

The Impact of Welfare Reform on Living Arrangements ¹

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Abstract

Labor market outcomes of welfare reform have been the subject of extensive research by economists, but there has been relatively little work on living arrangements, which was an important focus of reformers. Our research fills that gap by using data from the March CPS to examine the impacts of 1990s welfare waivers and the 1996 Federal welfare reform on living arrangements in samples of both children and women. Our findings suggest three main conclusions. First, welfare reform has had large effects on some important measures of living arrangements, including parental co-residence among children and marital status among women. Second, those effects are neither entirely aligned with the stated goals of reform nor entirely in spite of these goals. For example, TANF was associated with an 11 to 17 percentage point reduction in the fraction of black children living in central cities who live with an unmarried parent. However, the fraction of these children living with neither parent rose by 7 to 12 percentage points, more than doubling the baseline level. Third, there is a great deal of treatment heterogeneity both with respect to racial and ethnic groups (e.g., black, central-city women living in reform states are more likely to be divorced, while Hispanic women in reform states are less likely to be divorced) and with respect to whether reforms were waivers, TANF in states that had waivers, or TANF in states that did not (e.g., waiver effects on parental co-residence among black, central-city children were much smaller than were TANF effects). Standard approaches—using only data on adult women, pooling the data across racial and ethnic groups, focusing only on high school dropouts, and/or assuming homogeneous reform effects—would generally not uncover these important changes in living arrangements.

1 Introduction

Effects of government poverty alleviation programs on labor supply, fertility, marriage, and living arrangements have been the subject of a large and varied literature among economists, demographers, sociologists, and others. Over the last few decades, increasing numbers of children have spent time in single parent households, either because of divorce or because their mothers were never married. Few question that changes in family structure could have wide-ranging impacts on children's outcomes.

Concerns that welfare programs provide adverse incentives to work and form intact families have generated major state and federal reforms to welfare programs over the past 10 years. The widespread use in the United States of state waivers from the former Aid to Families with Dependent Children (AFDC) program in the early 1990s, followed by the passage of the 1996 Personal Responsibility and Work Opportunity Act (PRWORA), 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.

Existing literature on the effects of these reforms, which we discuss in more detail in section 2 below, focuses mainly on state welfare caseloads and labor market outcomes. While a few recent papers have considered marriage, headship, and fertility, relatively little attention has been paid to living arrangements defined more broadly. This relative dearth of research on living arrangements is somewhat surprising, given that changes in living arrangements were a key objective of the reforms of the 1990s. As stated in the text of PRWORA, the purpose of Temporary Assistance to Needy Families (TANF), which replaced AFDC, is to¹

increase State flexibility in operating programs designed to: (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-

¹The full text of PRWORA can be found by searching on "H.R. 3734" at

<http://thomas.loc.gov/home/c104query.html>.

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.

Each of these four goals explicitly involves issues related to living arrangements. In this paper we address these issues by examining comprehensively the impact of waivers and the 1996 Federal reform on household composition and living arrangements using the Current Population Survey (CPS).

In addition to considering a neglected topic, the paper also makes several other contributions. First, examining both state waivers and Federal welfare reform requires a “dual treatment” specification. This is complicated by the fact that not all states had waivers prior to the federal reform. We find that in some cases, the results depend on how one specifies the treatment variables. In particular, there seems to be important heterogeneity in the PRWORA treatment effect across states depending on whether they had a waiver in place prior to the Federal reform. Second, because of the way family relationships are coded in the CPS, there are major difficulties in interpreting the estimated impact of welfare reform on common measures of “family status” (e.g., presence of subfamilies in the household and female headship status) that would seem like natural candidates for study. We construct a set of measures of living arrangements that are not subject to these interpretation problems. Third, policymakers presumably are interested in the impacts of welfare reform on both child and maternal well-being. Thus, we present results using children as the unit of analysis, something that no econometric research on recent welfare reforms has considered previously.

We use data from the March CPS for survey years 1989–2000. In our children’s sample, we analyze the impact of reform on parental co-residence (e.g., whether the child lives with any parent, and if so whether the parent is married) and the propensity to be in a three-generation household. For the women’s sample, we analyze the impact of reform on marital status (never married, divorced/separated/widowed, married), and on the propensity to have an own child in the woman’s household. We augment the core CPS data with welfare reform variables including the presence and timing of state waivers and the timing of state implementation of the PRWORA legislation. We also include state-level controls for Medicaid generosity, labor market conditions, and other measures of welfare program parameters. We estimate pooled cross-sectional models relating the

family structure outcomes to these state-level variables, a small number of demographic controls, and state and year fixed effects. Hence, the effects of welfare reform are identified through variation in the timing and incidence of reforms across states. We also examine many extensions of the basic model by including state linear time trends, leads of policy variables, and alternative coding of reform variables. Because of differences in likelihood of being affected by welfare, our work allows for separate estimates for blacks, Hispanics, and whites.

Our results show that among black children living in central cities, TANF implementation is associated with a large—11 to 17 percentage points—reduction in the fraction of black, central-city children living with an unmarried parent and an increase in the fraction living with a married parent. However, we also find a large increase—7 to 12 percentage points—in the fraction of these children living with neither parent. For black, central-city women, we find that TANF implementation led to a 7 to 8 percentage point reduction in the fraction who have never married. However, we also find a substantial increase in the fraction divorced or separated. The effects of welfare waivers show the same patterns, but with somewhat smaller effects.

We find somewhat different effects for Hispanics. Among Hispanic children, the effect of reform is more mixed and is less likely to be statistically significant. We find that reform is associated with reductions in the fraction of Hispanic children living with an unmarried parent but find no evidence that reform is associated with an increase in the fraction of children living with neither parent. In contrast to the results for black, central-city women, welfare waivers and TANF implementation appear to have reduced the fraction of Hispanic women who are currently divorced or separated, while increasing the fraction who are currently married with spouse present.

While these differences across groups may seem surprising, as we discuss below, the impacts of these welfare reforms on marital status and living arrangements is theoretically ambiguous in many cases. One of the major findings in the paper is this treatment heterogeneity across subgroups of the population. Another finding is we find essentially no significant effects of reform among samples of whites or all adult female high school dropouts. For dropouts, this finding is due to heterogeneity in the effects across racial and ethnic groups.

The remainder of the paper proceeds as follows. Section 2 provides a selected review of the literature on welfare reform, labor market outcomes, marriage, and fertility. In section 3 we discuss the expected effects of welfare reform on family structure and living arrangements. In section 4 we

describe our data, while in section 5 we discuss our empirical models. We report the main results in section 6 and extensions in section 7. We conclude in section 8.

2 Literature Review

A large share of recent research on welfare reform has focused on reform’s impact on state welfare caseloads (e.g., Blank (2001), CEA (1997), CEA (1999), Moffitt (1999), Schoeni & Blank (2000), Wallace & Blank (1999), Ziliak, Figlio, Davis & Connolly (2000), and Haider & Klerman (2001)). For the most part, these authors find that welfare reform has led to significant reductions in welfare caseloads, with somewhat larger impacts of implementation of TANF compared to the state welfare waivers.²

The caseload literature is the subject of a recent comprehensive review by Bell (2001). Bell makes two important points that will be useful to keep in mind here. First, while researchers have made substantial efforts to attribute effects to specific characteristics of welfare reform—including time limits, sanctions, family caps, enhanced earnings disregards, and tightened work exemptions—no clear pattern of statistically significant results has developed in the literature. This is particularly striking given that most researchers have relatively strong priors concerning the likely effects of, say, time limits on caseloads. Expectations are weaker concerning the direction of effect reforms can be expected to have on living arrangements. It is not surprising, then, that we are also unable to attribute the effects of welfare reform on living arrangements to specific characteristics of reform. Second, Bell argues that there is no clear consensus regarding the importance of specifying the dynamic patterns of reform’s effects on caseloads (e.g., including leads and lags of policy variables). Partly for this reason, our preferred results do not consider dynamics; nonetheless, we do consider the sensitivity of our results to inclusion of leads of reform policy variables.

Another series of studies focuses on the relationship between welfare reform and employment, earnings, and income using nationally representative household survey data (e.g., Meyer & Rosenbaum (2000), Moffitt (1999), Ribar (2000), Schoeni & Blank (2000), Lewis (2002)). The studies consistently show that reforms led to increases in employment while the results for earnings, income,

²Grogger & Michalopoulos (1999) make the point that some women—namely those for whom time limits could be binding—may rationally respond to time limits by forgoing current welfare participation in order to “bank” years of welfare for future use, perhaps explaining part of the caseload reductions.

and poverty are less strong.³

More closely linked to our analysis, a number of recent papers have studied the effects of welfare waivers and TANF on headship, marriage, and fertility (e.g., Casper & Bryson (1998), Cox & Pebley (1999), Fitzgerald & Ribar (2001), Susin & Adler (2002), Horvath & Peters (2000), Kearney (2001), Kaestner & Kaushal (2001), Moffitt (1999), Lewis (2002), Dyer & Fairlie (2001), and Schoeni & Blank (2000)). This work has somewhat mixed findings. Schoeni & Blank (2000) find welfare waivers led to an increase in the fraction of high school dropouts who were married, with a similarly sized reduction in the fraction of these women who were coded as head of household; they also find a reduction in marriage rates in waiver states among women with a high school diploma but no more education. Fitzgerald & Ribar (2001) use SIPP data, finding little consistent association between waivers and female headship transitions. Using the CPS, Susin & Adler (2002) find that single mothers are more likely to live with their parents in states with the largest caseload reductions in AFDC participation, although the relationship is not monotonic in caseload reduction (as would be predicted if caseload reduction is an appropriate sufficient statistic for the impact of reform). Horvath & Peters (2000), using state-level aggregate data through 1996, find a negative association between nonmarital birth ratios and state-level waivers from AFDC. Kearney (2001) uses vital statistics data covering the waiver period and finds little association between family caps and fertility. Dyer & Fairlie (2001) find similar results using the CPS.

State-level randomized experiments present another source for results on the effects of reform on family structure and living arrangements. For example, Hu (2001) finds that California's waiver expanding eligibility for two-parent families led to increases in the likelihood of staying married. Duncan & Chase-Lansdale (2000) review the results of four major state experiments and again find mixed evidence on marriage. In one of the four studies (MFIP in Minnesota), marriage increased through reductions in divorce and increases in initial entry into marriage, while in the other studies there were no statistically significant changes.

If examining living arrangements is the goal, two important concerns arise regarding previous research. First, many studies use data only through 1996, so that only waiver effects may be iden-

³Also relevant are "leaver" studies that analyze random samples of women who leave welfare. The leaver studies find the majority of women who left welfare in the 1990s are employed (e.g., Moffitt & Stevens (2000)). While these data are very useful in understanding the circumstances of prior welfare recipients, they are not ideal for estimating the effect of welfare reform. They typically track only women who responded to the incentives embodied in the reforms. Moreover, by definition, these studies cannot identify outcomes for women who never take up benefits.

tified. Second, because of survey design and the focus on samples of women, some important effects of welfare reform may have been obscured. In particular we know essentially nothing regarding the effects of welfare reform on children's living arrangements per se.⁴

If one response to reform is for other older relatives to care for children, focusing on single mothers, welfare recipients, or even all women aged 16–54 will not tell us about these changes.⁵ Our understanding of the effects of welfare reform is surely incomplete in the absence of hard data concerning children's living circumstances.⁶

While some recent studies of welfare reform have focused on children, they rely only on time-series data. For example, in a report covered on the front page of the *New York Times*, Dupree & Primus (2001) use CPS data to examine national trends in children's living arrangements by race, ethnicity, and income. They find that between 1995 and 2000, there was a 1.5 percentage point decline in the number of children living with single mothers, virtually all of which can be accounted for by increases in cohabitation of single mothers with adult males to whom they are not married. Most of this change occurred between 1999 and 2000, and much of it was concentrated among black children. London (1998) also considers cohabitation and living arrangements, though her study focuses more on women than on children and includes data only through 1995. Additional recent research specifically concerning nationwide trends in children's living arrangements is reported by Ehrle et al. (2001) and Acs & Nelson (2001). While such research is informative, it does not make use of the substantial variation in the timing and incidence of state-level welfare reforms. As such,

⁴Paxson & Waldfogel (2001) look at one dimension of children's living arrangements using state-level administrative data from child welfare agencies and find that TANF is associated with increases in numbers of children in out-of-home care (e.g., foster care, living with relatives).

⁵For example, Casper & Bryson (1998) find that the share of children residing with grandparents has risen from 3.2% in the 1970s to 5.5% in the late 1990s, while Cox & Pebley (1999) find that grandparent-led families are more likely to receive welfare benefits than other families. Moreover, while overall caseloads have fallen drastically over the past five years, the child-only caseload has risen over much of the 1990s, reaching a peak of around 1 million cases in 1996 (Farrell, Fishman, Laud & Allen (2000)). While states have great latitude to define eligibility for TANF funds, Ehrle, Geen & Clark (2001) report that in all states but Wisconsin, children cared for by kin are eligible for child-only funds. Hence, changes in living arrangements are one way in which eligibility-restricting welfare reforms could be circumvented.

⁶Authors of completed and ongoing randomized studies have considered some measures of children's living arrangements. While randomized experiments have all the usual advantages in terms of uncontroversial identification, they typically have relatively small samples—at most a few hundred observations in each of the treatment and control groups. Even for measures of living arrangements that change substantially, we are skeptical that significant effects can be discerned in these sample sizes. For example, our sample of black, central-city children has about 13,000 observations, with significant results only for some cases. A further concern is that randomized experiments typically include at least some flow-sampled new welfare applicants, after some reforms have taken place. These flow-sampled women are likely very different from randomly drawn members of the population, given the economic boom of the last several years.

it is difficult to know how much of the trends in living arrangements can be attributed to sources other than welfare policy, and how much is explained by measures of welfare reform after partialing out these other effects. By controlling for state labor market conditions as well as state and year fixed effects, we address this concern.

Studies of recent welfare reforms complement a much larger body of literature assessing the effects of the older AFDC program on family structure. That literature examines the impact of AFDC benefit levels on a wide range of family structure and living arrangement outcomes including marriage, divorce, female headship, cohabitation, fertility, nonmarital fertility, and subfamily formation. Reviews of this literature (e.g., Hoynes (1997*b*), Moffitt (1992), and Moffitt (1998)) show that more generous welfare programs lead to higher rates of female headship and nonmarital births. However, with a few exceptions, the magnitude of the welfare effect is relatively modest and, in the aggregate, appears to play a small role in explaining the very significant changes over time in marriage and nonmarital birth rates. Work by Ellwood & Bane (1985) suggests that among the various behaviors thought likely to be affected by welfare policy, the choice between establishing one's own household and living with relatives is one of the most responsive. It is hard to know what this conclusion implies for impacts of welfare reform given the difference in the nature of the "treatment". As a result, movements of mothers and their children across households may be an important outcome for researchers to explore.

3 Expected Impact of Welfare Reform on Living Arrangements

To analyze the impact of welfare reform on family structure, we begin with a Becker-style economic model in which individuals make utility-maximizing choices about marriage, fertility, and living arrangements (e.g., Becker (1981)). Using these models, one can model the financial incentives in welfare programs to make predictions about impacts on family structure. For example, to analyze the female headship decision (e.g., Hoynes (1997*a*)), a woman compares her maximum attainable utility as a female head to her maximum utility as a nonhead. As a female head, she gains utility $U(Y(FH), FH)$ where $FH = 1$ if the woman becomes a head and 0 otherwise, and the (vector-valued) function Y provides the woman's optimal consumption bundle (e.g., labor supply and market goods) given her headship decision. Utility also may depend on FH directly because of

stigma or some other nonpecuniary effects of female headship. Because welfare benefits traditionally were available only to single-parent households, nearly all of which were female-headed, this model predicts unambiguously that the existence or expansion in welfare leads to an increase in female headship. Similar models can be used to predict that increases in welfare generosity lead to decreases in marriage and increases in divorce and nonmarital births.

This framework can also be used to make predictions about the impact of welfare reform on family structure and living arrangements. First, however, we need to describe the nature of the reforms more fully. Beginning in the early 1990s, many states were granted waivers to make changes to their AFDC programs. As shown in the top panel of Table 1, about half of 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 reform elements of PRWORA include work requirements and both continuous-use and lifetime time limits, as well as the potential for restricting eligibility for certain groups such as teen parents. However, the law leaves substantial scope for states to design and implement their TANF programs. While the nature of PRWORA and waivers varied considerably across states, in general the changes led to a reduction in the generosity of welfare, except in some cases when combined with work or marriage.

It is useful to classify the specific elements of welfare reform according to two dimensions: (1) whether the specific policy is “tightening” (making less generous) or “loosening” (making more generous), and (2) whether the policy affects the financial incentives associated with living arrangements directly or indirectly via effects on behavioral incentives more generally. Table 2 presents the major elements of welfare reform along these two dimensions. This range of reforms were present (to varying degrees) in both state waivers as well as the subsequent state TANF programs. Policies that are general and tightening include work requirements, financial sanctions, and time limits. General loosening provisions include liberalizing earnings disregards (lowering the tax rate on earned income while on welfare), and increasing asset limits. Tightening provisions that directly affect financial incentives associated with living arrangements include “family caps” (whereby welfare benefits do not increase with additional children born on aid), and residency requirements (whereby unmarried teen parents must live in the household of a parent or other guardian). Loosening provisions directly associated with family structure include expanding eligibility for two-parent families.

By combining this two-dimensional representation of welfare reform with the economic model

above, we can make predictions about the impact of welfare reform on family structure and living arrangements. We summarize these expected effects in Table 2 below the dotted lines. The general tightening aspects of reform reduce the attractiveness of being on welfare and therefore mitigate welfare's financial incentives on family structure. Thus, these provisions should lead to increases in marriage and decreases in nonmarital births. The increase in marriage could result from increases in the flow into marriage (initial marriage, remarriage) and/or decreases in the flow out of marriage (divorce/separation). These general tightening aspects of reform might also be expected to cause fiscal stress in heavily-dependent families. This expectation has been borne out empirically in the literature discussed above—reforms led to reductions in welfare participation without substantial increases in earnings and employment. Fiscal stress might cause a wide variety of changes in living arrangements. One *ex ante* concern among critics of welfare reform that is consistent with the model's predictions is that tightening would cause families to double up in the same household (e.g., a mother and child might move in with grandparents or some other relative). This outcome might be especially common for young parents facing termination due to teen residency requirements.⁷

Another possible effect of fiscal stress might be for children to leave their parent's household and move in with relatives. Conversely, parents might leave three-generation households, perhaps because of pressure from their own parents. These changes might also occur when parents face time limits or work sanctions, since children generally are eligible for child-only benefits if they live with neither parent, regardless of the income of the new household.

The general welfare loosening reforms would, of course, lead to the opposite predictions: decreases in marriage, increases in nonmarital births, and decreases in fiscal stress.

The reform provisions associated with specific elements of family structure lead, at least *ceteris paribus*, to clear and simple predictions concerning living arrangements. Family caps should reduce welfare fertility (specifically non-first births), and liberalizing the eligibility requirements for two-parent families should increase marriage. Teen residency requirements should lead to increases in household size, and in particular should lead to more three-generation households. Finally, more

⁷The welfare implications of changed living arrangements are often ambiguous. For example, in the case of teen parents moving in with their own parents, one might argue that one or both of the two youngest-generation children is better off living in a household with older adults. On the other hand, the household is likely to be more crowded, and there may have been good reasons for the middle generation to move out (e.g., physical abuse). DeLeire & Kalil's (2001) find that adolescent children of single parents living in three-generational households have outcomes at least as good as do children living in two-parent families, while replicating the findings of many researchers that teens living with only one parent and no grandparents suffer worse outcomes than teens living with two parents.

generous two-parent provisions, like the elimination of AFDC-UP’s 100-hour rule, should reduce the incentive for (nonmarital) female headship.

A conclusion from this section is that there is no single, clean prediction of the impact of welfare reform on living arrangements. In addition, different aspects of reform lead to opposite predictions about these outcomes. For example, most general elements of reform are predicted to increase marriage, but some (e.g., liberalizing earnings disregards) are predicted to decrease marriage rates. In the end, the net impact is an empirical question. Results will depend on the particular bundles of provisions used by the states, and impacts may vary across sub-groups depending on cultural or financial differences in their responses to different policies.

4 CPS Data, Coding Issues, and Outcomes

4.1 CPS data

We use data from the March Current Population Surveys (CPS) for 1989–2000. The March CPS is an annual demographic file of between 50,000 and 62,000 households and includes detailed information about the persons and families living in each household.⁸ The CPS provides information on demographics and family structure at the time of the survey as well as labor market and income information covering the preceding calendar year. We choose to begin the sample in 1989 for three reasons. First, there was essentially no activity in welfare waivers until the early 1990s, so adding earlier years would do little to identify effects of reform. Second, by starting in 1989, we are able to use data during a complete business cycle, from the peak in the late 1980s, through the early 1990s recession, and then through the long expansion of the 1990s. Third, beginning with the 1989

⁸We use the CPS for several important reasons. First, the March CPS provides very large samples. A recent National Research Council report argues that the CPS is the only nationally representative survey data with large enough sample sizes to identify the effects of welfare reform on subpopulations of interest (National Research Council (2001)). The CPS is also available through the year 2000, allowing us to include several years of data post-TANF implementation. Other data sets are available only with a long lag, making analysis of the post-1996 TANF reforms impossible. For example, the Survey of Income and Program Participation (SIPP) data are available only through the 1996 panel, and SIPP sample sizes are considerably smaller than those in the CPS. Moreover, the SIPP’s primary advantage for our purposes would be its “relationship” roster or matrix, which provides detailed pairwise relationship information for every person pair in a household. Unfortunately, the relationship module is available only in the second wave of SIPP panels. As a result, there would be no extended relationship data available for later years. Some other data sets do not cover the period of TANF implementation at all; for example, the most recent wave of the National Survey of Family Growth is 1995. Others, like the National Survey of American Families, have no pre-reform data at all. Longer panels such as the Panel Survey of Income Dynamics or the National Longitudinal Surveys of Youth offer small samples.

survey allows more continuity in our construction of key outcome variables.⁹

We use two main CPS samples. The first is a sample of all children, whom we define as those aged younger than 16.¹⁰ Our second sample includes women aged 16–54. Because of CPS design, a given CPS household is surveyed in two consecutive CPS March samples. However, if a household’s members move, they will appear only once. Instead of following the initial members, Census Bureau interviewers attempt to interview the current residents of the housing unit. To minimize any biases arising from the possibility of nonrandom movers, for each sample we select only those respondents whose households are in the first four months-in-sample.¹¹ Combining all the years 1989–2000, our samples contain 209,385 children and 240,343 women.

4.2 CPS coding issues and living arrangements

Before we discuss our estimation sample and outcome variables, it is important to explain why we do not analyze measures such as female headship or the presence of subfamilies. To begin, CPS households consist of a group of people who together occupy a housing unit. These persons can be related or unrelated to one another. The head of a CPS household is the person whose name is on the mortgage or lease for the housing unit. A CPS family is a group of two or more persons residing together and related by birth, marriage, or adoption. The primary family includes the head, while a CPS subfamily is a family that does not include the head of the household. There are related and unrelated subfamilies, depending on whether they are related to the primary family.

Female headship and subfamily formation would seem to be prime subjects in a paper on living arrangements. But as the discussion in the above paragraph makes clear, headship and subfamily status depend on who pays the rent. For instance, suppose that a woman and her child live with the woman’s male partner, who is neither the child’s father nor married to the woman. If the male partner pays the rent (he is the head), then the mother-child pair will be coded as an unrelated subfamily.¹² On the other hand, if the mother pays the rent, then the mother-child pair constitutes

⁹CPS coding changed in 1989. In some cases, within-household relationships were treated differently before then, so that earlier data would not necessarily be comparable to data for 1989–2000.

¹⁰We use this restrictive definition of children in order to avoid including a large number of potential teen parents as children, since teen parenthood is potentially endogenous to welfare reform.

¹¹This approach also allows us to avoid covariance estimation problems arising from dependent unobservables for repeatedly-observed households, an issue often left unaddressed in studies using the March CPS.

¹²The male partner would be counted as a primary individual in this case.

a primary family.¹³

This example suggests that in otherwise static living situations, changes in who pays the rent can interact with CPS coding rules to cause a “change” in subfamily status. We believe that in the absence of data constraints, most researchers would hesitate to characterize the two situations just described as different living arrangements.¹⁴ One might still argue that such changes in headship coding will average out under the assumption that the changes do not occur differentially in state-year cells with and without reform. While that argument is of course econometrically correct, we believe the assumption would be inappropriate in a study treating living arrangements as endogenous. We thus do not present estimates of impacts of reform on female headship or the presence of CPS subfamilies. Instead, we focus on other measures not subject to these interpretation problems.

In Table 3, we consider a number of possible changes in living arrangements in response to welfare reform and their implications for CPS family-based outcomes. In the table’s first row, we begin with the rent-changing example from above and summarize the interpretation problems already mentioned. Now consider the second case, in which a mother and her child move into a grandparent’s household. Here the head of household changes (the head was the mother and now is the grandparent). Using a sample of women aged 16–54 will often omit grandmothers and always omit grandfathers from the sample for the pre-reform period. This point is made even more clearly by the example in row 4 of the table (Child moves 1). There the child leaves the mother and joins the grandparent’s household. The grandparent in that case may not be in the sample in either the pre- or post-reform period, while the mother would be in the sample in both periods.

One should draw three conclusions from Table 3. First, depending on the outcome of interest, using a sample of children can make more sense than using a sample of women.¹⁵ Of course, making the child the unit of analysis is also attractive because we are directly interested in the impact of welfare reform on children. Second, one should be very careful about the choice of demographic controls. If the head’s identity is endogenous, then demographic characteristics of the head (e.g., education level and marital status) in households where children live could change endogenously

¹³In this case, the male partner is called a secondary or unrelated individual. Since 1996, such cohabitation cases have been identified in more detailed relationship variables, but the overall coding into subfamily status has remained the same.

¹⁴Although non-unitary models of the household suggest that differences in who holds the lease could affect the distribution of resources within the household.

¹⁵This conclusion need not always hold. For example, if the outcome of interest is fertility, then using a sample of women may be better (because presence of own children is assumed to be a function of reform).

with welfare reforms, making such variables inappropriate controls. Third, empirical results for family-level outcome variables, such as presence of subfamilies or number of families, would be subject to significant interpretation problems. For these reasons, we focus on outcomes that may be measured from CPS data but do not depend on CPS coding rules. While this criterion rules out family-based outcome variables, we are nonetheless able to construct a number of important indicators of living arrangements. We discuss these outcomes in the next subsection.

4.3 Outcome variables

Instead of using these family concepts discussed above, we construct a series of variables describing child and parent co-residence for the children’s sample, and marital status and presence of children in the women’s sample.¹⁶ For the sample of children, we construct three 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.¹⁷ We also create a dummy variable indicating whether the child lives with both a parent and a grandparent. We are not able to construct a satisfactory variable for whether the child lives with a grandparent independently of whether the child also lives with a parent.¹⁸

For the sample of women, we create three dummy variables indicating whether the woman is (*i*) never married, (*ii*) divorced/widowed/separated, or (*iii*) currently married. We also construct a dummy variable indicating whether the woman lives with at least one of her own children in her household.¹⁹

¹⁶In an earlier version of this paper (Bitler, Gelbach & Hoynes (2002)), we also presented results for the number of persons, children, adult men, and adult women in the household; to save space, this part of the earlier analysis has been moved to a separate paper.

¹⁷The CPS provides codes indicating the within-household line number of a person’s parent, if that parent lives in the household. We are thus able to correctly identify 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. The definition of parent here is biological, adoptive, or step-parent; this is how the Census Bureau classifies parental relationships for the March ADF. For the 1987 and 1988 March CPS files, no parent line variable was included, another reason for starting our sample with the 1989 March CPS.

¹⁸CPS coding allows a child’s grandparent to be identified in one of two ways: (*i*) if the grandparent is the head of household or (*ii*) the child’s parent also lives in the household. We believe the first approach may lead to systematically miscoding children as not living with grandparents when, say, a child lives with both a grandparent and an aunt who pays the rent. We therefore restrict our attention to children living with both a parent and a grandparent, an outcome that is observable because the CPS parent line of parents may be used to establish the presence of grandparents. We will still undercount some grandparents for children whose parents are unmarried but both live in the household. In this case, one parent will be coded as unrelated to the child, and if that parent’s parent lives in the household then our approach will fail to code the child’s grandparent as living in the household.

¹⁹We construct the dummy variable indicating presence of an own child using the parent line variable. For cases

Throughout the paper, we will focus primarily on subgroups for whom welfare participation rates prior to reform were relatively high. Our reasoning is that for a large share of women and, to a lesser extent, children, the theoretical effect of welfare reform is known *a priori* to be zero. Pooled samples will tend to average together the zero effect for these observations with the possibly nonzero effects among women and children likely to be affected by reform, possibly obscuring real changes where they occur. Moreover, if effects occur in opposing directions for different subgroups, then using pooled samples will tend to yield estimates of around zero. To the extent that we can identify subgroups for whom welfare reform is most likely to be binding, we can reduce this averaging problem.²⁰

Panel A of Table 4 reports household AFDC participation rates over the calendar years 1988–1992 for the children’s sample by race, ethnicity, and central-city residence.²¹

It is clear from the table that among children, blacks living in central cities are strongly tied to the welfare system, in both absolute and relative terms. About 37% of children in this group live in households that had some AFDC income last year. The closest non-black group is Hispanic children living in central cities, of whom 22% lived in households with some AFDC income.

Panel B reports household AFDC participation rates among women over the same period. It is interesting to note that among women, the population of high school dropouts pooled across race and ethnicity actually has a lower rate of AFDC participation (15.9%) than does the population of black women living in central cities (22.3%).²² In this sense, black, central-city women are actually a better high-impact subgroup than are high school dropouts. Given the participation rates in the

in which a woman is married to a man who is coded as the father of a child in the household, we code the child as also being the woman’s own child. The only cases we will miscode are those in which a woman cohabits with an unmarried partner who pays the rent and is the biological father of a child in the household.

²⁰The common use of educational status to identify high impact groups is not appropriate for the children’s sample because children are too young to have completed their own education. One might consider using the education level of the child’s parent, but not all children have parents in the household; indeed, this is an outcome on which we focus. Instead, one might consider using the education level of the household head. However, the household head can change endogenously with reform if children are switching households.

²¹Here, as elsewhere in the paper, black is shorthand for non-Hispanic black and white shorthand for non-Hispanic white.

²²In constructing Table 4, we did not require women to live with an own child. Since the literature typically considers either all women (or all high school dropouts) out of concern that fertility may be endogenous to reform, this is the appropriate basis for comparison.

table, we choose to focus on subgroups of blacks living in central cities²³ and Hispanics.²⁴ For comparison, we also present results for whites, and for dropout women.

Table 5 reports summary statistics for our child and women samples. These tables show that a large proportion of the women and children were exposed to waivers (13% of women and children) and TANF (more than 30% of both women and children). Most TANF exposure occurred in states that ever had a waiver (22% in both samples) rather than those that never had a waiver (9% of children and 10% of women). About 80% of each sample lived in an MSA, while about one-fourth of each sample lived in a central city. Not surprisingly, a larger share of the children sample than the women sample was black and Hispanic. Overall, more than two-thirds of the children lived with a married parent; 27% lived with an unmarried parent and 3% lived with neither parent. Six percent of all children lived with both a parent and a grandparent. Among the women in our sample, 43% lived with one of their own children. More than half the women were currently married, with 30% reporting never having been married and the remaining 15% being divorced, widowed, or separated. These means vary substantially across subgroups. For example, among black children living in central cities, 9% lived with neither parent, 63% lived with a parent who was unmarried, and 28% lived with a parent who was married. Also, 10% lived with both a parent and a grandparent. Hence, these children were three times as likely as a randomly drawn child to live with neither parent, more than twice as likely to live with an unmarried parent, less than half as likely to live with a married parent, and nearly twice as likely to live with a parent and a grandparent.

4.4 Simple before-after differences in outcomes

One way to assess the impact of welfare reform is to compare simple means of outcome variables before and after reforms were implemented. Table 6 reports such means, together with standard

²³In each metropolitan area, the largest city is designated the “central-city”. The central-city can include other cities if they meet population requirements. Therefore, central-city residence need not imply residence in any particular part of a city, e.g., the “inner city”. Hence, we believe that central-city residence is not likely to respond systematically to reform, an event that could invalidate our use of central-city residence to select the sample. We return to this issue below.

²⁴We would have preferred using only central-city Hispanics, but in estimating our models for this subgroup, collinearity appeared to be a serious problem, most likely because central-city Hispanics are concentrated in a small number of states. The overall AFDC participation rate for Hispanics was 17.2% among children and 10.4% among women, still well above the respective population averages of 10.9% and 5.5%.

errors, for the samples and subgroups on which we focus. In the table, the “Before reform, waiver state” cells report the mean and standard error of the outcome for observations in states that ever had a waiver, in the years before the waiver was implemented. The “After reform, waiver state” cells do the same for observations in these states in years after the waiver was implemented (including years during which TANF was implemented). For nonwaiver states, the before period includes all years before TANF implementation, and the after period includes all years after TANF implementation. By taking differences between the outcomes before and after welfare reform—whether waivers or TANF—we obtain crude estimates of the impact of reform on our outcomes of interest.

Table 6 shows that comparing means for the co-residence variables suggests very large changes in living arrangements for children. Among black, central-city children, the fraction living with neither parent nearly doubles after reform in waiver states, rising from 0.065 to 0.110. There are also large relative (though relatively small absolute) increases in the fraction of Hispanics living with neither parent in both waiver and nonwaiver states.

It is interesting to note that contrary to policy goals, in the white sample, the fraction of children living with an unmarried (married) parent actually rises (falls) slightly after reform, regardless of waiver history. However, the means for black, central-city children tell a different story: the fraction living with an unmarried parent falls by several percentage points. This reduction is almost entirely accounted for by the aforementioned increase in the fraction living with neither parent. Thus, the fraction of black, central-city children living with a married parent remains essentially unchanged. Means for Hispanic children do not show large or consistent changes in the fraction living with either a married or an unmarried parent.

Results for the sample of white women in the fifth column of Table 6 suggest reductions of 1–2 percentage points, or about 2–5%, in the fraction living with an own child. While there is little evidence of a change in this variable for Hispanic women, it falls by about 8% for both black, central-city women and dropouts. These changes could be explained by reductions in fertility, reductions in the rate of co-residence with own children among women who do have children, or both, but for our purposes the important point is that the changes among children and women are consistent in direction.

Among white women, there is evidence that the fraction of women who are currently married

fell slightly. Since the fraction who are divorced, separated, or widowed was unchanged, the fraction who are never married rises. These simple time-series statistics may simply reflect the well-known trend that women have been delaying marriage, rather than implying a reform-induced reduction in marriage rates.²⁵ Generally speaking, the same basic conclusions hold for the high-welfare use subgroups, although in all but one case there is evidence that the fraction who are currently married has actually fallen.

Taken together, these before-after comparisons suggest that welfare reform has been associated with important changes in the living arrangements of black, central-city children. There is less such evidence for Hispanic or other children, although the fraction living with neither parent did increase substantially for both groups. Among women, there is consistent evidence that co-residence with own children fell, while the fraction who were never married rose and the fraction currently married fell.

Before-after comparisons are interesting, but they do not establish either the presence or absence of causal effects of welfare reform on living arrangements. Economic conditions improved greatly at roughly the same time that reforms were implemented, and other trends may have been operating concurrently. Such concerns imply that more careful analysis is warranted in order to control for as many confounding factors as possible. We next describe our empirical approach to dealing with these issues.

5 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.

We estimate probit models²⁶

²⁵Bitler, Gelbach, Hoynes & Zavodny (2002) find evidence that flows into and out of marriage decrease on implementation of both waivers and TANF.

²⁶One might argue in favor of linear probability models in the presence of state and year fixed effects, since fixed effects do not “difference out” of nonlinear models. However, a number of the outcomes we consider have relatively little variance (e.g., a child living with neither parent or living with both a parent and a grandparent). Since linear probability models essentially average marginal effects over the *cdf* of the model’s unobserved components, they are less reliable when the conditional mean of the index function is concentrated in the tails of the *cdf*. This is the principal reason we prefer to use probits. There is also some simulation evidence (see Heckman (1981)) suggesting that relatively small numbers of observations per fixed-effect unit are necessary to yield approximately consistent

where the latent index y_{ist} indicates an outcome for individual i in state s in year t and has the following form:

$$y_{ist} = X_{ist}\delta + L_{st}\alpha + R_{st}\beta + \gamma_s + \nu_t + \epsilon_{ist}. \quad (1)$$

Here, X_{ist} is a vector of demographic characteristics, including controls for the person’s age and its square, race and ethnicity, as well as dummy variables indicating residence in an urban area (MSA) and a central city (we also include dummies indicating whether the CPS identifies a household’s MSA or central-city status). In the children’s sample, these variables all measure the children’s characteristics. Because of possible endogeneity of the household head’s identity, we do not include any characteristics of the head in X_{ist} . In the women’s sample we also include dummies for being a high school dropout and for being a high school graduate.

L_{st} is a vector of state-level labor market variables meant to control for economic opportunities in the state. These variables include current and one-year lags of unemployment and aggregate employment growth rates, as suggested by Blank (2001). L_{st} also includes 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). Unobservable determinants are captured by ϵ_{ist} . All regressions and summary statistics are weighted using the March Supplement person weight.

Our main focus is on the coefficients of R_{st} , a vector of dummy variables for state-level welfare reform. The welfare reform variables we use can be classified into two categories: those related to state waivers in the pre-PRWORA era and those related to post-PRWORA TANF programs. Our main focus is on simple dummy variables indicating whether or not the given reform—waiver or TANF—is in place in a state. 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 TANF plan had been implemented. In general, we coded states as

estimates; since our fixed effects are for states and years, we generally have enough observations so that this problem is likely of little concern.

having implemented a policy in a given month if the policy was implemented by the last day of the previous month. Since our data are collected in March, we code the policy as being in place in a given year if it was in place by the last day of February of that year. Our primary data source for the dating of state reforms is a set of tables available on the website of the Assistant Secretary for Planning and Evaluation (ASPE) for the Department of Health and Human Services.²⁷

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 reform’s effects are constant (over time and across states). In our view, this assumption would be unreasonable even if instantaneous effects could be presumed. Detailed aspects of state reforms and economic conditions can be difficult to observe. Moreover, there is no reason to think that different demographic groups will respond to the same reforms in the same way. Given all this, we strongly believe that the coefficients on R_{st} should be interpreted as averages of heterogeneous treatment effects over the post-reform period.

The top panel of Table 1 reports the first year for which we coded observations in each state as subject to a waiver. The table also lists the states that never implemented major statewide waivers according to ASPE. It is clear from the table, as well as previous literature, that there is substantial variation in the implementation of state waivers across states and time. Unfortunately for empirical researchers, variation in TANF implementation was much less extensive—all states implemented their TANF programs within a 14-month period. The bottom panel of Table 1 shows that for all states, the first March of TANF implementation occurred in either 1997 or 1998. One might thus expect imprecise estimates for the coefficients on TANF variables. However, the TANF coefficients are formally identified, so this precision issue is ultimately an empirical one.

Because we treat waivers and TANF implementation distinctly, our econometric models involve a “dual treatment” specification. It is thus important to consider carefully the interpretation of our estimated reform effects. States may be classified into four groups at any given time: (*i*) those who currently have neither an AFDC waiver nor TANF implemented; (*ii*) those who currently have AFDC waivers implemented; (*iii*) those who currently have TANF implemented and at some point

²⁷Specifically, 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 URL for the relevant ASPE website is http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm. More specific details regarding our construction of reform variables are available on request in a data appendix.

in the past implemented an AFDC waiver; and *(iv)* those who currently have TANF implemented and never implemented a waiver.²⁸

Our focus will generally be on the least restrictive specification of reform effects, which is simply to include dummy variables for three of these four categories. This approach allows distinct effects for each of the three reform regimes. We have chosen to make our baseline group be category *(i)*, state-year combinations for which neither reform is in place. The three coefficients are presented in stylized cases in Figures 1a and 1b. Each figure presents the trend in some outcome variable over time and marks the point where the waiver is implemented (if applicable) and when TANF is implemented. Figure 1a presents the case of a state that had a waiver and Figure 1b presents the case of a state that did not have a waiver. Our key coefficients are β_W , measuring the impact of waivers, β_{TE} , measuring the impact of TANF for a state that had an earlier waiver, and β_{TN} , measuring the impact of TANF for a state that did not have a waiver. Each of these coefficients is measured relative to the baseline period. In our empirical specifications, the waiver dummy is always set to 0 once the state’s TANF program is implemented. Hence, the coefficients on these variables are all comparable to each other (i.e., none of them must be added together to get the total effect of the given reform).

A more restrictive approach would be to constrain the (relative-to-baseline) effect for category *(iii)*—those who currently have TANF implemented and previously implemented a waiver—to be the same as the effect for category *(iv)*—those who currently have TANF and never had a waiver. This constraint allows the effects of TANF in states that ever had waivers to differ from the effects of waivers in those states. However, it entails assuming that TANF’s effects are the same in all states, regardless of waiver history, i.e., $\beta_{TE} = \beta_{TN}$. This specification is the norm in the literature on effects of welfare reform.

A second specification that imposes different restrictions is to constrain the reform effect for category *(iii)*—those who currently have TANF implemented and previously implemented a waiver—to be the same as for category *(ii)*—those who currently have a waiver. This constraint allows reform’s effects to differ in states as a function of waiver history—e.g., because states that ever had waivers may be more committed to reform. However, it entails assuming that within these state

²⁸There is a possible ambiguity in how to code states that implemented both a waiver and TANF after February but before the following March; we have coded these as category *(iii)*. We investigate alternatives in section 7.

groupings, all that matters is whether some reform program—be it waivers or TANF—is in place, so that $\beta_W = \beta_{TE}$. A third, and most restrictive, constraint is to assume that all reform effects—regardless of state waiver history or whether the reform is a waiver or TANF—are homogeneous, $\beta_W = \beta_{TE} = \beta_{TN}$. We do not estimate models with either of the two latter constraints imposed (though the comparison of before- and after-reform means in subsection 4.4 can be interpreted as a model imposing a stripped-down version of the second constraint). We do calculate and report formal test statistics imposing the equal TANF constraint.

If a constraint is appropriate, then there is an obvious efficiency gain to imposing it. However, if the constraint is not appropriate, then the effect we estimate will be some average of the treatment effects of individual policies (see Heckman & Robb (1985)). Particularly when there is no *a priori* basis for signing many of the effects we estimate, such averaging has the potential to mask important heterogeneity in the underlying treatment effects of the different reform regimes. Moreover, allowing the TANF effect to differ by waiver history has the added benefit of providing substantially greater cross-sectional variation than one would otherwise have when imposing equal TANF effects.

Lastly, we note that our standard errors have been adjusted to allow arbitrary correlation within state-by-year cells. Hence, our precision is not spuriously driven by the fact that we use microdata, while the policy variation occurs at the state-by-year level. An additional concern may arise in light of recent work (Kezdi (2001) and Bertrand, Duflo & Mullainathan (2002)) concerning serial correlation with difference-in-differences methodology using state policy reforms, particularly when the reforms stay on once implemented. In section 7, we explore extensions of the basic model that include state-specific trends to soak up unobservables that change over time at the state level, which should go some way toward addressing this issue. Given the nature of our left hand side variables, we believe that our results are likely to be relatively unaffected by this serial correlation issue.^{29,30}

²⁹It is not entirely clear what further steps we could take to address this concern. With only 50 states and state fixed effects included in our models, the asymptotic distributions of the approaches discussed in Kezdi (2001) and Bertrand et al. (2002) may not be appropriate. Nonetheless, we did re-estimate our covariance matrices using state-level clustering, with no apparent changes in the significance of our variables of interest.

³⁰An additional issue concerning standard errors apparently arises because we use microdata, i.e., we include a separate observation for each child or woman in a household. One might worry that we thus inappropriately treat multiple children from the same household as *iid* observations despite the obvious correlation of unobservables for children in the same household. In fact, when the left hand side variables do not vary within households, this issue is not a problem at all: since we weight the regressions and there are few RHS variables that vary within-household (age and race/ethnicity being the lone exceptions among the children, with age, race/ethnicity, and educational attainment varying among women), these specifications are nearly equivalent to running household-level regressions using weights equal to the sum of the individual observations' weights. For the outcomes that vary across individuals

6 Main Results

We report the main results in this section. We begin by presenting our results for our high impact samples by estimating the effect of welfare reform on black, central-city children and women (presented in section 6.1) and Hispanic children and women (presented in section 6.2). We then turn to analogous results for whites and high school dropout women in subsection 6.3. Each of our tables of estimates has two panels, with each column in each panel presenting estimates from a separate regression. The top panel presents estimates for the children’s samples while the bottom panel presents estimates for the women’s samples (with the exception of the dropout’s table, for which there is only one panel with results for women). All tables present probit marginal effects of switching on the reform dummies.³¹ We report only the coefficients on the welfare reform variables. However, as discussed above, each of the models also include controls for age (of the child or woman) and its square, MSA status, race/ethnicity (if applicable), educational attainment (for the women’s samples), central-city status (if applicable), state labor market conditions, state public assistance programs (other than reform variables), and state and year fixed effects.

6.1 Results for black, central-city children and women

Table 7 presents estimates for black, central-city children and women imposing the constraint that TANF effects are homogeneous across states that ever and never had waivers ($\beta_{TE} = \beta_{TN}$). The top panel, which presents the estimates for the children’s sample, suggest that reform is associated with significant effects on living arrangements: TANF is associated with a large increase in the fraction of children living with neither parent, as well as a large reduction in the likelihood of living with an unmarried parent. The results also suggest that reform leads to decreases in the likelihood that a child lives with both a parent and grandparent. For each of these outcome variables, the coefficients on the waiver and TANF reforms imply the same direction of reform, but the magnitudes and statistical significance vary considerably. The bottom panel, which presents the estimates for

(e.g., a child’s living with a married parent or a parent’s living with an own child), this issue may be somewhat more problematic.

³¹To generate these estimates, we used Stata’s `-dprobit-` command, which reports $\Phi_1 - \Phi_0$, where Φ_1 and Φ_0 are the normal *cdf* evaluated with the appropriate reform variable respectively turned on and off, holding all other variables at their sample means. An alternative would be to report average marginal effects over the sample, rather than the marginal effect at the sample mean, but this change rarely affects the results substantively.

the women’s sample, shows relatively consistent, but somewhat weaker results. Reform is associated with decreases in the fraction of women never married, increases in the fraction divorced/separated or widowed, increases in the fraction currently married, and decreases in the presence of an own child in the household.

In Table 8, we relax the constraint on the TANF effects, estimating three distinct treatment effects; any major waiver (β_W), TANF implementation in a state with a prior waiver (β_{TE}), and TANF implementation in a state without a prior waiver (β_{TN}). In general this strengthens the results, while revealing important differences in estimated reform effects for waivers vs. TANF. Wald tests imply that point estimates for TANF effects differ significantly according to ever-waiver status for columns 1 and 2 in Panel A, and for column 1 of Panel B. These differences suggest that the TANF homogeneity constraint may be inappropriate. For the remainder of the paper, we report results only for the unconstrained specifications, noting when the constraints are rejected.

The results for black, central-city children presented in Panel A of Table 8 show consistent, statistically significant, and important effects of reform on black, central-city children. Some of the findings are consistent with the goals of reform, while others are not. The first column shows that all three measures of welfare reform are associated with statistically significant and large increases in the probability of living with neither parent. Potential causes for this finding are a child’s moving in with a grandparent or other relative without her mother or the mother’s leaving the household without the child. The increases range from 3.6 percentage points for waivers, to 7.1 percentage points for TANF in states that did not have a waiver, to 11.8 percentage points for TANF in states that previously had a waiver. We can reject that the TANF effects are equal at the 5% level.

These results illustrate an important finding concerning the variation in the estimated effects of reform. For most outcomes we analyze, the effect of waivers is the smallest, followed by the impact of TANF in never-waiver states, with the effect of TANF in waiver states being the largest. This finding—larger effects of TANF—seems to be shared by studies looking at caseloads, employment and income. This regularity may be due to greater stringency in the TANF reforms, greater awareness of the changes on the part of recipients, and/or more attention placed on TANF reforms in welfare offices. Recall that all treatment effects are measured relative to the pre-reform baseline (e.g., the waiver dummy is turned off when the TANF dummy is turned on), so the larger effects for TANF in states that previously had waivers may be due to the longer time that some reform has

been in place. Alternatively, this may be capturing differences in reform-minded versus nonreform-minded states.

For the neither-parent outcome, the magnitudes of the TANF point estimates are extremely large compared to the low baseline rate. That is, TANF led to more than a doubling of the fraction of black, central-city children living with neither parent. One might worry that these estimated effects are “too large”—after all, it is rare in social science research to find such large effects of policy on behavior. We think this concern is misplaced for three reasons. First, while our probit results do suggest that the contribution of reforms is greater than the simple before-and-after difference of means, the large increase in the fraction of black, central-city children living with neither parent does show up clearly in the raw means reported in Table 6. Second, while the relative effects here are very large, the number of children affected is comparatively small as a fraction of all children: black, central-city children represent fewer than 8% of all children, so that even an increase of 10% in the number of black, central-city children living with neither parent would affect less than one percent of all children.³²

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 parents have left very low-income, 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, for which the dependent variables were indicators for whether the child (*i*) lived with neither parent in a household where total income was at or below the Federal poverty threshold for the appropriate number of residents and (*ii*) lived with neither parent in a household where total income was above the Federal poverty threshold.

The results for the neither-and-poor model implied marginal effects of 2.7, 7.8, and 4.1 per-

³²To estimate the total additional number of black, central city children living with neither parent, we weighted the estimated treatment effects for waivers and the two TANF estimates by the fraction of the post-reform sample falling into each cell. Approximately 25% of the sample’s post-reform black, central city children were in state-year cells for which a major waiver was implemented, with 51% being in ever-waiver state-year cells for which a TANF program was implemented and 24% being in never-waiver, post-TANF state-year cells. The weighted average of the three estimated treatment effects is thus 0.086. Intuitively, this means that if we were to randomly draw black, central city children from our post-reform sample and then assign the appropriate estimated treatment effect, the resulting average effect would be 0.086. Using the March CPS person weights, the estimated average annual number of black, central city children living in pre-reform state-year cells is slightly less than 2.4 million. Multiplying 0.086 by this figure yields a total increase of 206,000 in the number of black, central city children living with neither parent.

centage points for the waiver, TANF-ever, and TANF-never coefficients. Of these, the first and second were statistically significant at conventional levels (with estimated standard errors of 1.6 and 3.7 percentage points, and p-values of 0.03 and 0.01, respectively). For the neither-and-not-poor model, the estimated marginal effects were 0.9, 4.3, and 2.8 percentage points, with only the second estimate being statistically significant (the p-value is 0.06 and the standard error is 2.8). Of course, we do not know the counterfactual fraction of these “neither” children who would have lived in poor households in the absence of reform. Nonetheless, we believe it is reasonable to interpret these results as providing little support for the view that reform is causing children to move into well-off households with neither parent present.³³

Turning to the rest of Panel B of Table 8, we see that reforms are associated with significant decreases in the fraction of black, central-city children living with an unmarried parent, a goal often associated with reform intended to promote two-parent families. This result is particularly pronounced for TANF in ever-waiver states, which is associated with a 17.3 percentage-point (27% over baseline) reduction in the rate of living with an unmarried parent. The estimates are also important for the other welfare reforms, including a 6 percentage point decrease associated with waivers and an 11 percentage point decline with TANF in never-waiver states. The coefficient estimates in the third column show that welfare reform leads to a positive, but not statistically significant, increase in the fraction of black, central-city children living with a married parent. However, if one accepts the significant increase for the fraction living with neither parent and the significant decrease for the fraction living with an unmarried parent, it seems reasonable to take seriously the point estimates of between 2 and 7 percentage point increases in the fraction living with a married parent.³⁴

Results in the last column of Panel A show that the likelihood of a child living with both a parent and grandparent decreased with welfare reform, although the effect is not statistically significant for waivers. Together with the neither-parent results, this finding suggests that any increase in household size through the formation of three-generation households must be outweighed

³³Another natural question is whether the increase in living with neither parent is due to an increase in foster child status. To investigate this question, we estimated a similar probit model, using a dummy indicating foster child status. The coefficient estimates suggest positive but never significant effects of both waivers and TANF implementation in both ever-waiver and never-waiver states; the marginal effects were also insignificant for all the reform dummies.

³⁴The 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, rather than multinomial, probit models.

by mothers who are leaving the child’s household.

Next we turn to estimates for the sample of black, central-city women, presented in Panel B of Table 8. These results also show important effects largely consistent with the results for children but with weaker statistical significance. The results in column 2 show that TANF is associated with a significant and large reduction—between 7 and 8 percentage points (14–16% of baseline)—in the fraction of women who are never-married, a result that would appear consistent with commonly stated policy-making goals. However, this effect is almost entirely offset by an increase in the incidence of divorce and separation, hardly a goal of reform. In both these cases the effects of waivers are very small and insignificant. As with the results suggesting heterogeneous parental co-residence effects for children, we find this pattern striking. It appears that welfare reforms may lead to increases in marriage but poor mate selection, with divorce and separation the ultimate result. The results in column 1 of Panel B show negative effects of reform on the fraction of women who have an own child in the household, though these effects are not precisely estimated.³⁵

Putting these results together yields two important conclusions. First, among black children and women living in central cities, our highest-impact subgroup, there is clear evidence of significant and important effects of welfare reform on living arrangements. Many of these effects are large in both absolute and relative terms. The pattern of results for waivers and TANF is similar, though the effects tend to be larger for TANF relative to waivers, and for TANF in states that previously had waivers. Second, there is important heterogeneity in the nature of the effects. Two objectives often associated with “conservative” welfare reform policies—reducing the incidence of children living in single-parent families and increasing the incidence of living in two-parent families—appear to have been met to at least some degree. However, welfare reform in the 1990s does not appear to be exempt from the law of unintended consequences: there has been a large increase in the number of black, central-city children living with neither their mothers nor their fathers, and there is an increase in divorce and separation among black, central-city women. These are results that few, if any, policy makers sought.

One potential problem in interpreting results for the black, central-city samples concerns the composition of this subsample. If implementation of welfare reform were correlated—for either

³⁵For the Panel B results in Table 8, the TANF homogeneity constraints are rejected (at the 5% level) only for the probability of living with an own child.

causal or other reasons—with changes in central-city residence among blacks, then our results could be picking up compositional changes rather than real effects among a fixed group. For example, one might worry that our neither-parent results are driven by migration of children who already lived with neither parent from one household in the suburbs to another household in a central city. In fact, migration rates across central-city status are greater for those starting out in central cities. For example, among all black children in our sample the rate of migration out of central cities between 1996 and 1997 was 5.4%, by comparison to a 3.6% rate of migration into central cities. Nonetheless, to be sure, we estimated a probit model for central-city residence among all black children. This model has the same RHS variables as those above, and a dependent variable equal to 1 when a person lives in a central city and 0 otherwise. In all cases, the estimated coefficients and marginal effects for the welfare reform dummies were far from statistically significant. We also estimated our living arrangement models for a sample of all blacks and found qualitatively similar results.

6.2 Results for Hispanic children and women

We next move to Table 9, in which we explore how welfare reform impacts Hispanics. We did try estimating our models for Hispanics in central cities, but given their relative concentration in a small number of states (over 70% of central-city Hispanics live in California, New York, and Texas), the models were plagued by excessive collinearity. We have thus chosen to focus on all Hispanics. The results show quite different and somewhat weaker patterns compared to those for the black, central-city samples.

The results in Panel A shows that welfare waivers led to a statistically significant reduction in the propensity of Hispanic children to live with an unmarried parent, and an equivalent increase in the propensity to live with a married parent. The effects are substantial, leading to changes of between 6 and 10 percent over baseline. TANF effects for these outcomes are smaller and statistically insignificant. The results in the first column show that unlike black, central-city children, there is no evidence that welfare reform leads to increases in the propensity of Hispanic children to live with neither parent. Results in the final column also show no evidence of an association between welfare reform and the rate of co-residence with both a parent and a grandparent.³⁶

³⁶The TANF homogeneity constraints are not rejected for any of the models in Panel A.

The results for Hispanic women are presented in Panel B of Table 9. The results in the final column show that waivers and TANF implementation in never-waiver states are associated with significant increases in the propensity of Hispanic women to be married. The effects are relatively large: 3 to 4 percentage point increases (or 5 to 7 percent increases over baseline), although the TANF effect for ever-waiver states is essentially 0. By contrast to black, central-city women, reform is associated with decreases in divorce or separation, although the only significant effect is for TANF in never-waiver states. The results for never-married status are mixed and somewhat harder to interpret. Waivers are associated with significant decreases in the propensity of Hispanic women to be never married, while TANF in ever-waiver states is associated with significant increases in the propensity to be never married. The estimate for TANF in never-waiver states is not statistically significant. Lastly, we find that TANF implementation in ever-waiver states is associated with a significant increase in the fraction of Hispanic women living with an own child (an increase of 3.6 percentage points, or 6.7 percent over baseline). The analogous coefficient for TANF in never-waiver states in regressions for Hispanic children is not significant.³⁷

6.3 Results for white children and women and female high school dropouts

To complete the picture, we report in Table 10 estimates from a sample of white children and women. Given the large sample sizes involved, these results are estimated with a great deal of precision. Nonetheless, the estimates almost universally suggest no impact of reform on living arrangements for either children or women. In fact, the point estimates suggest responses that are the opposite of those found above (e.g., decreases in the fraction of children living with married parents or of women currently married, increases in the fraction of children living with unmarried parents, and decreases in the fraction of women divorced or separated). The two exceptions include a significant increase in never-married status and an equally-sized, significant decline in married status for women in TANF never-waiver states. We view this general lack of significant results as encouraging given that in the sample of Table 4, only 6% of white children and 4% of white women live in households with any AFDC income.

Lastly, we consider results for all women with less than a high school education. Dropout sam-

³⁷The TANF homogeneity constraint is rejected at the 1% level for living with an own child and for being currently married, is rejected at the 9% level for being divorced or separated, but is not rejected for being never married.

ples are commonly used in studies examining welfare programs. The results, presented in Table 11 show essentially no evidence of statistically or substantively significant effects of reform. Given the relatively high rate of welfare participation among high school dropouts and the significant effects for Hispanics and central-city blacks, one might have expected large effects among all dropouts. In results not shown here, when we estimate the models on subsamples of black, Hispanic, and white dropouts, we find the same patterns as above: significant findings for blacks, mixed evidence for Hispanics, and no effects for whites. This finding strongly demonstrates the importance of allowing for heterogeneity across racial and ethnic subgroups in welfare reform’s effects, even within educational groups.

7 Extensions

To gauge the robustness of our findings, we consider in this section a number of extensions to our basic models. These extensions include

1. adding state linear time trends,
2. investigating possible policy endogeneity by including leads of welfare reform variables,
3. exploring sensitivity to alternative coding of ever- and never-waiver states,
4. and including variables measuring detailed characteristics of welfare plans.

We discuss each of these in turn.³⁸ Overall, the extensions do not change the main conclusions in the paper.

First, we add state-specific linear time trends to the basic model. To do this, we can model the unobservable term in equation (1) as $\epsilon_{ist} = \theta_s t + u_{ist}$. The state-specific coefficient θ_s captures factors that evolve differentially across states and may be correlated with reform implementation. Results including these trends show all of the same patterns and results discussed above. For the black, central-city subsample, there are essentially no important differences in the results. For the Hispanic subsample, adding the trends increases the estimated variances so that several of

³⁸The earlier version of our paper, Bitler, Gelbach & Hoynes (2002), included state-specific time trends and an alternate coding of ever- and never-waiver states in its baseline models.

the estimated effects are no longer statistically significant, but the point estimates are basically unchanged. Results for whites and all dropouts are qualitatively almost identical with the state trends included.

Second, we explored the sensitivity of our results to the inclusion of leads of the reform dummies. Leads have been used in previous work to capture either announcement effects or the possibility that reforms are implemented partly because of changes in the dependent variable. Alternatively, lead variables might pick up effects of non-statewide pilot or partial reforms. To address these possibilities, we added a separate one-year lead for each reform dummy to our probit specifications. For the black, central-city children's sample, adding leads makes little difference. Among black, central-city women, adding the leads reduces the precision of our estimates, so that none is statistically significant, but the signs and patterns of the results are unchanged.

Among Hispanic children, estimates with the leads imply a significant TANF-induced increase of three to four percentage points in the fraction living with neither parent (the estimates are similar regardless of waiver history). The magnitude of this effect is comparable to the neither-parent effect for black, central-city children. The leads results also imply a marginally significant, 3.3 percentage point reduction in the probability of living in a three-generation household, due to TANF in ever-waiver states. For Hispanic women, results with leads are generally similar to those reported in section 6.

For white children, there are two substantial changes in the results when leads are included. First, the fraction living with an unmarried parent actually rises by a significant 3.7 percentage points when TANF is implemented in waiver states. Secondly, TANF is associated with a 3 (non-waiver states) to 4 (waiver states) percentage-point decline in the fraction of white children living with a married parent. Given the low baseline welfare participation rate for white children, it is difficult to believe that these figures are entirely causal effects. One possible explanation is that reform states were partly reacting to existing trends away from two-parent households. In fact, adding state-specific linear trends to the leads specification does reduce the TANF coefficient by about a fourth for ever-waiver states in the unmarried- and married-parent specifications. Of course, if there is misspecification for the whites, then one might question our use of the same methodology for blacks and Hispanics. On this point, we think it is important to bear in mind that the results for blacks and Hispanics go in the other direction—if anything suggesting increases in two-parent

families. It thus seems at least possible that whatever misspecification is present is less prevalent for blacks and Hispanics. Lastly, we note that the leads have essentially no impact on the estimates for white women.

A third robustness issue concerns the coding of whether states ever implemented a waiver. Several states implemented waivers late in the pre-TANF period, so that they implemented TANF shortly after implementing their waivers. In our base specification, we include those states in the ever-waiver category. An alternative is to classify these late-waiver states as never-waiver states. The first approach makes sense if having ever implemented a waiver signals that a state is willing to put effort into welfare reform; such states are likely to make more effort in implementing TANF as well. The second approach makes sense if what matters is not unobservable state effort, but rather the simple implementation of a program—for example, this would be the case if potential welfare participants change their behavior on hearing that the rules have changed. We believe that both effects likely have a role, and hence either coding approach is defensible. To gauge the sensitivity of our results to the coding used above, we recoded states as ever-waiver only if they implemented waivers by the end of February of a given year and did not implement TANF until at least the following February.³⁹ We also tried requiring ever-waiver states to have implemented a waiver for at least a year before implementing TANF. In general, these specifications look very much like those presented above, with the effects of TANF implementation among early-waiver states being quite similar to those for TANF implementation among late-waiver states.

7.1 Estimates including detailed reforms

A fourth alternative is to include variables capturing specific features of welfare reform plans. We re-estimated the models already discussed, broadening the set of variables included in the reform vector R_{st} to account for the following policies: time limits, sanctions for failing to comply with work and training requirements, income disregards, whether the state has liberalized treatment of two-parent cases, family caps, and policies requiring that minor case heads live with an adult (relative or otherwise). The variables used in the analysis are presented in Table 12.⁴⁰

³⁹This would classify a state that implemented a waiver in, say, August of 1997 and TANF in January of 1998 as a never-waiver state. Our baseline coding would classify this state as a ever-waiver state.

⁴⁰The main sources for this data are two documents from the Assistant Secretary for Planning and Evaluation (ASPE), available at http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm and

The first variable we added is one to which we refer as the monthly cutoff income: this variable is equal to the monthly amount of income at which a welfare participant would lose her income eligibility for assistance. This variable is constructed using the following formula:

$$C = F + \frac{M}{1 - R},$$

where C is the cutoff; F is the “flat disregard”, i.e., the amount of income the woman may have before any reduction is made to her welfare check; M is the maximum three-person benefit available in the state; and R is the “remainder disregard”, so that $1 - R$ is the state’s marginal benefit reduction rate. Table 12 reports F and R , rather than C , but C is used in the empirical specifications. Before reform, the cutoff variable is linear in the state’s maximum benefit, since the pre-waiver AFDC rules mandated a flat disregard of \$90 and a remainder disregard of 0%. Once reforms were implemented, either or both the disregards may change. We created one “overall” variable equal to the cutoff value, whether pre- or post-reform. However, we also interact the cutoff variable with the dummies representing implementation of each reform regime. We take this approach because it seems reasonable to believe that the effects of changed disregard policy will depend on whether other major reforms are in place.

The other detailed reform variables we use are all dummy variables, indicating whether

1. a state’s time limits result in termination or only reduction of benefits,
2. the state imposes sanctions that result in termination of benefits,
3. the state has eliminated the 100-hour rule governing eligibility for the AFDC Unemployed Parent program,⁴¹
4. the state has a family cap policy preventing benefits from rising when a new child is born,
5. or the state has a rule requiring that minor parents reside with adults (relatives or otherwise) in order to receive welfare benefits.

<http://aspe.hhs.gov/hsp/isp/waiver2/title.htm>. A data appendix providing a comprehensive description of these data is available upon request.

⁴¹While TANF no longer explicitly sets standards for two-parent families, many states have similar eligibility requirements for one-parent and two-parent families. Implicitly, this means that these states have loosened their UP rules relative to those in force under AFDC.

We constructed the detailed reform dummies for each state in its pre-reform, waiver (if applicable), and TANF period. These values were then interacted with the aggregate waiver and TANF-ever or TANF-never implementation dummies, so that we allow the effects of each detailed policy to vary with the reform regime. Main implementation effects are also included in all specifications.

The end result of this exercise is that we have 22 reform variables in each specification—1 main dummy and 6 detailed reform variables for each implementation regime, plus the main cutoff variable. With 4 outcomes per group and 7 groups, we do not have space to discuss all the results in detail. Instead, we touch on some of the statistically significant coefficient estimates.

In general, there is no shortage of statistically significant estimates. However, we do not believe the detailed results suggest any over-arching story concerning our earlier results. In some cases, the results appear to be internally consistent and informative. For example, in the Hispanic children sample, we find that sanctions implemented through waivers are associated with an increase in the probability of living with neither parent and a decrease in the probability of living with an unmarried parent. This could occur if sanctions cause women losing benefits to leave their children with relatives. Among the women sample results, we find that more generous cutoffs are positively associated with the fraction of black, central-city women who have never married. Similarly, more generous cutoffs are associated with an increase in the fraction of Hispanic women who are divorced or separated. These findings could be explained by greater self-sufficiency due to more generous disregards.

However, the results are difficult to rationalize in many other cases. For example, implementation of family caps under all three regimes is associated with a sizable increase in the fraction of Hispanic women who live with an own child. As another example, for the children's sample, minor residency requirements are significantly related to the probability of living with both a parent and a grandparent only among Hispanics, when implemented as part of TANF in a waiver state—but the effect is to *reduce* the number of multi-generation households. On reflection, we do not find the scattered nature of these detailed results particularly surprising. Despite the fact that these reforms are probably the most frequently mentioned in welfare reform discussions, states have implemented many others.⁴² Moreover, we have no way of measuring how strictly or uniformly states enforce

⁴²For example, a RAND report (Williamson, Jackson & Klerman (1997)) attempting to catalogue them all only

the various rules, nor do we have information regarding states' use of exemptions allowed under PRWORA. In at least some cases, states have emphasized caseworker discretion with individual cases as a part of their programs. Thus, it seems likely that substantial unobserved heterogeneity in state policies remains. This heterogeneity will cause bias in estimated effects of specific policies within welfare reform regimes. One advantage of the gross-treatment approach is that it is robust to this sort of bias, since only gross, average effects are estimated. Lastly, our inability to attribute the main findings of the previous sections to any specific reform characteristics is consistent with Bell (2001), as discussed in section 2.

8 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”. Understanding the effects of the significant policy reforms discussed here is of utmost importance. The first round of research has focused, justifiably, on the most immediate measures—welfare caseloads and female employment, earnings, and income. However, living arrangements have been a neglected area of study. In this paper, we push understanding forward by examining the impacts of reform on household composition, marital status of women, and parental co-residence status for 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. Using samples of children and women from the CPS, we estimate pooled cross-sectional models where the effects of reform are identified from differences in timing of the reforms across states. We focus on high-impact subgroups likely to be most affected by welfare reform.

Our results show important effects for children's co-residence with parents and for women's parental and marital status. For example, we find that more black, central-city children are living with a married parent, but more are also living with neither parent. Among black, central-city women, we find that TANF led to a significant reduction in the fraction never married. But this change was almost entirely offset by an increase in the fraction divorced or separated, suggesting

through 1997 runs nearly 100 pages.

the possibility of reform-induced “bad matches”.

Interestingly, we find different patterns among Hispanics than among blacks. While there is no evidence of an increase in the fraction of Hispanic children living with neither parent, there is a marked increase in the fraction of these children living with a married parent. Again, in contrast to effects for blacks, TANF implementation appears to have reduced the fraction of Hispanic women who are currently divorced or separated, while increasing the fraction who are currently married with spouse present.

These findings suggest several primary conclusions. First, welfare reform has had large effects on some important measures of living arrangements among subgroups in which one would have expected reform effects to be concentrated. Second, those effects are neither entirely aligned with the stated goals of reform nor entirely contrary to these goals. Third, there is a great deal of treatment heterogeneity. This heterogeneity concerns both subpopulations and whether reforms were implemented as waivers, as TANF in states that had waivers, or as TANF in states that did not. In numerous cases, standard approaches—pooling the race and ethnic groups, focusing only on high school dropouts, and/or assuming that TANF effects are the same in waiver and nonwaiver states—would lead researchers to erroneously conclude that reforms had been unimportant. Fourth, given the many dimensions along which state-level policies have changed, we may never be able to understand the specific features of welfare reform that lead 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.

Lastly, we wish to emphasize the fruitfulness—indeed, the necessity—of using children as the unit of analysis. Some of our most interesting and important findings concern children’s living circumstances. Results like those concerning the probability of living with neither parent simply could not have been uncovered in a sample using women as the unit of analysis.

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Figure 1a: Treatment Effects When State has Waiver

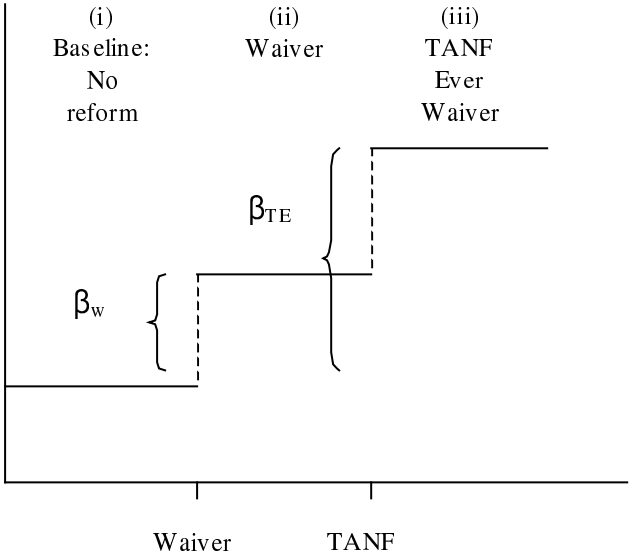
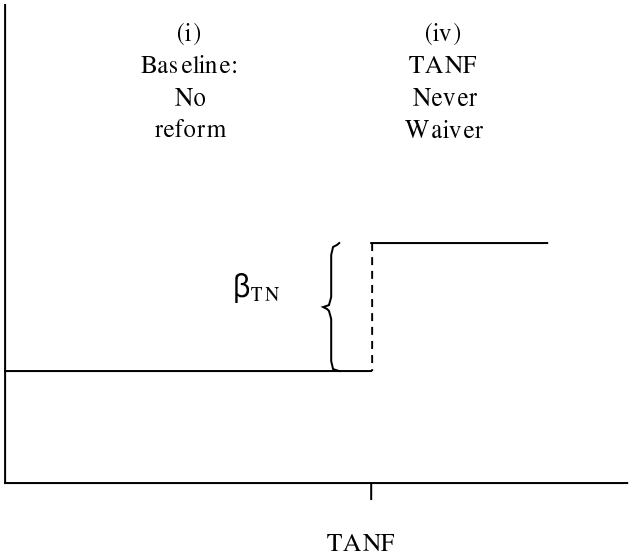


Figure 1b: Treatment Effects with No State Waiver



	Ever had a waiver:					Never had Waiver
	1993	1994	1995	1996	1997	
First Year for which Major Waiver Implemented by March 1	California	Georgia	Arkansas	Arizona	Hawaii	Alabama
	Michigan	Illinois	South Dakota	Connecticut		Florida
	New Jersey	Iowa	Vermont	Delaware		Kansas
	Oregon			Indiana		Kentucky
	Utah			Massachusetts		Louisiana
				Mississippi		Maine
				Missouri		Maryland
				Montana		Nebraska
				Virginia		Nevada
				Washington		New Hampshire
				West Virginia		North Carolina
				Wisconsin		Ohio
						Oklahoma
						South Carolina
						Tennessee
					Texas	
					Wyoming	
					Alaska	
					Colorado	
					DC	
					Idaho	
					Minnesota	
					New Mexico	
					New York	
					North Dakota	
					Pennsylvania	
					Rhode Island	
First Year for which TANF Implemented by March 1					1997	1998
					Alabama	Alaska
					Florida	Colorado
					Kansas	DC
					Kentucky	Idaho
					Louisiana	Minnesota
					Maine	New Mexico
					Maryland	New York
					Nebraska	North Dakota
					Nevada	Pennsylvania
					New Hampshire	Rhode Island
					North Carolina	Arkansas
					Ohio	California
					Oklahoma	Delaware
					South Carolina	Hawaii
					Tennessee	Illinois
					Texas	Mississippi
					Wyoming	New Jersey
					Arizona	Wisconsin
					Connecticut	
					Georgia	
					Indiana	
					Iowa	
					Massachusetts	
					Michigan	
					Missouri	
					Montana	
					Oregon	
				South Dakota		
				Utah		
				Vermont		
				Virginia		
				Washington		
				West Virginia		

Table 1: State implementation of AFDC waivers and TANF programs, by March 1

Note: See text for data sources and explanation.

Table 2: Provisions of welfare reform and the expected effects on family structure

	“Welfare Tightening” Reforms	“Welfare Loosening” Reforms
General Reforms	<i>Policy changes:</i>	<i>Policy changes:</i>
	Work requirements	Liberalized earnings disregards
	Financial sanctions	Liberalized asset tests
	Time limits	
	
	<i>Expected effects:</i>	<i>Expected effects:</i>
	↓ in financial incentives of welfare:	↑ in financial incentives of welfare:
	↑ marriage	↓ marriage
	↓ nonmarital births	↑ nonmarital births
	More fiscal stress:	Less fiscal stress
↑ doubling-up (larger households)	↓ doubling-up (smaller households)	
↑ child living without parents	↓ child living without parents	
Family Structure Specific Reforms	<i>Policy changes:</i>	<i>Policy changes:</i>
	Family caps	Expanded eligibility for two-parent families
	Residency requirements for unmarried teens	
	
	<i>Expected effects:</i>	<i>Expected effects:</i>
	↓ welfare fertility	↑ marriage
↑ 3 generation households	↓ divorce	
	More men in the household	

Table 3: Possible changes in family structure and implications for empirical analysis

Family structure change	Women sample	Children sample	Comments
Change in lease Woman lives with her child and an unmarried, cohabiting partner; name on lease changes from woman to man or vice-versa	Bad	Bad	Head of household changes Number of subfamilies changes
Double-up 1 Mother and children move in with grandparent or other relative	Bad	OK Child in sample both before and after doubling-up	Head of household change, so head's characteristics will change endogenously Family becomes subfamily
Double-up 2 Mother and children move in with non-relative	Unclear	OK Child in sample both before and after doubling-up	Using women sample will be OK only if new household is represented in sample before doubling-up occurs
Child moves 2 Child leaves mother and moves in with grandparent or other relatives	Bad	OK Child in sample both before and after moving	Relative may not be in women sample before child moves in
Unmarried man moves in/out Boyfriend or father moves in or out, or woman and child move in with him	Bad	Bad	Subfamily coding of both woman and child will depend on identity of leaseholder
Marital status change Woman gets married to cohabiting partner	Bad	Bad	Identity of head may change Number of unrelated persons will change Coding of identity of child's parent changes if partner was biological/adoptive father (before marriage leaseholder would be only parent who could be linked to the child, after marriage both could be)

Table 4: Household annual AFDC participation rates, 1988–92

	<i>Central-City Status</i>			<i>Educational Attainment</i>		
	<u>Live in Central city</u>	<u>Live outside Central city</u>	<u>Status Not identified</u>	<u>High school Dropout</u>	<u>High school Graduate</u>	<u>More than High school</u>
<i>A. Children</i>						
Black	0.369 (0.483)	0.267 (0.442)	0.246 (0.431)			
Hispanic	0.224 (0.417)	0.132 (0.338)	0.191 (0.393)			
White	0.084 (0.278)	0.054 (0.227)	0.057 (0.232)			
Pooled	0.200 (0.400)	0.080 (0.272)	0.094 (0.292)			
<i>B. Women</i>						
Black	0.223 (0.416)	0.150 (0.357)	0.143 (0.351)	0.339 (0.474)	0.183 (0.387)	0.069 (0.253)
Hispanic	0.134 (0.341)	0.075 (0.263)	0.121 (0.326)	0.158 (0.365)	0.086 (0.280)	0.039 (0.194)
White	0.042 (0.201)	0.034 (0.180)	0.035 (0.184)	0.100 (0.300)	0.037 (0.189)	0.014 (0.117)
Pooled	0.105 (0.306)	0.046 (0.210)	0.053 (0.225)	0.159 (0.366)	0.061 (0.240)	0.021 (0.144)

Note: All figures calculated using March *psupwgt* variable.

Table 5: Summary statistics, full child and full woman samples

	Children aged < 16	Women aged 16-54
Waiver implemented	0.13 (0.34)	0.13 (0.33)
TANF implemented, ever had a waiver	0.22 (0.41)	0.22 (0.41)
TANF implemented, never had a waiver	0.09 (0.29)	0.10 (0.29)
Termination time limit waiver	0.01 (0.08)	0.01 (0.07)
Full sanctions policy waiver	0.02 (0.15)	0.02 (0.15)
UP 100-hour liberalized by waiver	0.09 (0.28)	0.08 (0.28)
Family cap waiver	0.04 (0.20)	0.04 (0.20)
Minor residency waiver	0.02 (0.14)	0.02 (0.14)
Termination time limit, TANF	0.26 (0.44)	0.26 (0.44)
Full sanctions policy, TANF	0.19 (0.40)	0.20 (0.40)
UP 100-hour liberalized by TANF	0.27 (0.44)	0.27 (0.44)
Family cap, TANF	0.12 (0.33)	0.12 (0.33)
Minor residency, TANF	0.27 (0.45)	0.28 (0.45)
Real maximum benefits for a family of three	5.17 (2.08)	5.17 (2.07)
Unemployment rate	5.59 (1.52)	5.57 (1.51)
Employment growth rate	1.95 (1.50)	1.91 (1.51)
Living in central city	0.24 (0.43)	0.25 (0.43)
Central city status unidentified	0.15 (0.36)	0.15 (0.36)
Living in MSA	0.78 (0.41)	0.80 (0.40)
MSA status unidentified	0.01 (0.08)	0.01 (0.07)
Black	0.159 (0.366)	0.134 (0.340)
Hispanic	0.12 (0.33)	0.09 (0.28)
Age	7.4 (4.6)	34.4 (10.6)
Child lives with neither parent	0.03 (0.18)	
Child lives with unmarried parent	0.27 (0.44)	
Child lives with married parent	0.70 (0.46)	
Child lives with parent and grandparent	0.06 (0.23)	
Woman lives with own child		0.43 (0.50)
Woman never married		0.30 (0.46)
Woman divorced, separated or widowed		0.15 (0.36)
Woman currently married		0.55 (0.50)
N	209,385	240,343

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures in top row of each cell are means. Figures in bottom row are standard deviations. All figures weighted using March *psupwt* variable. See text for more information.

Table 6: Raw means before and after reform

	Children aged < 16				Women aged 16–54			
	Neither Parent	Unmarried Parent	Married Parent	Parent & Grandparent	Lives with Own child	Never Married	Divorced/ Separated	Currently Married
<i>White</i>								
Pre-reform, had waiver	0.017 (0.001)	0.185 (0.002)	0.798 (0.002)	0.037 (0.001)	0.424 (0.002)	0.253 (0.002)	0.144 (0.001)	0.603 (0.002)
Post-reform, had waiver	0.023 (0.001)	0.203 (0.002)	0.774 (0.002)	0.043 (0.001)	0.408 (0.002)	0.266 (0.002)	0.149 (0.001)	0.584 (0.002)
Pre-reform, no waiver	0.018 (0.001)	0.184 (0.002)	0.798 (0.002)	0.036 (0.001)	0.418 (0.002)	0.259 (0.002)	0.138 (0.002)	0.602 (0.002)
Post-reform, no waiver	0.023 (0.001)	0.200 (0.003)	0.777 (0.004)	0.044 (0.002)	0.394 (0.004)	0.271 (0.003)	0.148 (0.003)	0.581 (0.004)
<i>black, central-city</i>								
Pre-reform, had waiver	0.065 (0.004)	0.653 (0.007)	0.282 (0.007)	0.106 (0.005)	0.489 (0.008)	0.491 (0.008)	0.242 (0.006)	0.267 (0.007)
Post-reform, had waiver	0.110 (0.004)	0.607 (0.008)	0.283 (0.007)	0.103 (0.005)	0.449 (0.008)	0.525 (0.008)	0.224 (0.007)	0.251 (0.007)
Pre-reform, no waiver	0.088 (0.005)	0.627 (0.008)	0.285 (0.008)	0.112 (0.005)	0.472 (0.009)	0.511 (0.009)	0.226 (0.007)	0.264 (0.008)
Post-reform, no waiver	0.105 (0.008)	0.620 (0.013)	0.275 (0.013)	0.072 (0.009)	0.427 (0.014)	0.527 (0.014)	0.221 (0.012)	0.251 (0.012)
<i>Hispanic</i>								
Pre-reform, had waiver	0.030 (0.002)	0.281 (0.005)	0.689 (0.005)	0.079 (0.003)	0.543 (0.005)	0.295 (0.005)	0.143 (0.004)	0.562 (0.005)
Post-reform, had waiver	0.044 (0.002)	0.302 (0.004)	0.655 (0.004)	0.077 (0.002)	0.544 (0.004)	0.329 (0.004)	0.134 (0.003)	0.538 (0.004)
Pre-reform, no waiver	0.036 (0.003)	0.406 (0.007)	0.558 (0.007)	0.081 (0.004)	0.509 (0.007)	0.314 (0.006)	0.195 (0.005)	0.491 (0.007)
Post-reform, no waiver	0.050 (0.004)	0.359 (0.009)	0.591 (0.009)	0.082 (0.005)	0.490 (0.010)	0.316 (0.009)	0.157 (0.007)	0.528 (0.010)
<i>Dropout</i>								
Pre-reform, had waiver					0.389 (0.004)	0.448 (0.004)	0.158 (0.003)	0.394 (0.004)
Post-reform, had waiver					0.363 (0.004)	0.513 (0.004)	0.129 (0.003)	0.358 (0.004)
Pre-reform, no waiver					0.353 (0.005)	0.504 (0.005)	0.151 (0.004)	0.345 (0.005)
Post-reform, no waiver					0.315 (0.008)	0.554 (0.008)	0.144 (0.006)	0.301 (0.008)

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures in top row of cell are means. All figures in bottom row are standard errors. “Pre-reform” sample consists of all observations 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. All figures weighted using March *psupwt* variable. See text for more information.

Table 7: Children and women: Results for black, central-city sample, TANF effects constrained to be symmetric

A. Children's Sample: Parental residence				
	<u>Neither</u>	<u>Unmarried</u>	<u>Married</u>	<u>Parent & Grandparent</u>
Any major waiver	0.026 (0.019)	-0.051* (0.029)	0.017 (0.029)	-0.015 (0.014)
TANF enacted	0.090** (0.038)	-0.147*** (0.049)	0.060 (0.053)	-0.055*** (0.020)
Pre-reform mean	0.075	0.642	0.283	0.109
N	13,010	13,074	13,074	12,937
B. Women's Sample: Children & Marital Status				
	<u>Living with Own child</u>	<u>Never Married</u>	<u>Divorced/ Separated</u>	<u>Currently Married</u>
Any major waiver	-0.032 (0.022)	-0.005 (0.019)	-0.009 (0.013)	0.014 (0.016)
TANF enacted	-0.021 (0.040)	-0.074** (0.031)	0.057* (0.030)	0.002 (0.020)
Pre-reform mean	0.482	0.499	0.235	0.266
N	12,847	12,847	12,847	12,847

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are marginal effects and associated standard errors calculated (at sample means) by Stata's `-dprobit-` command. All specifications are weighted using March CPS *psupwgt* variable, with robust variance calculations to account for state-by-year-level clustering. Economic and welfare reform variables refer to the the survey year. Additional control variables are: age of child and its square; 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; income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; and year and state dummy variables.

Table 8: Children and women: Results for black, central-city sample, effects unconstrained

A. Children's Sample: Parental residence				
	<u>Neither</u>	<u>Unmarried</u>	<u>Married</u>	<u>Parent & Grandparent</u>
Any major waiver	0.036* (0.021)	-0.064** (0.030)	0.022 (0.030)	-0.012 (0.015)
<i>TANF in force:</i>				
Ever had waiver	0.118** (0.047)	-0.173*** (0.051)	0.071 (0.055)	-0.047** (0.020)
Never had waiver	0.071* (0.043)	-0.112** (0.056)	0.047 (0.059)	-0.056*** (0.017)
Pre-reform mean	0.075	0.642	0.283	0.109
N	13,010	13,074	13,074	12,937
B. Women's Sample: Children & Marital Status				
	<u>Living with Own child</u>	<u>Never Married</u>	<u>Divorced/ Separated</u>	<u>Currently Married</u>
Any major waiver	-0.022 (0.022)	-0.007 (0.020)	-0.008 (0.013)	0.016 (0.016)
<i>TANF in force:</i>				
Ever had waiver	-0.002 (0.040)	-0.078** (0.032)	0.060* (0.033)	0.005 (0.021)
Never had waiver	-0.051 (0.041)	-0.068* (0.037)	0.056 (0.035)	-0.001 (0.025)
Pre-reform mean	0.482	0.499	0.235	0.266
N	12,847	12,847	12,847	12,847

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are marginal effects and associated standard errors calculated (at sample means) by Stata's `-dprobit-` command. All specifications are weighted using March CPS `psupwgt` variable, with robust variance calculations to account for state-by-year-level clustering. Economic and welfare reform variables refer to the the survey year. Additional control variables are: age of child and its square; 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; income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; and year and state dummy variables.

Table 9: Children and women: Results for Hispanic sample, effects unconstrained

A. Children's Sample: Parental residence				
	<u>Neither</u>	<u>Unmarried</u>	<u>Married</u>	<u>Parent & Grandparent</u>
Any major waiver	-0.003 (0.006)	-0.032* (0.018)	0.037* (0.019)	-0.013 (0.009)
<i>TANF in force:</i>				
Ever had waiver	0.008 (0.007)	-0.006 (0.024)	-0.007 (0.028)	-0.004 (0.015)
Never had waiver	0.012 (0.010)	-0.018 (0.026)	0.004 (0.031)	0.004 (0.017)
Pre-reform mean	0.032	0.322	0.646	0.079
N	33,112	33,436	33,436	33,192
B. Women's Sample: Children & Marital Status				
	<u>Living with Own child</u>	<u>Never Married</u>	<u>Divorced/ Separated</u>	<u>Currently Married</u>
Any major waiver	0.004 (0.013)	-0.024* (0.013)	-0.009 (0.008)	0.028** (0.014)
<i>TANF in force:</i>				
Ever had waiver	0.036* (0.022)	0.030* (0.016)	-0.015 (0.013)	-0.000 (0.017)
Never had waiver	-0.002 (0.021)	0.013 (0.018)	-0.031** (0.013)	0.039** (0.019)
Pre-reform mean	0.531	0.301	0.162	0.537
N	30,543	30,543	30,543	30,543

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are marginal effects and associated standard errors calculated (at sample means) by Stata's `-dprobit-` command. All specifications are weighted using March CPS *psupwgt* variable, with robust variance calculations to account for state-by-year-level clustering. Economic and welfare reform variables refer to the the survey year. Additional control variables are: age of child and its square; 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; income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; and year and state dummy variables.

Table 10: Children and women: Results for white sample, effects unconstrained

A. Children's Sample: Parental residence				
	<u>Neither</u>	<u>Unmarried</u>	<u>Married</u>	<u>Parent & Grandparent</u>
Any major waiver	0.000 (0.002)	0.001 (0.007)	-0.001 (0.007)	0.005 (0.004)
<i>TANF in force:</i>				
Ever had waiver	-0.001 (0.006)	0.010 (0.012)	-0.009 (0.013)	0.007 (0.006)
Never had waiver	-0.002 (0.005)	0.005 (0.012)	-0.003 (0.013)	0.010 (0.007)
Pre-reform mean	0.017	0.185	0.798	0.037
N	138,816	139,000	139,000	139,000
B. Women's Sample: Children & Marital Status				
	<u>Living with Own child</u>	<u>Never Married</u>	<u>Divorced/ Separated</u>	<u>Currently Married</u>
Any major waiver	0.002 (0.007)	-0.007 (0.005)	0.003 (0.004)	0.002 (0.006)
<i>TANF in force:</i>				
Ever had waiver	-0.000 (0.011)	0.004 (0.008)	0.002 (0.007)	-0.005 (0.009)
Never had waiver	-0.009 (0.010)	0.018** (0.009)	0.005 (0.007)	-0.020** (0.009)
Pre-reform mean	0.422	0.256	0.141	0.603
N	172,931	172,931	172,931	172,931

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are marginal effects and associated standard errors calculated (at sample means) by Stata's `-dprobit-` command. All specifications are weighted using March CPS *psupwgt* variable, with robust variance calculations to account for state-by-year-level clustering. Economic and welfare reform variables refer to the the survey year. Additional control variables are: age of child and its square; 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; income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; and year and state dummy variables.

Table 11: Results for dropout women, effects unconstrained

	<u>Living with Own child</u>	<u>Never Married</u>	<u>Divorced/ Separated</u>	<u>Currently Married</u>
Any major waiver	-0.007 (0.012)	-0.003 (0.017)	-0.011* (0.006)	0.016 (0.011)
<i>TANF in force:</i>				
Ever had waiver	-0.004 (0.024)	0.003 (0.023)	-0.004 (0.010)	0.003 (0.015)
Never had waiver	-0.007 (0.024)	-0.022 (0.025)	0.009 (0.013)	-0.002 (0.018)
Pre-reform mean	0.375	0.470	0.155	0.375
N	44,766	44,766	44,766	44,766

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures are marginal effects and associated standard errors calculated (at sample means) by Stata's `-dprobit-` command. All specifications are weighted using March CPS *psupwgt* variable, with robust variance calculations to account for state-by-year-level clustering. Economic and welfare reform variables refer to the the survey year. Additional control variables are: age of child and its square; 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; income limit (as percentage of FPL) for pregnant women to be eligible for Medicaid; and year and state dummy variables.

Table 12: Selected characteristics of state AFDC waiver and TANF programs

	Waivers							TANF						
	Term TL	Full Sanc.	Flat Dis.	Rem. Dis.	100-hr. Rule	Minor Res.	Fam. Cap	Term TL	Full Sanc.	Flat Dis.	Rem. Dis.	100-hr. Rule	Minor Res.	Fam. Cap
Alabama								X	X		.2	X	X	
Alaska								X		150	.33	X	X	
Arizona	X				X	X	X	X	X		.3	X	X	X
Arkansas							X	X	X		.6	X	X	X
California			120	.33	X					225	.5	X	X	X
Colorado								X	X			X	X	
Connecticut	X	X		1.00	X	X		X	X		1.00	X	X	
DC								X					X	
Delaware					X	X		X	X			X	X	X
Florida								X	X			X	X	
Georgia							X	X	X			X	X	X
Hawaii	X		128	.488	X			X	X	128	.488	X	X	
Idaho								X	X		.4	X	X	X
Illinois		X		.67			X	X	X		.67	X	X	X
Indiana					X			X	X			X	X	X
Iowa	X	X		.6	X	X		X	X		.6	X	X	
Kansas								X	X		.4	X		
Kentucky								X	X				X	
Louisiana								X	X	120		X	X	
Maine								X		134	.2		X	
Maryland						X			X		.26	X	X	
Massachusetts		X	120	.5	X	X	X	X	X	120	.5	X	X	X
Michigan		X	200	.2	X	X			X	200	.2	X	X	
Minnesota					X			X			.36	X	X	
Mississippi					X		X	X	X				X	X
Missouri						X		X					X	
Montana					X			X		200	.25	X	X	
Nebraska					X			X	X		.2	X		
Nevada								X	X		.2	X	X	
New Hampshire								X			.5		X	
New Jersey							X	X	X		.5	X	X	X
New Mexico								X	X	150	.5	X	X	
New York								X			.42	X	X	
North Carolina								X				X	X	X
North Dakota								X	X		.27		X	X
Ohio								X	X			X	X	
Oklahoma								X	X	120	.5	X	X	
Oregon		X			X			X	X		.5	X	X	
Pennsylvania								X	X		.5		X	
Rhode Island										170	.5	X	X	
South Carolina								X	X			X	X	
South Dakota								X	X		.2		X	
Tennessee								X	X				X	X
Texas								X				X		
Utah								X	X	100	.5	X	X	
Vermont		X	150	.25	X	X			X	150	.25	X	X	
Virginia						X	X	X	X		1.00	X	X	X
Washington								X			.5	X	X	
West Virginia		X						X	X		.4	X	X	
Wisconsin					X		X	X	X			X	X	X
Wyoming								X	X	300		X	X	

Notes: Column headings refer to details of waiver/TANF policies as follows; Term TL refers to termination time limits, Full Sanc. refers to full sanctions, Flat and Rem. Dis. refer to flat or remainder earnings disregards, 100-hr. Rule refers to loosening of the UP 100-hour rule, Minor Res. refers to teen parent residency restrictions, and Fam. cap refers to family size caps for benefits. Flat disregards are measured in nominal dollars per month, with \$90 per month the default. Remainder disregards are measured as a rate, e.g., Hawaii disregards 48.8% of earnings. See text for more detailed explanation of these rules.