Welfare Reform and Female Headship

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In the 1990s, states launched dramatic modifications in Aid to Families with Dependent Children (AFDC), the largest cash welfare program in the United States. 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 President Clinton enacted the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), a major reform bill that replaced AFDC with Temporary Aid to Needy Families (TANF). Although many of the waiver and TANF reforms focused on work as a route out of dependence on welfare and toward economic self-sufficiency, policy makers were also concerned about demographic outcomes, including living arrangements, pregnancy, marriage, and cohabitation. Indeed, the stated goals of PRWORA included promoting marriage, reducing nonmarital 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 Bush administration's plans for reauthorizing welfare (see White House 2002).

A sizable literature has examined the effects of welfare reform on participation in welfare and economic well-being. More recently, analysts have investigated the implications of reform for demographic behavior. Some of this research has been based on experimental evaluations of state demonstration projects. Although marriage and other effects were initially reported in a few of these studies (e.g., Knox, Miller, and Gennetian 2000), a recent meta-analysis by Gennetian and Knox (2003) concluded that there were no consistent effects of these demonstration projects. The use of random assignment in these experiments should have provided convincing evidence regarding the projects' 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 demonstration projects were of limited duration. Fourth, they usually involved bundles of initiatives, making it difficult to isolate the effects of particular policies. Finally, the demonstration projects were limited to specific states or communities, making it difficult to generalize outcomes beyond the local context.

Other findings-also mixed-have come from nonexperimental, observational studies. Although it is more difficult to address problems such as the endogeneity of policies with observational data, the data can be used to examine broader portions of the population under wider sets of circumstances. Bitler et al. (2004), Horvath-Rose and Peters (2001), and Schoeni and Blank (2000) analyzed aggregated data on family structure, and Bitler, Gelbach, and Hoynes (2002) analyzed individual-level repeated cross-sectional data. To our knowledge, however, no previous observational study of the demographic effects of welfare reform has used nationally representative, individual-level, longitudinal data.

In the study presented here, we examined the relationship between changes in welfare policies and female headship using data from the 1990, 1992, 1993, and 1996 panels of the Survey of Income and Program Participation (SIPP). The longitudinal data in the SIPP allowed us to analyze the incidence of female headship, as well as transitions into and out of headship. The national coverage of the surveys permitted us to consider policy changes in different states, while the rich content of the surveys allowed us to control for a host of personal and family characteristics. The principal limitation of these data is the short duration of the panels (prospective longitudinal information is available for only two to four years), which leads to left-censored and right-censored data on spells in the transition analyses.

The specific aims of this investigation were to model female headship decisions (unmarried motherhood) and to assess the impact of the provisions of welfare reform on those decisions while controlling for local economic and social contextual conditions. In particular, we examined whether the provisions initiated as state waivers-and, in some cases, incorporated into subsequent TANF programs-deterred women from becoming and remaining unmarried mothers. We explored female headship in the context of a rationalchoice model in which changes in the programs' rules affect not only the direct incentives to marry and bear children but also the indirect incentives that arise through opportunities for jobs and marriage. In addition to the use of longitudinal, individual-level data on family outcomes and personal characteristics, we developed and used annual, county-level information on skill-specific wage and employment opportunities and on marriage-market conditions. We also used 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 to 1996, and many of these waiver provisions were continued by the states under PRWORA. In our study, we considered features of the states' waiver experiments and TANF programs, which allow the policies to have different impacts before and after TANF. If the reform provisions have significant effects, they should be detectable 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 (composed of unmarried women with their own children) has risen dramatically over the past three decades, from 12% of all families with children in 1970 to 26% 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 dependence on welfare 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).1

Some analysts, such as Murray (1984), have blamed public assistance programs for the rise in single headship, arguing 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 eligibility for welfare to poor married families with an unemployed primary earner (through AFDC-UP programs) were intended to address disincentives to marry. Some elements of 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 research. The large literatures on the flow components of female headship-marriage, cohabitation, divorce, and childbearing-have been surveyed elsewhere (Acs 1995; Hoynes 1997b; Moffitt 1995, 2001). Moffitt (1998) reviewed over 60 articles on the impact of levels of welfare benefits on marriage and fertility and concluded that the effects were in the expected direction, although likely to be small in magnitude.

Schultz (1994), Moffitt (1995, 1998), and others have cautioned that evidence regarding the demographic effects of the generosity of welfare benefits is sensitive to the methods used to control for differences in the economic, social, and institutional environments of the states. Since welfare reform has taken place at a time of significant changes in the labor market, disentangling the impact of reform from changes in employment conditions requires careful measurement of these contextual characteristics. In our study, we developed improved local measures of skill-specific employment opportunities, incorporated controls for marriage-market conditions, and accounted for other unobserved differences across states to address concerns about confounding environmental influences. Research by Grossbard-Shechtman (2003); Lichter, LeClere, and McLaughlin (1991); Lichter, McLaughlin, and Ribar (1997,2002); Matthews, Ribar, and Wilhelm (1997); Ribar (1998); Schultz (1994); and Wood (1995) has shown the importance of controlling for these types of circumstances in modeling family formation.

Although numerous studies have examined the demographic effects of welfare policies, nearly all 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 the results of social experiments and demonstration projects. Groeneveld, Hannan, and Tuma (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 and employment counseling. On balance, the treatments appeared to be substantially more generous than the existing combination of AFDC and food stamps, providing more assistance both inside and outside marital unions. After three years of exposure, rates of marital dissolution among blacks and whites in the experimental program were roughly 40% higher than among those in the control group. These results suggest that the independence effect from higher overall benefits dominated the effects associated with lower disincentives. However, Cain (1986) cautioned that the findings are sensitive to alternative statistical controls and that the actual changes in incentives for marriage 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 marital dissolution among recipients who were initially married. The Minnesota findings, however, appear to be an isolated result. Reports from several other state demonstration projects showed either no effects or negative effects on marriage (see, e.g., Blank 2002: table 9.C).

Gennetian and Knox (2003) reanalyzed individual-level data 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 activities, and imposed time limits. The researchers examined the programs individually, in groups according to policies, and all together and 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, whereas coresidence requirements for teenagers were associated with more nonmarital births.

Bitler et al. (2004) analyzed state-level vital statistics on marriages and divorces. They argued that these data may be more sensitive to policy changes because they represent flows into and out of marriage. However, the data also have 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 is associated with lower rates of both marriage and divorce. Bitler et al. (2002) used 1989-2000 individual-level data from the CPS to examine living arrangements. They found that the implementation of TANF had impacts on the black and Hispanic populations, but not on all groups combined. For example, TANF reduced the fraction of black central-city children who lived with unmarried parents, but increased the fraction who were living with neither parent. Bitler et al. 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 et al. (2002) and Schoeni and Blank (2000) focused on current living arrangements, not on transitions between those arrangements. The findings of Bitler et al. (2004), 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), indicate that it is important to consider transitions.

The observational studies by Bitler et al. (2004) and Horvath-Rose and Peters (2001) that have considered transitions in family structure did so using aggregate data, and hence they 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 the observational studies on the demographic effects of welfare reform have used relatively crude state-level controls, such as rates of general unemployment and of economic growth, 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 to work out of their counties of residence. Fitzgerald (1995) and Ribar (2002) have further shown that county-level economic conditions are important determinants of participation in welfare.

CONCEPTUAL FRAMEWORK

Headship outcomes result from several different family processes, including marriage, childbearing and child rearing. A woman could become a single parent by bearing and raising a child without marrying, by bearing a child within marriage and then divorcing, or through a more complicated series of transitions. Similarly, she could avoid single parenthood by remaining childless or by marrying before she became 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.

We framed our analysis of headship in terms of an economic rational-choice model (Becker 1981). Hoynes (1997b); Peters, Plotnick, and Jeong (2003); and Gennetian and Knox (2003) conducted detailed conceptual analyses of the implications of welfare reform for marriage and childbearing using this approach. Accordingly, this section only sketches the theoretical implications. In the rational-choice framework, people compare the perceived value of each alternative outcome and choose the outcome that they think 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-choice approach involves not only current considerations but future considerations as well. Thus, a woman may delay marriage or childbearing at some present cost if she believes that these actions will improve outcomes for her at some later time.

Within this framework, we can consider how the general features of cash assistance programs directly and indirectly change the incentives for childbearing and marriage. Clear direct incentives for fertility arise because eligibility for the programs is conditioned on the presence and number of children. Mothers may be eligible (depending on their incomes and other circumstances), while nonmothers are categorically ineligible.2 Thus, AFDC and TANF have the potential to subsidize parenthood outcomes but not other outcomes. The direct incentives of welfare programs for marriage are less clear, although 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 delay marriage by subsidizing additional searches for partners and leading women to be more selective regarding whom they marry. However, the programs may create offsetting long-term incentives to marry by reducing the risk of destitution in the event of divorce or widowhood.

Indirect incentives may also arise, especially to the extent that a program's rules, eligibility requirements, 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 may increase women's exposure to marriageable men, and the reduction in the time available to care for children may reduce fertility. These changes would lead to lower rates of headship.

In addition to these general features, we can also consider the specific reforms that states have implemented through waivers and then TANF (for reviews, see Blank 2002; Harvey, Camasso, and Jagannathan 2000; U.S. Department of Health and Human Services 1997). Some of these reforms 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 reforms are expected to reduce rates of female headship. Other reforms have reduced the generosity of welfare programs, such as by lowering benefits, or have made benefits more difficult to obtain, such as by enacting time limits, setting work requirements, or restricting eligibility among teenagers. These reforms are also expected to have reduced female headship-although, as the foregoing discussion indicated, 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 followed Hoynes (1997a) and Moffitt (1994) and considered 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, V_F and Y_N denote vectors representing the private economic resources available in each type of living arrangement (e.g., through own earnings, potential contributions from a partner, unearned income). B_F and B_N represent vectors of welfare benefits and policies under each arrangement. X represents a vector of personal characteristics, parameters of taste, 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. Later in the article, we briefly describe analyses in which we relaxed this restriction and examined the associations with the component childbearing and marriage processes. More details are presented in our longer working paper (Fitzgerald and Ribar 2003).

DATA CONSTRUCTION

For our analysis, we needed 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 PRWORA on women's decisions can best be estimated by using longitudinal data from the 1990s. We used data from the SIPP, a logical data set in terms of both its content and time coverage.3 The SIPP includes detailed information on monthly living arrangements, use of governmental programs, and other personal and family characteristics. The panels vary in length from 32 to 48 months. We pooled 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 20,000-40,000 households per panel. The survey oversamples 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 gave us an appreciable number of transitions.

In our study, we conducted two types of analyses. We first looked at the incidence of female headship and then at transitions into and out of female headship. In each analysis, we examined women aged 15 to 55 and defined female heads as women who are unmarried and living with related children aged 17 or younger.4 This definition includes unmarried mothers in subfamilies. For the analysis of headship levels, the headship indicator and the covariates were taken from the fourth month of each wave (the month immediately preceding each interview).

For the transition analyses, we examined spells, which we defined as consecutive months of headship or consecutive months of nonheadship. In most cases, the starting and ending dates of the spells were defined in terms of the observed transitions into or out of female headship. In addition, we allowed nonheadship spells to begin at age 15 (i.e., assuming that women enter the risk set for becoming female heads of households at age 15). Our analysis excluded left-censored spells (spells that were ongoing at the beginning of the panel) and used only the complete and right-censored spells that began during the panel. Doing so led 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.5 We allowed the analysis sample to include multiple spells per person and adjusted standard errors in our models accordingly. To reduce the number of observations in the data set, we took the covariates in the hazard models only from the fourth month of each wave.6

Along with the data on marriage and childbearing used to construct the headship variables, the SIPP contains other data on individual characteristics. From these data, we included measures of each woman's age, race, educational attainment, and urban residence as covariates in the multivariate analyses.

To provide contextual and policy information, we matched 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 respondents' counties of residence. They also mask metropolitan-area identifiers for some respondents and group states with only a few respondents. all these steps are taken to preserve the confidentiality of the 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, which allowed us to perform the necessary matches in the data.7

Welfare Program and Policy Parameters

One important indicator of welfare policy is the generosity of benefits. We measured this indicator as the maximum AFDC benefit available to a family of three with no other income in a state. 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. We adjusted the benefit for inflation using the Consumer Price Index for all Urban Consumers (CPI-U).

The other indicators of welfare policies came 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 and time that allowed us to estimate impacts. We relied mainly on the data that were assembled by the U.S. Department of Health and Human Services and Crouse (1999) and used by the Council of Economic Advisers in its 1997 and 1999 reports. These data were compared with print sources, such as the Green Book, and electronic sources like the Urban Institute's Welfare Rules Database (WRD). The indicators included 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 conceived while the mother was 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 rales under which married couples could receive benefits under the AFDC-Unemployed Parent program (AFDC-UP).8 The source data for the waivers included the month and year that a particular provision was adopted statewide.9 The values of the indicators in our analysis are O prior to a state adopting a waiver and 1 afterward.

For most of the analysis, we grouped waivers into two indicators: a family/term reform 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 (1) indicators for all the individual waivers, (2) an omnibus indicator for receiving any waiver at all, and (3) indicators for the number of waivers of each type. 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 waiver, 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 the 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 sensitive to the way in which we dated the waivers; we therefore just report the results based on adoption dates.

We also included an indicator for the implementation of TANF using data from Grouse (1999). Because the implementation of TANF varied by state, albeit within a 14-month window, its impact may be identified even when state and year fixed effects are included.

Besides the welfare policy variables, we also controlled for the generosity of the Earned Income Tax Credit (EITC). The EITC is a wage subsidy for low-income earners. As earnings rise, it has a phase-in range when the subsidy initially increases, a flat range when the subsidy is at its maximum, and a phase-out range when the subsidy is finally reduced. Following Dickert-Conlin and Houser (1999), we used the maximum EITC benefit available to a family with two children.10 In addition to the federal EITC, 10 states have incorporated EITCs into their own tax schedules, which leads to both cross-sectional and time-series variation in the maximum benefits.

Local Labor and Marriage Markets

For the descriptors of local labor-market conditions, we extended Ribar's (2003) work. Ribar constructed indirect annual measures for all counties from 1989 to 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 information on industry-specific payrolls 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 data on low-skill employment and wages from the SEDF and CPS files on a set of personal variables from those files and local payroll measures from the REIS. The employment and wage models accounted for county-specific effects and general time effects. In addition, the wage regressions were corrected for selectivity from the employment decision. For the present study, we combined coefficients from the employment and wage models with the payroll data from the REIS to impute wages for low-skill women workers across counties for the period 1989-2000.11 Earnings were adjusted for inflation using the CPI-U.

Table 1. **Sample Means for Variables: Women Aged 15–55, All-Person Waves for 1990, 1992, 1993, and 1996 SIPP Panels (Sample used in logits of female headship levels, unweighted)**

Variable	Mean
Proportion of Female Heads	0.128
Education: Highest Grade Completed	12.80
Person Lives in an MSA	0.81
Age	34.30
Black	0.13
AFDC Benefits for a Family of Three (in 1997 dollars)	365
Maximum EITC for a Family With Two Children	2,391
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 the 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)	654,327

The analyses also included a measure of the availability of spouses. 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 to 2000 using data from the decennial census and annual estimates of county populations. Counties with ratios that exceeded 5 or were less than 0.2 were trimmed to those values.

In addition to the time-varying covariates, our multivariate models also included 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. Tables 1 and 2 present the means of the variables used in the headship level and event-history analyses, respectively. 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-199Os and 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 (hereafter "adopting states") had a higher level of headship in 1990 (12.3%) than those that did not (hereafter "nonadopting states"; 11.7%). These initial differences in headship levels illustrate why it is important to account carefully for state attributes.

Table 2. **Sample Means for Variables: Women Aged 15–55, Means Averaged Over Spells (Unweighted)**

Variable	Spells of Nonheadship (for entry-rate hazard)	Spells of Headship (for exit-rate hazards)
Age	21.3	29.9
Black	0.16	0.22
Education: Highest Grade Completed	10.3	12.1
Person Lives in an MSA	0.79	0.82
Predicted Probability of Employment	0.51	0.71
Predicted Log Real Wage	3.55	1.52
AFDC Benefits for a Family of Three (in 1997 dollars)	355	351
Maximum EITC for a Family With Two Children	2,530	2,542
Sample Size (person waves)	52,839	13,822

In adopting states, female headship rose less rapidly through 1995 and fell less sharply afterward (rose to 13.1% in 1995 and fell to 12.4% in 2000) than in nonadopting states (13.3% in 1995 and 11.8% in 2000). These trends can be interpreted in two ways. On the one hand, evidence that the levels of headship increased by small and identical percentages in adopting and nonadopting states over the 1 0-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 5-year subperiods are consistent with first waivers and then TANF slowing the growth in female headship as policy makers 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. We specify our model as:

$$prob(FemaleHead_{ist} = 1) = F(X_{it}^{D}\beta^{D} + X_{it}^{E}\beta^{E} + X_{st}^{P}\beta^{P} + \alpha_{s} + \delta_{t}),$$

where *i* indexes the person, *s* indexes the person's state of residence, and t indexes time; *F* is the cumulative logistic function; X_u^D represents personal demographic characteristics age, race, education, and whether woman resides in a metropolitan statistical area (MSA); X_{it}^E represents county-environment variables: local wage rates, employment probabilities and sex ratio; X_{st}^p: represents policy variables that vary by state over time: indicators of welfare waivers and TANF indicators and interactions, AFDC benefits, and EITC maximum; α_s are state fixed effects (when they are included); and δ_t, represents annual calendar time dummy variables and panel dummy variables.

The results of the estimations are reported in Table 3. The models include indicators of waivers that turn on when the state adopted a waiver provision. The table lists results for two types of waiver specifications: the first groups waivers into the family/term and work categories, and 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 before and after TANF (initial tests indicated that the interactions were jointly significant in some specifications). Although one could conceivably measure the provisions of different state policies after the adoption of TANF, policies became more similar after TANF, and it would be difficult to tease out separate impacts. We believe that our set of waivers and the interaction of those waivers with the TANF indicator provide adequate measures of the policy environment.

Table 3. Logit Regressions for Female Headship

	No State Effects		With State Effects	
Variable	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		0.031 (0.97)		0.034 (0.84)
Relax AFDC-UP		0.089* (2.32)		-0.003 (0.07)
Work-Requirement Time Limit		0.036 (0.70)		0.014 (0.25)
JOBS Sanction		-0.006 (0.15)		-0.032 (0.73)
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 Coresidence		0.019 (0.49)		-0.007 (0.18)
TANF × Relax AFDC-UP		-0.075 (1.27)		-0.141* (2.39)
TANF × Work Requirements		0.093 (1.23)		0.213** (2.77)
TANF × Earnings Disregard		-0.012 (0.24)		-0.055 (1.09)
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)

(continued)

(Table 3, continued)

Variable	No State Effects: Model 1	Model 2	With State Effects: Model 3	Model 4
Education	–0.136**	–0.136**	–0.137**	–0.137**
	(24.05)	(23.99)	(23.41)	(23.42)
Predicted Log Real Wage	0.605**	0.643**	0.864**	0.861**
	(9.76)	(10.26)	(12.33)	(12.27)
Predicted Probability of Employment	–1.144**	–1.190**	–1.511**	–1.506**
	(7.67)	(7.94)	(9.28)	(9.25)
AFDC Benefits/100	0.026**	0.021*	0.053	0.068
	(3.02)	(2.27)	(1.32)	(1.46)
EITC Maximum/100	–0.025**	–0.025**	–0.010	–0.006
	(4.25)	(3.91)	(0.92)	(0.55)
Sex Ratio	–0.091	–0.081	–0.035	–0.033
	(1.09)	(0.98)	(0.42)	(0.40)
Log-Likelihood	–226,714.26	–226,615.35	–226,056.69	–226,022.47
Observations	654,327	654,327	654,327	654,327

Notes: The analysis sample consists of women aged 15–55 from the 1990, 1992, 1993, and 1996 panels of the SIPP. Besides the variables listed in the table, all the models include a set of panel dummy variables, calendar-year dummy variables, and dummy variables for each year of age prior to 19. Robust z-statistics (adjusted for clustering by person) appear in parentheses.

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

The state fixed effects account for unobserved attributes of the states. Likelihood ratio tests confirmed 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 3. The discussion in this section focuses, 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 the specifications with a positive coefficient, while the imputed employment probability appears with a negative coefficient. The opposing signs on these coefficients are difficult 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 interpretation. 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. Although negative, the insignificant coefficients differ from the strong findings reported by Dickert-Conlin and Houser (1999), who also examined data on headship from the SIPP. The race-specific county sex ratio is also estimated to be negative but insignificant. This finding 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 significant only 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 studies of headship, such as Dickert-Conlin and Houser (1999), Hoynes (1997a), and Moffitt (1994), that have included individual or area fixed effects. However, unlike some of these 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 the indicators are insignificant in the model with grouped waiver variables in the third column of Table 3. And only two of the indicators-TANF interacted with a change in AFDC-UP and TANF interacted with a work requirement-are significant in the model with individual component waivers in the fourth column. The results in the final column suggest that female headship fell after TANF was implemented in states that made it easier to qualify for AFDC-UP. They also suggest that female headship rose after TANF was implemented in states with more-stringent work requirements. The lack of confirmation from either the grouped specification or the uninteracted variables leads us to view these results cautiously, however.

A potential shortcoming of our analysis sample is that it included women who either were not eligible for welfare or were unlikely to consider participating in welfare. As a specification check, we reestimated our models using a restricted sample of women with less than 12 years of education. The impact of waivers and other welfare policy variables may be stronger for this group because they are more likely to need or consider receiving welfare. With this restricted sample, we found 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 3). However, when we reestimated the model with the separate component waivers (the specification corresponds to the fourth column of Table 3), none of the component waivers that we categorized as being work-related is individually significant. The only policy variable that is significant in this specification is the uninteracted indicator for relaxing the AFDC-UP requirements, which appears with a negative coefficient. Thus, the significant coefficients in Table 3 do not appear to be robust. Furthermore, less-educated women do not seem to be more sensitive to changes in welfare policy than are 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 3, 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 of Representatives, 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 may appear in the flows into or out of headship. We now 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 older than age 17 move out. In this section, we group these routes together and look at all exits from headship. In the next section, we discuss the separate routes out of headship.

To examine the determinants of leaving headship, we estimated proportional hazard models using the Cox partial-likelihood approach. Let t_i denote the (uncensored) length of the spell by the *i*th woman. We defined the hazard for this woman who lives in state *s* as

$$\Psi_{is}\left(t_i \middle| X_{is}\left(t_i\right)\right) = h\left(X_{is}\left(t_i\right)\right)\lambda_s\left(t_i\right), \qquad (1)$$

where

$$h\left(X_{is}\left(t_i\right)\right) = \exp\left(X_{it}^{D}\beta^{D} + X_{it}^{E}\beta^{E} + X_{st}^{P}\beta^{P} + \delta_t\right) \qquad (2)$$

and X^d, X^e, and X^P denote the time-varying demographic, environment, and policy variables as before; 8, represents annual calendar time and panel dummy variables; and $\lambda_s(t_i)$ is the baseline hazard in state *s*.

The Cox approach does not specify a parametric form for the underlying hazard, but 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 estimated the models allowing for state-specific underlying hazards. This is a generalization of state fixed effects called state-stratified partial-likelihood estimation. Although the stratified partial-likelihood method has been known for some time, it has rarely been 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 turned out, we dropped data from only a few states because they lacked complete spells. There is a possible selection bias in that we tended to ignore women from states with smaller samples, but the impact on the total sample is minor. Note that these women would have been dropped if we had used traditional fixed effects as well. Table 4 lists coefficients from different specifications of the multivariate exit hazard. As before, all the specifications include controls for demographic characteristics, local labor- and marriage-market conditions, year effects, and policy parameters. all the models use state-stratified hazards. Again we show four models: 1 and 3 with the aggregate term/ family and work-type waivers indicators and 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 on the interacted variables indicate how much the impact of a waiver changed after TANF was implemented. All the coefficients in the tables are exponentiated, so that a value less than 1 indicates a reduction in the hazard, while a value greater than 1 indicates an increase. Exponentiated coefficients help to illustrate the size of the effects. For example, the coefficient on being black in the first model of 0.629 indicates that the underlying hazard for black women is only about two thirds as high as that for other women.

Table 4. **Hazard for Exit From Female Headship**

			With TANF Interactions	
Variable	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-Requirement 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 Coresidence				0.956 (0.21)
TANF × Relax AFDC-UP				1.473 (1.22)
TANF × Work Requirement				0.467† (1.96)
TANF × Earnings Disregard				1.034 (0.13)
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)

(continued)

(Table 4, continued)

Variable	Model 1	Model 2	With TANF Interactions Model 3	Model 4
MSA Residence	0.931	0.931	0.931	0.939
	(0.70)	(0.71)	(0.70)	(0.62)
Predicted Log Real Wage	0.792	0.793	0.792	0.797
	(0.86)	(0.86)	(0.86)	(0.84)
Predicted Probability of Employment	1.092	1.064	1.101	1.051
	(0.15)	(0.10)	(0.16)	(0.08)
AFDC Benefits/100	1.253	1.135	1.252	1.093
	(1.51)	(0.71)	(1.47)	(0.50)
EITC Maximum/100	0.983	0.980	0.981	0.970
	(0.43)	(0.48)	(0.47)	(0.67)
Sex Ratio	1.285	1.266	1.287	1.258
	(1.07)	(0.98)	(1.08)	(0.96)
Log-Likelihood	−3,253.5184	−3,250.8988	−3,253.3811	−3,244.318
Chi-Square for All Coefficients = 0	156.87	162.57	157.18	175.72
Observations	13,822	13,822	13,822	13,822
Persons	3,774	3,774	3,774	3,774
Exits	797	797	797	797

Notes: The analysis sample consists of women aged 15–55 from the 1990, 1992, 1993, and 1996 panels of the SIPP. Estimates are from Cox proportional hazard models with baseline hazards that are stratified by state. Besides the variables listed in the table, the models include panel dummy variables, calendar-time dummy variables, and dummy variables for each year of age prior to 19. Robust z-statistics (adjusted for clustering by person) appear in parentheses.

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

Being black is also estimated to reduce exits from headship in the other models, while being older is estimated to increase exits. AFDC benefits, EITC subsidies, and local employment conditions do not have significant effects.14

Neither the grouped waivers nor the component waivers are significant in the models without interactions. A likelihood-ratio test failed to reject the null hypothesis that the grouped waivers have the same impacts before and after TANF or that the impacts of component waivers are jointly the same before and after TANF. Among component waivers, the family-cap waiver has a significant negative effect on exits prior to TANF (it reduces exits by half) but no net effect after TANF. The family cap reduces the generosity of welfare, so it is unclear why it would reduce exits; indeed, a strategy for increasing benefits would involve leaving welfare for a short time. The work time limit is estimated to have a significant negative impact on exits after the adoption of TANF, again an odd result. Both results raise suspicions about robustness.

In a model (not shown) for women with less than 12 years of education, the results were somewhat different. When we allowed different effects post-TANF, we found that the grouped family/termination waivers have a significant, positive impact on exits after TANF, but little impact before. When we looked at the components to see the source of this result, we saw that the result 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. Furthermore, the teenage-coresidence waiver may be thought to reduce exits by marriage, but a later analysis of exits by marriage showed that the teenage-coresidence waiver has no impact. Thus, estimating the model with a sample of less-educated women produces no stronger evidence that welfare reform was effective in encouraging exits from headship.

The mixed results and the large size of some of the waiver coefficients make us some-what skeptical that the results can be taken at face value. We note as well that we may have a small sample problem because only a few transitions were observed in some states after waivers took effect. Multicollinearity among the waiver variables may be affecting the estimates as well.

Entry Rates Into Female Headship

A woman enters female headship in one of two ways: an unmarried woman has a premarital birth (or children move in) or a married woman with children becomes divorced. We first estimate headship-entry hazard models that combine both 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 5.

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 variables for the local labor market are not significantly associated with entry.15 They are significant in the unstratified model (not shown), but within the stratified model, there is apparently not enough variation to get precise estimates. We found a counterintuitive positive effect of the male/female ratio on entry rates. In these models, likelihood-ratio tests failed to reject that waivers have the same impact before and after TANF (at the 5% level); that is, the TANF interactions are not jointly significant. As for waivers, 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 appears to have increased headship in the preTANF years.

In the models for the low-education population, the overall results for entry hazards are similar to those just presented. None of the waiver aggregates or component waivers has significant effects on entry, even when split out by pre- or post-TANF. To test sensitivity further, 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 changed 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 the moving out or aging of a child-we estimated a hazard for exit by marriage, treating other types of exits as censoring. For brevity, we do not report the detailed results from this model or the others that were broken out by reasons of exit or entry; complete results are available in an appendix in our working paper (Fitzgerald and Ribar 2003). The model for exits from marriage is essentially a competing-risk model. We estimated the model using the general sample containing all educational groups. We had hoped that the model might help us to see the impact of teenage coresidence, which was 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, we again have mixed results.

Table 5. **Hazards for Entry Into Female Headship**

Variable	Model 1	Model 2	With TANF Interactions 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-Requirement 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 Coresidence				0.775 (1.11)
TANF × Relax AFDC-UP				0.887 (0.35)
TANF × Work Requirement				0.652 (1.08)
TANF × Earnings Disregard				1.310 (0.97)
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)

(continued)

(Table 5, continued)

Variable	Model 1	Model 2	With TANF Interactions Model 3	Model 4
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 Probability of Employment	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	−3,450.5465	−3,447.242	−3,450.447	−3,444.2302
Chi-Square for All Coefficients = 0	783.80	796.65	786.17	811.84
Observations	52,839	52,839	52,839	52,839
Persons	10,952	10,952	10,952	10,952
Exits	734	734	734	734

Notes: The analysis sample consists of women aged 15–55 from the 1990, 1992, 1993, and 1996 panels of the SIPP. Estimates are from Cox proportional hazard models with baseline hazards that are stratified by state. Besides the variables listed in the table, the models include panel dummy variables, calendar-time dummy variables, and dummy variables for each year of age prior to 19. Robust *z*-statistics (adjusted for clustering by person) appear in parentheses.

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

Entry by unmarried, childless women. Transitions into female headship also involve distinct mechanisms: births by unmarried women and the loss of husbands by married women with children. To try to sort out our results better, 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 nonheadship. The former group entered headship by giving birth, the latter by losing a husband.

In the sample of unmarried, childless women, we found two significant component waivers in the entry hazard (births). The work time limit 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 earnings, thus making welfare more desirable) had a significant negative effect on births prior to TANF. Because 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 evermarried 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 did states that did not adopt waivers. This finding 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 found that 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 changes in the sample.

Two potential problems could produce weak effects among the component waivers. First, there may be collinearity among the component waivers. States adopted bundles of waivers, which limits the variation in adoption dates among the components. Yet, we think that the proper way to test for the effects of waivers is to include all component waivers simultaneously or to use grouped waivers. If component waivers are tested one by one, a researcher could never be sure if the estimated impact was due to that component or to some other waiver that was commonly bundled with it. second, our sample might not have included 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 we mentioned earlier, it may take more time for people to respond to the provisions of waivers.

An additional limitation-the exclusion of left-censored spells brought about by the short durations of the SIPP panels-contributed to some statistically weak results in the transition models. While longer panels and larger sample sizes would have strengthened the hazard analyses, it is not clear that they would have overturned our findings regarding the welfare policy variables. A sensitivity analysis in which we were able to consider most left-censored spells did not lead to different results. Also, the policy variables were generally weak and insignificant in the longitudinal logit models for the incidence of headship, which used all the observations available in the SIPP panels and did not exclude left-censored spells.

In short, our intent was to control adequately for many confounding influences, such as local-area labor markets, the EITC, 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 the effects of waivers if they were strong and robust. If we had used fewer controls, we would not be sure that we were isolating the effects of waivers. In the end, we found little robust evidence that waivers were effective in reducing female headship of families.

Footnote

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Notes

1. The literature in this regard, however, is far from unanimous. Many studies of the impact of family structure on child well-being have reported insignificant associations (see, e.g., Amato's 1993 review). Some studies have reported that particular types of nonmarital arrangements, such as living with a single mother in a multigenerational household (see, e.g., Deleire and Kalil 2002), are more beneficial than is living with two parents. Studies have also found that marriage has negative associations for children whose parents fight and argue often (Amato, Loomis, and Booth 1995; Jekielek 1998; Morrison and Coiro 1999) and for children from different marriages in blended families (Hofferth and Anderson 2003).
2. An exception would be a teenager who is covered under either her parent's grant or a child-only grant.

3. 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 cohort were at least 30 years old in the mid-1990s), while the latest cohort was too young (aged 12-17 in 1997).
4. Women who reported their status as "married-spouse absent" were counted as married.
5. We also estimated models using retrospective information on marriage and fertility that was collected in the SIPP using the 1990, 1992, and 1993 panels. The retrospective information allowed us to extend each woman's headship history back to age 15. Unfortunately, the SlPP lacks retrospective information on some covariates, such as children's residence, so we had to make strong assumptions to impute these values. Hazard models that were estimated using the augmented data produced results that were similar to those reported here.
6. "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 previous interview, when the condition actually 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 reestimated our models by including a dummy variable for interview dates or by using wave-by-wave, rather than month-by-month, transitions.
7. The results in this study were screened to ensure that identifying information is not revealed and were approved for release by the U.S. Census Bureau.
8. 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 U.S. Department of Heath and Human Services 1997).
9. Data regarding teenage coresidence requirements came from the WRD; these data were measured yearly. There were some inconsistencies between the old and new versions of this database. In cases of conflict, we used the most recent information.
10. In a model of welfare transitions, Grogger (2003) found that using the maximum ElTC benefits and the EITC credit rate produce similar results.
11. Ribar's model coefficients were estimated using data through 1997; thus, the imputed employment and wage outcomes for 1998-2000 are based on out-of-sample predictions. The results that we report in this article did not change when we reestimated the models using data from the 1990, 1992, and 1993 panels of the SIPP and in-sample predictions of the economic variables.
12. The underlying hazard can be recovered as a step function with one step for each completed length of spell in the sample (with a separate underlying hazard for each state in the stratified version).
13. Chamberlain (1985) and Kalbfleisch and Prentice (1980) suggested 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, and Fitzgerald (2004) used it to control for location-specific effects in an analysis of the durations of spells on welfare.
14. As a check, our models were run adding the county unemployment rate as an additional control. In the specifications, the unemployment rate did not have a statistically significant effect, and the wage rate and probability of employment remained insignificant.
15. This result is not changed by adding an unemployment variable (not shown).
16. Horvath-Rose and Peters (2001) also observed a positive sign on teenage coresidence in a related context in which they estimated the proportion of out-of-wedlock births by unmarried women using state panel data. They noted that the result is counterintuitive because the presumption is that the waiver would discourage pregnancy by teenagers who become pregnant to become independent or to avoid a bad home situation. As we mentioned earlier, they argued that the odd sign may indicate that the waiver may add some security for a teenage mother who knows that she will be living either at home or in a group situation.

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