nuisance function that is eliminated from the likelihood. The covariates in these models serve to shift the underlying hazards up or down.¹² We estimate the models allowing for state-specific underlying hazards. This is a generalization of state fixed effects called state stratified partial likelihood estimation. While the stratified partial likelihood method has been known for some time, it has been rarely used.¹³

The estimation method allows the form of duration dependence to vary freely across states but constrains the proportional effects of the observed covariates to be the same across states. The method requires at least two spells in each location with at least one completed spell. States with no complete spells are ignored (that is, they cancel out of the likelihood). As it turns out, we drop data from only a few states because they lack complete spells. There is a possible

¹² The underlying hazard can be recovered as a step function with one step for each completed spell length in the sample (with a separate underlying hazard for each state in the stratified version).

¹³ Chamberlain (1985) and Kalbfleish and Prentice (1980) suggest the potential for a partial likelihood approach to eliminate fixed effects. Ridder and Tunali (1999) used this approach to control for family-specific effects in an analysis of child mortality, while Fitzgerald (2003) used it to control for location-specific effects in an analysis of welfare durations.

selection bias in that we tend to be ignoring women from states with smaller samples, but the impact on total sample size is minor. Note that these women would be dropped if we used traditional fixed effects as well. The likelihood for the stratified model is developed in the Appendix.

Table 3 lists coefficients from different specifications of the multivariate exit hazard. As before, all of the specifications include controls for demographic characteristics, local labor and marriage market conditions, year effects, and policy parameters. All of the models use state stratified hazards. Again we show four models: 1 and 3 with the aggregate term/family and work-type waivers indicators, 2 and 4 using component waivers. The first two columns show results without the TANF interactions and the second two columns include the interactions. The coefficients in the tables are exponentiated so that a value less than one is a reduction in the hazard and a value greater than one is an increase in the hazard. Exponentiated coefficients help to interpret the size of the effects. For example, the coefficient on being black in the first model of .629 indicates that the underlying hazard for black women is only about two-thirds as high as that for other women.

Among demographic variables, being black has a significant effect to reduce exits from headship in all models as expected. Being older increases exits. AFDC benefits and EITC do not have a significant effects. The local employment conditions variables do not have significant effects.¹⁴

¹⁴ As a check, the model was run adding county unemployment rates as well. The unemployment rate did not have a statistically significant effect and the wage rate and probability of employment remain insignificant. The county unemployment rates had more

Neither the grouped waivers nor the component waivers are significant in the models without interactions. A likelihood ratio test fails to reject that the grouped waivers impacts are the same pre- and post TANF or that the component waiver impact are jointly the same pre- and post (at a five percent level). Among component waivers, the family cap waiver has a significant negative effect on exits prior to TANF but no net effect after TANF. (Note that post-TANF the net impact would be found by multiplying the pre- and post-TANF (interacted) exponentiated coefficients rather than adding them.) The family cap reduces welfare generosity so it is unclear why that would reduce exits. The work time limit has a significant negative impact on exits after TANF adoption, again an odd result. Both results raise suspicions about robustness.

In a model (not shown) for women with less than 12 years of education, results are somewhat different. When we allow different effects post-TANF, we find that the grouped family/termination waivers have a significant, positive impact on exits after TANF, but little before. When we look at the components to see the source of this result, we see it is due primarily to the teenage coresidence waiver. This waiver appears to have a significant negative effect on exits from welfare prior to TANF, but no impact after TANF. Further, the teenage coresidence waiver might be thought to reduce exits by marriage, but later work showing exits by marriage shows no impact of the teenage coresidence waiver. Thus the low education exit model produces at best mixed evidence that welfare reform was effective at encouraging exits from headship.

The mixed results and the large size of some of the waiver coefficients make us

missing values because the county codes matched less well than for wages and probability of employment.

somewhat skeptical that the results can be taken at face value. We note as well that we may have a small sample problem: some of the waiver indicators will have a relatively small number of exits in each state after the waiver takes effect.

Entry Rates into Female Headship

A woman enters female headship in one of two ways: an unmarried women has a pre-marital birth (or children move in) or a married women with children becomes divorced. We first estimate a headship entry hazard models which combine both of these paths, and later briefly report on models that distinguish the paths. All models are state stratified hazards. The estimation results are shown in Table 4.

The demographic variables produce expected results: being black increases entry into headship, whereas being older or more educated reduces entry. In these state stratified models, the local labor market variables have insignificant effects on entry.¹⁵ These labor market variables are significant in the unstratified model (not shown) but within the stratified model there is apparently not enough variation to get precise estimates. We find a counterintuitive positive effect of the male/female ratio on entry rates. In these models, likelihood ratio tests fail to reject that waivers have the same impact pre- and post-TANF (at the 5 percent level); that is, the TANF interactions are not jointly significant. As for waivers, we find that the teenage coresidence waiver increases entry prior to TANF, but has no separate impact after the adoption of TANF.¹⁶ Recall that the teenage coresidence waiver decreased exits in the earlier model, so it

¹⁵ This result is not changed by adding an unemployment variable (not shown).

¹⁶ Horvath and Peters (2001) also observe a positive sign on teenage coresidence in a related context where they estimate the proportion of out-of-wedlock births by unmarried women

appears to have increased headship in the pre-TANF years.

In the models for the low education population, the overall results for entry hazards are similar to those above. None of the waiver aggregates or component waivers has significant effects on entry, even when split out by pre- or post-TANF. To further test sensitivity, we added political variables and found that the results were robust. Alternative methods of dating the waivers (use of implementation rather than adoption dates or use of lags) made little difference, although it changes the specific waivers that become marginally significant.

Separate paths for exit and entry

Exits by Marriage. Because the transitions out of female headship could be due to different mechanisms, marriage or move out/aging of a child, we estimated a hazard for exit by marriage, treating other types of exits as censoring.¹⁷ This is essentially a competing risk model. We used the all education group sample described above. We had hoped that this might help us see the impact of teenage coresidence, the only significant waiver in the earlier exit hazard. But in the marriage hazard teenage coresidence is no longer statistically significant. Instead, the family cap is estimated to have a negative effect on exits prior to TANF but no net impact following the adoption of TANF. Thus again we have mixed results.

using state panel data. They note that the result is counterintuitive because the presumption is that the waiver would discourage pregnancy by teens that become pregnant in order to become independent or avoid a bad home situation. As mentioned earlier, they argue that the odd sign might be explained because the waiver might add some security for a teenage mother who knows she will either be living at home or in a group situation.

¹⁷ Results were made available to referees and are available from the authors.

Entry by Unmarried, Childless Women. Transitions into female headship also involve distinct mechanisms: births by unmarried women, and loss of husbands by married women with children. To try to better sort out our results, we split the entry sample into two mutually exclusive groups: unmarried women who were childless and women who were ever married during their spell of non-headship. The former group entered headship by giving birth, the latter by losing a husband.

In the sample of unmarried, childless women we find two significant component waivers in the entry hazard (births). The work time limit waiver encourages entry prior to TANF, but has a significant negative net effect post-TANF. The post-TANF coefficient could reflect a longer adjustment period and suggests that the work time limit reduced fertility by making welfare less desirable. We also found that relaxing the earnings disregard (allowing recipients to keep more thus making welfare more desirable) had a significant negative effect on births prior to TANF. Since these women were not already on welfare before the birth, this result seems odd and points to the mixed nature of the results.

Entry by Ever-Married Women. In the sample of ever-married women who become heads through marital disruption, the coefficient on the teenage coresidence waiver is significantly, positive. This result seems spurious since few women in this sample were teenagers. Higher EITC benefits do appear to reduce entry into headship among ever-married women; these benefits may encourage families to stay together.

Overall, there were few significant results among the welfare policy variables in the analyses that split out some of the paths into and out of headship. In particular, the estimated impacts of waiver and TANF policies remained weak and inconsistent.

CONCLUSION

We began by noting that states that adopted waivers did not see as large a rise in female headship as states that did not adopt waivers. This suggested that waivers may have reduced female headship. Yet in a multivariate model of levels of headship, only the grouped work-type waiver indicator was significant, but positive (increasing headship). The mechanism by which work-type waivers increase headship is not clear: waivers that make welfare less attractive would be expected to reduce headship. In transition models for exit from and entry into female headship based on new spells, we sometimes find waivers have an impact. For example, termination time limits and work time limits appear to increase exits, but the estimates are not robust to specification or sample changes.

Two potential problems could produce weak effects among the component waivers. One is simply the colinearity among the component waivers. States adopt bundles of waivers, which limits the variation in adoption dates among the components. Yet, we think the proper way to test for waiver effects is to group waivers or to include all component waivers simultaneously. If component waivers are tested one by one, a researcher could never be sure if the estimated impact was due to that component or some other waiver that was commonly bundled with it. Second, our sample might not include enough time after the adoption of waivers. Especially in models with fixed state effects or stratified by state, we put great demands on the time series variation within each state to sort out effects. As mentioned earlier, it may take more time for people to respond to waiver provisions.

In short, our intent was to adequately control for many confounding influences such as local area labor markets, marriage markets, and unobserved state and time effects, and let the data show us the impact of welfare waivers. We believe that we used appropriate data and good

controls, and that we would have picked up waiver effects if they were strong and robust. Had we used fewer controls, we would not be sure that we were isolating the waiver effects. In the end, the paper produces little robust evidence that waivers were effective at reducing female headship of families.

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Appendix: Development of the Likelihood Function for Partial Likelihood Estimation of the Cox Proportional Hazard Model

Let t_i denote the (uncensored) spell length by the i th woman. Define the hazard for this woman in location j as

$$(1) \quad \Psi_j(t_i | X_{ij}(t_i)) = h(X_{ij}(t_i)) \lambda_j(t_i)$$

where

$$(2) \quad h(X_{ij}(t_i)) = \exp(B'X_{ij}(t_i)) \text{ and } X \text{ denotes the vector of potentially time varying covariates; } B \text{ is a vector of unknown coefficients; } \lambda_j(t_i) \text{ is the baseline hazard in location } j.$$

$$(3) \quad L_i(B) = \frac{\Psi_j(t_i | X_{ij}(t_i))}{\sum_{k \in \mathbb{R}_j(t_i)} \Psi_j(t_i | X_{kj}(t_i))}$$

$$(4) \quad = \frac{h(X_{ij}(t_i)) \lambda_j(t_i)}{\sum_{k \in \mathbb{R}_j(t_i)} h(X_{kj}(t_i)) \lambda_j(t_i)}$$

$$(5) \quad = \frac{\exp(B'X_{ij}(t_i))}{\sum_{k \in \mathbb{R}_j(t_i)} \exp(B'X_{kj}(t_i))}$$

Everything is measured at t_i , the spell length for person i . Note that the underlying hazard λ_j in (4) has canceled out. In essence, the risk set for a person includes only those in her location, and the estimation makes comparisons only among those who live in location j . The likelihood for the whole sample is

$$(6) \quad L = \prod_{i=1}^N L_i(B) \text{ where } L_i(B) \text{ is from (3).}$$

Table 1
Sample Means for Variables

A. For Women age 15-55, All person waves for 1990,92,93,96 SIPP panels
(Sample used in Female Headship Levels Logits, Unweighted)

Variable	Mean
Proportion of Female Heads	0.128
Education: Highest Grade Completed	12.8
Person Lives in an MSA	.81
Age	34.3
Black	.13
AFDC benefits for family of 3 (\$1997)	365
Maximum EITC for family with 2 children	2391
Predicted Local Log Real Wage	1.59
Predicted Local Probability of Employment	0.72
Proportion of States that Ever Adopted the Indicated Waiver through 1992-1996 (Not from SIPP data)	
Any major waiver	0.75
Term Limits	0.28
Work Requirement time limit	0.23
Family Cap	0.49
Jobs sanctions	0.47
Enhanced earnings disregard	0.44
Teenage Mother Coresidence Required	0.49
Sample size (person waves)	654327

B. For Women Age 15-55, Means averaged over spells (Unweighted)

	Spells of Non-headship (for entry rate hazard)	Spells of Headship (for exit rate hazards)
Age	21.3	29.9
Black	0.16	.22
Education: Highest Grade Completed	10.3	12.1
Person Lives in MSA	.79	.82
Predicted Probability of Employment	0.51	0.71
Predicted Log Real Wage	3.55	1.52
AFDC benefits for family of 3 (\$1997)	355	351
Maximum EITC for family with 2 children	2530	2542
Sample size (person waves)	52839	13822

Table 2
Logit Regressions for Female Headship

	No State Effects		With State Effects	
	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	0.008 (0.22)		0.022 (0.55)	
Work Waiver	-0.008 (0.22)		0.011 (0.28)	
Tanf	0.110* (2.04)	0.101* (2.48)	0.067 (1.28)	0.057 (1.53)
Tanf * Term/Family	0.043 (0.69)		-0.029 (0.46)	
Tanf* Work Waiver	-0.115* (2.32)		-0.032 (0.66)	
Term. Time Limit		0.024 (0.52)		0.073 (1.37)
Family Cap		-0.055 (1.60)		-0.018 (0.47)
Teenage Coresidence		.031 (0.97)		.034 (0.84)
Relax AFDC UP		0.089* (2.32)		-0.003 (0.07)
Work Req. Time Limit		0.036 (0.70)		0.014 (0.25)
JOBS Sanction		0.031 (0.97)		0.034 (0.84)
Earnings Disregard		-0.090** (2.66)		-0.028 (0.71)
Tanf *Term Limit		0.021 (0.37)		0.067 (1.23)
Tanf *Family Cap		-0.032 (0.75)		-0.038 (0.92)
Tanf *Teenage Cores.		0.019 (0.49)		-0.007 (0.18)
Tanf *Relax AFDC-UP		-0.075 (1.27)		-0.141* (2.39)
Tanf *Work Req.		0.093 (1.23)		0.213** (2.77)
Tanf *Earning Dis.		-0.012 (0.24)		-0.055 (1.09)

Table 2 (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Tanf *JOBS Sanctions		-0.089+ (1.84)		0.002 (0.05)
Age	-0.037** (29.07)	-0.037** (29.17)	-0.039** (29.30)	-0.039** (29.27)
Black	1.414** (52.96)	1.421** (53.08)	1.462** (52.78)	1.462** (52.77)
MSA Residents	-0.086** (2.86)	-0.088** (2.91)	-0.085** (2.65)	-0.083* (2.57)
Education	-0.136** (24.05)	-0.136** (23.99)	-0.137** (23.41)	-0.137** (23.42)
Predicted Log Real Wage	0.605** (9.76)	0.643** (10.26)	0.864** (12.33)	0.861** (12.27)
Predicted Employment Probability	-1.144** (7.67)	-1.190** (7.94)	-1.511** (9.28)	-1.506** (9.25)
AFDC Benefits/100	0.026** (3.02)	0.021* (2.27)	0.053 (1.32)	0.068 (1.46)
EITC Maximum/ 100	-0.025** (4.25)	-0.025** (3.91)	-0.010 (0.92)	-0.006 (0.55)
Sex Ratio	-0.091 (1.09)	-0.081 (0.98)	-0.035 (0.42)	-0.033 (0.40)
Log Likelihood	-226714.26	-226615.35	-226056.69	-226022.47
Observations	654327	654327	654327	654327

Notes: Women aged 15-55 from 1990, 92, 93 panels of SIPP. All models include a set of panel dummies, calendar year dummies, and dummies for each year of age prior to 19. Robust z-statistics in parentheses (adjusted for clustering by person).

+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3
Hazard for Exit from Female Headship

	<u>With TANF Interactions</u>			
	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	1.040 (0.24)		0.988 (0.06)	
Work Related Waiver	1.049 (0.29)		1.080 (0.39)	
Tanf			0.968 (0.10)	0.770 (0.82)
Tanf *Term/Family			1.163 (0.50)	
Tanf *Work Waiver			0.925 (0.31)	
Termination Limit		1.304 (1.30)		1.489 (1.03)
Family Cap		0.972 (0.17)		0.517* (2.36)
Teenage Coresidence		1.181 (1.09)		1.228 (0.99)
Relax AFDC UP		0.886 (0.75)		0.705 (1.41)
Work Req. Time Limit		0.950 (0.25)		1.472 (1.35)
JOBS Sanctions		1.104 (0.57)		1.480 (1.59)
Earnings Disregard		1.016 (0.10)		1.046 (0.20)
Tanf *Term Limit				0.838 (0.41)
Tanf *Family Cap				2.168** (2.72)
Tanf *Teenage Cores.				0.956 (0.21)
Tanf * Relax AFDC-UP				1.473 (1.22)
Tanf *Work req.				0.467+ (1.96)
Tanf *Earnings Dis.				1.034 (0.13)

Table 3
(continued)

	Model (1)	Model (2)	Model (3)	Model (4)
Tanf *JOBS Sanctions				0.677 (1.41)
Age	1.036** (6.69)	1.036** (6.70)	1.036** (6.70)	1.036** (6.79)
Black	0.629** (4.29)	0.627** (4.31)	0.630** (4.27)	0.629** (4.28)
Education	0.988 (0.61)	0.990 (0.53)	0.988 (0.62)	0.989 (0.56)
MSA residence	0.931 (0.70)	0.931 (0.71)	0.931 (0.70)	0.939 (0.62)
Predicted Log Real Wage	0.792 (0.86)	0.793 (0.86)	0.792 (0.86)	0.797 (0.84)
Predicted Employment Probability	1.092 (0.15)	1.064 (0.10)	1.101 (0.16)	1.051 (0.08)
AFDC Benefits/100	1.253 (1.51)	1.135 (0.71)	1.252 (1.47)	1.093 (0.50)
EITC Maximum/ 100	0.983 (0.43)	0.980 (0.48)	0.981 (0.47)	0.970 (0.67)
Sex Ratio	1.285 (1.07)	1.266 (0.98)	1.287 (1.08)	1.258 (0.96)
Log Likelihood	-3253.5184	-3250.8988	-3253.3811	-3244.318
Chi-square for all coeffs=0	156.87	162.57	157.18	175.72
Observations	13822	13822	13822	13822
Persons	3774	3774	3774	3774
Exits	797	797	797	797

Notes: Women aged 15-55 from 1990,92,93 panels of SIPP. Cox proportional hazard, stratified by state. All models include a set of panel dummies, calendar year dummies, and dummies for each year of age prior to 19. Robust z-statistics in parentheses (adjusted for clustering by person).
+ significant at 10%; * significant at 5%; ** significant at 1%.

Table 4
Hazards for Entry into Female Headship

	<u>With TANF Interactions</u>			
	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	1.168 (0.84)		1.223 (0.90)	
Work Related Waiver	0.848 (0.91)		0.811 (0.91)	
Tanf			1.102 (0.29)	1.647 (1.45)
Tanf *Term/Family waiver			0.898 (0.35)	
Tanf *Work Waiver			1.095 (0.33)	
Termination Limit		1.111 (0.48)		0.839 (0.43)
Family Cap		1.196 (1.06)		1.421 (1.45)
Teenage Coresidence		1.335+ (1.75)		1.573* (2.09)
Relax AFDC UP		0.927 (0.44)		1.051 (0.18)
Work Req. Time Limit		0.994 (0.03)		1.276 (0.87)
JOBS Sanctions		0.792 (1.28)		0.889 (0.41)
Earnings Disregard		0.855 (0.93)		0.706 (1.40)
Tanf *Term Limit				1.266 (0.53)
Tanf *Family Cap				0.769 (1.03)
Tanf *Teenage Cores.				0.775 (1.11)
Tanf *Relax AFDC-UP				0.887 (0.35)
Tanf *Work Req.				0.652 (1.08)
Tanf *Earnings Dis.				1.310 (0.97)

Table 4
(continued)

	Model (1)	Model (2)	Model (3)	Model (4)
Tanf *JOBS Sanctions				0.914 (0.30)
Age	0.956** (7.64)	0.956** (7.64)	0.956** (7.63)	0.955** (7.67)
Black	1.991** (7.89)	2.004** (7.94)	1.992** (7.88)	1.999** (7.89)
Education	0.945* (2.08)	0.944* (2.10)	0.945* (2.08)	0.946* (2.05)
MSA residence	1.134 (1.14)	1.123 (1.05)	1.136 (1.16)	1.136 (1.15)
Predicted Log Real Wage	1.275 (0.82)	1.273 (0.81)	1.272 (0.81)	1.269 (0.80)
Predicted Employment Probability	0.395 (1.46)	0.392 (1.47)	0.396 (1.45)	0.383 (1.50)
AFDC Benefits/100	1.186+ (1.66)	1.163 (1.40)	1.181 (1.59)	1.138 (1.18)
EITC Maximum/ 100	0.940 (1.22)	0.935 (1.31)	0.943 (1.16)	0.935 (1.27)
Sex Ratio	1.393** (3.14)	1.383** (3.07)	1.394** (3.14)	1.393** (3.15)
Log Likelihood	-3450.5465	-3447.242	-3450.447	-3444.2302
Chi-square for all coeffs=0	783.80	796.65	786.17	811.84
Observations	52839	52839	52839	52839
Persons	10952	10952	10952	10952
Exits	734	734	734	734

Notes: Women aged 15-55 from 1990,92,93,96 panels of SIPP. Model is Cox proportional hazard estimated, stratified by state. Models include panel dummies, calendar time dummies, and dummies for each year of age prior to 19. Robust z-statistics in parentheses (adjusted for clustering by person). + significant at 10%; * significant at 5%; ** significant at 1%.

Appendix Table A
Hazard for Exit from Female Headship by Marriage
With TANF Interactions

	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	1.097 (0.41)		1.146 (0.50)	
Work Waiver	1.168 (0.70)		1.037 (0.13)	
Tanf			0.565 (1.26)	0.395* (1.98)
Tanf * Term/Family			1.031 (0.07)	
Tanf * Work Waiver			1.320 (0.78)	
Term. Time Limit		0.649 (1.48)		0.828 (0.38)
Family Cap		0.779 (1.09)		0.326** (2.87)
Teenage Coresidence		1.409 (1.56)		1.356 (0.99)
Work Req. Time Limit		1.262 (0.81)		1.556 (1.19)
JOBS Sanction		2.169** (3.24)		2.709** (3.01)
Earnings Disregard		0.945 (0.25)		1.095 (0.29)
Relax AFDC UP		1.022 (0.10)		0.867 (0.43)
Tanf * Family Cap				3.027** (2.72)
Tanf * Term Time Limit				0.806 (0.38)
Tanf * Teen Cores.				1.052 (0.16)
Tanf * Work Time Limit				0.711 (0.67)
Tanf * Earnings Disregard				0.826 (0.55)
Tanf * JOBS Sanc.				0.745 (0.76)
Tanf * Relax AFDC- UP				1.298 (0.59)
Age	0.979** (2.70)	0.978** (2.81)	0.978** (2.74)	0.978** (2.73)
Black	0.382** (5.39)	0.382** (5.38)	0.383** (5.38)	0.381** (5.40)
Education	0.981 (0.63)	0.980 (0.66)	0.981 (0.63)	0.980 (0.67)
MSA Resident	0.740* (2.21)	0.726* (2.34)	0.742* (2.19)	0.731* (2.27)
Predicted Log Real Wage	1.616 (1.23)	1.662 (1.30)	1.628 (1.25)	1.648 (1.28)

Table A (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Predicted Employment Probability	2.016	2.007	1.975	2.027
	(0.84)	(0.83)	(0.82)	(0.84)
AFDC Benefits/100	1.320	1.229	1.364	1.222
	(1.45)	(0.94)	(1.62)	(0.93)
EITC Maximum/ 100	1.040	1.001	1.040	0.987
	(0.71)	(0.01)	(0.70)	(0.20)
Sex Ratio	1.547	1.449	1.539	1.438
	(1.58)	(1.33)	(1.56)	(1.29)
Observations	13822	13822	13822	13822

Notes: Women aged 15-55 from 1990,92,93 panels of SIPP with 12 or fewer years of education. All models include a set of panel dummies, calendar year dummies, and dummies for each year of age prior to 19. Model is Cox proportional hazard, stratified by state. Robust z-statistics in parentheses (adjusted for clustering by person). + significant at 10%; * significant at 5%; ** significant at 1% *

Appendix Table B
Hazard for Entry into Female Headship by Ever Married Women
With TANF Interactions

	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	1.022 (0.09)		1.120 (0.40)	
Work Waiver	0.931 (0.31)		0.792 (0.78)	
Tanf			1.881 (1.37)	1.761 (1.17)
Tanf * Term/Family			0.835 (0.47)	
Tanf * Work Waiver			1.317 (0.79)	
Term. Time Limit		0.791 (0.85)		0.761 (0.51)
Family Cap		1.105 (0.45)		1.180 (0.46)
Teenage Coresidence		1.693* (2.41)		2.007* (2.38)
Work Req. Time Limit		0.800 (0.88)		0.628 (1.14)
JOBS Sanction		1.002 (0.01)		0.956 (0.11)
Earnings Disregard		0.912 (0.43)		1.057 (0.17)
Relax AFDC UP		0.985 (0.07)		0.749 (0.78)
Tanf * Family Cap				0.928** (0.21)
Tanf * Term Time Limit				0.917 (0.15)
Tanf * Teen Cores.				0.798 (0.75)
Tanf * Work Time Limit				1.210 (0.36)
Tanf * Earnings Disregard				0.933 (0.20)
Tanf * JOBS Sanc.				1.042 (0.10)
Tanf * Relax AFDC- UP				1.599 (1.05)
Age	0.979* (2.20)	0.979* (2.24)	0.979* (2.19)	0.979* (2.24)
Black	1.956** (5.33)	1.973** (5.46)	1.946** (5.28)	1.966** (5.41)
Education	0.910** (2.82)	0.907** (2.88)	0.911** (2.79)	0.912** (2.72)
MSA Resident	1.294+ (1.83)	1.260 (1.64)	1.308+ (1.89)	1.274+* (1.71)
Predicted Log Real Wage	0.965 (0.09)	1.022 (0.06)	0.950 (0.13)	1.045 (0.11)

Table B (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Predicted Employment Probability	1.021 (0.03)	0.962 (0.05)	1.026 (0.03)	0.849 (0.19)
AFDC Benefits/100	1.200 (1.48)	1.139 (2.03)	1.141 (0.98)	1.099 (0.71)
EITC Maximum/ 100	0.918 (1.21)	0.870* (2.03)	0.932 (0.98)	0.875+ (1.90)
Sex Ratio	1.427 (0.76)	1.442 (0.85)	1.431 (0.75)	1.459 (0.87)
Observations	9194	9194	9194	9194

Notes: Women aged 15-55 from 1990,92,93 panels of SIPP with 12 or fewer years of education. Women was ever-married during non-headship spell. All models include a set of panel dummies, calendar year dummies, and dummies for each year of age prior to 19. Model is Cox proportional hazard, stratified by state. Robust z-statistics in parentheses (adjusted for clustering by person).

+ significant at 10%; * significant at 5%; ** significant at 1% *

Appendix Table C
Hazard for Entry into Female Headship by Unmarried, Childless Women
With TANF Interactions

	Model (1)	Model (2)	Model (3)	Model (4)
Term/Family Waiver	1.351 (0.99)		1.376 (0.90)	
Work Waiver	0.829 (0.62)		0.888 (0.33)	
Tanf			0.657 (0.77)	1.836 (1.15)
Tanf * Term/Family			0.894 (0.22)	
Tanf * Work Waiver			0.926 (0.18)	
Term. Time Limit		1.254 (0.64)		0.624 (0.80)
Family Cap		1.231 (0.79)		1.692 (1.48)
Teenage Coresidence		1.136* (0.47)		1.256 (0.66)
Work Req. Time Limit		1.070 (0.19)		2.490* (2.11)
JOBS Sanction		0.803 (0.77)		1.388 (0.76)
Earnings Disregard		0.778 (0.93)		0.388* (2.24)
Relax AFDC UP		0.996 (0.01)		1.470 (0.86)
Tanf * Family Cap				0.516 (1.62)
Tanf * Term Time Limit				1.768 (0.84)
Tanf * Teen Cores.				0.867 (0.40)
Tanf * Work Time Limit				0.153** (2.80)
Tanf * Earnings Disregard				2.465+ (1.94)
Tanf * JOBS Sanc.				0.637 (0.98)
Tanf * Relax AFDC-UP				0.632 (0.81)
Age	0.917** (9.46)	0.917** (9.49)	0.916** (9.48)	0.916** (9.59)
Black	1.621** (3.49)	1.622** (3.47)	1.630** (3.51)	1.617** (3.41)
Education	1.010 (0.24)	1.008 (0.20)	1.011 (0.27)	1.016 (0.42)
MSA Resident	0.912 (0.52)	0.907 (0.55)	0.914 (0.51)	0.932 (0.40)
Predicted Log Real Wage	1.828 (1.35)	1.833 (1.35)	1.841 (1.36)	1.805 (1.30)

Table C (continued)	Model (1)	Model (2)	Model (3)	Model (4)
Predicted Employment Probability	0.215	0.219	0.211	0.211
	(1.56)	(1.54)	(1.58)	(1.58)
AFDC Benefits/100	0.918	0.896	0.961	0.822
	(0.37)	(0.47)	(0.17)	(0.89)
EITC Maximum/ 100	0.965	0.982	0.962	0.957
	(0.41)	(0.21)	(0.43)	(0.49)
Sex Ratio	1.219	1.209	1.226	1.190
	(1.13)	(1.08)	(1.15)	(1.06)
Observations	43645	43645	43645	43645

Notes: Women aged 15-55 from 1990,92,93 panels of SIPP with 12 or fewer years of education. Woman was never married and childless during non-headship spell. All models include a set of panel dummies, calendar year dummies, and dummies for each year of age prior to 19. Model is Cox proportional hazard, stratified by state. Robust z-statistics in parentheses (adjusted for clustering by person). + significant at 10%; * significant at 5%; ** significant at 1% *