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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 household size, 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, in states that never had waivers, TANF was associated with a reduction of 14 percentage points 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 8 percentage points, essentially doubling the baseline level. Third, there is a great deal of treatment heterogeneity both with respect to racial and ethnic groups, 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 was 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 that TANF effects are the same in waiver and nonwaiver states - would generally not uncover these important changes in living arrangements.

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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) considerably changed the economic incentives facing low income individuals with children or considering having children. In particular, these reforms dramatically reduced the generosity of state welfare programs by imposing time limits, strengthening work requirements, and limiting the population eligible.

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 to[1]

> 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-

[1]The 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 importantly 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.[2] 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.

Our data come from the March CPS for survey years 1989–2000. We augment these data with

[2]A number of other possible data sets have different coding rules; using them could to some extent avoid these problems. We selected the CPS for several reasons. The March CPS provides both a large sample (62,000 households) along with detailed information about who lives in the household. It 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 relationship data available for years after 1996. 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.

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 outcomes discussed above to these state-level variables, a small number of demographic controls, state and year dummy variables, and state-specific time trends. Hence, the effects of welfare reform are identified through variation in the timing and incidence of reforms across states. We estimate reform effects using general samples including all children or women, as well as samples focusing on populations highly likely to be affected by welfare reform.

Our results show that among Black children living in central cities, welfare reform leads to increases in household size, with most of the growth attributable to increases in the number of children and not to increases in the number of adult men. Among states that did not have waivers, we find that TANF implementation is associated with a large—14 percentage point—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—8 percentage points—in the fraction of these children living with neither parent. We find somewhat different effects for Hispanic children, with increases in the number of adult men in the household and increases in the fraction of children living with married parents, but no evidence of an increase in the fraction of children living with neither parent.

For Black, central-city women, we find that TANF implementation in states that did not have waivers led to a significant reduction in the fraction who have never married. However, we also find an increase in the fraction divorced or separated. In contrast, 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. Estimating the same models among a pooled sample of all children or all adult female high school dropouts, we find essentially no significant effects of reform. Among the full sample of women, there is evidence of a moderate reduction in the fraction currently married and an increase in the fraction who are divorced, as well as a moderate reduction in the fraction living with an own child.

Estimates including more detailed measures of specific aspects of state reforms (e.g., sanctions, time limits, or more generous earnings disregards) yield no consistent implications concerning the channels through which reforms have operated.

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 we conclude in section 7.

2 Literature Review

The effects of recent welfare reform measures is the subject of a now-extensive literature. A large share of this research has focused on the impact of welfare reform 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)). This literature finds 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.[3] Blank (2000) cautions that controlling for local economic conditions is very important for identifying the impact of welfare reform. These caseload studies generally find that the strong macroeconomy played a relatively large role in the welfare caseload declines in the welfare waiver period and a somewhat smaller role in the TANF period, with some differences across studies in the magnitude of these effects related to how they model persistence.

This 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—across states, no clear pattern of statistically significant results seems to have developed in the literature. This lack of a systematic pattern is particularly striking given that most researchers have relatively strong priors concerning the likely effects of, say, time limits on caseloads (the relationship should be negative). Expectations are weaker concerning the direction of effect of

[3] Grogger & 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.

many reforms on living arrangements. Our inability to attribute the effects of welfare reform on living arrangements to specific characteristics of reform is thus not surprising given the caseload literature.

Bell's (2001) second observation is 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). Given the discussion in Bell (2001), we believe our choice not to consider dynamic issues causes is appropriate. While a systematic analysis of dynamic issues could be fruitful, for space considerations it is beyond the scope of our study.

Another series of studies focuses on the relationship between welfare reform and employment, earnings, and income using nationally representative household survey data. For example, Schoeni & Blank (2000) find that welfare waivers lead to increases in women's own earnings and total family income, and decreases in poverty among high school dropouts. They find that the 1996 federal welfare reform was associated with a reduction in poverty rates but no significant change in women's earnings. Using micro data from the CPS, Moffitt (1999) finds that waivers led to increased work and earnings among the least educated groups of women. Other recent studies show that the estimated impacts of welfare reform are somewhat sensitive to the richness of the controls for labor market conditions and other policy factors. For example, Ribar (2000) uses detailed location information together with the SIPP and finds that state welfare reforms had little effect on labor force participation after controlling for location- and skill-specific economic opportunities. Meyer & Rosenbaum (2000) use CPS data to examine the effects of many programs for the poor, including welfare waivers, the Earned Income Tax Credit (EITC), and Medicaid. They conclude that welfare waivers led to moderate increases in the employment of low educated women, while expansions in the EITC led to more substantial effects.[4]

Several recent papers have studied the effects of welfare waivers and TANF on headship, marriage, and fertility (e.g., Moffitt (1999), Schoeni & Blank (2000), Hu (2001), Casper & Bryson (1998), Cox & Pebley (1999), Fitzgerald & Ribar (2001), and Horvath & Peters (2000)). Schoeni & Blank (2000) find welfare waivers led to an increase in the fraction of high school dropouts who

[4]Also 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.

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. Horvath & Peters (2000), using state-level aggregate data through 1996, find a negative association between nonmarital birth ratios and state level waivers from AFDC.

If examining living arrangements is the goal, a number of concerns arise regarding previous research. First, many of the studies use data only through 1996, so that only waiver effects may be identified. Second, while caseloads, employment, marriage, and fertility are important outcomes, studies focusing on them do not address broader issues concerning living arrangements. For example, none of these outcomes addresses concerns regarding "doubling-up". Third, 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. 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, rather than using the cross-state variation in timing and incidence of recent welfare reforms. For example, in a report covered on the front page of the *New York Times*, Dupree &

[5]For 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.

[6]Authors 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 central-city Black 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.

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, 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 and state-specific linear trends, we address this concern in this paper.

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), 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". This suggests that an important dimension of living arrangements to explore under welfare reform is movements of mothers and their children into other households.

3 Family structure, living arrangements, and welfare reform in the 1990s

Empirical studies of family structure typically begin with a Becker-style economic model, in which individuals make utility-maximizing choices about marriage, fertility, and living arrangements. In models of the female headship decision (e.g., Hoynes (1997)), a woman compares her maximum attainable utility if she becomes a female head to her maximum utility if she does not. 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 (as long as the direct utility of female headship is not too large). Similar models can be used to predict that increases in welfare generosity lead to decreases in marriage and increases in divorce and nonmarital births. Whether welfare programs encourage the establishment of separate households depends on the treatment of income received by unmarried women's cohabiting partners. If partners' income is disregarded by the welfare system, then marriage might be affected even if cohabitation is not.

Applying these types of models to predict the effects of welfare reform on living arrangements requires a detailed understanding of the nature of reform. 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 (the details of which we explain below), 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. As noted in the introduction, the goals of both types of welfare reform are to aid children in the care of their parents or other relatives, encourage work, reduce dependency on public assistance, reduce nonmarital births and female headship, and, more generally, increase the formation of two-parent families.

In what ways did the reforms change AFDC in order to achieve these goals? The nature of PRWORA and waivers varied somewhat across states, but in general the changes led to a reduction in the generosity of welfare, except in some cases when combined with work or marriage. PRWORA includes 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. Many features later incorporated in PRWORA appeared first in state waivers. We have summarized several of the most common and prominent features of state waivers and TANF programs. These are presented in Table 2.[7] One can classify these specific changes to AFDC as "tightening" (making less generous) or "loosening" (making more generous). The tightening aspects include termination time limits and monetary sanctions to penalize participants for not complying with work or other rules. There were also loosening, pro-work policies, such as liberalizing the disregard and earnings reduction policies, as well as expanding welfare for two parent families. Overall the reforms are usually characterized as pro-work, pro-marriage, and welfare-tightening.

3.1 Expected effects of reform

Some aspects of reform lead, at least *ceteras paribus*, to clear and simple predictions concerning living arrangements. For example, liberalizing the 100-hour rule for two-parent families should increase marriage. More generally, as families leave welfare for work and distance themselves from other marriage-discouraging aspects of public assistance programs, we would also expect to see marriage rates increase. While clean predictions can be made for marriage, predictions are ambiguous for other interesting outcomes. Consider cohabitation: if partners can no longer receive benefits while cohabiting and remaining unmarried, then the woman may bring less financially to the partnership, reducing her attractiveness to potential male partners. Another way of making this point is to observe that both the demand for and the supply of men to live in the household may change with welfare reform.

While some reforms directly affect incentives for specific living arrangements, welfare reform might also be expected to cause significant 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

[7] The main source for this data is the Assistant Secretary for Planning and Evaluation (ASPE), http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.htm. A data appendix providing a comprehensive description of these data is available upon request.

might cause a wide variety of changes in living arrangements. Consider household size. One *ex ante* concern among critics of welfare reform was 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.[8]

Lastly, consider co-residence of parents and their children. One 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.

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 data at the household, family, and person level. The survey 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.[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

[8]The 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).

[9]CPS coding also 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.

[10]We use this restrictive definition of children in order to avoid including a large number of potential teen parents

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 household. 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 will be useful to begin with a review of family and household concepts as they relate to the CPS. 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. A CPS subfamily is a family that does not include the head of the household. More specifically, if a family includes both the household head and a group of people who by themselves are related as parents, children, or spouses, then the group not including the head is called a related subfamily. The related subfamily and other members (e.g., the household head) of the larger family together constitute the household's primary family.[12] If a group of people in the household are related, but none of these people is related to the head, then the group is called an unrelated subfamily.

Subfamily formation would seem to be a prime subject in a paper on living arrangements. But as the discussion in the above paragraph makes clear, coded subfamily status depends on who pays the rent, since headship determines family coding. 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

as children, since teen parenthood is potentially endogenous to welfare reform.

[11]The CPS interviews residents of a household for four consecutive months, after which they are left alone for 8 months. The household is then recontacted for another four months. Whether respondents in later months are representative of those in earlier months depends on whether people moving into households experiencing either changes in membership or the departure of all former members are representative of the people leaving such households. To minimize any problems related to this issue, we simply exclude respondents whose households are in months 5–8 of survey participation. This exclusion 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.

[12]Even in the absence of any related subfamilies, the family that includes the head is called the primary family.

woman. If the male partner pays the rent (he is the head), then the mother-child pair will be coded as an unrelated subfamily.[13] On the other hand, if the mother pays the rent, then the mother-child pair constitutes a primary family.[14]

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.[15] 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 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 the grandmother's household. Here the head of household changes (the head was the mother and now is the grandmother). Using a sample of women aged 16–54 will often omit the grandmother 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 grandmother's household. The grandmother 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.[16] Of course, making the child the unit of analysis is also attractive because we are directly interested in the impact of

[13]The male partner would be counted as a primary individual in this case.

[14]In 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.

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

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

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 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

Our first set of outcome variables relates to household composition and is constructed identically for the children and women samples. We count the total number of people in each household, as well as the number of children (aged less than 16), adult women (16 or over), and adult men (aged 16 or older). These variables give us a simple yet important snapshot of household composition. First, measuring the total number of people in households allows us to address the question of doubling-up, if somewhat indirectly. Second, by tracking adult men separately, we can examine whether welfare reform has increased the number of men living in households where children live. This variable thus allows us to assess indirectly the two-parent policy goal. Counting the number of children and adult women completes the picture of household composition.

For the children sample, we also create variables to investigate whether children are more or less likely to live with parents after welfare reform. We also examine the marital status of their parents using 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 child lives with

[17]The 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 head. The definition of parent here is biological, adoptive, or step-parent; this is how the Census Bureau classifies parental relationships. 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.

a grandparent independently of whether the child also lives with a parent.[18]

For the sample of women, we augment the household composition measures with variables capturing current marital status and co-residence of parents and children. Specifically, we look at the impact of reform on the propensity to be never married, divorced/separated/widowed, or 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]

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.

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.[20] 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. More than 35% of children in this group live in households that had some AFDC income. The closest non-Black group is Hispanic children living in central cities,

[18] CPS 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.

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

[20] The 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.

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%).[21] 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 table, we choose to focus on subgroups of Blacks living in central cities[22] and Hispanics.[23]

Tables 5 and 6 report summary statistics for our two pooled 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). This exposure was evenly divided between states that had ever had a waiver (15% in both samples) and those that never had a waiver (16%). 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 (16% of the children sample vs. 13% of the women sample) and Hispanic (12% of the children sample vs. 9% of the women sample). The average age of the children was around 7 and that of the women was about 34. Again not surprisingly, children's households contained more people than did women's households (4.3 people per household for the children sample vs. 3.1 for the women sample), with the difference in number of children (2.3 per household for the children sample vs. 1.0 for the women sample) playing a major role. In both samples, the average number of men 16 or older was slightly less than one.

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

[21] In 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.

[22] It is important to understand that central-city residents are those people whose households are located in a city that is at the center of a metropolitan area. 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.

[23] We 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%.

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.

In a number of cases, means for the outcome variables change dramatically when we look at the Black, central-city and Hispanic subgroups.[24] For example, among Black children living in central cities, 9% lived with neither parent, 63% lived with a parent who was unmarried, 28% lived with a parent who was married, and 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. Differences from the pooled sample are much less pronounced among Hispanics. Among these children, 4% lived with neither parent, 65% lived with a parent who was married, and 32% lived with an unmarried parent; these figures are relatively similar to the pooled sample. The one major difference between the Hispanic and pooled samples is that 8% of Hispanics lived with both a parent and a grandparent vs. only 6% of the pooled sample.

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. Tables 7 and 8 report such means, together with standard errors, for the pooled samples and the 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 7 suggests little evidence of changes in the household composition variables for the children sample. One might argue that relatively small changes in household size in the pooled sample are

[24]These means are reported in Tables 9–16 below, so we do not report them in Tables 5 and 6.

substantively significant given the small fraction of households for which welfare reform is likely to matter. For example, the .07-person reduction of total household size in the pooled sample would have to be quite large if it were driven entirely by changes in welfare-relevant households. However, the Black central-city and Hispanic subsamples, for which welfare use is comparatively frequent, show little evidence of changes in household composition. These simple before-after tabulations thus provide no evidence of either doubling-up or increases in the presence of men. Similarly, the means for women in Table 8 do not suggest large changes in household size or composition for them.

By contrast, comparing means for the co-residence variables suggests very large changes in living arrangements. For example, in the pooled sample, the fraction of children living with neither parent rises after reform by 52% in waiver states and 31% in nonwaiver states. Among Black, central-city children, the fraction living with neither parent nearly doubles after reform in waiver states, rising from 0.062 to 0.117. 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 pooled 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. As for co-residence with both a parent and a grandparent, the fraction of Black, central-city children in this situation rises by two percentage points, or about 20%, in waiver states. By contrast, this fraction falls by 4.5 percentage points, or about 40%, in nonwaiver states. There is little change in the fraction of Hispanic children living with both a parent and a grandparent.

Results for the pooled sample of women in the fifth column of Table 8 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 10% 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.

In the pooled sample, 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, which would be contrary to expectations. 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. There is little evidence of changes in household composition from these simple comparisons.

While before-after comparisons are interesting, 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.

Our typical linear[25] regression has the form

[25] A number of our outcomes are binary. For those outcomes, we can consider y_{ist} a latent index and use probit

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

Here y_{ist} is some outcome for individual i in state s in year t. 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 that the CPS does not identify 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 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, a dummy indicating whether the state extends benefits to two-parent families (AFDC-UP), and measures of a state's Medicaid generosity. The γ_s and ν_t terms represent state and year fixed effects, while $\theta_s t$ is a state-specific linear time trend. The state (year) fixed effects control for unobserved factors that differ across states and not over time (over time and not across states), while the state-specific trends are meant to capture factors that are unrelated to state reforms but evolve differentially 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

rather than OLS estimation of the scaled coefficients. One 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 estimates; since our fixed effects are for states and years, we generally have enough observations so that this problem is likely of little concern.

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 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.[26]

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

Our focus will generally be on the least restrictive specification of reform effects, which is simply

[26]Specifically, 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.

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 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.

The econometric structure just described allows us to estimate gross effects of reform in a relatively simple evaluation framework. However, that approach cannot tell us which aspects of reform are driving any estimated effects. For this reason, we also estimate specifications that include variables representing state reform policies in more detail. The policies considered include 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). We discuss the specification of these variables in somewhat more detail in subsection 6.3.

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 (2001)) concerning serial correlation with difference-in-differences methodology using state policy reforms, particularly when the reforms stay on once implemented. Part of our motivation for including state-specific trends is 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.[27,28]

[27] It 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, we are skeptical that the asymptotic distributions of the approaches discussed in Kezdi (2001) and Bertrand et al. (2001) will 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.

[28] An 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

6 Main results

We report the main results in this section. We begin in subsection 6.1 by reporting estimates for the children's sample using the "gross-treatment" specification (i.e., using only the overall implementation dummies). We then turn to analogous results for women in subsection 6.2. Each of our tables of estimates has two panels, with each column in each panel presenting estimates from a separate regression. The top panel of the table presents coefficient estimates from OLS regressions of household composition measures on reform and other variables. The bottom panel presents probit marginal effects of switching on the reform dummies for measures of parental co-residence and marital status (for the children's sample) or own marital status and presence of own children in the household (for the women's sample).[29] 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), central-city status (if applicable), state labor market conditions, state public assistance programs (other than reform variables), state and year fixed effects, and state-specific linear time trends.

In subsection 6.3, we turn to a discussion of estimates using detailed reform characteristics.

6.1 Gross-treatment results for children

Table 9 presents estimates for Black, central-city children imposing the constraint that TANF effects are homogeneous across states that ever and never had waivers ($\beta_{TE} = \beta_{TN}$). The estimates in the top panel suggest little significant impact of reform on household composition. Results in Panel B suggest more clearly that reform is associated with significant effects on living arrangements: TANF brings a large increase in the fraction of children living with neither parent in nonwaiver states, as

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 (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.

[29] To generate these estimates, we used Stata's -dprobit- command, which reports $\Phi_1 - \Phi_0$, where Φ_1 and Φ_0 are the normal c.d.f. 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.

well as a large reduction in the likelihood of living with an unmarried parent.

In Table 10, 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}).

Panel A of Table 10 provides a clear picture of changes in household composition for central-city households with Black children. In states that had waivers, household size grew with the implementation of both waivers and TANF. In both cases, the increase in household size comes primarily through increases in the number of children in the household, though this increase is not precisely estimated for the waiver dummy. There is also evidence that the number of adult women increased with TANF implementation in waiver states. Some of these effects are quite large. For example, the coefficient estimate of 0.669 children in response to TANF in these states represents an increase of 25% over the baseline. These results are consistent with more doubling-up. The ratio of increased children to increased women for TANF implementation in waiver states was about 4 to 1, suggesting that several children were added for every woman. One interpretation of this result is that any doubling-up occurred for relatively large families. A complementary explanation is that some children joined households where other children lived, but did so without their mothers. Interestingly, there is no evidence of any increase in the number of adult men living in central-city households with Black children.

The results in Panel A suggest that the homogeneity constraints imposed in Table 9 lead to substantively quite different results for the total number of people and the number of children in households. For both of these outcomes, removing the constraint of homogeneous TANF effects causes the sign and magnitude of the any-waiver coefficient to change. For the total number of people in the household, the estimated effect is significant without the constraint and insignificant with it. The estimated effects of the two TANF dummies are quite dissimilar for these two outcomes, and it is thus not surprising that the F statistics testing equality of the two coefficients reject at the 0.02 and 0.03 levels, respectively, for columns [1] and [2]. For the other two columns, the TANF homogeneity constraint is not rejected at conventional levels. For the remainder of the paper, we report results only for the unconstrained specifications, noting when the constraints would have been rejected.

In the bottom panel of Table 10 we turn to the impact of reform on parental co-residence.

These results show striking heterogeneity in the the impact of reform across children, even within the group of Black children living in central cities. In particular, the first column shows that welfare reform has been associated with an increase in the probability of living with neither parent. While the effect on living with neither parent is statistically significant only for the TANF-never-waiver dummy (the p-value for TANF implementation in waiver states is 0.108), the estimates are positive for both the waiver and TANF-ever-waiver dummies. Potential causes for this finding are a child's moving in with a grandparent without her mother or the mother's leaving the household without the child.

The magnitudes of the TANF point estimates are extremely large compared to the low baseline rate (e.g., the 7.8 percentage-point estimate in nonwaiver states implies a doubling of the fraction of Black, central-city children living with neither parent).[30] 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 7. 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 a change for 7.8% of Black, central-city children affects substantially less than one percent of all children.[31]

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 and entered households headed by other relatives with relatively high 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

[30] Recall from Table 7 that before reform, the fraction of Black, central-city children living with neither parent was 6.2% in waiver states and 8.0% in nonwaiver states.

[31] Nonetheless, the human effects of a 7.8 percentage-point effect are not small: this effect means that approximately 180,000 Black, central-city children newly live without a parent as a result of TANF in nonwaiver states.

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 1.8, 7.9, and 4.8 percentage points for the waiver, TANF-ever, and TANF-never coefficients. Of these, only the last was statistically significant at conventional levels (with an estimated standard error of 2.5 percentage points). For the neither-and-not-poor model, the estimated marginal effects were 0.4, 3.7, and 3.2 percentage points, with only the final estimate being marginally statistically significant (the p-value is 0.09 and the standard error is 3.2). 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.[32]

Turning to the rest of Panel B of Table 10, we see that reforms are associated with a decrease 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 never-waiver states, which is associated with a 14.2 percentage-point (23% over baseline) reduction in the rate of living with an unmarried parent. Again, the estimates for the waiver and TANF-ever-waiver dummies have the same sign, though they are not statistically significant. The coefficient estimates in the third column show that the fraction of Black, central-city children living with a married parent is not significantly different from zero for any of the three reform dummies. However, if one accepts the significant TANF-never increase of 7.8% in the fraction living with neither parent and the significant decrease of 14.2% living with an unmarried parent, it seems reasonable to take seriously the point estimate of 6.4% (23% over baseline) for the fraction living with a married parent.[33] However, given that there is no increase in the number of men living in central-city households with Black children, increases in the fraction of these children living with

[32]Another 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 and highly significant effects of both waivers and TANF implementation in waiver states. However, the marginal effects were not significant for any of the reform dummies. This contrast occurs because of the very low baseline rate of being a foster child: only 1.3% of Black, central-city children were reported in the CPS to be in formal foster care. Given this low baseline rate, the model fit is quite poor, with the predicted baseline being only 0.00367% at the means of the RHS variables. Re-estimating this probit for all Black children yielded substantively similar findings.

[33]The dummy variables for neither parent, unmarried parent, and married parent are exhaustive and mutually exclusive. However, the estimated marginal effects need not sum exactly to zero because the results come from single-equation, rather than multinomial, probit models.

a married parent must be due, on net, to new marriages of women already cohabiting with male partners.

Results in the last column of Panel B show that the likelihood of a child living with both a parent and grandparent decreased with TANF implementation, with the effect again being significant only in nonwaiver states. Together with the neither-parent results, this finding suggests that any increase in doubling-up through the formation of three-generation households must be outweighed by mothers who are leaving the child's household.[34]

Putting these results together yields two important conclusions. First, among Black children living in central cities, our highest-impact subgroup, there is clear evidence of significant and important TANF effects among states that did not implement major statewide waivers. These effects are large in both absolute and relative terms, suggesting that in at least some places welfare reform has had substantively significant effects on the living circumstances of children likely to be subject to reforms. The pattern of results for waivers and TANF in states that had waivers is similar, though the results are not statistically significant. Second, there is important heterogeneity in the nature of the reforms. 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. Clearly this is a result that few policy makers sought. At the same time, there is no evidence that the number of men living with these children has increased, suggesting that on net, any increase in marriage comes from previously cohabiting partners.

One potential problem in interpreting results for Black, central-city children concerns the composition of this subsample. If implementation of welfare reform were correlated—for either causal or other reasons—with changes in central-city residence among Black children, 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

[34] The homogeneity constraint is not rejected in any column of Panel B of Table 10.

central cities. For example, among all Black children in our sample the rate of migration out of central cities between 1996 and 1997 was 6.4%, by comparison to a rate of migration into central cities from non-central-city locations equal to 3.6%. 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 the results 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.[35]

We next move to Table 11, in which we explore how welfare reform impacts our next-most at-risk group, Hispanic children. We did try estimating our models for Hispanic children in central cities, but given their relative concentration in a small number of states, we had trouble obtaining convergence with some of the probit routines. While the probit log-likelihood function is concave, we have many RHS variables, and over 70% of central-city Hispanics live in three states (California, New York, and Texas), making collinearity a serious issue. We have thus chosen to focus on all Hispanics. The results show quite different patterns from those for Black, central-city children. First, there is less evidence of an increase in household size or number of children, though the number of children per household rises by 0.232 when TANF is implemented in nonwaiver states. Second, the number of men living in households with Hispanic children rises by 0.119 with implementation of waivers.[36]

Panel B shows that welfare reform in waiver states led to significant decreases in the propensity of Hispanic children to live with an unmarried parent—about 11 percentage points for waiver implementation and 9 percentage points for TANF implementation. This effect is entirely accounted for by a significant increase in the fraction living with a married parent. These treatment effects are again large relative to baseline, but, we believe, not implausible. Interestingly, for waiver implementation, the 0.119 increase in number of men per household from Panel A almost exactly matches the 11.7 percentage-point increase in the fraction of Hispanic children living with a married parent, since the baseline number of men per household is about 1. Unlike those for Black, central-city children, the results for Hispanic children provide no evidence that welfare reform leads to

[35] We also estimated a central-city residence model for our sample of Black women living in central cities, with the same result.

[36] Homogeneity of TANF effects across waiver history can be reject at the 7% and 8% levels, respectively, for both the total number of people per household and the total number of children.

increases in the propensity to live with neither parent. Results in the final column show that in waiver states, both waiver and TANF implementation brought a statistically significant reduction of about three percentage points in the rate of co-residence with both a parent and a grandparent. The estimate for TANF implementation in nonwaiver states is of opposite sign but is statistically insignificant.[37]

To complete the picture, we report in Table 12 estimates from a pooled sample including all children. Given the large sample sizes involved, these results are estimated with a great deal of precision. Nonetheless, the estimates universally suggest no impact of reform on household composition or parental co-residence. We view this finding as encouraging given that in the sample of Panel A of Table 4, only 12 percent of children live in households with any AFDC income. Again, this lack of discernible aggregate impact points to the importance of looking at high-impact samples.

6.2 Gross-treatment results for women

Next we turn to estimates for the sample of women. We again present estimates for both Black, central-city women and Hispanic women, beginning in Table 13 with results for Black, central-city women. Results in the top panel tell a similar story to that suggested by the results for Black, central-city children: households in waiver states appear to grow with TANF implementation (though the estimate in the first column is not statistically significant), with the change coming primarily from an increase in the number of children. Again, there is no evidence that the number of men per household has risen.[38]

Panel B of Table 13 reports estimates for outcomes related to the woman's marital status and the presence of own children in household. These results, particularly for TANF in states without prior waivers, show that reform is associated with a significant and large reduction—8.5 percentage points (17% of baseline)—in the number of never-married women, 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. As with

[37]The TANF homogeneity constraints are not rejected for the neither-parent model. However, they are rejected at below the 1% level for both living with a married parent and living with a never-married parent, and at the 7% level for the probability of living with both a parent and a grandparent.

[38]The TANF homogeneity constraints are rejected at the 6% and 1% levels, respectively, for the total number of people and the number of children per household.

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. Results for the fraction of women who have an own child in the household are not precisely estimated.[39, 40]

Results for Hispanic women, presented in Table 14, again show different patterns than those for Blacks. Results in Panel A show that welfare reform is generally not associated with changes in household composition for Hispanic women. The significant reduction in the number of men associated with TANF implementation in waiver states is the sole exception.[41] Turning to Panel B, we find that TANF implementation in waiver states is associated with a significant and large increase—7.1 percentage points (13% over baseline)—in the fraction of Hispanic women living with an own child. This change is consistent with the increase in the number of children in these households.

The results for marital status among Hispanic women suggest that welfare reform is not associated with any significant changes in never-married status. However, reform does appear to lead to a significant reduction in the share of women currently divorced or separated; for TANF implementation in nonwaiver states, this effect is statistically significant. Each reform regime is associated with an increase in the fraction of Hispanic women who are currently married, with the significant increase of 3.3 percentage points (6% over baseline) for TANF implementation in nonwaiver states approximately offsetting the reduction in the rate of being never married.[42] As with the children samples, comparing the Black, central-city and Hispanic results seems to reveal very different patterns of response in the impacts of welfare reform on living arrangements.

Lastly, we consider results for samples of high school dropouts (Table 15) and the full sample

[39] For the Panel B results in Table 13, the TANF homogeneity constraints are rejected (at the 8% level) only for the probability of living with an own child.

[40] One might wonder how we could obtain a point estimate of 0.071 for TANF implementation in waiver states given the large and positive (though marginally insignificant) coefficient estimate for the neither-parent outcome in the Black, central-city children sample. However, the present outcome concerns whether a woman lives with *any* own child. The observed results can be explained by a combination of the following; (*i*) an increase in the propensity to live with neither parent among families with disproportionately large numbers of children before the split-up and (*ii*) either increases in first births or increases in living with own children among women with relatively few children. Unfortunately, we cannot test this hypothesis, because we do not observe earlier living arrangements of children living with neither parent.

[41] The TANF homogeneity constraint is rejected at the 5% level for this outcome, but not for any other Panel A outcome.

[42] The TANF homogeneity constraint is rejected at the 1% level for living with an own child but is otherwise not rejected for Panel B outcomes.

of women (Table 16). For dropouts, we find essentially no evidence of statistically or substantively significant effects of reform, with the one exception being a small reduction (relative to baseline) in the number of men in the household associated with TANF among never-waiver states. 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 dropouts. This finding again points to the importance of allowing for heterogeneity across racial and ethnic subgroups in welfare reform's effects.

For the full sample of women, the only household composition outcome showing any statistically significant effect is the number of men, which falls slightly (about 3%) with TANF implementation in waiver states. For the Panel B outcomes, we find that TANF implementation in nonwaiver states is associated with a statistically significant reduction in the fraction of all women living with an own child. The fraction who are currently married falls by 1.7 percentage points (about 4% of baseline), with most of this change accounted for by an increase in the rate of being divorced or separated. While we did not expect to find much for this pooled sample, the marital status effects are not particularly large relative to baseline, or by comparison to the relative-to-baseline effects for the high-impact subgroups (where those results were statistically significant).

6.3 Estimates including detailed reforms

While we believe the results in the previous two subsections are important and interesting, they do not tell us which aspects of reform are driving changes in living arrangements. We therefore re-estimated the models already discussed, broadening the set of variables included in the reform vector R_{st}.

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 2 reports F and R, rather than C, but C is used in the empirical specifications. The table shows that there is a great deal of variation in the components of the monthly cutoff variable.[43] 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%.[44] 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 (waivers, TANF in waiver states, and TANF in nonwaiver states). 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. Because of lack of space, we report only the interacted cutoff variables.

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. whether the state imposes sanctions that result in termination of benefits;
3. whether the state has eliminated the 100-hour rule governing eligibility for the AFDC Unemployed Parent program;[45]
4. whether the state has a family cap policy preventing benefits from rising when a new child is born; and
5. whether the state has a rule requiring that minor parents reside with adults (relatives or otherwise) in order to receive welfare benefits.

We present data on these characteristics in Table 2. We constructed the detailed reform dummies for each state in its pre-reform, waiver (if applicable), and TANF period. These values were

[43] In Connecticut and Virginia, 100% of income is disregarded up to the Federal poverty line (FPL); hence the FPL for a family of three was used as the cutoff for these states. A number of other states have non-uniform disregard policies, so that we had to make assumptions regarding either or both of F and/or R. Details for these and other judgments we made take many pages to describe fully, so we will generally not discuss them here; on request, we will provide a data appendix with all details.

[44] Actually, the flat and remainder disregards were, respectively, $120 and 67% during the first four months on AFDC. In both this case and for state welfare reforms, we code disregard variables according to their long run values under the assumption that spells last long enough to be affected by the long run policy.

[45] While 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.

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 8 outcomes per group and 7 groups, we do not have space to discuss all the results in detail. Instead, we report results for only the statistically significant coefficient estimates, without their standard errors. These results appear in Tables 17 and 18. Each table has three columns for each subgroup considered above. These three columns are for waivers (columns headed "W"), TANF implementation in states that ever had waivers (columns headed "TE"), and TANF implementation in nonwaiver states (columns headed "TN").

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 Black, central-city children sample we find that sanctions implemented either through waivers or TANF in nonwaiver states are associated with a large increase, of between 0.6 and 0.7, in the number of children in the household—enough to at least account for the changes in Table 10. We also find that termination time limits are associated with a large increase in the number of men living in households where Hispanic children live, which could occur if Hispanic women with children move in with men (or vice versa) because of concerns about paying the bills once time limits bind. Looking at the results for the women samples, we find that more generous monthly cutoffs are associated with across-the-board reductions in the number of men with whom Black, central-city women live; this finding could be explained by greater self-sufficiency due to more generous disregards. A result in line with this finding is that more generous cutoffs are also 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.

However, the results are difficult to rationalize in many other cases. For example, elimination of the 100-hour rule is associated with a large increase—0.21—in the number of men with whom Black, central-city women live when the policy change happens under a waiver, but when the change happens under TANF in a nonwaiver state, the estimated effect is -0.29. Or, consider the fact that 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 a last example, observe that 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.[46] Moreover, we have no way of measuring how strictly or uniformly states enforce 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. An additional explanation for the lack of clarity in the detailed results involves interactions. It is hard to imagine that sanctions would have the same impact in a state with a low monthly cutoff (e.g., Mississippi) as they do in a state with a high cutoff (e.g., Connecticut). Presumably losing benefits due to sanctions in Connecticut—where the cutoff is the Federal poverty line—is much more costly, and thus much more likely to cause changes in living arrangements due to fiscal stress. Because of the large number of detailed reform categories and the small number of states, we are not able to explore possible interaction effects.[47] Lastly, our inability to attribute the main findings of the previous two subsections to any specific reform characteristics is consistent with Bell (2001), as discussed in section 2.

[46] For example, a RAND report (Williamson, Jackson & Klerman (1997)) attempting to catalogue them all only through 1997 runs nearly 100 pages.

[47] For example, among the Black, central-city children, collinearity forced the dropping of the main TANF implementation dummy in waiver states for all detailed specifications except the neither-parent model, despite the absence of any interactions.

7 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 the number of persons in households, 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 household composition, marital status, and parental status. For example, among Black children living in central cities, we find that welfare reform leads to increases in household size, with most of the increase attributable to increases in the number of children. We also find that parental co-residence changes substantially for Black, central-city children. While more of them are living with a married parent, more are also living with neither parent. Among Black, central-city women, in states that did not have waivers, 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 the possibility of reform-induced "bad matches".

Interestingly, we find different patterns among Hispanics than among Blacks. For Hispanics, reform leads to weaker increases in overall family size, as well as increases in the number of adult men in the household. 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, by contrast to 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.

In an effort to understand what led to these changes in living arrangements, we also estimated models allowing distinct effects for several important features of state reforms. Unfortunately, these results are fairly muddled, leaving little basis to attribute effects to particular policies.

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 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 data, 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. Many of our most interesting and important findings concern children's living circumstances. It is not clear how, say, the results for the probability of living with neither parent could 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

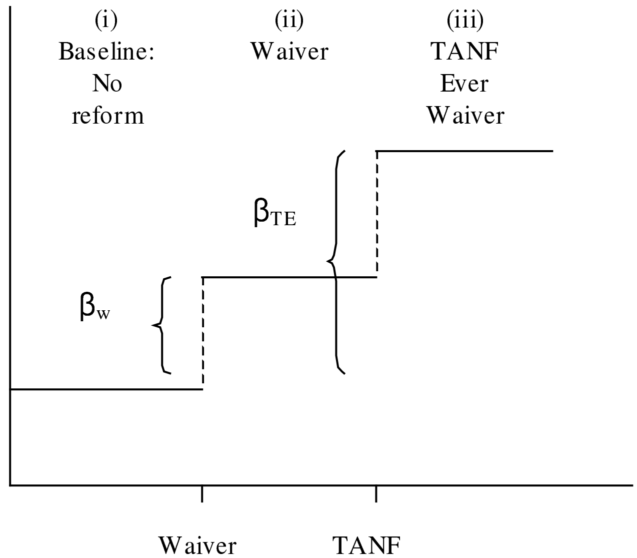

Figure 1b: Treatment Effects with No State Waiver

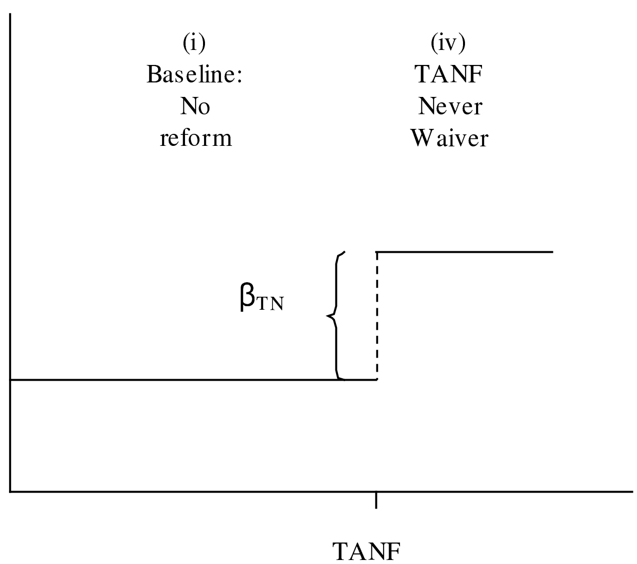

Table 1: State implementation of AFDC waivers and TANF programs, by March 1

	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
					1997	1998
First Year for which TANF Implemented by March 1					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	

Note: See text for data sources and explanation.

Table 2: 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			225	.5	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		128	.488	X		
Idaho								X	X		.4	X	X	X
Illinois	X	X		.67			X	X	X		.67	X	X	X
Indiana					X		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							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.

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 Relative may not be in sample before doubling-up	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 father (before marriage leaseholder would be only parent who could be linked to the child, after marriage both could be)

Table 4: Household AFDC participation rates, 1988–92

	Central City Status			*Educational Attainment*		
	Live in Central city	Live outside Central city	Status Not identified	High school Dropout	High school Graduate	More than High school
A. Children						
Black	0.369	0.267	0.246			
	(0.006)	(0.007)	(0.010)			
Hispanic	0.224	0.132	0.191			
	(0.003)	(0.002)	(0.005)			
White	0.084	0.054	0.057			
	(0.002)	(0.001)	(0.001)			
Pooled	0.200	0.080	0.094			
	(0.002)	(0.001)	(0.001)			
B. Women						
Black	0.223	0.150	0.143	0.339	0.183	0.069
	(0.006)	(0.006)	(0.009)	(0.009)	(0.006)	(0.004)
Hispanic	0.134	0.075	0.121	0.158	0.086	0.039
	(0.004)	(0.004)	(0.007)	(0.005)	(0.004)	(0.003)
White	0.042	0.034	0.035	0.100	0.037	0.014
	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Pooled	0.105	0.046	0.053	0.159	0.061	0.021
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)

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

Table 5: Summary statistics: child sample

	Mean	Std. Dev.	Min.	Max.
Waiver implemented	0.13	0.34	0.00	1.00
TANF implemented, ever had a waiver	0.15	0.36	0.00	1.00
TANF implemented, never had a waiver	0.16	0.36	0.00	1.00
Termination time limit waiver	0.02	0.12	0.00	1.00
Full sanctions policy waiver	0.02	0.15	0.00	1.00
UP 100-hour liberalized by waiver	0.09	0.28	0.00	1.00
Family cap waiver	0.04	0.20	0.00	1.00
Minor residency waiver	0.02	0.14	0.00	1.00
Termination time limit, TANF	0.26	0.44	0.00	1.00
Full sanctions policy, TANF	0.19	0.40	0.00	1.00
UP 100-hour liberalized by TANF	0.27	0.44	0.00	1.00
Family cap, TANF	0.12	0.33	0.00	1.00
Minor residency, TANF	0.27	0.45	0.00	1.00
Real maximum benefits for a family of three	5.17	2.08	1.39	12.60
Unemployment rate	5.59	1.52	2.20	11.40
Employment growth rate	1.75	1.56	-4.87	9.86
Living in central city	0.24	0.43	0.00	1.00
Central city status unidentified	0.15	0.36	0.00	1.00
Living in MSA	0.78	0.41	0.00	1.00
MSA status unidentified	0.01	0.08	0.00	1.00
Black	0.159	0.366	0.000	1.000
Hispanic	0.12	0.33	0.00	1.00
Age	7.4	4.6	0.0	15.0
Number of people in household	4.26	1.44	1.00	25.00
Number of children in household	2.34	1.20	1.00	12.00
Number of women in household	1.19	0.53	0.00	8.00
Number of men in household	0.98	0.61	0.00	8.00
Child lives with neither parent	0.03	0.18	0.00	1.00
Child lives with unmarried parent	0.27	0.44	0.00	1.00
Child lives with married parent	0.70	0.46	0.00	1.00
Child lives with parent and grandparent	0.06	0.23	0.00	1.00
N	209,385	209,385	209,385	209,385

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures are means for children aged < 16. All figures weighted using March *psupwgt* variable. See text for more information.

Table 6: Summary statistics: women sample

	Mean	Std. Dev.	Min.	Max.
Waiver implemented	0.13	0.33	0.00	1.00
TANF implemented, ever had a waiver	0.15	0.36	0.00	1.00
TANF implemented, never had a waiver	0.16	0.37	0.00	1.00
Termination time limit waiver	0.02	0.12	0.00	1.00
Full sanctions policy waiver	0.02	0.15	0.00	1.00
UP 100-hour liberalized by waiver	0.08	0.28	0.00	1.00
Family cap waiver	0.04	0.20	0.00	1.00
Minor residency waiver	0.02	0.14	0.00	1.00
Termination time limit, TANF	0.26	0.44	0.00	1.00
Full sanctions policy, TANF	0.20	0.40	0.00	1.00
UP 100-hour liberalized by TANF	0.27	0.44	0.00	1.00
Family cap, TANF	0.12	0.33	0.00	1.00
Minor residency, TANF	0.28	0.45	0.00	1.00
Real maximum benefits for a family of three	5.17	2.07	1.39	12.60
Unemployment rate	5.57	1.51	2.20	11.40
Employment growth rate	1.72	1.57	-4.87	9.86
Living in central city	0.25	0.43	0.00	1.00
Central city status unidentified	0.15	0.36	0.00	1.00
Living in MSA	0.80	0.40	0.00	1.00
MSA status unidentified	0.01	0.07	0.00	1.00
Black	0.134	0.340	0.000	1.000
Hispanic	0.09	0.28	0.00	1.00
Age	34.4	10.6	16.0	54.0
Number of people in household	3.15	1.53	1.00	25.00
Number of children in household	0.97	1.17	0.00	12.00
Number of women in household	1.45	0.71	1.00	8.00
Number of men in household	0.99	0.72	0.00	8.00
Woman lives with own child	0.43	0.50	0.00	1.00
Woman never married	0.30	0.46	0.00	1.00
Woman divorced, separated or widowed	0.15	0.36	0.00	1.00
Woman currently married	0.55	0.50	0.00	1.00
N	240,343	240,343	240,343	240,343

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures are means for women aged 16–54. All figures weighted using March *psupwgt* variable. See text for more information.

Table 7: Raw means before and after reform: child sample

	Household composition				Parental co-residence			
	Total	Children	Females >= 16	Males >= 16	Neither	Unmarried	Married	Grandparent & Parent
Pooled								
Before reform, waiver state	4.29	2.35	1.19	0.99	0.023	0.256	0.721	0.052
	(0.01)	(0.01)	(0.00)	(0.00)	(0.001)	(0.002)	(0.002)	(0.001)
After reform, waiver state	4.32	2.40	1.20	1.01	0.035	0.269	0.696	0.063
	(0.01)	(0.00)	(0.00)	(0.00)	(0.001)	(0.002)	(0.002)	(0.001)
Before reform, non-waiver state	4.24	2.32	1.19	0.96	0.032	0.268	0.701	0.056
	(0.01)	(0.00)	(0.00)	(0.00)	(0.001)	(0.002)	(0.002)	(0.001)
After reform, non-waiver state	4.17	2.27	1.18	0.96	0.042	0.279	0.679	0.058
	(0.01)	(0.01)	(0.00)	(0.00)	(0.001)	(0.002)	(0.003)	(0.001)
Black, central city								
Before reform, waiver state	4.06	2.62	1.29	0.62	0.062	0.674	0.263	0.097
	(0.04)	(0.03)	(0.01)	(0.01)	(0.006)	(0.010)	(0.009)	(0.006)
After reform, waiver state	4.09	2.67	1.31	0.64	0.117	0.620	0.263	0.117
	(0.03)	(0.03)	(0.01)	(0.01)	(0.005)	(0.009)	(0.008)	(0.006)
Before reform, non-waiver state	4.23	2.72	1.36	0.66	0.080	0.629	0.291	0.114
	(0.02)	(0.02)	(0.01)	(0.01)	(0.004)	(0.007)	(0.006)	(0.004)
After reform, non-waiver state	4.11	2.53	1.29	0.65	0.099	0.597	0.304	0.069
	(0.03)	(0.03)	(0.01)	(0.01)	(0.006)	(0.010)	(0.009)	(0.006)
Hispanic								
Before reform, waiver state	4.81	2.79	1.33	1.13	0.026	0.307	0.666	0.073
	(0.02)	(0.02)	(0.01)	(0.01)	(0.002)	(0.006)	(0.006)	(0.003)
After reform, waiver state	4.70	2.73	1.34	1.16	0.042	0.315	0.643	0.077
	(0.02)	(0.01)	(0.01)	(0.01)	(0.002)	(0.004)	(0.004)	(0.002)
Before reform, non-waiver state	4.48	2.54	1.28	1.00	0.035	0.331	0.634	0.084
	(0.02)	(0.01)	(0.01)	(0.01)	(0.002)	(0.005)	(0.005)	(0.003)
After reform, non-waiver state	4.42	2.48	1.27	1.04	0.051	0.297	0.652	0.081
	(0.02)	(0.02)	(0.01)	(0.01)	(0.003)	(0.006)	(0.006)	(0.004)

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures are means for children aged < 16. "Before reform" sample consists of all observations for which no reform (waiver or TANF) has been implemented. "After reform" sample consists of all observations for which some reform (waiver or TANF) has been implemented. All figures weighted using March *psupwgt* variable. See text for more information.

Table 8: Raw means before and after reform: women sample

	Household composition				Child co-residence and marital status			
	Total	Children	Females >= 16	Males >= 16	Living with Own child	Never Married	Divorced/ Separated	Currently Married
Pooled								
Before reform, waiver state	3.18	0.97	1.45	1.01	0.440	0.292	0.151	0.557
	(0.01)	(0.01)	(0.00)	(0.00)	(0.002)	(0.002)	(0.002)	(0.002)
After reform, waiver state	3.17	0.99	1.47	1.00	0.428	0.310	0.150	0.540
	(0.01)	(0.00)	(0.00)	(0.00)	(0.002)	(0.002)	(0.001)	(0.002)
Before reform, non-waiver state	3.16	0.97	1.44	0.97	0.440	0.286	0.153	0.561
	(0.01)	(0.00)	(0.00)	(0.00)	(0.002)	(0.002)	(0.001)	(0.002)
After reform, non-waiver state	3.07	0.93	1.42	0.96	0.419	0.298	0.159	0.543
	(0.01)	(0.01)	(0.00)	(0.00)	(0.003)	(0.002)	(0.002)	(0.003)
Black, central city								
Before reform, waiver state	3.12	1.27	1.56	0.70	0.497	0.515	0.240	0.245
	(0.03)	(0.03)	(0.02)	(0.02)	(0.011)	(0.011)	(0.009)	(0.009)
After reform, waiver state	3.05	1.19	1.58	0.66	0.446	0.537	0.225	0.238
	(0.03)	(0.03)	(0.01)	(0.01)	(0.009)	(0.009)	(0.008)	(0.008)
Before reform, non-waiver state	3.26	1.30	1.63	0.71	0.475	0.492	0.233	0.275
	(0.02)	(0.02)	(0.01)	(0.01)	(0.007)	(0.007)	(0.006)	(0.006)
After reform, non-waiver state	3.05	1.11	1.55	0.68	0.440	0.513	0.221	0.266
	(0.03)	(0.03)	(0.02)	(0.02)	(0.010)	(0.010)	(0.008)	(0.009)
Hispanic								
Before reform, waiver state	3.86	1.50	1.63	1.25	0.540	0.308	0.140	0.552
	(0.02)	(0.02)	(0.01)	(0.01)	(0.007)	(0.006)	(0.005)	(0.007)
After reform, waiver state	3.83	1.54	1.65	1.22	0.540	0.341	0.130	0.529
	(0.02)	(0.01)	(0.01)	(0.01)	(0.005)	(0.005)	(0.003)	(0.005)
Before reform, non-waiver state	3.58	1.30	1.58	1.04	0.525	0.297	0.175	0.528
	(0.02)	(0.01)	(0.01)	(0.01)	(0.005)	(0.005)	(0.004)	(0.005)
After reform, non-waiver state	3.59	1.32	1.56	1.10	0.526	0.299	0.153	0.549
	(0.02)	(0.02)	(0.01)	(0.01)	(0.007)	(0.006)	(0.005)	(0.007)
Dropout								
Before reform, waiver state	3.69	1.29	1.72	1.09	0.388	0.462	0.155	0.383
	(0.02)	(0.01)	(0.01)	(0.01)	(0.005)	(0.005)	(0.004)	(0.005)
After reform, waiver state	3.79	1.34	1.79	1.12	0.363	0.517	0.125	0.358
	(0.02)	(0.01)	(0.01)	(0.01)	(0.004)	(0.005)	(0.003)	(0.004)
Before reform, non-waiver state	3.59	1.22	1.71	1.02	0.367	0.475	0.155	0.370
	(0.01)	(0.01)	(0.01)	(0.01)	(0.004)	(0.004)	(0.003)	(0.004)
After reform, non-waiver state	3.54	1.17	1.72	1.01	0.336	0.531	0.145	0.325
	(0.02)	(0.02)	(0.01)	(0.01)	(0.006)	(0.006)	(0.004)	(0.006)

Note: Tabulations from the March CPS, 1989–2000, using only respondents in households in months 1–4 of sample. All figures are means for women aged 16–54. "Before reform" sample consists of all observations for which no reform (waiver or TANF) has been implemented. "After reform" sample consists of all observations for which some reform (waiver or TANF) has been implemented. All figures weighted using March *psupwgt* variable. See text for more information.

Table 9: Results for Black, central city children sample, TANF effects constrained to be symmetric

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	-0.010	-0.030	-0.025	0.044
	(0.147)	(0.135)	(0.044)	(0.047)
TANF enacted	0.224	0.189	0.092*	-0.056
	(0.270)	(0.230)	(0.051)	(0.078)
Mean of LHS variable	4.622	2.655	1.323	0.644
N	13,075	13,075	13,075	13,075

B. Parental residence				
	Neither	Unmarried	Married	Parent & Grandparent
Any major waiver	0.015	-0.045	0.019	-0.013
	(0.018)	(0.031)	(0.031)	(0.017)
TANF enacted	0.082**	-0.131***	0.044	-0.052***
	(0.035)	(0.046)	(0.051)	(0.020)
Mean of LHS variable	0.089	0.629	0.282	0.104
N	13,010	13,074	13,074	12,937

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 10: Results for Black, central city children sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.338*	0.257	0.016	0.065
	(0.188)	(0.179)	(0.060)	(0.063)
TANF in force:				
Ever had waiver	0.808**	0.669**	0.160*	-0.021
	(0.369)	(0.321)	(0.089)	(0.108)
Never had waiver	0.040	0.037	0.070	-0.067
	(0.255)	(0.224)	(0.054)	(0.083)
Mean of LHS variable	4.622	2.655	1.323	0.644
N	13,075	13,075	13,075	13,075

B. Parental residence				
	Neither	Unmarried	Married	Parent & Grandparent
Any major waiver	0.030	-0.026	-0.017	0.000
	(0.030)	(0.044)	(0.043)	(0.028)
TANF in force:				
Ever had waiver	0.127	-0.100	-0.015	-0.031
	(0.079)	(0.078)	(0.074)	(0.032)
Never had waiver	0.078**	-0.142***	0.064	-0.054***
	(0.036)	(0.049)	(0.053)	(0.018)
Mean of LHS variable	0.089	0.629	0.282	0.104
N	13,010	13,074	13,074	12,937

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 11: Results for Hispanic children sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	-0.013	-0.149	0.017	0.119*
	(0.160)	(0.111)	(0.050)	(0.069)
TANF in force:				
Ever had waiver	-0.202	-0.108	-0.031	-0.063
	(0.203)	(0.147)	(0.070)	(0.060)
Never had waiver	0.267	0.232*	0.065	-0.030
	(0.174)	(0.136)	(0.056)	(0.032)
Mean of LHS variable	5.043	2.646	1.309	1.089
N	33,442	33,442	33,442	33,442

B. Parental residence				
	Neither	Unmarried	Married	Parent & Grandparent
Any major waiver	-0.003	-0.108***	0.117***	-0.029**
	(0.011)	(0.026)	(0.029)	(0.013)
TANF in force:				
Ever had waiver	0.005	-0.093***	0.086***	-0.028*
	(0.013)	(0.028)	(0.032)	(0.017)
Never had waiver	0.013	0.021	-0.040	0.019
	(0.009)	(0.025)	(0.028)	(0.019)
Mean of LHS variable	0.039	0.315	0.646	0.079
N	33,112	33,436	33,436	33,192

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 12: Results for full children sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.038	0.024	0.001	0.013
	(0.038)	(0.037)	(0.011)	(0.013)
TANF in force:				
Ever had waiver	-0.002	0.036	-0.005	-0.033*
	(0.066)	(0.062)	(0.018)	(0.019)
Never had waiver	-0.008	0.011	0.009	-0.028
	(0.057)	(0.045)	(0.016)	(0.018)
Mean of LHS variable	4.513	2.342	1.191	0.980
N	209,382	209,382	209,382	209,382

B. Parental residence				
	Neither	Unmarried	Married	Parent & Grandparent
Any major waiver	0.004	-0.014	0.009	0.005
	(0.004)	(0.010)	(0.010)	(0.005)
TANF in force:				
Ever had waiver	0.005	-0.007	-0.000	-0.001
	(0.006)	(0.016)	(0.017)	(0.008)
Never had waiver	0.005	0.010	-0.016	-0.002
	(0.004)	(0.012)	(0.013)	(0.006)
Mean of LHS variable	0.032	0.267	0.700	0.058
N	209,382	209,382	209,382	209,382

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 13: Results for Black, central city women sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.135	0.128	0.007	-0.000
	(0.141)	(0.098)	(0.059)	(0.064)
TANF in force:				
Ever had waiver	0.330	0.408***	0.008	-0.087
	(0.214)	(0.147)	(0.091)	(0.112)
Never had waiver	-0.070	-0.005	0.042	-0.107
	(0.119)	(0.106)	(0.080)	(0.083)
Mean of LHS variable	3.511	1.232	1.589	0.690
N	12,847	12,847	12,847	12,847

B. Children and Marital Status				
	Has child	Never Married	Divorced/ Separated	Currently Married
Any major waiver	0.018	-0.008	-0.016	0.032
	(0.033)	(0.032)	(0.020)	(0.028)
TANF in force:				
Ever had waiver	0.071	-0.079	0.041	0.034
	(0.054)	(0.052)	(0.045)	(0.040)
Never had waiver	-0.029	-0.085**	0.070**	-0.002
	(0.040)	(0.034)	(0.034)	(0.024)
Mean of LHS variable	0.466	0.510	0.230	0.260
N	12,847	12,847	12,847	12,847

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 survey year. Additional control variables are: age of woman and its square; dummies for being a high school dropout and for having at least a high school diploma; 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 14: Results for Hispanic women sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.018	-0.003	0.038	-0.017
	(0.110)	(0.065)	(0.047)	(0.053)
TANF in force:				
Ever had waiver	-0.010	0.124	-0.012	-0.121*
	(0.146)	(0.088)	(0.062)	(0.062)
Never had waiver	0.168	0.130*	0.007	0.031
	(0.106)	(0.075)	(0.056)	(0.046)
Mean of LHS variable	4.183	1.422	1.610	1.151
N	30,543	30,543	30,543	30,543

B. Children and Marital Status				
	Has child	Never Married	Divorced/ Separated	Currently Married
Any major waiver	0.022	-0.025	-0.015	0.030
	(0.022)	(0.026)	(0.015)	(0.024)
TANF in force:				
Ever had waiver	0.071**	0.044	-0.028	0.003
	(0.028)	(0.035)	(0.020)	(0.030)
Never had waiver	-0.012	0.007	-0.023*	0.033*
	(0.022)	(0.020)	(0.012)	(0.018)
Mean of LHS variable	0.533	0.314	0.149	0.536
N	30,543	30,543	30,543	30,543

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 survey year. Additional control variables are: age of woman and its square; dummies for being a high school dropout and for having at least a high school diploma; 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 15: Results for dropouts women sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.000	-0.066	0.034	0.032
	(0.061)	(0.049)	(0.027)	(0.026)
TANF in force:				
Ever had waiver	0.027	0.069	-0.013	-0.029
	(0.099)	(0.094)	(0.041)	(0.036)
Never had waiver	-0.014	0.100	-0.051	-0.063*
	(0.095)	(0.071)	(0.043)	(0.032)
Mean of LHS variable	4.052	1.258	1.733	1.061
N	44,766	44,766	44,766	44,766

B. Children and Marital Status				
	Has child	Never Married	Divorced/ Separated	Currently Married
Any major waiver	-0.017	-0.022	-0.005	0.018
	(0.017)	(0.026)	(0.009)	(0.017)
TANF in force:				
Ever had waiver	-0.012	-0.022	0.010	-0.008
	(0.031)	(0.036)	(0.016)	(0.023)
Never had waiver	-0.003	-0.006	0.002	0.001
	(0.022)	(0.026)	(0.012)	(0.017)
Mean of LHS variable	0.366	0.492	0.145	0.363
N	44,766	44,766	44,766	44,766

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 survey year. Additional control variables are: age of woman and its square; dummies for being a high school dropout and for having at least a high school diploma; 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 16: Results for full women sample, effects unconstrained

A. Household composition				
	Number of People			
	Total	Children	Females >= 16	Males >= 16
Any major waiver	0.028	0.013	0.018	-0.003
	(0.024)	(0.017)	(0.013)	(0.010)
TANF in force:				
Ever had waiver	0.010	0.025	0.014	-0.030*
	(0.034)	(0.026)	(0.018)	(0.017)
Never had waiver	-0.056	-0.031	0.001	-0.026
	(0.034)	(0.020)	(0.017)	(0.016)
Mean of LHS variable	3.406	0.970	1.449	0.987
N	240,335	240,335	240,335	240,335

B. Children and Marital Status				
	Has child	Never Married	Divorced/ Separated	Currently Married
Any major waiver	0.006	-0.006	-0.002	0.006
	(0.007)	(0.006)	(0.004)	(0.006)
TANF in force:				
Ever had waiver	0.008	0.007	0.006	-0.013
	(0.012)	(0.010)	(0.007)	(0.010)
Never had waiver	-0.017**	0.004	0.012**	-0.017**
	(0.008)	(0.008)	(0.005)	(0.008)
Mean of LHS variable	0.433	0.296	0.153	0.551
N	240,335	240,335	240,335	240,335

Note: ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All figures in panel *A* are OLS coefficients and standard errors. All figures in panel *B* 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 survey year. Additional control variables are: age of woman and its square; dummies for being a high school dropout and for having at least a high school diploma; 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 state has AFDC-UP program; 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; state-specific time trends; and year and state dummy variables.

Table 17: Statistically significant detailed results for kids samples

	Black, Central City			Hispanic			All		
	W	TE	TN	W	TE	TN	W	TE	TN
Total									
Term. time limits					0.80				
Full sanctions								-0.20	
100-hour UP elim.				-0.51					
Monthly cutoff									
Family cap	-0.87				0.69				
Minor res. reqt.					-2.76			0.63	
Children									
Term. time limits					0.73				
Full sanctions	0.63		0.67					-0.15	
100-hour UP elim.									
Monthly cutoff									
Family cap	-0.71				0.51				
Minor res. reqt.					-2.01				
Women (age >= 16)									
Term. time limits									
Full sanctions						-0.16			
100-hour UP elim.				-0.22			-0.03		
Monthly cutoff									
Family cap		0.20							
Minor res. reqt.				0.22					
Men (age >= 16)									
Term. time limits			0.37	0.16		0.52			0.11
Full sanctions			-0.26	-0.30		0.20	-0.04		
100-hour UP elim.						0.21			
Monthly cutoff									
Family cap									
Minor res. reqt.	0.41							0.22	
Neither parent									
Term. time limits					-0.04			-0.01	
Full sanctions		0.12		0.09					
100-hour UP elim.									
Monthly cutoff				-0.01	-0.01		0.00	0.00	
Family cap				-0.03	-0.03	-0.02			
Minor res. reqt.	-0.06				-0.15		-0.01		
Unmarried parent									
Term. time limits				-0.13					
Full sanctions			0.16	0.24		-0.12			
100-hour UP elim.								-0.04	
Monthly cutoff									
Family cap									
Minor res. reqt.									
Married parent									
Term. time limits				0.17					
Full sanctions				-0.32		0.12			
100-hour UP elim.				-0.12					
Monthly cutoff									
Family cap		-0.15							
Minor res. reqt.									
Parent & grandparent									
Term. time limits				-0.04					
Full sanctions	-0.08					-0.04	-0.02	-0.02	
100-hour UP elim.				-0.04	-0.08				0.02
Monthly cutoff									
Family cap									
Minor res. reqt.					-0.20				

Note: See text for details.

Table 18: Statistically significant detailed results for women samples

	Black, Central City			Hispanic			Dropouts			All		
	W	TE	TN	W	TE	TN	W	TE	TN	W	TE	TN
Total												
Term. time limits					0.63				0.78	0.10		0.19
Full sanctions									-0.28			
100-hour UP elim.			-0.36	-0.35								
Monthly cutoff												
Family cap	-0.54			0.42								
Minor res. reqt.		1.42			-1.13						0.38	
Children												
Term. time limits				-0.17	0.55		-0.16					
Full sanctions						-0.27			-0.16			-0.08
100-hour UP elim.								-0.18				-0.05
Monthly cutoff				0.09	0.09	0.06						
Family cap	-0.38			0.47	0.60							
Minor res. reqt.		0.95	-0.71				0.12					
Women (age >= 16)												
Term. time limits							0.16		0.42			
Full sanctions												
100-hour UP elim.		0.24		-0.27		0.21		0.09				
Monthly cutoff			0.07						0.03			0.02
Family cap												
Minor res. reqt.				0.16								
Men (age >= 16)												
Term. time limits			0.41			0.33			0.20	0.06		
Full sanctions			-0.26	-0.18		0.19						
100-hour UP elim.	0.21		-0.29				-0.08					
Monthly cutoff	-0.11	-0.11	-0.11									-0.01
Family cap	-0.18											
Minor res. reqt.		0.75	0.33		-0.57						0.19	
Live w/own child												
Term. time limits					0.18	-0.18						
Full sanctions				-0.11			-0.05					
100-hour UP elim.								-0.08			-0.04	-0.03
Monthly cutoff				0.03	0.03							
Family cap	-0.10			0.15	0.24	0.19						
Minor res. reqt.			-0.21									
Never married												
Term. time limits					0.18				0.15			
Full sanctions			0.10	0.21	-0.15		0.09			0.03		0.03
100-hour UP elim.									-0.09			
Monthly cutoff	0.06	0.06	0.05			-0.02						
Family cap			0.13		-0.17							
Minor res. reqt.					0.71							
Div. or separated												
Term. time limits				-0.03								
Full sanctions	-0.08							0.04		-0.01		
100-hour UP elim.												
Monthly cutoff				0.03	0.03	0.02						0.00
Family cap												
Minor res. reqt.					-0.16						-0.04	
Currently married												
Term. time limits				0.09					-0.12			
Full sanctions			-0.07	-0.20			-0.05	-0.07				
100-hour UP elim.									0.05			
Monthly cutoff			-0.04									
Family cap			-0.08		0.17							
Minor res. reqt.			0.19	-0.07								

Note: See text for details.