Welfare Reform and Children's Living Arrangements*

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Abstract

Little is known about welfare reform's effects on family structure and children's living arrangements, an important focus for reformers. Using March CPS data, we find that state welfare waivers are associated with children being less likely to live with unmarried parents, more likely to live with married parents, and more likely to live with neither parent. Children living with neither parent are living with grandparents or other relatives, or rarely, in foster care. The estimates vary somewhat by children’s race and ethnicity. Due to the limited variation in TANF’s implementation timing across states, we focus on the waiver results.

I. Introduction

The 1990s saw major reforms to public assistance programs in the United States. Beginning in the late 1980s, many states received waivers and implemented sweeping reforms to their Aid to Families with Dependent Children (AFDC) programs. This widespread experimentation led to passage of the 1996 Personal Responsibility and Work Opportunity Act (PRWORA), which eliminated AFDC and replaced it with Temporary Assistance for Needy Families (TANF). These reforms substantially changed the economic incentives facing low-income individuals with children or considering having children. In particular, these reforms dramatically reduced the lifetime generosity of state welfare programs by imposing time limits, strengthening work requirements, and limiting the population eligible for assistance. We discuss the large and growing literature on the effects of these reforms in section III below, focusing mainly on state welfare caseloads and labor market outcomes. While a few papers have considered the impacts of reform on family structure, they have mostly focused on marriage, headship, and fertility. Relatively little attention has been paid to children's living arrangements defined more broadly or children's well-being in general. This relative dearth of research on living arrangements is somewhat surprising, given the longstanding concern that AFDC provided adverse incentives to form and maintain intact families, and the hypothesized negative consequences for children. In fact, changes in living arrangements appeared as a key stated objective in the PRWORA legislation which aimed to:[1] "(1) provide assistance to needy families so that children may be cared for in their own homes or in the homes of relatives; (2) end the dependence of needy parents on government benefits by promoting job preparation, work, and marriage; (3) prevent and reduce the incidence of out-of-wedlock pregnancies and establish annual numerical goals for preventing and reducing the incidence of these pregnancies; and (4) encourage the formation and maintenance of two-parent families."

In this paper we consider comprehensively the impact of waivers and the 1996 federal reform on children's living arrangements using March Current Population Survey (CPS) data. We first examine reform's impact on three outcome variables: (i) whether the child lives with an unmarried parent, (2) whether the child lives with a married parent, and (3) whether the child lives with neither parent. The importance of evaluating the effects of reform on the first two living arrangements is clear given the goals of PRWORA. We believe the propensity to live with neither parent is important as well. Living with no parent but other relatives—usually grandparents—is a common outcome for children whose parents are in jail, deceased, or otherwise unable to care for their children. Tracking the growing trend in grandparents being primary caregivers for their grandchildren was a concern of welfare reformers, who mandated in PRWORA that the Census Bureau measure the prevalence of custodial grandparenting. The 2000 Decennial Census found that 2.4 million adults were grandparents who had primary responsibility for caring for their grandchildren under 18 (Simmons and Dye 2003). As yet we know little about how welfare reform has affected the propensity of children to live without parents.

We further analyze who children live with if not with their parents, in an effort to begin to assess the implications for child well-being. We explore how reform changes the propensity for children to live with no parent and a grandparent who is the head of household. Work by Solomon and Marx (1995) and Moyi, Pong, and Frick (2004) suggests that children living with a grandparent and no parent have better outcomes than children living with a single parent. We explore how reform changes the propensity to live with no parent in a poor household (one with income under the federal poverty level) as well as to live with no parent in a household that is not poor.

We also consider several measures of whether children live in doubled-up households. The first such measure is living with an unmarried parent and grandparent. Past research (DeLeire and Kalil 2002) finds that teen children of single parents living in three-generation households have outcomes at least as good as do children of two-parent families. Our second measure of doubling-up is living with a parent and a potential non-marital partner of that parent. If cohabitation brings a nonbiological parent into the household, child well-being may be negatively impacted. McLanahan and Sandefur (1994) and Case, Lin, and McLanahan (2000) suggest that living with nonbiological parents may be disadvantageous.[2]

One novel feature of this work is our use of the child as the unit of observation and our analysis of reform's effects on children's living arrangements. As we allude to above, this approach seems natural given children's central role in social policy in the United States, where low-income public assistance programs are targeted to families with children, the elderly, and the disabled. As also noted above, affecting children's living arrangements was a central focus for the 1996 reformers. Interest in the living arrangements of children also stems in part from the fact that measures of children's economic and psychological well-being are worse in single-parent families (McLanahan and Sandefur 1994); of course, this negative association does not establish causality.

Data restrictions provide another reason to focus on children's rather than women's living arrangements. We are interested in determining whether there are any changes in the propensity for child-parent co-residence. This issue could not be examined with a sample of women using standard data sets such as the CPS, as they have no information about absent family members (for example, children living elsewhere). By instead looking at children, and with whom they live, we can track such changes. We use the CPS because it is the largest and most current data set available (and the large samples turn out to be important for identifying reform's effects).

We estimate pooled cross-sectional models relating children's living arrangements to welfare reform in a model with state and year fixed effects, thereby identifying the impact of reform using the timing and incidence of reforms across states. Our models also control for demographic variables, other state policy variables, and labor market conditions. While this identification strategy is compelling for waivers, we argue that identifying TANF's impacts is more tenuous given the lack of variation in the timing of TANF implementation across states. Consequently, while we present results for both TANF and waivers, we focus on the waiver results.

To target our analysis on children potentially eligible for AFDC/TANF, our sample includes all children in families where the head has a high school education or less; we drop all families with a family head who has completed more education than a high school diploma. Because of differences in baseline living arrangements and welfare participation, we also estimate separate models for blacks, Hispanics, and whites. Overall, the results show that state welfare waivers are associated with decreases in the likelihood that children live with an unmarried parent, increases in the likelihood that children live with a married parent, and increases in the likelihood that they live with neither parent. The results for the impact of TANF are mixed and less precisely estimated.

The estimates are somewhat larger and more precisely estimated for blacks, compared to Hispanics and whites. However, if we construct impacts relative to baseline mean living arrangements or baseline welfare participation rates, the results are much more similar across the groups—with a few exceptions. The finding that reform leads to an increased likelihood that children live with neither parent is concentrated among black children-with smaller impacts for whites and no statistically significant impacts for Hispanics. Further, the positive impact of reform on marriage is concentrated among Hispanics, with more mixed evidence for whites and no statistically significant findings for blacks.

The remainder of the paper proceeds as follows. We discuss the expected effects of welfare reform on living arrangements in section II. We provide a review of the literature in section III. In section IV we describe our data, while in section V we discuss our empirical models. We report the main results in section VI. We report extensions to our main results in section VII and we conclude in section VIII.

II. Expected Impact of Welfare Reform on Living Arrangements

We begin this section by describing the nature of recent reforms. Beginning in the early 1990s, many states were granted waivers to change their AFDC programs. About half of the states implemented some sort of welfare waiver between 1992 and 1995. On the heels of this state experimentation, PRWORA was enacted in 1996, replacing AFDC with TANF. Table 1 reports state implementation dates for state waivers and TANF.

The key elements of reform in the state waivers and TANF legislation include: work requirements, lifetime time limits, financial sanctions, and enhanced earnings disregards. These changes were designed to increase work and reduce welfare participation. Other changes adopted by some states include: expanding eligibility for two-parent families, “family caps” (freezing benefits at the level associated with current family size), and imposing residency and schooling requirements for unmarried teen recipients. For a detailed discussion of the policy changes, see Blank and Haskins (2001) and Grogger and Karoly (2005).

Prior to these reforms, welfare benefits were targeted primarily to unmarried women with children. Thus, appealing to Becker's models of the family (for example, Becker 1981), welfare is predicted to reduce marriage and increase nonmarital fertility. To the extent that the main thrust of welfare reform is reducing the generosity of welfare, then reform is expected to increase marriage and reduce nonmarital births. In our analysis, therefore, welfare reform (both waivers and TANF) would be expected to decrease the probability that a child lives with an unmarried parent and increase the probability that a child lives with a married parent.

However, there are other factors at work besides the overall decrease in welfare generosity. The reforms are work-promoting, so we expect hours of work and earnings to increase among potential welfare recipients (and in fact, this is what we see). It is possible that the increase in work effort could lead to a reduction in marriage—through either a reduction in leisure time or the "independence" effect of increased earnings. Over the long run, the total impact on marriage will depend on the relative size of feedback effects like these.

What about welfare reform and living arrangements more broadly? Are children more or less likely to live with their parents? What about three-generation households? If welfare reform leads to financial stress—from reductions in income—then we might expect more doubling-up with extended family members in the same household. For example, a mother and child might move in with the child's grandparents or some other relative. Alternatively, children might leave their parent's household and move in (alone) with relatives. Increases in income, on the other hand, may lead to reductions in shared living-unless the shared living sufficiently reduces the fixed costs (for example, child care) of working. This discussion suggests that knowing how welfare reform affects income is important for generating predictions about living arrangements. However, the literature is quite mixed on this issue (Blank 2002). In fact, Bitler, Gelbach, and Hoynes (2003b) provide evidence that Connecticut's Jobs First waiver increased income for some while decreasing it for others.

Aside from their impacts through income and employment, changes such as work requirements and financial sanctions may have more direct impacts on parent-child co-residence. In their interviews with caseworkers and welfare administrators in 12 states, Geen et al. (2001) report that "parents sometimes feel as if they must choose between TANF and keeping their children because they cannot possible meet all of the requirements of both systems [child welfare and TANF] at the same time" (p.36). In some situations, a child residing with a relative other than the parent can lead to an increase in welfare benefits.[3]

Finally, impacts of reform may vary across race and ethnicity. One might expect that a large share of the children at risk of being on cash welfare live in single parent families or families with no parent present. If so, then estimated effects of reform might be larger for black children, a larger share of whom do not live with a married parent, than for Hispanic or white children. There is another reason to expect effect sizes to vary across race/ethnicity, namely differences in baseline welfare participation rates. Black and Hispanic children are much more likely to participate in welfare before reform and thus are likely to be at higher risk of being affected than other children. This fact suggests that reduced-form estimates of the effects of reform may be larger for blacks and Hispanics simply because the share of the black and Hispanic population affected by reform is larger.

Overall, welfare reform is expected to increase the probability that a child lives with a married parent and decrease the probability that a child lives with an unmarried parent. It is also possible, through reductions in income or increases in the cost of complying with welfare rules, that welfare reform may lead to an increase in the probability that a child lives with neither parent. We may also see differences across race and ethnicity.

III. Literature Review

Of the large volume of research on welfare reform, most of the studies focus on impacts on welfare participation, employment, and earnings. The broader literature is well summarized in Blank (2002) and Grogger and Karoly (2005). Here we limit our attention to the much smaller literature on the impact of welfare waivers and TANF on living arrangements.

A wide range of outcomes has been examined in the literature on welfare reform and living arrangements. The most commonly measured are the marital and cohabitation status of women, with evidence coming from nonexperimental studies and experimental evaluations of AFDC waivers. Nonexperimental studies of welfare reform and marriage include: Acs and Nelson (2004); Bitler et al. (2004); Ellwood (2000); Fitzgerald and Ribar (2004); Kaestner and Kaushal (2005); Lewis (2003); Rosenbaum (2003); Schoeni and Blank (2000); and Susin and Adler (2002). The results in the nonexperimental literature are mixed. For example, some studies find that reform leads to increases in marriage (Schoeni and Blank 2000), others find reform leads to decreases in marriage (Rosenbaum 2003; Bitler et al. 2004; Fitzgerald and Ribar 2004), and finally others find small or insignificant effects (Ellwood 2000; Kaestner and Kaushal 2005).

The evidence from experimental studies of welfare reform and marriage is also mixed-with few statistically significant results and both positive and negative treatment effects (see reviews by Grogger and Karoly 2005; Fein et al. 2002; and the meta-analysis by Gennetian and Knox 2003). The study by Harknett and Gennetian (2003) is particularly notable in this regard:

in their analysis of Canada's SSP program, they find a statistically significant increase in marriage in one province and a statistically significant decrease in marriage in another. Both Gennetian and Knox (2003) and Grogger and Karoly (2005) present results suggesting that the most TANF-like waivers show more consistently negative (not always significant) impacts on marriage while reforms with generous earnings disregards but lacking stringent work requirements or sanctions lead to increases in marriage.

This literature largely takes the woman as the unit of observation. This analysis may be incomplete, however. If one response to reform is for other, older relatives to care for children, then focusing on samples of single mothers, welfare recipients, or even all women of childbearing age will miss these changes.[4] As such, it is surprising how little econometric research focuses directly on welfare reform and children's living arrangements.

One of the only published papers on the topic of which we are aware is Acs and Nelson (2004). Acs and Nelson (2004) use data from two panels of the National Survey of American Families (NSAF) to examine the impacts of specific features of TANF-taken one a time-on children's and women's living arrangements. Overall, their findings are mixed, but they suggest that family caps may have increased the probability that children live in two-parent families and aggressive child support enforcement may have led to fewer single-parent and more two-parent families. They note that their estimates are identified by specific policy changes between 1997 and 1999 in the 13 states identified within the NSAF, and thus may reflect only short-term differences. Their data do not span the pre-PRWORA period, so they cannot identify effects of waivers. They do include state fixed effects, one year fixed effect, and individual covariates along with the unemployment rate and its lag, but they do not control for other policy changes.[5]

Our approach has several advantages compared to the limited existing work on the effects of reform on children's living arrangements. Our data span the period 1989–2000, including some pre-reform data and data from the waiver period. Consequently, we can use variation in the timing of state welfare waivers implemented from 1992–1996 to identify effects of reform. In contrast, it is difficult to identify effects of TANF as it was implemented quite quickly. Thus, in a model with a full set of year fixed effects, TANF identification comes from cross-sectional variation in the timing of TANF implementation within 1997.[6] We include data from more states and have a much larger sample than would be possible with the NSAF or the SIPP. The CPS sample should provide a snapshot of living arrangements for all children not in institutional settings or in transient homeless facilities. We also include a number of other policy variables to account for other factors that may be driving changes in living arrangements.

IV. Data

We use data from the March Current Population Surveys (CPS) for survey years 1989–2000. The March CPS is an annual demographic file of between 50,000 and 62,000 households and includes detailed information about members of each household including demographics and family structure at the time of the survey and income covering the preceding calendar year. This time period encompasses the main state waiver activity as well as the pre- and post-TANF period.[7]

Our sample consists of March CPS children, whom we define as those aged younger than 16. We exclude older children in order to avoid including potential teen parents as children, since teen parenthood is possibly endogenous to welfare reform. In order to target our analysis to households at higher risk of participating in welfare, we also restrict our sample to children living in families whose head had at most a high school diploma. We use the head's education rather than the parent's education because if both parents are absent from the household, it is impossible to determine either parent's education.[8] Our full sample contains observations on 146,572 children.[9,10]

Our main outcomes consist of three mutually exclusive dummy variables indicating whether the child (i) lives with neither parent, (ii) lives with a parent who is currently unmarried, or (iii) lives with a parent who is currently married.[11] In robustness tests, we analyze living with no parent in a household under the federal poverty limit, living with no parent in a household at or above the federal poverty limit, and living with no parent and a grandparent who is the householder. To further explore the results for living with an unmarried parent we also examine living with an unmarried parent and a grandparent, and living with an unmarried parent and a potential cohabitor.[12]

We estimate models separately for black, Hispanic, and white children. (We define "black" as non-Hispanic black and "white" as non-Hispanic white.) To illustrate why the estimated effects of reform may vary across these groups, Table 2 presents means of the main outcome variables, welfare participation, and poverty rates for the three groups of children in 1989 (pre-reform). (Appendix Table 2 provides means for the control and welfare-reform variables for the full sample.) The table shows that among families where the head has a high school education or less, 38 percent of black children lived in households that had some AFDC income in the previous year, compared to 22 percent of Hispanics and only 9 percent of whites. The table also shows that limiting the white sample to children in high school dropout families leads to welfare participation and poverty rates much closer to the rates for blacks and Hispanics in the high school graduate sample.

One very strong reason for estimating separate models by race and ethnicity is the differences in baseline living arrangements themselves. All three groups have faced the same basic welfare programs over the decades preceding the reforms of the 1990s. Yet these groups somehow arrived at very different living arrangements on average. As Table 2 shows, baseline living arrangements are very different for blacks, Hispanics, and whites. For example, while 59 percent of black children in our high school graduate or less sample live with an unmarried parent, only 35 percent of Hispanic and 23 percent of white children do. This fact tells us that, even if the pre-reform rates of welfare participation were the same, we might well expect different responses to the same policy change. Pooling across race and ethnicity would average together possibly very different treatment effects. Reduced-form estimates of the impact of reform for race/ethnic groups with higher baseline welfare participation rates may also be larger in magnitude simply because a larger share of these groups may be at risk of being affected by reform.

One rough way to assess the impact of welfare reform is to compare simple means of outcome variables before and after reforms were implemented. Table 3 reports such means, together with standard errors, for our main samples and subgroups. In the table, “Pre-reform” cells report the mean and standard error of the outcome for observations before any reform (whether waivers or TANF) was implemented. “Post-reform” cells do the same for observations after any reform (waivers or TANF) was implemented. By taking differences between the outcomes before and after some welfare reform was implemented, we obtain crude estimates of the impact of reform on our outcomes of interest (reported in the table).

These simple results suggest substantial changes in living arrangements for some children. Among black children, the probability of living with neither parent rises by 2.7

percentage points after reform, from 9.3 percent to 11.9 percent. The probability of living with a married parent falls by 2.2 percentage points, while there is no significant change in the probability of living with an unmarried parent. For Hispanics the pattern is different—a large (2.9 percentage point) reduction in the probability of living with an unmarried parent, with a similar sized increase in the probability of living with a married parent and little change in the probability of living with neither parent. Among white children, there was a 5.6 percentage point reduction in the probability of living with a married parent, a 1.9 percentage point increase in the probability of living with neither parent, and a 3.7 percentage point increase in the probability of living with an unmarried parent. We take these as a starting point, but of course they are only suggestive, since they do not control for any confounding factors such as economic conditions or secular social trends.

V. Empirical Model

The standard approach in much of the literature discussed above is to use pooled cross-sections and run regressions of outcome measures on demographic covariates, state-level controls, policy variables, and state and year fixed effects. We follow this basic approach, estimating probit models of a latent dependent variable having the following form:

(1) $y_{ijst} = X_{ist}\delta + L_{st}\alpha + R_{st}\beta + \gamma_s + \nu_t + \mu_j + \varepsilon_{ijst}$

where the latent index y_{ijst} indicates an outcome for individual *i* in month in sample *j* in state *s* in year *t*. Here, X_{ist} is a vector of demographic characteristics, including controls for the child's age and its square, a dummy for whether the child's family head is a high school dropout, and dummy variables indicating residence in an urban area (MSA) and a central city (as well as dummies indicating whether the CPS suppresses a household's MSA or central city status—as is done for small areas). The vector L_{st} contains state-level labor market variables meant to control for economic opportunities in the state. These variables include current and one-year lags—as suggested by Blank (2001)—of unemployment and aggregate employment growth rates, as well as public assistance program variables (other than the reform variables) including the real maximum welfare benefit level for a family of three and measures of a state's Medicaid generosity. The γ_s and ν_t terms represent state and year fixed effects. The state (year) fixed effects control for unobserved factors that differ across states and not over time (over time and not across states).[13] The μ_j terms represent dummy variables for the household's month-in-sample (one through eight) at the time of the March interview. Unobservable determinants are captured by ε_{ijst}. All regressions and summary statistics are weighted using the March basic monthly person weight.

Our main focus is on the coefficients of R_{st}, a vector of simple dummy variables indicating whether or not the given reform—waiver or TANF—is in place in state *s* for year *t*. Following the convention in the literature, we code a waiver as being in place only if it was "major", in the sense of involving a significant deviation from the state's AFDC program, and if it was in place statewide. For TANF, we construct a dummy variable indicating whether the state's TANF plan had been implemented. Note that the waiver dummy is set to 0 once the state's TANF program has been implemented. Thus, the waiver and TANF treatment effects are both measured relative to a baseline of no reform.[14]

Some observers object that the simple dummy-variable approach taken here assumes that reform effects occur instantaneously at the time of implementation. However, this objection is on target only if one assumes that the reform's effects are constant. Given that the treatment effects are likely to vary over time (for example, due to lags in information and response) and/or across states (for example, due to differences in implementation) the coefficients and associated marginal effects for R_{st} can and should be interpreted as averages of heterogeneous treatment effects over the post-reform period.

Using this estimation strategy, the impacts of welfare reform are identified by differences in the timing and incidence of reforms across states. As shown in the top panel of Table 1, there is substantial variation in the implementation of state waivers across states and years. TANF implementation, however, took place in all states in a relatively narrow time period (between September 1996 and January 1998). As shown in the bottom panel of Table 1, the TANF implementation dummy is equal to one in all states by the 1998 March CPS survey year. Thus, as discussed in Bitler, Gelbach, and Hoynes (2003a), the use of year fixed effects removes all identifying variation from the post-March 1997 data and the TANF effects are identified by the cross-sectional implementation-date variation within 1997, when some states have implemented TANF and others have not. (See also footnote 6.) Given these circumstances, we are less confident that the TANF effects indeed identify interesting impacts of reform. Therefore, we concentrate our discussion on the better-identified waiver effects, though for completeness we also report estimated TANF effects.

Estimating standard errors correctly requires us to account for the fact that we use microdata, while the policy variation occurs at the state-by-year level. Also, households can appear in two successive March surveys, so we have multiple observations for most of the children in our sample; lastly, there may be more than one child from any given household. We cluster at the state level, which addresses all these concerns.[15]

VI. Results

We report the main results in this section. All tables present probit marginal effects of switching on the reform dummies.[16]

A. Main Findings

Panel A of Table 4 presents estimates for black children where the head has at most a high school degree. The results show that waivers are associated with a significant increase of 4.8 percentage points in the probability of living with neither parent, a significant decrease of 5.5 percentage points in the probability of living with an unmarried parent, and no effect on the probability of living with a married parent.[17] The 4.8 percentage point treatment effect for living with neither parent may seem large relative to the baseline neither-parent probability of 9.3 percent, but it may be more relevant to consider the increase in neither parent compared to the most likely population of children at risk: the 61.7 percent living with an unmarried parent before reform. This comparison suggests an 8 percent effect (4.8/61.7 = 0.078).[18] Also note that the increase in the probability that black children live with neither parent does show up clearly in the raw means reported in Table 3. The results for TANF are less precisely estimated, possibly reflecting the weaker TANF identification strategy. They show a positive impact on the probability of living with neither parent and a negative impact on the probability of living with a married parent.

Results in Panel B of Table 4 concern the subsample of black children living in a family whose head has fewer than 12 years of education; recall from Table 2 that these children are somewhat more disadvantaged than the larger sample of black children considered in Panel A. The Panel B results show that the increase in the probability of living with neither parent associated with waivers is considerably larger for this dropout sample. However, the baseline rate of living with neither parent is greater by almost an identical proportion (in each case, the figures for the dropout sample are about 5/3 those for the larger sample). Overall, the waiver results for the dropout subsample are qualitatively similar to those for the larger sample, with the TANF estimates being less precise.

We next move to the top panel of Table 5, in which we explore how welfare reform impacts Hispanic children, whose family head has at most a high school degree. The results show that welfare waivers are associated with a significant decrease in the probability of living with an unmarried parent, a significant increase in the probability of living with a married parent, and no significant effect on the probability of living with neither parent. None of the TANF effects are significant.

Specifically, the results show that waivers lead to a 3.1 percentage point decrease in the probability of living with an unmarried parent, which is a nine percent effect compared to the baseline share of 0.347. Further, waivers are associated with a 4.3 percentage point increase in the probability of living with a parent who is married, for a seven percent increase over the baseline of 61 percent. Panel B results for Hispanic children living in families headed by dropouts are almost identical to those for the larger Panel A sample of Hispanic children; given that baseline living arrangements for Hispanic children vary little across these samples, the near-equality of estimates is heartening.

To complete the picture, the top panel of Table 6 reports estimates for the sample of white children whose family head has at most a high school degree. Given the large sample sizes involved, these results are estimated with a great deal of precision. The results show that welfare waivers are associated with a significant increase in the probability of living with neither parent, a significant decrease in the probability of living with a married parent, and no impact on the probability of living with an unmarried parent. TANF leads to a different pattern—a statistically significant decrease in the probability of living with an unmarried parent and a statistically significant increase in the probability of living with a married parent (no significant effect on neither parent). As stated above, we focus on the waiver results because of the weaker identification of TANF's effects.

In particular, the estimates for white children show that waivers led to an increase of 0.7 percentage points in the probability of living with neither parent, which implies a 26 percent impact relative to the baseline rate for neither parent or a three percent impact if we assume that children living with unmarried parents are the at risk group (0.7/23.6 = 0.030).

As discussed earlier, Table 2 shows that this sample of white children living with a head with at most a high school degree is much less likely to be involved with the welfare system pre-PRWORA compared to the samples of black and Hispanic children. A better comparison might be to compare white children whose family heads are high school dropouts to black and Hispanic children whose family heads have at most a high school diploma. To examine the importance of these differences, we re-estimate the models for white children for this lowest education subsample of white children.

Panel B estimates for white children in the lowest education sample generally are larger in magnitude, as would be expected given the greater welfare use of white children living in families headed by dropouts. The unexpected negative impact of waivers on living with a married parent disappears. However, due to the large reduction in sample size when we condition on head's dropout status, none of the waiver coefficients is significant in this subsample.

B. Extension and Discussion

The qualitative pattern of impacts of welfare reform is quite consistent across the three groups. Where the results are significant, we find that reform is associated with an increase in the probability of living with neither parent, a decrease in the probability of living with an unmarried parent and an increase in the probability of living with a married parent (for waivers). The exception is the mixed results for white children, and in particular the unexpected negative impact of waivers on the probability of living with a married parent for white children. (Although this result goes away when we limit the sample to dropouts.)

It is difficult, however, to make a direct comparison in the magnitudes of the marginal effects across the samples. As discussed above, the samples of children have very different underlying baseline family structures and live in families with different probabilities of participating on welfare. One way to compare the estimates across the samples in a meaningful fashion is to divide each marginal effect by p_s, where p_s is the pre-reform baseline welfare participation rate for subgroup *s*. Normalizing this way yields comparable treatment effects under the assumptions that (i) holding constant the propensity to use welfare, the average treatment effect on the treated does not vary across race and ethnicity, and (ii) reform does not change the distribution across race and ethnicity of the propensity to use welfare. Each of these assumptions is debatable, but this normalization approach nonetheless provides a way to consider effects under an interesting counterfactual. These results are presented in Table 7 for the statistically significant marginal effects, for the three samples of children whose family heads had at most a high school degree.[19]

The normalized impacts in Table 7 show that where any of the estimates are significant, they are more similar between groups. For example, the impact of waivers on living with neither parent is 13 percent for blacks and 7 percent for whites—the black effect is still nearly twice the size of the white effect, though the normalized estimates are much closer than the raw ones. The impact of waivers on living with an unmarried parent is -15 percent for blacks and -14 percent for Hispanics. Some of the qualitatively different results, of course, remain—increases in living with married parents are concentrated among Hispanics and increases in living with neither parent are only found for blacks and whites.[20] The lack of a positive impact of reform on living with a married parent for black children may be due to black women's facing worse marriage markets (see, for example, Ellwood and Jencks 2001 or Wilson 1987).

In general, drawing welfare conclusions can be difficult when considering changes in living arrangements, and the neither-parent results are a good case in point. One might surmise that the children newly living with neither parent have left very poor, welfare-dependent households headed by a low-income parent, entering households headed by other relatives with higher incomes. At least from a financial perspective, these children could be better off. To investigate this hypothesis, we estimated two separate probits, the dependent variables of which were indicators for whether the child (i) lived with neither parent in a household where total income in the year before the survey was at or below the federal poverty threshold for their household size and (ii) lived with neither parent in a household where total income was above the federal poverty threshold.

The results are presented for the three subsamples of children in columns 1 and 2 of Table 8. The results in the top panel for black children show that the estimates for the neither-and-poor model imply significant marginal effects of 1.8 percentage points for the waiver coefficient and 3.5 percentage points for the TANF coefficient. For the neither-and-not-poor model, the estimated marginal effects were 3.1 and 0.2 percentage points, with only the waiver estimate being statistically significant. Of course, we do not know the counterfactual probability that these "neither" children would have lived in poor households in the absence of reform. Nonetheless, we believe these results suggest that reform caused an increase in the propensity for black children to live in both poor and non-poor households with neither parent present.

To further explore the rise in children living with neither parent, we estimated models of living with a grandparent who is the householder while not living with a parent.[21] These results provide some information about the people with whom the child lives when her parents are absent, and are important for assessing the relevance of the neither-parent findings for child well-being. As mentioned earlier, work by Moyi, Pong, and Frick (2004) and Solomon and Marx (1995) suggests that children living with a grandparent and no parent have better outcomes than children living with a single parent.

These estimates, presented in the third column of Table 8, suggest that for waivers, about two-fifths of the increase in the propensity of black children to live with neither parent is due to increases in children's propensity to live with a grandparent while not living with a parent (the estimated marginal effect is a significant 2.0 percentage points, compared to the overall effect of 4.8 points as reported in Table 4). For TANF, the estimated marginal effect is an insignificant 1.1 points (compared to the significant estimate of 3.6 points in Table 4).

Another possible response to any fiscal tightening due to reform is for the parent and child to move in with relatives. To examine this possibility, we examined models of living with an unmarried parent and a grandparent; we report these results in column 4 of Table 8. While the point estimates consistently show a positive impact of reform on the probability of living with an unmarried parent and grandparent, none are statistically significant. Finally, column 5 of Table 8 explores the possibility that unmarried parents live with an opposite sex unmarried person to whom they are not related (the closest proxy to cohabitation we can create). Here we see that for blacks, waivers are associated with a decrease in the probability of living with a potential cohabiter.

VII. Sensitivity and Robustness

To gauge the robustness of our findings, we consider a number of extensions to our basic models. Full tables of results for these extensions are available on request; we provide a brief summary of them here.[22] First, we consider more parsimonious controls for time. In place of year fixed effects, we include a more flexible cubic in time. With a cubic in place of year fixed effects, the estimates for TANF are somewhat smaller in magnitude, but overall the results are statistically and substantively unchanged. This finding is important: it suggests that the most conservative interpretation of our TANF estimates that they represent the effects for 1997 only may be unnecessarily restrictive here.

We also include variables capturing specific features of reform plans in an effort to unpack the black box of welfare reform. The detailed reforms we considered included indicators for stringent time limits, elimination of the 100-hour work rule for the Unemployed-Parent program, family caps, minor coresidency requirements, and more generous earnings disregard policies; and a monthly cutoff income variable equal to the income-level at which a welfare participant would lose welfare eligibility. We explored many specifications using these detailed reform variables. We also examined summary measures of the main aspects of reform. For example, we coded state TANF policies by strength of work incentive as in Blank and Schmidt (2001). In general, there is no shortage of statistically significant estimates. However, we do not believe the detailed results suggest any clear story concerning our earlier results. In some cases, the results appear to be internally consistent and informative, but they are difficult to rationalize in many other cases.

It is possible that some other index of TANF severity/generosity would provide estimates more in line with expectations. However, in comparing alternate approaches to characterizing state policies, Grogger and Karoly (2005) find considerable disagreement. More generally, our systematic exploration of detailed aspects of reform suggests that we are unlikely to determine which specific policies lead to changes in children's living arrangements.[23]

VIII. Conclusion

The 1990s ushered in a new era for welfare programs. The U.S. has moved away from public assistance as an entitlement, focusing instead on "temporary assistance for needy families." In this paper, we examine the impacts of reform on the living arrangements of children. By all accounts, living arrangements are an important factor in child well-being. Moreover, influencing living arrangements was an explicitly stated goal of welfare reformers. We examine two sources of reform: state welfare waivers in the 1990s and state implementation of PRWORA (TANF). Using samples of children in families where the head has a high school education or less from the CPS, we estimate pooled cross-sectional models separately for black, Hispanic, and white children where the effects of reform are identified from differences in timing of reforms across states.

One concern with our TANF findings lies in TANF's short implementation period between September 1996 and January 1998. The most conservative interpretation of our TANF estimates is that they represent impacts for 1997 (though results from the cubic-in-time specification discussed in section VII suggest that interpretation may be unnecessarily cautious). However, we do find significant impacts of waivers on children's living arrangements. Waivers were implemented over a long period of time, and thus, we are more confident that we are finding meaningful effects for waivers.

The qualitative pattern of reform's impacts is generally similar for blacks and Hispanics. Where the results are significant, we find that reform is associated with an increase in the probability of living with neither parent, a decrease in the probability of living with an unmarried parent and an increase in the probability of living with a married parent. For whites, we find mixed results, including an unexpectedly negative impact of waivers on the probability of living with a married parent. However, this unexpected finding is no longer present when we limit ourselves to the dropout white sample.

While the magnitudes of the welfare reform effects vary somewhat across the three groups of children, when we normalize the reform effects by the group specific pre-reform welfare participation rate, the impacts are more consistent across groups. Some important differences remain. First, the positive impacts of reform on a child's residing with neither parent is concentrated among black children, and to a lesser extent, white children. Second, the positive impact of reform on living with married parents is concentrated among Hispanics, with more mixed evidence for whites and no statistically significant findings for blacks.

Additional analyses suggest that reform's positive impact on living with neither parent may increase child well-being. We find sizable portions of our impacts on living with neither parent are due to increases in the propensity to live with a grandparent. This finding may be important because child well-being has been estimated to be higher in child-grandparent coresiding families compared to single-parent families. Further, we find evidence that reform increased black children's propensity to live in both poor and non-poor households where neither parent is present.

These findings suggest several conclusions. First, welfare reform is associated with large effects on some important measures of living arrangements. Second, those effects are neither entirely aligned with the stated goals of reform (black children are more likely to live with neither parent) nor entirely contrary to these goals (there is evidence that children are more likely to live with married parents and less likely to live with unmarried parent). Third, analyzing the living arrangements of women, as is typical in the literature, would not have revealed the impacts on the probability of living with neither parent. Fourth, given the many dimensions along which state-level policies have changed, we may never be able to understand which specific features of welfare reform led to the measured impacts. With so many kinds of reforms and a “laboratory” of only 50 states, any particular set of reforms may simply proxy for unmeasured differences across states rather than true policy responses.

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1 The full text of PRWORA can be found at http://thomas.loc.gov/cgi-bin/query/z?c104:H.R.3734.ENR:htm.

2 Cohabitation is not a focal outcome for us because it is impossible to directly identify cohabiting partners in the CPS before 1996, and only possible for partners of the householder from 1996 on. We use a proxy for cohabitation—living with an unmarried opposite sex adult—

common to the literature. Further, we do not know the biological relationship between children and unmarried partners. Ginther and Pollak (2003) suggest it is living in a blended family, rather than living with a nonbiological parent, that is associated with worse child outcomes.

3 In some states after PRWORA, children generally are eligible for child-only benefits if they live with neither parent, regardless of the income of the new household.

4 This is also an issue with experimental analyses. The data on children are generally collected only as part of surveys of former recipients (treatment and control members) several years after random assignment. Therefore, these data contain detailed information only for children who are still co-resident with their parents.

5 Several recent studies also examine children's living arrangements but focus on trends over time (Acs and Nelson 2001; Acs and Nelson 2003; Cherlin and Fomby 2002; Dupree and Primus 2001; Ehrle, Geen, and Clark 2001; and London 1998). The studies show that over the 1990s the share of children living with an unmarried mother declined, while those living with neither parent increased, with larger changes for blacks. This research is informative but does not identify the role of welfare reform. Another recent study in its early stages–Dunifon, Hynes, and Peters (2005)—uses a pooled cross-section design and Survey of Income and Program Participation (SIPP) data to track children's living arrangements and how they respond to specific aspects of welfare reform.

6 The CPS interview occurs in March of each year. We have coded states as having implemented TANF (or waivers) for a given survey if they did so by the end of February of that year. Since all states implemented TANF between August 1996 and January 1998, in the presence of year dummies, TANF identification really comes from differences in outcomes

between 1997 and earlier years among states who have implemented by February 1997 and those who have not.

7 Due to survey changes in 1989, choosing this starting date also allows more continuity in our construction of key outcome variables.

8 As discussed in Bitler, Gelbach, and Hoynes (2002), there is a potential bias from conditioning on education of the head given that head (and perhaps then the education of the head) itself is endogenous. However, the advantages of using a sample in which we expect large impacts of reform are likely to outweigh these possible endogeneity concerns.

9 One sample selection issue concerns the set of state/years represented in our data. Our main results present 12 subgroup-outcome combinations: three racial/ethnic groups and four outcomes. To maintain comparability of estimated effects, we keep the set of states and years fixed for all 12 combinations. Our samples ensure that state-year cells are represented for all subgroup-outcome combinations (this means that there is variation in the left-hand side variable within each state-year cell for each subgroup-outcome combination). Table 1 lists states included in the sample and years for which any state is omitted.

10 We also drop the oversample of Hispanic households (identified by having a March supplement weight that is positive but zero basic monthly survey weight). By design, this oversample excludes less stable Hispanic households (in order to maximize the Hispanic sample) which is a concern given our focus on living arrangements. See U.S. Department of Labor and U.S. Census Bureau (2002) for more information on this oversample.

11 The CPS indicates the line number of a person's parent, if that parent lives in the household. We can thus tell whether a person lives with at least one parent. Together with the CPS's

marital-status variable, we can construct these three variables without relying on relationships to the household or family reference person.

12 We can construct a measure for living with a parent and that parent's parent for every child. The grandchild of householder variable can only be constructed for some children. The variable living with a potential cohabiter is a dummy for living with an opposite sex adult who is unmarried and not in one's own CPS family. The "live with unmarried partner" variable in the 1996 and later CPSs is not available in the early period. Casper, Cohen, and Simmons (1999) discuss the reliability of such proxy measures during the period when the direct "unmarried partner" measure is available for the householder. They find that the imputed measure contains measurement error, thus we view it as less reliable than our main outcomes.

13 Expansions in the federal Earned Income Tax Credit will be absorbed by the year fixed effects. As discussed in Eissa and Hoynes (2000) and Ellwood (2000), the EITC may increase marriage incentives for nonworking single women but decrease incentives for working single women.

14 A waiver is considered "major" only if it related to one of the following policies: termination time limits, work exemptions, sanctions, increased earnings disregards, family caps, or work requirement time limits. The waiver and TANF reform data comes from the Assistant Secretary for Planning and Evaluation (ASPE) for the Department of Health and Human Services: http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm. We code states as having implemented a policy in March (the survey month) if the policy was implemented by the end of February of that year.

15 One concern has been raised in recent work by Kezdi (2002) and Bertrand, Duflo, and Mullainathan (2004). They show that when there is serial correlation in the residual part of the

(latent, in our case) dependent variable, methods using variation in state policy reforms may lead to greatly understated standard errors unless they account for within-state autocorrelation. Clustering at the state level accounts for this concern as well as the repeated-households concern. Note that the number of states included in the probit models must be large enough to justify an appeal to some law of large numbers in averaging cross-state elements of the middle matrix in the estimated covariance matrix for this approach to work; we do not pursue this technical point further here.

16 Because we have two treatment variables, we calculate a baseline predicted probability (where both reform dummy variables are set to 0). We then switch on the given reform variable (that is, the waiver dummy or the TANF dummy) and calculate the estimated probit value. The difference between this estimated treated value and the estimated baseline is then the individual's estimated treatment effect. Standard errors are estimated using the delta method. Stata code to implement this routine via the command margfx is available at http://glue.umd.edu/~gelbach/ado/index.html#margfx.

17 We note again that the dummy variables for neither parent, unmarried parent, and married parent are exhaustive and mutually exclusive. However, the estimated marginal effects need not sum exactly to zero because the results come from single-equation models, rather than multinomial ones that constrain the sum of all predicted probabilities to be one. To see whether this constraint matters, we also estimated marginal effects using multinomial logit models. Results for the waiver coefficients were nearly identical in almost all cases; for the TANF estimates, the results are qualitatively similar in almost all cases, though they are quantitatively more variable with respect to the probit marginal effects. We have chosen to report the probit results for two reasons. First, we are uneasy about the appropriateness of the independence of

irrelevant alternatives (IIA) assumption here; Small-Hsiao tests did not reject the IIA assumption, but Hausman tests frequently returned non-positive definite covariance matrices, which cannot happen in large samples if the IIA assumption is satisfied. Second, delta method standard errors are very time-consuming to calculate using the multinomial logit.

18 We thank an anonymous referee for suggesting this comparison.

19 To be precise, this is only accurate if the share of people at risk of being affected by the policy does not itself respond to reform.

20 PRWORA significantly altered the eligibility rules for TANF and other social insurance programs for new and in some cases, all documented immigrants but left them more or less unchanged for the native born. These changes for non-natives are much more likely to have affected Hispanics than blacks or whites. We reestimated our main models while also controlling for state-only funded “fill-in” programs which maintained coverage for immigrants for cash assistance, food assistance and SSI, and Medicaid, following Borjas (2003) and using the coding from Tumlin, Zimmerman, and Ost (1999). Adding these extra variables does not substantively change the results for Hispanics (or blacks and whites).

21 This measure is incomplete, as it misses grandparent-grandchild co-residence in no-parent households where the grandparent is not the householder. It is, however, the best measure we are able to construct.

22 In addition to robustness tests discussed at length below, we performed three other sensitivity tests. We added to our main models state-specific linear time trends (to capture factors changing over time that may be correlated with reform). We added leads of the reform variables (to test for possible legislative endogeneity or announcement effects). Neither change made a large difference. We also separated the waivers by whether they were implemented early or late in the

waiver period. Later ones had larger impacts but are rarely significant, not surprising given the lack of identifying variation for the later ones, all of which were implemented during 1996.

23 This fact may be no surprise. We have focused on the most commonly mentioned reforms, yet states have implemented many others. Further, some argue that what really matters is how states implement and enforce policies, which is difficult to measure. Finally, our inability to attribute our main findings to specific policy changes is consistent with other papers in this literature.

Table 1
State Implementation of AFDC Waivers and TANF Programs, by March 1

	Ever had a waiver:			Never had waiver
	1993	1994	1996	
First year for which major waiver implemented by March 1	California	Georgia (89 96)	Arizona (92)	Alabama (90 96 97)
	Michigan	Illinois	Connecticut	Florida
	New Jersey		Delaware (92)	Kansas
	Oregon (94)		Indiana (94)	Louisiana (91)
			Massachusetts	Maryland
			Missouri (89 90 92 93 94 100)	Nebraska
			Virginia	Nevada
			Washington	North Carolina
			Wisconsin (97)	Ohio
				Oklahoma
				Texas
				Wyoming (90 92 96 97 98)
				Colorado
				Minnesota
				New Mexico
				New York
				Pennsylvania
				Rhode Island
			1997	1998
First year for which TANF implemented by March 1			Alabama (90 96 97)	Colorado
			Florida	Minnesota
			Kansas	New Mexico
			Louisiana (91)	New York
			Maryland	Pennsylvania
			Nebraska	Rhode Island
			Nevada	California
			North Carolina	Delaware (92)
			Ohio	Illinois
			Oklahoma	New Jersey
			Texas	Wisconsin (97)
			Wyoming (90 92 96 97 98)	
			Arizona (92)	
			Connecticut	
			Georgia (89 96)	
			Indiana (94)	
			Massachusetts	
			Michigan	
			Missouri (89 90 92 93 94 100)	

	Oregon (94)
	Virginia
	Washington

Notes: Numbers in parentheses list years for which no observation from the state appears in our final sample; states with no parentheses are represented every year between 1989–2000. The following states are not represented in any year: Alaska, Arkansas, District of Columbia, Hawaii, Iowa, Idaho, Kentucky, Maine, Mississippi, Montana, North Dakota, New Hampshire, South Carolina, South Dakota, Tennessee, Utah, Vermont, and West Virginia. Maryland, Nebraska, North Carolina, Ohio, and Texas all implemented waivers between March 1, 1996 and February 28, 1997; because no observations in our sample from these states are ever coded as subject to waivers, they are listed here as never having had a waiver. See footnote 9 for information on sample selection and see text for data sources for reform.

Table 2

Welfare Participation, Extreme Poverty, and Living Arrangements in 1989 for children, by Race and Ethnicity

	Blacks	Hispanics	Whites
A. Family head has at most 12 years of education:			
Family had AFDC income last year	0.38	0.22	0.09
Family had income under half the FPL last year	0.30	0.20	0.08
Child lives with neither parent	0.09	0.02	0.02
Child lives with unmarried parent	0.59	0.35	0.23
Child lives with married parent	0.33	0.63	0.75
N	2,402	1,781	9,291
B. Family head has fewer than 12 years of education:			
Family had AFDC income last year	0.47	0.25	0.21
Family had income under half the FPL last year	0.38	0.23	0.17
Child lives with neither parent	0.15	0.03	0.05
Child lives with unmarried parent	0.61	0.35	0.32
Child lives with married parent	0.24	0.63	0.63
N	1,011	1,127	2,291

Notes: Tabulations from the 1989 March CPS using only black, white, or Hispanic children. Sample in Panel A is black, white, or Hispanic children whose family head had 12 or fewer years of schooling. Sample in Panel B is black, white, or Hispanic children whose family head had fewer than 12 years of schooling. Family denotes primary family or unrelated subfamily. All figures weighted using March weight variable. Variables denoting child living arrangements do not sum to one because rounding was done independently. See text for more information.

Table 3
Child Living Arrangements Before and After Reform, Unconditional Means

	Neither Parent	Unmarried Parent	Married Parent
A. Black children			
Pre-reform	0.093	0.617	0.290
	(0.002)	(0.004)	(0.004)
Post-reform	0.119	0.612	0.268
	(0.003)	(0.005)	(0.004)
Difference	0.027***	-0.005	-0.022***
	(0.004)	(0.006)	(0.006)
B. Hispanic children			
Pre-reform	0.040	0.347	0.613
	(0.002)	(0.004)	(0.004)
Post-reform	0.048	0.318	0.633
	(0.002)	(0.004)	(0.004)
Difference	0.008***	-0.029***	0.021***
	(0.002)	(0.005)	(0.006)
C. White children			
Pre-reform	0.028	0.236	0.736
	(0.001)	(0.002)	(0.002)
Post-reform	0.047	0.273	0.680
	(0.001)	(0.002)	(0.002)
Difference	0.019***	0.037***	-0.056***
	(0.001)	(0.003)	(0.003)

Notes: ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively, for the difference rows only. Tabulations from the March CPS, 1989–2000, using only black, white, or Hispanic children whose family head had 12 or fewer years of schooling. All figures in the top row of each cell are means for the relevant race/ethnic group. All figures in the second row of each cell in parentheses are standard errors. "Pre-reform" sample consists of all observations (for relevant race/ethnic group) for which no reform (waiver or TANF) has been implemented. "Post-reform" sample consists of all observations for which some reform (waiver or TANF) has been implemented. Difference sample consists of all observations (for relevant race/ethnic group). All figures weighted using March weight variable. See text for more information.

Table 4
Impacts of Welfare Reform on Living Arrangements of Black Children

	Neither	Unmarried	Married
A. Family head has at most 12 years of education:			
Any major waiver	0.048***	-0.055***	0.006
	(0.013)	(0.021)	(0.018)
TANF enacted	0.036*	0.008	-0.050*
	(0.020)	(0.035)	(0.029)
Pre-reform mean	0.093	0.617	0.290
N	26,770	26,770	26,770
B. Family head has fewer than 12 years of education:			
Any major waiver	0.082***	-0.089***	-0.004
	(0.031)	(0.034)	(0.025)
TANF enacted	0.056	-0.015	-0.037
	(0.044)	(0.052)	(0.038)
Pre-reform mean	0.155	0.653	0.192
N	9,914	9,914	9,914

Notes: ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. All figures are marginal effects and associated standard errors in parentheses. Marginal effects calculated by averaging individual-specific marginal effects. Marginal effects for each reform dummy are calculated with all other reform dummies set to 0. Sample in Panel A is all black children in families whose head had 12 or fewer years of schooling. Sample in Panel B is all black children in families whose head had fewer than 12 years of schooling. All specifications are weighted using March CPS weight variable, with robust variance calculations to account for state-level clustering. Economic and welfare reform variables refer to survey year. Additional control variables are: age of child and its square; dummy for family head's being a high school dropout (in top panel); real maximum AFDC/TANF benefits for a family of three; current and one-year lagged values of state rates of unemployment and employment growth; dummies for residence in central city and MSA; dummy for central-city status being censored; dummy for MSA status being censored; dummy for whether any Medicaid expansion has been enacted in the state; maximum income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; dummies for interview month in sample; and year and state dummy variables.

Table 5
Impacts of Welfare Reform on Living Arrangements of Hispanic Children

	Neither	Unmarried	Married
A. Family head has at most 12 years of education:			
Any major waiver	-0.011	-0.031*	0.043**
	(0.008)	(0.017)	(0.018)
TANF enacted	-0.008	-0.001	0.008
	(0.006)	(0.034)	(0.035)
Pre-reform mean	0.040	0.347	0.613
N	30,746	30,746	30,746
B. Family head has fewer than 12 years of education:			
Any major waiver	-0.011	-0.032	0.046**
	(0.012)	(0.028)	(0.022)
TANF enacted	-0.009	-0.011	0.020
	(0.012)	(0.038)	(0.037)
Pre-reform mean	0.048	0.351	0.601
N	19,097	19,097	19,097

Notes: ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. All figures are marginal effects and associated standard errors in parentheses. Marginal effects calculated by averaging individual-specific marginal effects. Marginal effects for each reform dummy are calculated with all other reform dummies set to 0. Sample in Panel A is all Hispanic children in families whose head had 12 or fewer years of schooling. Sample in Panel B is all Hispanic children in families whose head had fewer than 12 years of schooling. All specifications are weighted using March CPS weight variable, with robust variance calculations to account for state-level clustering. Economic and welfare reform variables refer to survey year. Additional control variables are: age of child and its square; dummy for family head's being a high school dropout (in top panel); real maximum AFDC/TANF benefits for a family of three; current and one-year lagged values of state rates of unemployment and employment growth; dummies for residence in central city and MSA;

dummy for central-city status being censored; dummy for MSA status being censored; dummy for whether any Medicaid expansion has been enacted in the state; maximum income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; dummies for interview month in sample; and year and state dummy variables.

Table 6
Impacts of Welfare Reform on Living Arrangements of White Children

	Neither	Unmarried	Married
A. Family head has at most 12 years of education:			
Any major waiver	0.007*	0.011	-0.017*
	(0.004)	(0.010)	(0.009)
TANF enacted	-0.005	-0.021*	0.026**
	(0.007)	(0.012)	(0.011)
Pre-reform mean	0.028	0.236	0.736
N	89,056	89,056	89,056
B. Family head has fewer than 12 years of education:			
Any major waiver	0.010	-0.019	0.014
	(0.017)	(0.018)	(0.017)
TANF enacted	-0.049*	-0.017	0.074*
	(0.027)	(0.039)	(0.041)
Pre-reform mean	0.075	0.320	0.605
N	17,847	17,847	17,847

Notes: ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. All figures are marginal effects and associated standard errors in parentheses. Marginal effects calculated by averaging individual-specific marginal effects. Marginal effects for each reform dummy are calculated with all other reform dummies set to 0. Sample in Panel A is all white children in families whose head had 12 or fewer years of schooling. Sample in Panel B is all white children in families whose head had fewer than 12 years of schooling. All specifications are weighted using March CPS weight variable, with robust variance calculations to account for state-level clustering. Economic and welfare reform variables refer to survey year. Additional control variables are: age of child and its square; dummy for family head's being a high school dropout (in top panel); real maximum AFDC/TANF benefits for a family of three; current and one-year lagged values of state rates of unemployment and employment growth; dummies for residence in central city and MSA;

dummy for central-city status being censored; dummy for MSA status being censored; dummy for whether any Medicaid expansion has been enacted in the state; maximum income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; dummies for interview month in sample; and year and state dummy variables.

Table 7
Impacts of Welfare Reform on Living Arrangements for Children, Normalized by Pre-Reform Welfare Participation Rate

	Neither	Unmarried	Married
A. Black children			
Waiver coefficient, inflated	**0.127**	**-0.148**	0.016
TANF coefficient, inflated	**0.097**	0.022	**-0.135**
B. Hispanic children			
Waiver coefficient, inflated	-0.048	**-0.141**	**0.196**
TANF coefficient, inflated	-0.035	-0.005	0.037
C. White children			
Waiver coefficient, inflated	**0.071**	0.105	**-0.168**
TANF coefficient, inflated	-0.054	**-0.208**	**0.260**

Notes: Table reports marginal effects from top panels of Tables 4, 5, and 6; normalized by the pre-reform welfare participation rate for each group (number in bold if the marginal effect was significant at the 10 percent level). Panel A reports normalized marginal effects from Table 4. Sample in Panel A is all black children in families whose head had 12 or fewer years of schooling. Panel B reports normalized marginal effects from Table 5. Sample in Panel B is all Hispanic children in families whose head had 12 or fewer years of schooling. Panel C reports normalized marginal effects from Table 6. Sample in Panel C is all white children in families whose head had 12 or fewer years of schooling. See text for more information.

Table 8

Impacts of Welfare Reform on Living Arrangements for Children

	Child lives with neither parent and:			Child lives with an unmarried parent and:	
	Is poor	Is not poor	Grandparent is household head	Grandparent	Opposite sex unmarried adult
A. Black children					
Any major waiver	0.018**	0.031***	0.020***	0.017	-0.013*
	(0.008)	(0.010)	(0.008)	(0.013)	(0.007)
TANF enacted	0.035*	0.002	0.011	0.005	-0.013
	(0.020)	(0.013)	(0.015)	(0.019)	(0.020)
Pre-reform mean	0.048	0.045	0.056	0.104	0.060
N	26,663	26,727	26,663	26,678	26,711
B. Hispanic children					
Any major waiver	0.002	-0.014**	0.001	0.014	0.003
	(0.005)	(0.005)	(0.003)	(0.009)	(0.010)
TANF enacted	0.002	-0.011*	0.011	-0.006	0.012
	(0.007)	(0.006)	(0.008)	(0.008)	(0.012)
Pre-reform mean	0.019	0.021	0.02	0.047	0.049
N	30,233	30,517	29,803	30,246	30,657
C. White children					
Any major waiver	0.004	0.004	0.001	0.004	0.005
	(0.002)	(0.004)	(0.004)	(0.003)	(0.007)
TANF enacted	-0.003	-0.002	-0.008	0.007	-0.003
	(0.003)	(0.005)	(0.005)	(0.007)	(0.011)
Pre-reform mean	0.005	0.022	0.014	0.032	0.057
N	89,056	89,056	89,056	89,056	89,056

Note: ***, **, and * indicate statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. All figures are marginal effects and associated standard errors in parentheses. Marginal effects calculated by averaging individual-specific marginal effects. Marginal effects for each reform dummy are calculated with all other reform dummies set to 0. Dependent variable for column 1 is a dummy indicating whether the child lives with no parent and her family has income below FPL. Dependent variable for column 2 is a dummy indicating whether the child lives with neither parent and her family has income at or above FPL. Dependent variable for column 3 is a dummy indicating whether the child lives with neither parent and with a grandparent who is the household head. Dependent variable for column 4 is a dummy indicating whether the child lives with an unmarried parent and a grandparent. Dependent variable for column 5 is a dummy indicating whether the child lives with an unmarried parent and an unrelated unmarried adult of the opposite sex. Sample in Panel A consists of all black children in families whose head had 12 or fewer years of schooling. Sample in Panel B consists of all Hispanic children in families whose head had 12 or fewer years of schooling. Sample in Panel C consists of all white children in families whose head had 12 or fewer years of schooling. All specifications are weighted using March CPS weight variable with robust variance calculations to account for state-level clustering. Economic and welfare reform variables refer to the survey year. Additional control variables are: age of child and its square; dummy for family head's being a high school dropout; real maximum AFDC/TANF benefits for a family of three; current and one-year lagged values of state rates of unemployment and employment growth; dummies for residence in central city and MSA; dummy for central-city status being censored; dummy for MSA status being censored; dummies for being black and for being Hispanic; dummy for whether any Medicaid expansion has been enacted in the state; maximum income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; dummies for interview month in sample; and year and state dummy variables. See text for more information.

Appendix Table 1
Summary Statistics by Race and Ethnicity

	Blacks	Hispanics	Whites	Pooled
Waiver implemented	0.10	0.21	0.10	0.13
	(0.00)	(0.00)	(0.00)	(0.00)
TANF implemented	0.30	0.33	0.28	0.29
	(0.00)	(0.00)	(0.00)	(0.00)
Real maximum benefits for a family of three ($1000)	4.75	5.80	5.32	5.33
	(0.01)	(0.01)	(0.01)	(0.01)
Unemployment rate	5.62	6.14	5.55	5.71
	(0.01)	(0.01)	0.00	0.00
Employment growth rate	1.84	2.07	1.77	1.86
	(0.01)	(0.01)	(0.01)	0.00
Living in central city	0.53	0.45	0.15	0.30
	(0.00)	(0.00)	(0.00)	(0.00)
Central city status unidentified	0.12	0.11	0.16	0.14
	(0.00)	(0.00)	(0.00)	(0.00)
Living in MSA	0.87	0.91	0.73	0.80
	(0.00)	(0.00)	(0.00)	(0.00)
MSA status unidentified	0.00	0.00	0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Age	7.40	7.00	7.50	7.30
	(0.00)	(0.00)	(0.00)	(0.00)
Family head is high school dropout	0.42	0.68	0.24	0.38
	(0.00)	(0.00)	(0.00)	(0.00)
N	26,770	30,746	89,056	146,572

Notes: Tabulations from the March CPS, 1989–2000, using only black, white, or Hispanic children whose family head had 12 or fewer years of schooling. All figures in top row of each cell are means. Figures in bottom row are standard deviations. All figures weighted using March weight variable. See text for more information.