THE IMPACT OF WELFARE REFORM ON MARRIAGE AND DIVORCE*

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The goal of the 1996 Personal Responsibility and Work Opportunity Reconciliation Act was to end needy parents' dependence on governmental benefits, in part by promoting marriage. The prereform welfare system was widely believed to discourage marriage because it provided benefits primarily to single mothers. However, welfare reform may have actually decreased the incentives to be married by giving women greater financial independence via the program's new emphasis on work. This article uses vital statistics data on marriages and divorces during 1989–2000 to examine the role of welfare reform (state waivers and implementation of Temporary Assistance to Needy Families) and other state-level variables on flows into and out of marriage. The results indicate that welfare reform has led to fewer new divorces and fewer new marriages, although the latter result is sensitive to specification and the choice of data.

The U.S. welfare system changed dramatically during the 1990s, beginning with various state-implemented experimental programs and culminating in passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996. A primary goal of PRWORA is to "end the dependence of needy parents on government benefits by promoting" marriage as well as by encouraging job preparation and work.[1] Despite the burgeoning literature on the effect of welfare reform on welfare caseloads, women's labor-force outcomes, and children's well-being (see, e.g., recent reviews by Blank 2002; Duncan and Chase-Lansdale 2001; Grogger, Karoly, and Klerman 2002; Moffitt 2002b), few studies have examined whether welfare reform has affected transitions into and out of marriage. The effect of the welfare system on marital transitions has important policy implications; the Bush administration plans to use federal funds to promote marriage as an alternative to public assistance. Marriage is increasingly viewed as the route to exiting welfare and poverty (Horn and Sawhill 2001; Lichter, Graefe, and Brown 2003; Murray 2001).

Prior to the 1990s reforms, the welfare system was widely regarded as providing disincentives to marriage because it allocated benefits primarily to single women with children. Many studies have concluded that more-generous welfare programs are associated with higher rates of female household headship and lower rates of marriage (e.g., Grogger and Bronars 2001; Hoynes 1997; Lichter, McLaughlin, and Ribar 2002; Schultz

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1. The full text of PRWORA can be found by searching on "H.R. 3734" in the 104th Congress at http://thomas.loc.gov/home/c104query.html. Other stated goals of PRWORA include reducing the incidence of nonmarital pregnancies and encouraging the formation and maintenance of two-parent families.

1994; and the references therein).[2] Welfare reduces transitions from cohabitation into marriage (Manning and Smock 1995). It is also positively associated with divorce, although empirical findings are weaker than for other measures of family structure, such as female headship (e.g., Ellwood and Bane 1985; Hoffman and Duncan 1995). Overall, the estimated effects of welfare are relatively small in magnitude and cannot explain the secular decline in U.S. marriage rates and rise in divorce rates since the 1960s, during which average real welfare benefits declined (Moffitt 2001).

A central goal of welfare reform is to increase self-sufficiency though increases in employment and decreases in welfare participation. Temporary Assistance for Needy Families (TANF) gives states greater flexibility in determining eligibility rules and benefit levels and in imposing time limits, financial sanctions, and work requirements (along with enhanced earnings disregards).

Welfare reform also seeks to encourage marriage and the formation of two-parent families. Theory does not provide clear predictions of the expected impact of welfare policies on marriage. One reason is that few policy changes directly target marriage. Time limits and other restrictions on welfare benefits, such as work requirements and financial sanctions, could lead to *more* marriage by making the receipt of welfare a less attractive and less viable option for women (Moffitt 2002a). However, there may be *less* marriage if the increased emphasis on work leads to greater financial independence for women while reducing the need or desire to be married. This fundamental ambiguity of the impact of reform on marriage is recognized in the research community (e.g., Moffitt 2002b; Grogger et al. 2002) but is not widely acknowledged in the policy debates.[3]

In this study, we examined the effect of welfare reform on marriages and divorces using flow data from vital statistics for 1989 through 2000. Vital statistics data offer several advantages over the CPS data. They are a near-complete universe of marriages and divorces, and the data measure flows into and out of marriage instead of stocks of the number of people in various marital-status categories. Marital behavior and changes in policies and the labor market are more closely linked in flow data than in stock data, as Lichter et al. (2002) noted, and specification bias is minimized (Klerman and Haider forthcoming).

We also examined the effects of federal welfare reform after PRWORA and state waivers prior to PRWORA. The results suggest that welfare reform is associated with significantly reduced flows into both marriage and divorce. In the case of marriage, the robustness and statistical significance of this result depends on the specification and data source used. However, we found no evidence of increased flows into marriage: our estimated coefficients for welfare reform variables are sometimes statistically insignificant, but they are never positive.

EXPECTED EFFECTS OF REFORM: THEORY AND EVIDENCE

Marriage Model

Economic models of marriage and divorce posit that individuals marry when the benefits less costs (net utility) of being married are higher than the net benefits of remaining single, and analogously for divorce (Becker 1973). In a typical utility-maximizing model

2. However, the estimated effects of welfare tend to be sensitive to the inclusion of state and individual fixed effects, which generally result in lower levels of significance (Hoynes 1997; Moffitt 1994). Earlier studies tended to find no significant effects on marriage and fertility, whereas more recent studies have tended to find significant effects (Moffitt 1998).

3. In a related literature on the effect of welfare reform on female headship of households, Fitzgerald and Ribar (2004) found weak evidence in the Survey of Income and Program Participation that pre-1996 waivers reduced female headship. Schoeni and Blank (2000) similarly found that waivers are negatively associated with female headship among high school dropouts in data from the Current Population Survey (CPS).

of marriage, an individual's utility from being single depends on the individual's earned income if single, other income, and individual characteristics, such as education and race. An individual's utility from being married depends on the individual's earned income if married, the spouse's income, other income, and individual characteristics. An individual then chooses the utility-maximizing state: marriage or remaining single. More complex models add cohabitation as another category (e.g., Brien, Lillard, and Waite 1999; Clarkberg, Stolzenberg, and Waite 1995). This simple framework can incorporate most theories that explain differences in marriage rates across time or among individuals (Ellwood and Jencks 2001).[4]

Unfortunately, utility-maximizing models yield ambiguous predictions of the effect of an increase in a woman's own earnings on marriage and divorce. Higher earnings raise utility in *both* the married state and the single state. Depending on tastes for marriage versus nonmarriage and on the income-sharing rule among spouses, an increase in a woman's own income may make being single more attractive and discourage marriage. This is known as the "independence effect" (Becker 1973; Becker, Landes, and Michael 1977). However, increased income can also have a "stabilizing effect" on unions, thereby encouraging marriage and discouraging divorce. In addition, men may prefer to marry economically independent women (Goldscheider and Waite 1986; Oppenheimer 1997). The net effect of higher own income is therefore ambiguous (Fitzgerald and Ribar 2004). At the same time, increases in women's earnings opportunities that are due to strong labor-market conditions, the Earned Income Tax Credit, and welfare reform have ambiguous theoretical impacts on marriage and divorce rates.

An increase in a potential spouse's income, in contrast, clearly increases the utility of being married relative to being single. Policies that enhance men's earnings opportunities may therefore promote marriage and decrease divorce.

The empirical literature has typically found that better labor-market opportunities for women are negatively associated with marriage rates—suggesting that the independence effect dominates for unmarried women—while better labor-market opportunities for men are positively associated with marriage rates (e.g., Blau, Khan, and Waldfogel 2000; Brien 1997; Wood 1995). Divorce rates have the expected negative association with men's labor-market opportunities, such as the unemployment rate and average earnings. The relationship between women's labor-market opportunities and divorce is more mixed, with some studies finding a negative impact of women's labor-market opportunities and others finding a positive impact (see the review by Ellwood and Jencks 2001 and studies by Hoffman and Duncan 1995; Lichter et al. 2002; Ressler and Waters 2000; Tzeng and Mare 1995). The mixed findings for women suggest that the importance of the independence effect versus the stabilization effect of women's own earnings on divorce is not clearly established in the literature.

The utility-maximizing model makes clear predictions about the impact of welfare on marriage and divorce prior to welfare reform. TANF's predecessor, Aid to Families with Dependent Children (AFDC), provided benefits primarily to single women with dependent children. An increase in welfare benefits raises utility in the unmarried state relative to the married state, leading to predictions that welfare leads to reductions in marriage and increases in divorce. The empirical literature generally supports this prediction, but the results are less strong in models that include state fixed effects. With levels of welfare

4. Ellwood and Jencks (2001) presented eight major hypotheses that have been explored in the literature to explain the secular decline in marriage (and accompanying increase in female headship): declines in male labor-market performance, increases in female labor-market opportunities, expansion in public aid, changes in marriage markets, new contraceptive technologies, legal changes regarding abortion and divorce, changes in women's sense of control and efficacy, and changes in attitudes (on sex, cohabitation, nonmarital fertility, marriage, and divorce).

benefits held constant, the presence of an AFDC-Unemployed Parent program (AFDC-UP), which made two-parent families eligible for benefits, should have led to increases in marriage. However, most previous research has suggested that the presence of an AFDC-UP program did not significantly influence marriage rates (Schultz 1994; Winkler 1995).

Welfare Reform

In this article, we use the utility-maximizing model to frame our investigation of the expected impact of welfare reform on marriage and divorce. We first summarize the major policies that states implemented as part of welfare reform. Beginning in the early 1990s, many states were granted waivers to make changes to their AFDC programs. About half the states implemented some sort of welfare waiver between 1993 and 1995. On the heels of this state experimentation, PRWORA was enacted in 1996, replacing AFDC with TANF. The main features of state waiver and TANF programs include work requirements, financial sanctions, time limits, liberalized earnings disregards (lower tax rates on earned income while on welfare to promote work), increased limits on assets, and expanded eligibility for two-parent families.

The expansion of welfare for two-parent families through the program is the only aspect of welfare reform that directly affects the incentive to marry. Theoretically, this feature should lead to increases in new marriages and decreases in new divorces. However, the actual impact on marriage may be muted because women who marry are eligible for welfare only if their husbands' earnings are low enough to keep the total family income lower than the welfare income limit (Moffitt 2002b). Such low-earning husbands may not be attractive to most women, so extending benefits to more married-couple families may give single women little incentive to marry. For currently married women, in contrast, extending welfare benefits to more married two-parent families discourages divorce by removing the incentive to become single to qualify for welfare benefits.

The other policy changes adopted as part of welfare reform affect marriage only indirectly. Most of these policies (with the exception of the enhanced earnings disregard) make welfare less generous. This fact has led many to conclude that welfare reform unconditionally promotes marriage. This conclusion would be valid if marriage were the only outside option to participating in welfare. However, even prior to welfare reform, many women exited welfare and entered the labor force; about 27% of the exits from 1968 to 1992 were due to an increase in women's hours of work, compared with about 22% that were due to women marrying or cohabiting (U.S. Department of Health and Human Services 1998). Since welfare reform, studies of those who have left welfare have shown that exits from welfare to employment are gaining importance relative to exits through marriage. For example, Loprest (2001) reported that among single parents who left TANF (between 1997 and 1999), fully 71% were employed in 1999. Consequently, to understand the impact of reform on marriage and divorce, one has to understand the impact on work incentives and earnings opportunities.

Virtually all the elements of welfare reform increase the incentive to work. Prior research has consistently shown that these greater work incentives lead to significant increases in employment among women (Blank 2002; Corcoran et al. 2000; Grogger et al. 2002; Moffitt 2002b). However, the same policies have *ambiguous* effects on marriage and divorce. As Harknett and Gennetian (2003) indicated, a welfare reform–induced increase in employment may lead to *more* marriage through increases in self-esteem and/or changing the pool of potential mates. On the other hand, an increase in employment may lead to *reductions* in marriage through increases in the cost of time. In analyzing focus groups' responses, Harknett and Gennetian also found that recipients voiced both these experiences. Furthermore, reform-induced increases in income (from increased earnings disregards, for example) could increase marriage through greater attractiveness or stability or decrease marriage via an independence effect.

In sum, the expected effect of welfare reform on marriage and divorce is unclear. In particular, the net effect of welfare reform on marriage and divorce for any given woman will depend on her preferences and attitudes, marriage markets, and economic opportunities.

The findings of the empirical literature on welfare reform and marriage have been mixed, reflecting this theoretical ambiguity. The literature includes both nonexperimental and experimental evidence, and most studies have relied on stock measures of marriage. Among the nonexperimental studies, Schoeni and Blank (2000) found a positive effect of waivers on the fraction of those who were married and insignificant impacts of TANF, whereas Bitler, Gelbach, and Hoynes (2002) reported increases in the fraction of black central-city women who were divorced, separated, or widowed but decreases in the fraction of Hispanic women who were divorced, separated, or widowed. Kaestner and Kaushal (forthcoming), in contrast, concluded that both TANF and waivers had negligible effects on the fraction of female non-college graduates who were married. Ellwood's (2000) results suggest that the fraction of low-income mothers who were married declined slightly more between 1986 and 1999 in states with the most-aggressive welfare reform policies than in states with the least-aggressive policies, but the findings were not conclusive. In a study using flow data, Rosenbaum (2000) found that waivers led to reductions in transitions into marriage.

The evidence from experimental studies has also been mixed, with few statistically significant results and findings of both positive and negative impacts of welfare reform on marriage. Grogger et al. (2002) noted that reforms that are similar to TANF have had more consistently negative (but insignificant) impacts on marriage. In addition, some of the limited evidence on flows into divorce has indicated that reform led to declines in divorce among married couples. Furthermore, Fraker et al. (2002), in an evaluation of Iowa welfare reform, found some evidence that reform reduced both marriage and divorce. In summary, the net effects of welfare reform on marriage and divorce are theoretically ambiguous and have not been resolved in the empirical literature.

EMPIRICAL METHODS

Almost all previous studies of the relationship between welfare and marriage patterns have used either individual-level data to examine the determinants that an individual is never married, single, or divorced or state-level data to examine the determinants of state-level averages of the fractions of the population that are married and divorced, pooling data across states and years. Such methods measure the prevalence of marriage. In contrast, we used a flow measure of entry into and exit from marriage; in other words, we focused on the incidence of marriage instead of on its prevalence. Welfare reform should have a more immediate effect on flows than on stocks because data on the prevalence of marriage include many long-term marriages that were at a low risk of dissolution as the welfare system changed. Changes in the incidence of marriage and divorce will eventually lead to appreciable impacts on the prevalence of marriage and divorce, but such changes in stocks are difficult to measure in the short run. In addition, when specifying a model that relates individual behavior to changes in an economic or policy environment, examining flows allowed us to relate outcomes to changes occurring during the same period.

We used a state-level approach and regressed new marriage and divorce rates on measures of welfare reform, other social assistance programs, economic and demographic factors, and other controls:

$$y_{st} = \mathbf{W_{st}}\boldsymbol{\beta} + P_{st}\delta + \mathbf{E_{st}}\boldsymbol{\phi} + \mathbf{D_{st}}\eta + \boldsymbol{\gamma_s} + \boldsymbol{\nu_t} + \varepsilon_{st} .$$

The dependent variable, y_{st}, denotes the log of the number of new marriages or divorces divided by the population at risk of marrying or divorcing in state s and year t. To capture the at-risk population, we focused on the number of new marriages per 1,000 *unmarried* women and divorces per 1,000 *married* women. Each ratio was defined for women aged 15 and older. $\mathbf{W}_{st}$ are the variables of interest, namely, indicators for welfare reform that were in effect in the state. We include other variables to control for factors that correlated with state policies. P_{st} controls for the generosity of public assistance in the state, $\mathbf{E}_{st}$ is a vector of controls for local labor-market conditions, and $\mathbf{D}_{st}$ is a vector of demographic factors. The vectors $\boldsymbol{\gamma}_s$ and $\boldsymbol{\nu}_t$ are state and year fixed effects (some specifications also include state-specific linear time trends). As we discuss later, not all these controls are included in each specification.

We focused on two measures of welfare reform. $\mathbf{W}_{st}$ measures the share of the year that the state had a major waiver in place prior to the TANF (waiver) and the share of the year that TANF was implemented in the state (TANF).[5] The coefficients for the welfare reform variables give the estimated effect of each particular welfare reform relative to the traditional AFDC program. In other words, the coefficient of the TANF variable gives the estimated average effect of TANF relative to the AFDC program without waivers, and the waiver coefficient gives the estimated average effect of waivers from AFDC relative to AFDC without waivers.[6]

Some specifications also include state and year fixed effects, and some further add state-specific linear time trends. The state fixed effects, $\boldsymbol{\gamma}_s$, control for time-invariant differences across states, and the year fixed effects, $\boldsymbol{\nu}_t$, control for changes in marriage and divorce rates in a given year that are common to all states. The time trends control for unobservable factors that change linearly over time within states and affect marriage and divorce rates. Our preferred specification includes both state and year fixed effects, and we present results with and without time trends because the trends absorb much of the variation in the dependent variables. Unobservable factors that affect flows into marriage and divorce are captured by ε_{st}, and the covariance matrix estimates are White/Huber corrected, which allows for arbitrary heteroscedasticity. All data and regressions are weighted by the population of women used in the denominator of the marriage or divorce rate.

Our focus was on the coefficients $\boldsymbol{\beta}$, which measure the impact of waivers and TANF on flows into and out of marriage. The regression coefficients measure average treatment effects of reform relative to the AFDC program prior to implementation of an AFDC waiver and/or TANF. In this model, identification comes from variation in the timing of reform across states and over time. The variation in waivers is rich, and thus the impacts are well identified in this model. Two states first implemented major waivers in 1992, and by 1997, 29 states had major waivers.

The identification of TANF, as discussed by Blank (2001) and formalized in Bitler, Gelbach, and Hoynes (2003), is much weaker. All states implemented TANF within a 16-month period (between September 1996 and January 1998). More specifically, 19 states implemented TANF in 1996, 1 implemented TANF in 1998, and the remainder implemented TANF in 1997. Thus, TANF is identified almost entirely by cross-state variation in calendar year 1997 implementation status (early versus late implementers). Data

5. We experimented with using two separate TANF variables, one for states that ever implemented an AFDC waiver and one for other states, instead of one combined TANF variable. Theoretically, implementation of TANF may have resulted in fewer changes in welfare policies during the TANF period in states that had waivers from the AFDC program than in states without waivers, or many individuals might have adjusted their marital status when waivers were implemented, either of which would lead to a smaller magnitude for the TANF coefficient for waiver states than for nonwaiver states. Alternatively, states with AFDC waivers might have implemented more extensive reforms under TANF than did states without waivers, leading to larger effects in the waiver states during the TANF period. The estimated coefficients of the two TANF variables were not significantly different in any of the specifications, however, so we report the results for the combined variable.

6. The waiver variable is set equal to the share of the year that the waiver was in effect before TANF was implemented and to zero after a state with an AFDC waiver implemented TANF. Thus, the effects can be compared directly.

after 1998 do not contribute to the TANF identification because by then, all states had implemented TANF.[7]

To interpret the TANF coefficient, imagine that some states implemented TANF on January 1, 1997, and all other states did so on January 1, 1998. In the absence of data on any covariates, our estimates would then be equivalent to using a standard difference-in-differences estimator that subtracts the change in the rate of new marriage (divorce) for late-implementing states from this change for early-implementing states. This estimate would reflect only the impact for 1997. If the treatment effect was constant across time, then this effect would also be the effect for all years. But if there was over-time heterogeneity in the treatment effect, the estimate would be entirely uninformative about treatment effects in later years, which could be either greater or less than the 1997 effect. A further disadvantage is that because TANF implementation occurred during only a 16-month period, random factors that affect state marriage (divorce) rates and that differ at a point in time may create bias in our TANF estimates. If we had several years of TANF variation—as we do with the waiver variable—we would be more confident that such random factors average out to zero. Given these identification issues, we have more confidence that the estimated coefficients of the state waiver variable represent causal effects than we do for the TANF estimates.

DATA

Our main data are from vital statistics, which we augmented with data from various sources on labor-market conditions, other policy variables, and state characteristics.

We used vital statistics data on marriages and divorces for several reasons. First, vital statistics data are more useful than are stock data from the CPS in examining flows into and out of marriage. Second, using flow data leads to a better specification of the timing of the impact of changes in policy and labor-market environment on the marriage and divorce outcomes. Klerman and Haider (forthcoming) concluded that stock models (using welfare caseloads) are misspecified and that flows are the correct measure for detecting the immediate impacts of welfare reform. Third, the CPS underreports both marriages and divorces (Goldstein 1999). The vital statistics data, in contrast, are a near-universe of new marriages and divorces. Fourth, as Thorton and Rodgers (1987) noted, survey data, such as the CPS, contain more measurement error than do vital statistics registration data because survey respondents may report inaccurate or incomplete information about household members' marital histories.

There are also disadvantages to using vital statistics data. We used state-level data, which do not allow comparisons of effects across different age, racial, and educational groups. Detail vital statistics data, which were samples of marriages and divorces that include such demographic variables, are last available for 1995, prior to the implementation of TANF. In addition, the aggregate data are by state of occurrence for marriages and divorces, not by state of residence. If the likelihood of marrying or divorcing outside the state of residence is systematically related to welfare reform, then our results would be biased. The detail data do report state of occurrence and state of residence for most women; we discuss the robustness of the results to using the available detail data later. Despite these drawbacks, the gains from using flow data that measure entries into marriage and divorce (which are the variables that should respond to reform) arguably outweigh the disadvantages.

7. In regressions that excluded data after all states had implemented TANF, the estimated effects of TANF were not qualitatively different from those shown here. Furthermore, dropping years prior to TANF also led to little change in the estimated impacts of TANF. Including the longer time period yielded more precise estimates of the other explanatory variables, thereby reducing the overall residual variation.

We used the aggregate vital statistics data for the years 1989–2000.[8] Data on the number of new marriages are available for all 50 states and the District of Columbia, a total of 612 state-year groups, but data on the number of new divorces are available only for 572 state-year groups.[9] Data on divorce were restricted to the balanced panel of states reporting these data for each year from 1989 to 2000, or a total of 552 state-year groups.[10] The aggregate flows into marriage and divorce were turned into a rate by dividing by the at-risk population: unmarried women for marriages and married women for divorces. These "at-risk" numbers were calculated by aggregating state totals from the March CPS.

In the regression model, P_{st} is the real value of cash AFDC/TANF benefits in the state. The value of cash benefits was measured by the real maximum AFDC/TANF payment to a family of four with one adult in a given state and year. (Data sources and details on this and all other variables are in the appendix.)

$\mathbf{E_{st}}$ includes four controls for local labor-market conditions. The first two, taken from the Bureau of Labor Statistics and the Census Bureau, respectively, are the overall unemployment rate and real median family income. These variables are annual averages. We also included men's and women's real median weekly wages (calculated from the March CPS for each year) to capture changes in the male and female labor markets, respectively.

$\mathbf{D_{st}}$ includes several additional variables to capture demographic and other factors that influence state-level marriage and divorce rates. The regressions include variables measuring the fraction of the population that is black and that is Hispanic to control for differences in marriage and divorce patterns across racial and ethnic groups (see, e.g., Bennett, Bloom, and Craig 1989). The regressions also control for the fraction of the state population living in metropolitan areas because urban residence tends to be negatively associated with women's marriage rates (Moffitt 1990). We also included controls for the age distribution of the female population across various age groups, for their distribution across three of four educational groups, and for the fractions of women enrolled in high school and college. These demographic variables were constructed from the March CPS. Finally, the regressions contain a dummy variable indicating whether a state has a "covenant marriage" option. We expect that covenant marriages should have little effect on total marriage rates because in these states, couples can choose the type of marriage, regular or covenant. Covenant-marriage laws may have an impact on divorce rates if couples are unable to predict how good their match is.[11]

Table 1 presents summary statistics for the main variables used in the analysis. The table shows that almost 5% of unmarried women marry each year and that 2% of married women divorce each year.[12] Not shown here, conditioning on all women (as opposed to the at-risk population) leads to smaller transition rates: 2% of all women marry each year and 1% of women divorce each year. Across the full period, waivers were in place 13% of the time and TANF was in place 32% of the time.

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8. We chose this period because it started immediately prior to the onset of the 1990–1991 recession and ended immediately prior to the onset of the 2001 recession. In addition, AFDC waivers were first implemented during the early 1990s. The results for the welfare reform variables are qualitatively similar if the time period is extended back to 1981.

9. Data on divorce are not available for California, Indiana, and Louisiana in 1991–2000; Colorado in 1994–2000; and Nevada in 1991–1993. The marriage results are not sensitive to including observations from states for which any data on divorce are available.

10. If states have different underlying trends in the absence of reform, then using an unbalanced panel of states and years could confound these trends with other effects. The divorce results are not sensitive to this restriction.

11. Whether a state allows unilateral divorce is also likely to affect marriage and (particularly) divorce rates, but we did not include this as a covariate because it did not vary over time within any state during 1989–2000.

12. The maximum value for the marriage rate of 0.62 corresponds to Nevada. Nevada is a clear outlier, with marriage rates about six times the rates of the next-highest marriage-rate state (Hawaii). As we discuss later, the results are not sensitive to dropping data from Nevada.

Table 1. Sample Means, 1989–2000

Variable	Mean	*SD*	Maximum	Minimum
Marriages per 1,000 Unmarried Women	0.048	0.036	0.620	0.015
Divorces per 1,000 Married Women	0.021	0.005	0.042	0.008
Share of Year That Major AFDC Waiver Was in Effect	0.129	0.316	1.000	0.000
Share of Year That TANF Was in Effect	0.322	0.456	1.000	0.000
Real Max. AFDC/TANF Benefits, Family of Four ($1,000)	6.034	2.358	14.088	1.710
Overall Unemployment Rate	0.056	0.015	0.114	0.022
Real Median Income, Family of Four ($1,000)	52.595	6.916	78.410	35.419
Real Median Weekly Wages Among Women ($100)	3.484	0.514	5.769	1.879
Real Median Weekly Wages Among Men ($100)	5.534	0.755	8.122	2.554
State Has Covenant Marriage Option	0.009	0.094	1.000	0.000
Share of Population Living in Metropolitan Areas	0.794	0.161	1.000	0.202
Share of Population That Is Black	0.126	0.081	0.666	0.003
Share of Population That Is Hispanic	0.101	0.103	0.421	0.005
Share of Women Aged 15–18	0.068	0.010	0.115	0.038
Share of Women Aged 19–24	0.105	0.013	0.167	0.057
Share of Women Aged 25–29	0.095	0.014	0.150	0.050
Share of Women Aged 30–39	0.208	0.018	0.287	0.134
Share of Women Aged 40–49	0.177	0.019	0.254	0.123
Share of Women Aged 50 and Older	0.347	0.032	0.439	0.171
Share of Women Aged 15–24 Enrolled in High School	0.041	0.007	0.080	0.019
Share of Women Aged 15–24 Enrolled in College	0.041	0.008	0.083	0.008
Share of Women Aged 19 and Older and High School Dropouts	0.175	0.042	0.332	0.064
Share of Women Aged 19 and Older and High School Graduates	0.342	0.052	0.497	0.220
Share of Women Aged 19 and Older and Some College	0.235	0.045	0.373	0.112
Share of Women Aged 19 and Older and College Graduates	0.180	0.037	0.375	0.090

Notes: Observations are weighted by the state-year population of women aged 15 and older except for marriages and divorces relative to unmarried and married women, which are weighted by the respective denominator. Data are at the state level for 1989–2000 except divorce data, which are only for states that reported divorce data for all years. Dollar amounts are in real 1997 dollars. The age distribution is measured among women aged 15 and older, and the age and education distributions are calculated from the March CPS. The number of state-year combinations is 612 (51 states) except for the divorce rate, which is 552 state-year combinations (46 states).

As an initial exploration of the impacts of welfare reform on marriage and divorce, Table 2 reports mean marriage and divorce rates classified by welfare reform regime. The means suggest that marriage rates were lower in state-years during the waiver period (0.041) and the TANF period (0.046) than in state-years during the prereform AFDC period (0.052). The average divorce rate was slightly lower in state-years with waivers and TANF than in state-years during the prereform period.

Table 2. Marriage and Divorce Rates, by State Welfare Reform Status, 1989–2000

Variable	AFDC	Waiver	TANF
Marriages per 1,000 Unmarried Women Aged 15 and Older	0.052	0.041	0.046
	(0.041)	(0.010)	(0.036)
	[333]	[83]	[196]
Divorces per 1,000 Married Women Aged 15 and Older	0.022	0.019	0.020
	(0.005)	(0.004)	(0.004)
	[299]	[75]	[178]

Notes: Shown are average marriages and divorces per 1,000 women aged 15 and older who were at risk of marriage or divorce, by welfare reform regime. Standard deviations are in parentheses, and the number of state-year combinations is in brackets. Column 1 is state-year combinations with no welfare reform, column 2 is state-year combinations with a major AFDC waiver, and column 3 is state-year combinations with TANF. Data are weighted by the female population aged 15 and older who were at risk.

The differences suggested by Table 2 may be due to many factors other than welfare reform. Differences in states' demographic composition, economic conditions, or other forms of state heterogeneity could underlie the differences in the means. In addition, time trends in new marriages and divorces unrelated to welfare reform could skew the interpretation of the means in Table 2, since waivers were implemented during the middle of the sample period and TANF was implemented toward the end. The booming economy during the late 1990s, which coincided with welfare reform, also could underlie much of the change suggested by the sample means. We therefore turn to multivariate analysis to examine the effect of welfare reform on marriage and divorce rates.

RESULTS

The main results for marriage and divorce are presented in Tables 3 and 4. We provide four specifications to highlight the role played by the control variables and fixed effects. In the first column, only the two welfare reform measures are included in the regressions. The second column adds the other measure of public assistance generosity and the state-level controls for economic conditions and demographics. The third column adds state and year fixed effects, and the fourth column adds state-specific linear time trends.

The results consistently show that waivers from the AFDC program and implementation of the TANF program are negatively associated with marriage rates. The waiver coefficients are statistically significant below the 10% level in every specification presented in Table 3, and all the TANF coefficients are statistically significant at the 1% level. In the specifications that include state and year fixed effects (columns 3 and 4), the results imply that waivers from the AFDC program are associated with a 5% reduction in transitions into marriage.

As was found in prior studies, controlling for state and year fixed effects has a large impact on estimated program effects. These fixed effects control for differences across states that chose to implement waivers and for the secular decline in marriage rates. Without these controls, the estimated waiver effect is biased upward; the estimated waiver coefficient drops noticeably to about 5% (negative) when the fixed effects are included. Implementation of TANF is associated with a 23% decline relative to marriage rates during the AFDC program. The results for waivers and TANF are not significantly changed when state time trends are added to the specification that includes state and year fixed effects (column 4).

The estimated magnitude of the effect of TANF on marriage rates is sizable. However, as we discussed earlier, the TANF coefficient is identified by relatively little variation over a short period and may be more sensitive to unmeasured factors than is the waiver coefficient.

Table 3. Determinants of Marriage Rates, 1989–2000

Variable	(1)	(2)	(3)	(4)
Share of Year That Major AFDC Waiver Was in Effect	–0.190**	–0.156**	–0.048†	–0.046†
	(0.032)	(0.031)	(0.028)	(0.024)
Share of Year That TANF Was in Effect	–0.137**	–0.134**	–0.233**	–0.214**
	(0.036)	(0.041)	(0.052)	(0.043)
Log of Real Maximum AFDC/TANF Benefits, Family of Four		–0.240**	0.101	–0.182
		(0.037)	(0.093)	(0.135)
Overall Unemployment Rate		–4.163**	0.727	–0.262
		(0.849)	(0.653)	(0.870)
Log of Real Median Income, Family of Four		–0.250	–0.188	–0.058
		(0.171)	(0.309)	(0.217)
Log of Women's Real Median Wages		–0.108	–0.069	–0.012
		(0.104)	(0.062)	(0.050)
Log of Men's Real Median Wages		–0.036	0.034	0.049
		(0.114)	(0.054)	(0.063)
Covenant-Marriage State		–0.022	0.018	–0.066
		(0.065)	(0.053)	(0.075)
Share of Population Living in Metropolitan Areas		0.018	–0.102	–0.493
		(0.096)	(0.453)	(0.638)
Share of Population That Is Black		–0.559**	–2.037	–3.673
		(0.162)	(2.280)	(4.006)
Share of Population That Is Hispanic		–0.054	–1.367	–1.696
		(0.162)	(0.967)	(1.770)
State and Year Fixed Effects	No	No	Yes	Yes
State-Specific Time Trends	No	No	No	Yes
Adjusted R^2	0.054	0.409	0.892	0.913
State-Year Combinations	612	612	612	612

Notes: Shown are coefficients from regressions of the determinants of marriage rates during 1989–2000 for 51 states. The dependent variable is the natural log of new marriages per 1,000 unmarried women aged 15 and older. The regressions in columns 2–4 also control for the age, education, and enrollment distribution of the female population. Robust standard errors are in parentheses. Data are weighted by the number of women who were at risk of marriage.

†$p < .10$; *$p < .05$; **$p < .01$

With regard to the control variables, the results show that increases in welfare benefits significantly reduce marriage (column 2) but have insignificant effects once state and year fixed effects are added (column 3).[13] The models without state and year effects show that increases in state unemployment rates are associated with reductions in marriage. As we discussed earlier, it is difficult to interpret coefficients on such variables, given that they may have an impact on both women's and men's incomes. We therefore included real median weekly wages separately for men and women by state and year. These results tend to show positive impacts of men's wages and negative impacts of

13. The finding of a weakening of the impact of welfare benefits with the addition of state fixed effects was common in the older AFDC literature. Few of these older studies examined transitions.

women's wages on marriage once state and year fixed effects are included, although none of the coefficients are statistically significant.

The demographic controls show that marriage rates are lower in states that have a larger black population. The controls for the age, enrollment, and education distributions within states (not shown) also exhibit the expected signs. Marriage rates are significantly higher for states with larger shares of young women and significantly lower for states with higher shares of women who are currently enrolled in high school or college. Marriage is significantly more common in states with a higher share of women aged 19 and older with some college education but no college degree.

The insignificant associations between economic conditions and most demographic factors and marriage rates in columns 3–4 may be surprising, given that previous studies suggested that marriage rates are related to macroeconomic conditions and demographic factors. However, with state and year fixed effects, the coefficients are identified only by deviations from state-level averages. There is not enough variation over time within states to identify significantly most of the economic and demographic variables when fixed effects are included in the model.

The main results for the impact of welfare reform on divorce are presented in Table 4. These results show that implementation of statewide waivers from the AFDC system leads to lower divorce rates. Waivers are associated with a 5%–6% average reduction in divorce rates when other state-level factors are controlled. When state and year fixed effects are included, the estimated impact of TANF is negative but significant only at the 10% level, and including state-specific time trend further reduces the significance level of the estimated coefficient. The point estimates, however, are robust to these alternative specifications.

The results for the economic and demographic control variables in the divorce regressions tend to be somewhat less robust than in the marriage regressions. Many lose statistical significance with the addition of state and year fixed effects. Contrary to expectations, welfare benefits are negatively associated with divorce transitions. Although not statistically significant, most of the point estimates imply that higher wages for women and men are both associated with lower divorce rates. This is suggestive evidence that the "stabilizing" impact of own income dominates the "independence" effect for married women. In contrast, the results in Table 3 for marriage suggest that the independence effect dominates for unmarried women. Column 2 of Table 4 indicates that more urban states and states with higher shares of black women have higher divorce rates.

The coefficients on the age, enrollment, and education distributions within states show that divorce is less common in states with larger shares of women enrolled in high school and in college and that divorce is more common in states with larger shares of women who are high school dropouts or have some college but no four-year degree, relative to the share with at least a college degree. Here, too, the patterns of significance are less clear than in the results for marriage.

The results also indicate that states with a covenant-marriage option have lower divorce rates than states that do not, once state-specific time trends are included. Because the result holds in regressions that include state fixed effects and state trends, it suggests that adoption of a covenant-marriage law affects divorce rates instead of merely reflecting preexisting lower propensities for divorce in states that pass such laws.

Discussion of the Main Results

Overall, the results from the aggregate vital statistics data show that waivers are associated with reductions in transitions into marriage *and* reductions in transitions from marriage to divorce. Furthermore, the TANF estimates suggest that TANF has larger impacts than do waivers, with the caution that the identification of TANF comes from substantially less variation than does the identification of waivers (Blank 2001). Finally, the negative impact

Table 4. Determinants of Divorce Rates, 1989–2000

Variable	(1)	(2)	(3)	(4)
Share of Year That Major AFDC Waiver Was in Effect	–0.118*	–0.063*	–0.055**	–0.056**
	(0.042)	(0.027)	(0.018)	(0.018)
Share of Year That TANF Was in Effect	–0.075*	0.057†	–0.126†	–0.102
	(0.030)	(0.032)	(0.065)	(0.070)
Log of Real Maximum AFDC/TANF Benefits, Family of Four		–0.095*	–0.033	–0.216†
		(0.042)	(0.113)	(0.125)
Overall Unemployment Rate		0.247	–1.461*	–1.772*
		(0.749)	(0.676)	(0.721)
Log of Real Median Income, Family of Four		–0.983**	–0.082	0.114
		(0.152)	(0.219)	(0.180)
Log of Women's Real Median Weekly Wages		–0.141†	–0.083†	–0.025
		(0.074)	(0.044)	(0.039)
Log of Men's Real Median Weekly Wages		0.089	–0.075†	–0.018
		(0.071)	(0.041)	(0.044)
Covenant-Marriage State		0.058	–0.055	–0.156*
		(0.057)	(0.041)	(0.070)
Share of Population Living in Metropolitan Areas		0.182*	–0.232	–0.184
		(0.074)	(0.279)	(0.324)
Share of Population That Is Black		0.377*	–0.845	–6.669
		(0.151)	(1.662)	(4.092)
Share of Population That Is Hispanic		–0.236	–5.163**	–4.172*
		(0.164)	(0.925)	(1.912)
State and Year Fixed Effects	No	No	Yes	Yes
State-Specific Time Trends	No	No	No	Yes
Adjusted R^2	0.026	0.618	0.892	0.923
State-Year Combinations	552	552	552	552

Notes: Shown are coefficients from regressions of the determinants of divorce rates during 1989–2000 for 46 states that reported aggregate divorce data for all years. The dependent variable is the natural log of new divorces per 1,000 married women aged 15 and older. The regressions in columns 2–4 also control for the age, education, and enrollment distribution of the female population. Robust standard errors are in parentheses. Data are weighted by the number of women who were at risk of divorce.

†$p < .10$; *$p < .05$; **$p < .01$

of TANF on divorce is estimated with substantially less precision than the negative impact of TANF on marriage.

Our results (although not always statistically significant) provide consistent evidence that the independence effect dominates for transitions into marriage and the stabilization effect dominates for transitions out of marriage. For example, in the marriage regressions, improvements in women's labor-market opportunities (the log of women's real median wages) *and* welfare reform both lead to reductions in marriage. In the divorce regressions, increases in women's wages and reform both lead to reductions in divorce. This finding is consistent with Ellwood and Jencks's (2001) observation that women's labor-market opportunities are generally negatively associated with marriage. This evidence in favor of the independence effect is not uniformly present in the literature on divorce—about half the studies found negative impacts of women's labor-market opportunities on divorce (as we did) and half found positive impacts.

Last, our finding that increases in labor-market opportunities (directly and indirectly through welfare reform) negatively affect marriage flows is not inconsistent with our finding of a negative effect on divorce flows as well. As we discussed earlier, the net effect on marriage (which depends on preferences, marriage markets, and economic opportunities) can vary across women. What we estimated is an average treatment effect. Harknett and Gennetian (2003) presented an excellent and convincing illustration of this point. They analyzed the impacts of the Self Sufficiency Project (SSP), a welfare reform program in Canada. Like the U.S. welfare reform that we examined, SSP also has had a theoretically ambiguous impact on marriage, but the SSP reforms have many fewer features. SSP has a generous earnings disregard and a reduction in marriage disincentives (Michalopoulos et al. 2000). Harknett and Gennetian's data are experimental, based on random assignment with all participants in the SSP group receiving the same treatment. Harknett and Gennetian found that SSP had no effect on marriage, on average, but led to a significant increase in marriage in one province (New Brunswick) and a significant decrease in marriage in another (British Columbia). They explored many explanations for this finding and concluded that "unmeasured characteristics at a state or provincial level can significantly alter the estimated relationship between policies and marriage" (p. 474).

A novelty in our study was the use of flows into and out of marriage, rather than stocks. By examining flows, we averaged over a different population than have studies that have analyzed stocks and hence may have found a different average treatment effect. To address this possibility, we used the June 1995 supplement to the CPS, which provides data on marital histories. These results are presented in Table 5. We provided statistics for four samples: women who married in the previous year (column 1), women who divorced in the previous year (column 2), all married women (column 3), and all divorced women (column 4). The top panel presents means for all women aged 15–65, and the bottom panel presents means for women aged 15–65 with living children younger than age 18.

There are several important differences between the flow (newly married and divorced) and stock (currently married and divorced) samples. For example, women who enter marriage are younger and less likely to have children than is the stock of married women. Notably, the flow sample is more representative of the population who is at risk of being affected by welfare policies than is the stock sample. In particular, among women with children, the flow samples of marriages and divorce are more likely to have low levels of education compared with the stock samples. The flow samples are also composed of younger women.

SENSITIVITY TESTS AND ROBUSTNESS OF RESULTS

Additional Controls

Our results are robust with respect to specifications with a wide variety of additional control variables and to other sensitivity tests.[14] For example, using an unbalanced sample of states did not affect the results, nor did using other population measures to weight the regressions. We also explored the sensitivity of the results to functional form, estimating models in which the dependent variable was either the marriage (divorce) rate or the number of marriages (divorces), with population as an explanatory variable, and estimating

14. In addition to the changes discussed here, an earlier version of this study also included controls for states' rates of growth in employment, the fraction living in poverty, the presence of an AFDC-UP program, and the extent of Medicaid expansions. The results were not sensitive to dropping each of these variables, which were not significantly associated with marriage and divorce rates.

Table 5. Average Characteristics of Married and Divorced Women: June 1995 Current Population Survey

Variable	Recently Married (1)	Recently Divorced (2)	Currently Married (3)	Currently Divorced (4)
All				
Age	28.94	38.10	41.55	43.19
High school dropout and 19 or older	0.09	0.14	0.12	0.13
High school graduate and 19 or older	0.33	0.39	0.38	0.37
Some college and 19 or older	0.32	0.31	0.27	0.32
College graduate and 19 or older	0.22	0.15	0.23	0.17
No children	0.50	0.17	0.15	0.16
White	0.84	0.83	0.88	0.81
Black	0.11	0.14	0.08	0.15
Number of individuals	1,137	648	26,634	5,040
Have Children Younger Than Age 18				
Age	28.87	33.58	35.94	36.31
High school dropout and 19 or older	0.14	0.12	0.11	0.14
High school graduate and 19 or older	0.45	0.44	0.37	0.40
Some college and 19 or older	0.28	0.31	0.28	0.34
College graduate and 19 or older	0.10	0.12	0.24	0.13
White	0.82	0.83	0.87	0.80
Black	0.14	0.15	0.08	0.16
Number of individuals	481	378	13,815	2,159

Notes: Shown are weighted sample means for women aged 15–65 in the supplement to the June 1995 Current Population Survey (the number of individuals is not weighted). Newly married (divorced) women are women who married (divorced) within the past 12 months.

log-odds-ratio regressions. These models all provided similar estimates of the impact of welfare reform.

Adding other state variables to control for labor and marriage markets also did not lead to different results. Including separate controls for men's and women's unemployment rates did not qualitatively affect the estimated coefficients of the variables for welfare reform in any of the regressions.[15] Including men's rate of labor-force participation or employment-to-population rate also did not affect the results for the welfare reform variables, although men's employment rate was negatively associated with the marriage rate in some specifications. We also included the incarceration rate in the regressions to control further for the number of available male marriage partners. The results for the welfare reform variables were similar to those shown in the tables, and the incarceration rate was not significantly associated with the marriage rate or the divorce rate. Including a variable measuring the fraction of births to unmarried women (potentially endogenous to the decision to marry and divorce and to reform), which may affect marriage rates if having a nonmarital birth influences the likelihood that a woman will soon marry, did not

15. All results discussed in the article but not shown in tables are available on request.

appreciably affect the magnitudes of the estimated coefficients of the welfare reform variables. The results for welfare reform are also robust to controlling for the sex ratio, measured as the ratio of men aged 15 and older to women aged 15 and older.

Specific Aspects of Reform

We also investigated the effect of specific aspects of welfare reform on marriages and divorces. This approach is motivated by the fact that welfare reform policies are heterogeneous across states, and it is important and interesting to know how the different policies affect the outcomes of interest. The difficulty, as many scholars have noted, is that the dimensions along which policies differ are almost as numerous as the policies themselves. Accordingly, it is difficult to capture the features of reform in a parsimonious way. Furthermore, there will always be unmeasured attributes of reform, and if these attributes are correlated with both the included detailed reforms and the outcomes of interest, then estimates for detailed reforms will be biased.

Nonetheless, to examine which aspects of reform affected marriage and divorce, we adopted an approach used in several recent studies of welfare reform and used summary measures on features of reform. We included a dummy variable equal to 1 if the state expanded benefits under the AFDC-UP program (e.g., removed the 100-hour rule). We also included dummy variables for whether overall work incentives in TANF are “strict” or “medium” (the omitted category was weak incentives). This coding came from Blank and Schmidt (2001) and was used by Schoeni and Blank (2003). The categorization was based on information on benefit generosity, earnings disregards, time limits, and sanctions. Overall, the results were consistent with our main findings and provided little insight into the specific policies that were driving the results.

Detail Vital Statistics Data

One drawback of the aggregate vital statistics data is that marriages and divorces are counted by state of *occurrence*. One obvious check of sensitivity is to drop observations from Nevada and Hawaii, states with many weddings to nonresidents. The results presented in Tables 3 and 4 are robust to dropping these states. However, if more general nonresidential marriage and divorce patterns occur and if these patterns are systematically related to welfare policies, then our estimates will be biased. We directly examined these possible biases by using the detail vital statistics sample.

The detail data, like the aggregate data, can be used to construct counts of marriages and divorces by state, but the detail data report both state of occurrence and state of residence. Unfortunately, the detail data are not collected for every state. Thus, although we could use these data to examine whether using state of occurrence biases the results, the results would be only suggestive, since totals for state of residence would be underreported. This underreporting of totals for state-of-residence marriage and divorce is due to the fact that marriages and divorces that involve residents of states that report detail data but occur in states that do not report detail data will be missing from the state-of-occurrence totals. This issue is of greater concern for marriages than for divorces during the period under examination, particularly if states with relatively lax marriage requirements, such as Nevada, did not report detail data. Only 88% of the marriages that took place in states that reported detail data were to state residents, compared with 93% of the divorces.

Another advantage of the detail data is that they report the ages and, in some cases, races of the women who were getting married or divorced. We used these data to examine the sensitivity of our results to including controls for the age and race of the individual and to explore how the impact of reform varies across age and racial groups. (Unfortunately, information on educational attainment was dropped from the detail-data files beginning with the 1989 detail data.)

The detail data are available only through 1995, so the sample is limited to 1989–1995. In addition, not all states reported detail data. Because the data stopped in 1995, we estimated and report only the impact of waivers on marriage and divorce. Late-waiver states were also not captured in this analysis. We aggregated the detail data by either state of occurrence or state of residence, year, and five-year age groups. We again restricted our sample to create a balanced panel and included states that reported marriage (divorce) for each year from 1989–1995, so that we had seven years of data on 41 states for marriage and 31 states for divorce (neither included Nevada). The denominators for these age-aggregate data were CPS estimates of the at-risk population disaggregated by age.

We present estimates based on the detail data in Tables 6 (for marriage) and 7 (for divorce). Table 6 reports the results of specifications that were designed to examine the sensitivity of our results on waivers and marriage to the use of data from the detail data set (rather than from the aggregate vital statistics), state of residence (rather than state of occurrence), and the use of controls for age group (rather than for the states' share of the female population in each age group). The first column reports the waiver coefficient from a specification that is identical to that in column 3 of Table 3, except that only data for 1989–1995 are included (and thus there is no TANF variable). The estimated effect of waivers is again negative and significant and rises considerably (from –0.048 in Table 3 to –0.081), suggesting larger treatment effects for the early-waiver states. Column 2 includes the same specification, except that only the 41 states that reported detail marriage data are included. This change had essentially no impact on the estimated impact of waivers. Column 3 presents the replication of the specification in column 2, except that the detail data set was used, with data aggregated across age groups to the state-year level. Switching to the detail sample led to a substantial reduction in the estimated waiver coefficient, which is no longer significant. Presumably, this change occurred because the detail data were sampled; if they were universe data (like the aggregate data), then the coefficient estimates would have to be numerically identical.

In column 4, we disaggregate the data by eight 5-year age groups. The coefficient on the waiver dummy variable is now –0.072 and is again statistically significant.[16] In column 5, we used the same data source and level of aggregation as in column 4 but added seven dummy variables for five-year age groups. The estimated coefficient of –0.057 is still significant (and does not differ significantly from the column 4 coefficient). Thus, switching to the detail data and then controlling for age while continuing to use state of occurrence to define the marriage rates reduced the estimated coefficient on the waiver dummy variable by as much as 30% but did not change the qualitative conclusion that waivers reduced marriage inflows.

Column 6 of Table 6 examines the sensitivity of our main results to the use of state of occurrence (as in our Table 3 results). In this specification, marriage rates are computed on the basis of state of residence, rather than state of occurrence. In all other respects, the column 6 specification is identical to the one in column 3 (it uses the detail data, aggregated across age groups).[17] The result is a substantial drop in the magnitude of the waiver coefficient, to a statistically insignificant –0.022. In column 7, the data are disaggregated by age group, so that the appropriate basis of comparison to state of occurrence is in column 4. This estimate is still insignificant and smaller than the one in

16. The coefficient can change because the dependent variable is the log marriage rate and the weight is the level of the denominator for each cell. If we were to use the level of the marriage rate instead of the log, the estimated coefficients in columns 3 and 4 would necessarily be identical (because none of the right-hand-side variables in the two specifications vary at the substate level).

17. Recall that this state-of-residence measure is an underestimate of the true number of marriages to women living in a state if many women in that state got married in other states (such as Nevada) for which there were no detail data.

Table 6. Determinants of Marriage Rates, Comparison of Aggregate and Detail Vital Statistics Data, 1989–1995

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of Year That the Major AFDC Waiver Was in Effect	–0.081* (0.041)	–0.088† (0.050)	–0.051 (0.031)	–0.072† (0.038)	–0.057† (0.032)	–0.022 (0.027)	–0.046 (0.034)	–0.031 (0.028)
Women Aged 15–19					1.395** (0.025)			1.388** (0.026)
Women Aged 20–24					2.837** (0.022)			2.820** (0.021)
Women Aged 25–29					3.032** (0.027)			3.018** (0.025)
Women Aged 30–34					2.703** (0.023)			2.697** (0.022)
Women Aged 35–39					2.358** (0.025)			2.360** (0.024)
Women Aged 40–44					2.030** (0.022)			2.034** (0.022)
Women Aged 45–49					1.702** (0.024)			1.706** (0.024)
Adjusted R^2	0.885	0.804	0.960	0.067	0.967	0.955	0.040	0.968
State-Year-Age Combinations	357	287	287	2,296	2,296	287	2,296	2,296
Number of States	51	41	41	41	41	41	41	41
Data Source	Aggregate	Aggregate	Detail	Detail	Detail	Detail	Detail	Detail
Aggregated Across Age Groups	Yes	Yes	Yes	No	No	Yes	No	No
State of Occurrence	Yes	Yes	Yes	Yes	Yes	No	No	No

Notes: Shown are coefficients from regressions of the determinants of marriage rates during 1989–1995. The dependent variable is the natural log of the number of marriages per 1,000 women who were at risk of marrying (currently unmarried). Columns 1 and 2 are based on aggregate data, and columns 3–8 were based on detail data. In columns 6–8, data are by state of residence instead of state of occurrence. In columns 4–5 and 7–8, data are disaggregated into eight age groups, and the denominators of the marriage rates are age specific. Columns 5 and 8 include age-group fixed effects. All regressions control for socioeconomic and demographic variables; the age, education, and enrollment distribution of the female population; and state and year fixed effects. Standard errors are in parentheses. All standard errors allow arbitrary heteroscedasticity; for columns 4–5 and 7–8, arbitrary within-state-year correlation is also allowed. Data are weighted by the denominator of the marriage rate.

†$p < .10$; *$p < .05$; **$p < .01$

column 4, although it is twice the size (in magnitude) of the estimate in column 6. Finally, in column 8 the age-group dummy variables are added to the specification in column 7, the result being a relatively small and statistically insignificant estimate. These results show that the waiver coefficient in the marriage specifications is never significant when state of residence is used to sort the data and to compute marriage rates. On the other hand, this coefficient is never positive, and, in general, the use of dummy variables for age does not substantially change the estimated coefficient relative to specifications in which marriage rates are aggregated across age groups within states.

Table 7 repeats the same set of specification checks with the divorce rate as the left-hand-side variable. Specifications in this table include fewer states because 10 of the states

Table 7. Determinants of Divorce Rates, Comparison of Aggregate and Detail Vital Statistics Data, 1989–1995

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of Year That the Major AFDC Waiver Was in Effect	–0.044** (0.017)	–0.038 (0.024)	–0.029† (0.016)	–0.042* (0.017)	–0.035* (0.018)	–0.030† (0.016)	–0.048** (0.018)	–0.040* (0.017)
Women Aged 15–19					1.639** (0.090)			1.659** (0.090)
Women Aged 20–24					1.849** (0.068)			1.879** (0.068)
Women Aged 25–29					1.662** (0.068)			1.697** (0.068)
Women Aged 30–34					1.405** (0.067)			1.441** (0.067)
Women Aged 35–39					1.208** (0.068)			1.247** (0.068)
Women Aged 40–44					1.017** (0.071)			1.056** (0.071)
Women Aged 45–49					0.666** (0.073)			0.704** (0.072)
Adjusted R^2	0.966	0.955	0.969	0.066	0.790	0.967	0.053	0.798
State-Year-Age Combinations	322	217	217	1,697	1,697	217	1,697	1,697
Number of States	46	31	31	31	31	31	31	31
Data Source	Aggregate	Aggregate	Detail	Detail	Detail	Detail	Detail	Detail
Aggregated Across Age Groups	Yes	Yes	Yes	No	No	Yes	No	No
State of Occurrence	Yes	Yes	Yes	Yes	Yes	No	No	No

Notes: Shown are coefficients from regressions of the determinants of divorce rates during 1989–1995. The dependent variable is the natural log of the number of divorces per 1,000 women at risk of divorcing (currently married). Columns 1 and 2 are based on aggregate data, and columns 3–8 are based on detail data. In columns 6–8, data are by state of residence instead of state of occurrence. In columns 4–5 and 7–8, data are disaggregated into eight age groups, and the denominators of the divorce rates are age specific. Columns 5 and 8 include age group fixed effects. All regressions control for socioeconomic and demographic variables; the age, education, and enrollment distribution of the female population; and state and year fixed effects. Standard errors are in parentheses. All standard errors allow arbitrary heteroscedasticity; for columns 4–5 and 7–8, arbitrary within-state-year correlation is also allowed. Data are weighted by the denominator of the divorce rate.

†$p < .10$; *$p < .05$; **$p < .01$

that reported detail marriage data did not report detail divorce data. Restricting consideration to the 1989–1995 period while using the main data and specification from column 3 of Table 4 does not change the estimated waiver coefficient. Switching to the detail data, restricting consideration to the 31 states with detailed divorce data, and adding dummy variables for age all have relatively minor impacts on the estimated waiver coefficient. Moreover, using state of residence, rather than state of occurrence, has no important impact on the estimated waiver coefficient for the divorce specifications.[18]

18. The detailed data can also be used to explore how the impact of reform varies across different age groups. The results, not shown here, suggest that the decline in marriage is concentrated among the youngest

In sum, Table 6 suggests that the magnitude and statistical significance of the estimated impact of waivers on marriage is sensitive to the period used; the use of detail data; controls for age dummy variables; and, especially, the computation of marriage rates on the basis of state of residence. However, there is no evidence to support a positive effect of waivers on marriage rates. Table 7 indicates that the sign, magnitude, and significance of the waiver estimates for divorce specifications are robust.

DISCUSSION AND CONCLUSIONS

Transitions into and out of marriage have significant implications for the economic well-being and self-sufficiency of women and their children. A major goal of welfare reform therefore includes raising marriage rates and lowering nonmarital birth rates, while evaluating the effects of reform on marriage and divorce that are of interest to policy makers. We used vital statistics data on marriages and divorces during 1989–2000 to examine the role of welfare reform (state waivers and the implementation of TANF) and other state-level variables on flows into and out of marriage. A strength of this analysis was the use of data on flows into marriage and divorce, data that respond more quickly to welfare reform than do stock data.

Our results indicate that the transitions into marriage are negatively associated with AFDC waivers and with the implementation of TANF. The magnitude and statistical significance of these estimates are somewhat sensitive to specification checks regarding our use of state of occurrence to define marriage rates. However, the sign of our estimates is always negative and sometimes statistically significant. Thus, we can say with some confidence that welfare reform is not "pro-marriage," on balance. With respect to divorce, the sign, magnitude, and significance of our estimated waiver coefficients are robust, suggesting that waivers reduce flows out of marriage and into divorce. Our estimated TANF coefficient is also negative in divorce specifications, although it is statistically significant only at the 10% level when we included state and year fixed effects.

The effects of welfare reform on marriage and divorce are not clearly predicted by utility-maximizing theory. Given this theoretical ambiguity, our findings that welfare reform leads to reductions in marriage and divorce are perhaps surprising. However, changes in welfare programs may have different effects on marriage across individuals, as Harknett and Gennetian (2003) found in their experimental analysis of Canada's SSP. Welfare reform may have different effects on single persons than on married persons. Because welfare reform encouraged or required more work, single women may have been less likely to get married because their earnings rose or the independence effect dominated for these women. For married women, welfare reform may have increased the number of hours they would have to work if they divorced, thereby discouraging divorce. In addition, welfare reform may have introduced considerable uncertainty about the future and made people less likely to change their current marital status, consistent with our finding of a reduction in transitions into and out of marriage.

The increased eligibility of married two-parent families for welfare may have had more effect on married women than on single women because in married two-parent families, the women's husbands would have to have low earnings for the families to be eligible for welfare. Low-earning men may not be desirable spouses, so welfare reform may have created little incentive for single women to marry. For married women, in contrast, relaxing the two-parent rule would discourage divorces that were aimed at qualifying for welfare. Consistent with these differing incentives, welfare reform may have discouraged

age groups, while divorce is concentrated in the higher age groups. Estimates using the population (rather than the population at risk) and disaggregating by race (black, white, and other), five-year age group, and state and year were similar to the estimates presented in Tables 6 and 7, columns 5 and 8.

divorce among married individuals but may have had a much smaller effect among never-married individuals. If the number of divorces has declined as a result of welfare reform, the number of remarriages would be expected to fall as well; most divorced individuals remarry, and the average time between divorce and remarriage is only about three years (Kreider and Fields 2002).

In the final analysis, it is difficult to draw strong conclusions regarding the impact of TANF, given the short period of implementation and the lack of available data on comparison groups. There is little evidence here that welfare reform has encouraged more marriage. Furthermore, while the study addressed many of the shortcomings of the vital statistics data, the lack of consistency in the coefficients on some of the key explanatory variables highlights the need for additional research using individual-level longitudinal data.

APPENDIX

Aggregate number of marriages and divorces: National Center for Health Statistics, *Vital Statistics of the United States* and *Monthly Vital Statistics Report*, various years.

Detail data on number of marriages and divorces: National Center for Health Statistics, Data File Documentations, Marriage and Divorce, 1989–1995 (machine readable data file and documentation, CD-ROM Series 21, No. 6), Hyattsville, MD.

AFDC waivers and implementation of TANF: The primary source for the dating of state reforms is the tables on the web site of the assistant secretary for planning and evaluation for the Department of Health and Human Services: http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm. A state is coded as having an AFDC waiver if it has a "major" waiver or there was a significant deviation from the state's AFDC program and the waiver was in place statewide. More details on the coding of the welfare reform variables are presented in Bitler et al. (2002) and are available on request.

Maximum AFDC/TANF welfare benefits for a four-person family with one adult: Robert Moffitt's web site: www.econ.jhu.edu/People/Moffitt/DataSets.html. Benefits are deflated using the personal-consumption expenditures deflator (1997 = 100). Benefits are annual and presented in thousands.

Population, by age, sex, race and ethnicity: Bureau of the Census web site: http://eire.census.gov/popest/estimates.php.

Percentage of the population living in metropolitan areas: Bureau of the Census, *Statistical Abstract*, various years. Data for 1989, 1991, 1995, and 1999 were linearly interpolated.

Population of unmarried and married women aged 15 and older, men's and women's median weekly wages, distribution across age and educational groups, and the fraction enrolled in high school and college: Calculated from the March CPS, 1989–2000. Totals are weighted sums within states and years. Distributions are weighted averages. All are for women aged 15 and older and use the variable "psupwgt."

Average annual unemployment rates: Bureau of Labor Statistics, *Employment and Earnings* and *Geographic Profile of Employment and Unemployment*, various years.

Real median income for a family of four: Bureau of the Census web site: http://www.census.gov/hhes/income/4person.html. Deflated using the personal-consumption expenditures deflator (1997 = 100).

Covenant marriage: Coding based on the coding in the Americans for Divorce Reform web site: http://www.divorcereform.org/cov.html.

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