

## **The Impact of Earnings Disregards on the Behavior of Low Income Families**

Jordan D. Matsudaira  
Cornell University  
[jordan.matsudaira@cornell.edu](mailto:jordan.matsudaira@cornell.edu)

and

Rebecca M. Blank  
University of Michigan, Brookings Institution, and  
National Bureau of Economic Research  
[rblank@brookings.edu](mailto:rblank@brookings.edu)

May 2008

Corresponding author:  
Rebecca M. Blank  
Brookings Institution  
1775 Massachusetts Avenue, NW  
Washington, D.C. 20036  
202-797-6299  
[rblank@brookings.edu](mailto:rblank@brookings.edu)

We would like to thank Brian Kovak, Emily Beam, Cody Rockey, Tori Finkle, and Ari Kushner, for excellent research assistance.

## **Abstract**

This paper investigates the impact of changes in earnings disregards for welfare assistance received by single mothers following welfare reform in 1996. Some states adopted much higher earnings disregards (women could work full time and still receive welfare), while other states did not. We explore the effect of these changes on women's labor supply and income using several data sources and multiple estimation strategies. Our results indicate these changes had little effect on labor supply or income. We show this is because few women used these earnings disregards. This is surprising and we discuss why this might occur.

## I. Introduction

The welfare reforms enacted by the U.S. Congress in 1996 gave states substantial leeway to design cash assistance programs for low income and predominantly female-headed families with children. States used this discretion to implement a wide variety of changes in their welfare programs. One change made by many states was to disregard a higher share of the earnings of working women in calculating their eligibility for welfare benefits. Higher earnings disregards are typically viewed as equivalent to reduced tax rates, leading to an increase in the effective wage rate. In general, economic theory would predict such a change should induce greater labor supply among low-wage workers. Furthermore, even in the absence of any labor supply effects, higher earnings disregards should increase income among workers by allowing them to receive more welfare benefits at a given level of earnings.

States that adopted higher disregards in the mid-1990s used these arguments, claiming that they would increase work incentives, thereby reinforcing other program changes also designed to push welfare recipients into employment, as well as supplement the income of single mothers as they left welfare and entered work. In many states, these changes in earnings disregards were large, with reductions in the implicit tax on earnings of 50 percentage points or more. Despite a large literature that evaluates the effects of welfare reform<sup>1</sup>, we are not aware of previous research that focuses on the effects of these benefit disregard changes. This paper investigates whether enhanced benefit disregards produced increases in labor supply and also investigates their effects on income.

Despite very large differences in earnings disregards across states, our results suggest that states with higher disregards do not show substantially larger increases in

---

<sup>1</sup> For instance, see summaries of this research in Blank (2002) or Grogger and Karoly (2005).

labor supply among low-skilled single mothers. This is true whether we look at labor force participation or hours of work. Even more surprising, we find no income supplementation effect from these disregards. This is puzzling, since higher disregard states should be providing greater subsidies to low-wage women as they enter work. We verify these results across several data sets and multiple specifications. Why should this be true? Our data suggest that very few working women in high disregard states appear to take advantage of earnings disregards to receive ongoing income supplements from welfare; instead they leave welfare entirely once they are working. We discuss a number of reasons why women might choose to forego ongoing support from the public assistance system that high earnings disregards could provide.

The next section discusses the changes that occurred in the mid-1990s in more detail. The third section reviews the literature on changes in behavior and well-being among single mothers in response to welfare program incentives. The fourth section describes the comparative patterns in the data over time among states that raised their earnings disregards and those that didn't, using both graphical and difference-in-difference comparisons. The fifth section provides a more parameterized test of these effects. The sixth section looks at reasons why the effects of these earnings disregards are so small. The final section concludes.

## **II. Earnings Disregards and Labor Supply Incentives**

The Aid to Families with Dependent Children (AFDC) program was the primary cash welfare program in this country prior to the 1996 welfare reforms. It provides the base comparison point for our analysis, so we describe it here in some detail. We then

look at the changes implemented following the 1996 welfare reforms that abolished AFDC and gave states the authority to design their own cash welfare programs.

The AFDC program provided a maximum benefit level, or benefit guarantee ( $G$ ), to those who did not work. As women went to work, earnings disregards determined the amount of earnings that was ignored in the ongoing calculation of welfare benefits, and hence determined how quickly income rose with earnings by determining how rapidly benefit reductions offset earnings increases. Under AFDC, earnings were disregarded entirely in the calculation of benefits for an initial period up to a certain earnings level; we will refer to this as the initial earnings disregard (IED). This initial AFDC earnings disregard included a mandatory \$30 in earnings each month, but (at state discretion) could also include disregards related to child care expenses and other work expenses.

When earnings exceeded this disregard, benefits were reduced at a rate  $t$  ( $0 \leq t \leq 1$ ), which we will refer to as the benefit reduction rate (BRR). Earnings are disregarded in the calculation of benefits at a rate of  $1-t$ ; that is, for each hour worked at wage  $w$ , income rises by  $(1-t)w$ , while the remainder is lost through an offsetting reduction in welfare benefits. We use the umbrella term “earnings disregards” to refer to both the initial (100 percent) earnings disregard as well as the more graduated earnings disregards built into the benefit reduction rate.

Figure 1 depicts the budget constraint that results from this program design. A non-worker receives  $G$ , the maximum benefit level. A woman who begins to work at wage  $w$  will see her income rise dollar for dollar as her earnings increase, until the initial earnings disregard, IED, is exhausted (point A on figure 1). Beyond this point, income rises at a rate of  $(1-t)w$ , with benefits reduced by  $t$  cents for every dollar of additional

earnings. At point B, known as the break-even point, benefits have been reduced to zero. Beyond B, income rises again dollar-for-dollar with earnings (ignoring the effects of other tax or transfer programs.)

Under the AFDC program, states determined the maximum state benefit level, G, leading to widely varying benefit levels across states.<sup>2</sup> After 1967, however, the federal government enacted national rules for earnings disregards that all states were required to follow. Table 1 shows how the earnings disregard rules changed over time under AFDC starting in 1979. Federal rules about earnings disregards tightened after 1981, with caps on state-determined child care and work expense disregards. A gross income cap on eligibility was also imposed, which prevented anyone from receiving welfare whose income exceeded 150 percent of the state need standard.<sup>3</sup> In states where the break-even point is higher than the income cap, this creates a notch in the budget constraint.

By the early 1990s, when welfare reform was enacted, women on welfare who went to work received a standard \$30 initial earnings disregard and were potentially eligible for further disregards depending on their child care and work expenses. Once these initial disregards were exhausted, they faced a benefit reduction rate of 67 percent, which rose to 100 percent after four months of work. This implies that three key parameters are important in understanding benefit payout and (by implication) labor supply incentives in AFDC<sup>4</sup>: the benefit level G, the benefit reduction rate t, and the

---

<sup>2</sup> For instance, in January 1990 the lowest benefit state paid \$118/month, while the highest benefit state paid \$846/month, with a median of \$364 in monthly benefits.

<sup>3</sup> State need standards were correlated with but not always identical to their benefit guarantees. Like the benefit guarantees, they varied substantially across states.

<sup>4</sup> In fact, incentives are more complicated than described here, as states vary in the manner in which they apply these four parameters to calculate benefits. For more information on the detailed formulae used by states to calculate welfare benefits, contact the authors.

initial earnings disregard amount IED. A fourth parameter, an income or earnings cap beyond which welfare could not be received, was also important in some circumstances.

In general, increases in earnings disregards, either through increases in the IED or decreases in the BRR, should increase hours worked among non-workers or low-hours workers. This effect is indicated on Figure 1 by arrows 1 and 2. On the other hand, as the break-even point increases, more people who are at or near the old breakeven point may find it beneficial to reduce their hours. This is indicated on Figure 1 by arrows 3 and 4. The net effect is theoretically ambiguous, and depends upon the magnitude of the labor supply responses among non-workers versus workers near the breakeven point. Similar arguments suggest that increased earnings disregards should result in increased income for nearly all workers, except those who decrease their hours significantly from above the break-even point B.

Note that there is similar ambiguity if one asks about the effect of earnings disregards on income levels. If the dominant effect is to increase labor supply among non- or low-hours workers, then these disregards should raise income; but if the effect is to reduce hours among workers who would otherwise be off welfare, then the income effect is negative. In the welfare reform era, when all the emphasis was on moving people off welfare and into work, the 50 percent decline in caseloads suggests that few persons reduced work to receive welfare; in this situation, one would expect expanded income disregards to raise the income of women, allowing them to continue to receive some income supplements as they enter work. Of course, if the push to leave welfare discourages them from combining welfare and work, then this may not occur.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 abolished the AFDC program and replaced it with a federal block grant to the states, known as the Temporary Assistance to Needy Families (TANF) block grant.<sup>5</sup> Under TANF, federal rules about benefit reductions were abolished and states could now determine these parameters in any way they wished.

The result was enormous state variation in the design of TANF-funded welfare plans by the late 1990s. Different states made very different choices about a range of new program options, including work requirements, time limits on benefits, sanctions (punishments for those who didn't comply with the new rules), and a variety of eligibility restrictions. States also chose very different earnings disregard policies, with variation in the initial earnings disregards they provided, in their benefit reduction rates and in the gross income caps that they imposed. In short, all four parameters became state-specific. Further state variation occurred because states also allowed these parameters to change in differing ways over time as a women's employment spell lengthened. In some states, earnings disregards and benefit reduction rates were set at one level in the first few months after a women entered employment, changed again within six months of employment, and changed again after 12 months of employment.

Table 2 provides a quick snapshot of how earnings disregards changed over the 1990s across the states, showing earnings disregards at months 1 and 6 in 1990, 1995 and 2000. All states were subject to uniform AFDC rules in 1990; by 1995 a few states were

---

<sup>5</sup> Blank (2002) and Grogger and Karoly (2005) describe the 1996 welfare reform and summarize research about its impact.

deviating from AFDC requirements.<sup>6</sup> By 2000, four years after welfare reform, states were all over the map in their earnings disregard rules, from 0 to 100 percent.

To provide a concrete example of this state diversity, let us describe the programs in Florida, Illinois, and Texas. Figure 2a shows the income constraint facing a low-wage single mother in Florida in 1990 and 2000 who entered work from welfare and is in her sixth month of work with a wage of \$7/hour.<sup>7</sup> In 1990, the initial earnings disregard allowed her to earn \$120 before her benefits were reduced dollar-for-dollar with earnings. She hit the breakeven point at \$595 of monthly earnings (85 hours of work at \$7/hour). Essentially, if she worked more than 20 hours/week in 1990, she was no longer eligible for welfare. By 2000, Florida had raised its initial earnings disregard to \$200, and lowered its benefit reduction rate to 50 percent. The breakeven point was now \$1190. At \$7/hour of earnings, this woman had to work 170 hours/month (over 40 hours/week) before she lost her welfare eligibility. Clearly, by 2000 there were stronger incentives for non-workers and low-hours workers to increase employment and a substantial benefit subsidy to low-wage work.

In contrast, the changes in Illinois' income constraints were more ambiguous. Figure 2b plots the 1990 and 2000 income constraint facing this same woman in her sixth month of work in Illinois at \$7/hour in wages. Her budget constraint in 1990 under AFDC was identical to that in Florida, except that Illinois' monthly guarantee  $G$  was \$367, \$73 higher than in Florida. This shifts up the entire budget constraint but does not change its shape. By 2000, Illinois eliminated all initial earnings disregards, but had

---

<sup>6</sup> Some states were granted federal waivers, allowing them to alter their AFDC program. Michigan and California were the first to do this in 1993; a total of 6 states changed their disregard policies before 1996.

<sup>7</sup> Because we want to emphasize the changes in earnings disregard parameters, we ignore inflation adjustments in Figures 2a to 2c. In reality, benefit guarantee levels eroded due to inflation in most states between 1990 and 2000, shifting the 2000 income constraint line down relative to 1990.

enacted a lower benefit reduction rate of 66 percent. As a result, women who were not working had less incentive to enter the labor market and work only a few hours; but they had a greater incentive to work more hours. The breakeven point expanded to \$1155 (165 hours/month at \$7/hour, or about 40 hours/week), and many more women who left welfare to work in low-wage and part-time jobs would have received subsidies.

Finally, Figure 2c shows that the state of Texas made very few changes after the 1996 reforms. The income constraint facing this same woman is identical in 1990 and 2000 in Texas, with only a very slight change in the guarantee rate. Eight states essentially kept the AFDC rules post-1996; some other states made only small changes.

Table 3 summarizes how the variation across states widens between 1990 (when all states ran AFDC programs) and 2000. The first six columns show the total welfare benefits paid to a welfare recipient who is in her first, sixth, and thirteenth month of employment in 1990 and 2000. We assume this woman earns \$7/hour and works 30 hours/week. The variation in 1990 comes from variation in state benefit levels and federal variation in earning disregard rules (see Table 1). The variation in 2000 comes from state variation in benefit levels and earnings disregards.

Table 3 shows that welfare benefits for working welfare recipients are much more extensive for women in 2000 compared to 1990. In some states the welfare payments available during the first month of work is actually lower in 2000. This is because the initial earnings disregards were quite high under AFDC for a woman with child care and work expenses, and because there was inflation erosion in the guarantee level in most states between 1990 and 2000. But the continuing subsidy to work after the first few months is much higher in most states by 2000.

Table 3 also indicates that the variation across states in benefits available to a woman leaving welfare and working for a year rises significantly after welfare reform. We calculate what we call the “expected income gain from work” in each state for 1990 and 2000, which is affected by both benefit levels and earnings disregard rules. We first calculate the difference between total income received during each of the first 12 months of work at a given number of work hours for a woman earning \$7/hour, minus what she would receive if she did not work during these twelve months (essentially, the guarantee level in the state since we assume no other income sources than work and welfare.) If there were no earnings disregards, this calculation would simply be her total earnings minus the annual benefit maximum; the greater the earnings disregard, the higher is estimated income since earnings are supplemented by welfare benefits. We do this calculation for each month at 25, 30, 35, and 40 hours of work.

The last column in Table 3 shows the difference in the expected income gain from work at 30 hours/week in 2000 versus 1990 (all of these numbers are adjusted for inflation and expressed in 2000 dollars.) This is an estimate of how the incentive to enter work has changed, largely due to changes in earnings disregards.<sup>8</sup> It is clear in Table 3 that the income benefits to work have risen substantially in a number of states. At the maximum, women in Connecticut can expect to earn \$5132 more in 2000 during their first 12 months of work at 30 hours/week than they did in 1990. Fourteen states show income gains of more than \$2000. Relative to annual pre-tax earnings of about \$10,500 (1500 hours times \$7/hr) these gains are substantial.

---

<sup>8</sup> These calculations are also affected by changes in benefit guarantee levels. Most states, however, made relatively minor changes in benefits over this decade; most of the benefit changes are due to inflation erosion. Because we subtract benefit levels from potential earnings (that is, we calculate the incentive to work by estimating the difference between work income and non-work income), pure inflation effects that shift the guarantee down over time are differenced out in this calculation.

To calculate a broad measure of the state increase in work incentives due to expanded earnings disregards, we average the difference in the expected income gains from the first 12 months of work at 25, 30, 35, and 40 hours of work for each state. Based on these calculations, we define three groups of states. “Low-change” states are the fifteen states that have the lowest changes in their average expected income gain to work between 1990 and 2000, based on our calculations. These are largely states that made little change in their earnings disregards after the 1996 reforms.<sup>9</sup> The net gain to work in these states (averaged across the four hours categories) varies from -\$648 (NM) to \$477 (GA). “High-change” states are the fifteen states with the largest changes in average expected income gain to work between 1990 and 2000; their average gains vary from \$1842 (NV) to \$5757 (CT). These are states that significantly increased their initial earnings disregard and/or significantly decreased their benefit reduction rates.<sup>10</sup> The remaining 21 states are designated as middle-change states. (The District of Columbia is included, so we have 51 ‘states.’) The middle category includes states like Illinois, whose earnings disregard changes might increase work subsidies for some hour/wage combinations and decrease them for others.

We show the evolution in the amount of welfare benefits available to workers in these three groups of states from 1984 through 2003 in Figures 3a and 3b. Figure 3a graphs the trends in real yearly benefits from welfare in the first year after a welfare recipient (a single mother with two children) goes to work for 30 hours/week at \$7/hour (assumed constant in real terms over the period). The thick solid line shows average real

---

<sup>9</sup> These states include AL, AR, AZ, CO, GA, KY, MD, MS, NM, NY, OR, PA, SC, SD, VA. Note that this is a mix of states from all parts of the country.

<sup>10</sup> These states include AK, CT, DC, DE, HA, IA, MA, MO, NV, NH, NJ, ND, OH, RI, WI. Again, this is a mixed group of states.

yearly benefits in the high-change states. As expected, there is a sharp increase in the ongoing average welfare benefits available to a working welfare recipient after 1995 in these states, after a decade of little change. In contrast, there is very little increase in the middle-change states (thin solid line) or low-change states (dotted line).<sup>11</sup>

One's initial reaction might be that the strongest comparison is between the high-change and the low-change states. But note that from the standpoint of drawing inferences about the effects of changes in earnings disregard policies, Figure 3a suggests that the better comparison might be between high- and middle-change states. These two states show identical trends in earnings disregards prior to 1994 (the differences in amounts are largely due to higher benefit levels in high-change states, which lead to higher benefits at 30 hours of work.) By the year 2000, however, women working 30 hours per week in high-change states were eligible to receive almost \$2,000 more per year than similar women in middle-change states. Low-change states have lower benefits available to a 30-hour-per-week worker throughout this period, and the trends are somewhat different.

Figure 3b shows the same plot for a woman who enters work from welfare, but works 40 hours per week. The differences across states are even sharper in this plot. Indeed, our high-change states are almost all states whose earnings disregards have increased so much that they subsidize full-time work for at least some period after women leave welfare. There is very little subsidy to full-time work after 1995 in either the middle-change or the low-change states.<sup>12</sup>

---

<sup>11</sup> The lines in Figures 3a and 3b are not population weighted; every state counts the same in the group averages. Creating group averages that are population weighted by states produces the same conclusions.

<sup>12</sup> One might object to characterizing work incentives by the total amount of cash welfare a woman might continue to receive in the year after she enters work, on the grounds that many welfare recipients do not

Figures 3a and 3b indicate that the returns to work for women in high-change states increased much more than those facing women in middle-change or low-change states. The comparisons across these groups of states will be key to our analysis of whether or not these state changes actually increased work behavior. Before turning to that analysis, however, we summarize the prior literature on the expected effects of income disregards on labor supply.

### **III. Literature on the Labor Supply and Income Effects of Welfare Program Design**

Changes in benefit disregards are typically viewed as equivalent to changes in tax rates. An extensive literature has investigated the elasticity of labor supply to changes in wage and tax rates.<sup>13</sup> Heckman (1993) summarizes this literature by noting that labor supply elasticities appear to be quite low for those already working; that is, the impact of changes in wages on hours of work among workers is small. Most of the elasticity of labor supply appears to occur on the extensive margin; that is, the decision to participate in work or not. This is true for both male and female labor supply, although the responsiveness of female labor supply is greater.<sup>14</sup> Most of this literature, however, focuses on the responses of men and married women, while we are interested in female household heads with children.

Four different U.S. public assistance programs have been used to study the specific question of how earnings disregards affect labor supply among single mothers:

---

understand the complicated set of rules for benefit determination. However, if we focus only on welfare benefits available to women in their first month of work—information that is likely communicated by local welfare offices—the trends across the three groups of states are nearly identical. Furthermore, given the large number of women leaving welfare in the mid-1990s, we would expect that approximate information about the availability of ongoing benefits would become known within low-income neighborhoods.

<sup>13</sup> For instance, see Auerbach and Slemrod (1997) or Blundell and MaCurdy (1999).

<sup>14</sup> As Heckman notes, male labor supply responsiveness appears to have increased as men's overall participation rates have fallen, providing more leeway for a participation response.

the Aid to Families with Dependent Children (AFDC) program; the experimental Negative Income Tax programs run in the 1970s; the experimental “waiver programs” run in the early and mid 1990s by some states; and the Earned income Tax Credit.

The early AFDC-based literature is summarized in Danziger, Haveman and Plotnick (1981), who conclude that there are big differences in the estimated effect of the benefit reduction rates in different studies, and that the estimates seem very sensitive to specification and data definitions.<sup>15</sup> Moffitt (1992, 2002) provides a more updated summary of this literature and concludes that the labor supply of female heads is remarkably inelastic, with little response to major changes in benefit levels, benefit reduction rates, and labor market opportunities. In his specific discussion of benefit reduction rates, Moffitt concludes that the increase in labor supply induced by lower benefit reduction rates among welfare recipients is offset by the decrease in labor supply among workers near the break-even point. Note that this conclusion is a judgment, however, with regard to AFDC. In most of these studies there is no separate estimate of these effects; only an aggregate impact is estimated. Hence, one cannot tell from this research whether the impact of lower benefit reductions rates on labor supply behavior is non-existent, or whether there are positive and negative effects that cancel each other out.

The Negative Income Tax (NIT) experiments that operated in the 1970s were designed to explicitly test the behavioral and income effects of varying levels of  $G$  and  $t$ . Burtless (1986) concludes that lower benefit reduction rates appear to create positive work incentives for welfare recipients who are not working. The net effect in the total

---

<sup>15</sup> This is perhaps not surprising since the federal government imposed a standard benefit reduction rate across states. State differences arose from differences in initial earnings disregards for child care and work expenses, but the data on these were extremely limited. As a result, there was no agreed-upon way of calculating effective earnings disregards and different studies take different approaches. In comparison, state benefit guarantees were well-defined and well-documented and hence estimated with more precision.

low-income population is slightly lower work incentives, however, suggesting that the negative work incentives among those near the break-even point must also be significant.

The welfare-to-work experiments run by the states in the early 1990s also included changes in earnings disregard policies. States received waivers from the Federal government that allowed them to offer alternative programs to AFDC with stronger work incentives. The Federal government required that states evaluate these programs with a rigorous random assignment design. In most cases, this meant that a “bundle” of reforms (mandatory welfare-to-work, time limits, changes in benefit disregards, etc) was compared to the old AFDC program, making it difficult to separate out the impact of any one of these reforms alone. Hence, although virtually all of these experiments resulted in increases in labor supply and reductions in welfare utilization, it is hard to say how much of this might be due to the lower benefit reduction rate.

The Minnesota Family Investment Program (MFIP) had a more complex experimental design that allowed separate evaluation of the mandatory welfare-to-work program and the lower benefit reduction rate (Miller, et. al., 2000). The results indicated that the lower BRR appears to have had little labor supply effect. The increase in labor supply seems to have come almost entirely from the mandatory welfare-to-work program and its associated sanctions. The lower BRR did provide substantial wage subsidies to those workers who left welfare, however, and significantly increased their incomes and lowered poverty rates. A review of a large number of these experimental state programs (Bloom and Michalopolous, 2001) indicates results consistent with MFIP. Programs with greater earnings disregards generally seemed to have larger increases in income and

greater declines in poverty, but appeared to have no greater labor supply effects than programs that included only mandatory welfare-to-work requirements.

In contrast, the Canadian Self-Sufficiency Project (SSP), which operated in the mid-1990s, seemed to suggest that financial incentives could both increase labor supply and reduce poverty. SSP was a randomized controlled trial study of an earnings supplement given to full-time workers who had been on welfare for over one year. Relative to a control group, a program group offered a subsidy equal to about CA\$10,000 per year in 1992 had about 15 percent higher labor force participation while receiving the subsidy, as well as significantly lower poverty rates (Michalopoulos, et al, 2002). This is (very roughly) a bit more than \$8,000 in US-2000 dollars, or about 4 times the average difference between income gains in high and low-change states. This suggests that very large financial incentives can induce work, although the labor supply (and income) effects faded very quickly after the subsidy ended.

The expansion of the Earned Income Tax Credit (EITC) provided an alternative opportunity to study the impact of changes in implicit marginal tax rates among single mothers in the 1990s. The EITC increases the implicit wage for non-workers as they enter the labor market, which should increase the labor force participation incentive for non-workers. But the subsidy is capped at a maximum amount over a range of hours, and then phases out; this should reduce labor supply among higher earners. Between 1990 and 2000 the EITC expanded substantially. For instance, the initial wage subsidy increased from 14 percent to 34 percent (40 percent for mothers of two or more children), while the maximum credit more than doubled and the phase-out rate increased from 10 percent to 16 percent (21 percent for mothers of two or more children.) For single

mothers, the evidence indicates a significant positive effect of the EITC expansions on labor force participation. Meyer and Rosenbaum (2001) estimate that 60 percent of the increase in female labor supply between 1984 and 1996 was due to EITC expansion. There seems to be little effect on hours among those already working.<sup>16</sup> Hence, in comparison to the AFDC/NIT literature, the EITC literature shows a clearer net positive effect on labor supply among female household heads, with strong initial participation effects and few offsetting declines in labor supply among higher earners.<sup>17</sup> Of course, the EITC operates through the tax system. Given the difficulty in understanding changing marginal tax rates within the tax system, it is perhaps not surprising that women who are already working do not respond to the higher marginal tax rates from the EITC; it is not clear that they understand them.

In summary, the early evidence from the NIT experiments and from early AFDC studies show mixed (but generally small) effects of earnings disregards on labor supply. This is consistent with the experimental evidence on actual earnings disregard changes in state waiver programs from the early-1990s that show few labor supply effects. In contrast, the SSP and EITC evidence suggests that sizeable earnings subsidies can induce significant labor supply effects.

The impact of earnings disregards on poverty has generally received less attention than their impact on labor supply. Perhaps surprisingly, there is little research on the impact of AFDC on overall income levels. Some results from the Negative Income Tax suggest that housing and educational outcomes improved among NIT recipients (Hanushek, 1986). The experimental studies of state waiver programs in the 1990s (cited

---

<sup>16</sup> A simulation in Meyer (2002) confirms these results. Eissa and Liebman (1996) show similar results.

<sup>17</sup> In contrast, married women's labor supply seems negatively affected by the EITC, as one might expect (Eissa and Hoynes, 1998).

above) provide the strongest evidence that greater earnings disregards can have positive income effects and negative poverty effects. The tax subsidies in the EITC also appear to produce income and poverty benefits. The Council of Economic Advisors (2000) estimated that the EITC removed 4.3 million persons from poverty in 1998.<sup>18</sup>

In all, this literature suggests that the large increases in state earnings disregards following welfare reform might not have very large effects on labor supply, but should help increase income. To our knowledge, there are no studies that focus on the impact of these state earnings disregard changes. The next two sections investigate this question.

#### **IV. A Simple Investigation of the Effect of Earnings Disregards on Labor Supply and Income**

In this section, we perform some simple tests of whether low-skilled single mothers in the states that substantially increased earnings disregards showed larger increases in labor supply or income. In the next section we undertake regressions to test the impact of changing earnings disregards in a more structured estimation process.

##### *A. The Data*

We utilize three sources of data in this research and describe them briefly here. A Data Appendix provides more detailed information on data sources and variable definitions. First, we start by looking at the Outgoing Rotation Group (ORG) data from the Current Population Survey (CPS). The ORG data includes information from all persons in their fourth and eighth interview months (one fourth of the CPS is in the ORG each month). By aggregating this data across all months we have quite large annual samples, even when selected by gender and skill level. For instance, in the 1990 data

---

<sup>18</sup> Hotz and Scholz (2001) summarize the research that indicates positive effects of the EITC on income.

there are 3709 single mothers with less than a high school degree. In the 2000 data there are 2322 such women. We use ORG data from 1984 through 2003.

The ORG contains information on current workforce participation and hours of work. It has no information on income or public assistance usage, however. We supplement the ORG with the March CPS data. A special supplemental survey each March asks CPS respondents about their income sources in the previous year. (Hence we use data from the 1985-2004 March CPS to get information on calendar years 1984-2003.<sup>19</sup>) Because this comes from only one month's CPS sample, the sample sizes are much smaller. For instance, in the 1990 data there are 1572 less-skilled single mothers, while there are 991 in 2000.

Finally, to test the robustness of our results with an alternative source of data, we use information from the Survey of Income and Program Participation (SIPP). The SIPP is a longitudinal data set, which selects a panel of respondents and collects monthly information (based on interviews every four months) from them for an extended period of time. We use data from the 9 SIPP panels that were launched in 1986, 1987, 1988, 1990, 1991, 1992, 1993, 1996, and 2001. These lasted from 24 months (1988 panel) to 52 months (1996 panel). The longitudinal nature of this data set lets us look at behavioral changes for the same woman over time in our econometric estimation in the next section. The data include both labor market and income information. We have SIPP data from 1986 through 2003, with sample sizes for less-skilled single mothers ranging from 947

---

<sup>19</sup> The labor market information we use from the CPS is based on questions about labor force involvement last week (consistent with ORG), so for this information we use the 1984-2003 CPS data.

(1993) to 334 (1986).<sup>20</sup> We use one month's observation from each woman in each year (so annual averages do not contain multiple observations from the same woman), using the data she reports in her last interview month in each year.

For all of these data sets, we look at single mothers, defined as unmarried women between the ages of 18 and 54, living with children age 18 or younger. In most cases, we look only at single mothers with less than a high school education, the group most highly affected by changes in welfare. We refer to this group as less-skilled single mothers.<sup>21</sup>

Our earnings disregard information by state are taken from the Urban Institute's Welfare Rules Database. This database provides information on state-specific program parameters for the state TANF programs that replaced AFDC following welfare reform. The database includes detailed annual information not only on state earnings disregard policies, but on all other policies related to cash welfare programs. We also use this data source to define the welfare policy variables we include in the regressions below. (More detail is in the Data Appendix.)

### *B. A Few Graphical Comparisons*

To provide a sense of the data, we compare differential behavior among less-skilled single mothers across three groups of states. The high-change states are the 15 states where the expected income gain from work increased the most between 1990 and 2000, as discussed above. The low-change states are the 15 states whose expected

---

<sup>20</sup> Because the SIPP panels overlap, we often have women from multiple panels in the same year. For instance, our 1988 data includes women from the last interview of the 1986 SIPP, from the ongoing 1987 SIPP and from the early interviews of the 1988 SIPP.

<sup>21</sup> We weight the SIPP data by person weights throughout our analysis (important because of attrition in the SIPP over a panel.) Results that we present from the CPS and ORG are not weighted, but are very similar to weighted results.

income gain from work increased the least, while the middle-change states are the remaining 21 states. We saw in Figures 3a and 3b how much these states differed in the income gains available to a single mother who left welfare for work following welfare reform.

Figure 4a graphs the average probability that a less-skilled single mother works in each of these three groups of states, using the ORG data. The thick solid line shows the high-change states, the thin solid line shows the middle-change states and the dashed line shows the low-change states. About one-third of our sample work in the late 1980s in all groups of states. That percentage increases rapidly from the mid-1990s through 2000. The percentage working slows or decreases after 2000, as the economy slows down.

All three groups of states show quite similar trends in Figure 4a, although the high-change states appear to have suffered a greater decline in work in the early 1990s and show a somewhat more rapid rise after 1993. This greater cyclicity of employment in high-change states relative to the other state groups poses a serious challenge to estimating the causal effects of earnings disregard policies. Although work rises more rapidly in these states after 1993, part of this might be due only to the business cycle and would have occurred in the absence of policy changes. We attempt to control for the independent effects of economic conditions in the regression analyses below, but the data in the Figure 4a foreshadow our results. Although work appears to decline in the low-change states when the economy slows down in the early 2000s, there is little evidence of different behavior among women in the high- and middle-change states, despite quite different work incentives.

Figure 4b graphs average hours of work in each of these three groups of states (based on ORG data), including the zeroes for those who don't work. Figure 4b looks almost identical to Figure 4a, with large increases in hours of work within this population, but little evidence of a faster increase among less-skilled single mothers in the high-change states.

Figure 5 graphs average annual income for less-skilled single mothers in each of these three groups of states, based on the March CPS data. Women in high-change states have slightly higher monthly income levels than in low- or middle-change states. The pattern over time is identical, especially among the high- and middle-change states after 1998, showing no particularly higher incomes reported in states with significantly increased earnings disregards.

In short, the raw data do not indicate that low-skilled single mothers in high-change states either worked substantially more or had higher incomes than similar women in states with fewer subsidies. We have looked at similar plots using the SIPP data and for a wide variety of other variables without seeing noticeably different patterns. We summarize these results with simple difference-in-difference calculations.

### *C. Difference-in-Difference Comparisons*

We compare the years 1991-93 to the years 2001-03, which includes a period several years before welfare reform is enacted and a period several years after welfare reform is implemented.<sup>22</sup> We compare the change in a variable (take hours of work as an example) between these two groups of years in state group 1 versus state group 2. Thus we difference across years and between two different state groups. We make three state

---

<sup>22</sup> 1991-93 is also before almost any of the state waiver programs were implemented as well.

group comparisons: high-change to middle-change states, high-change to low-change states, and middle-change to low-change states. For state groups  $s_1$  and  $s_2$ , and years  $y_1$  and  $y_2$ , our difference in difference calculation is

$$(1) \quad (\text{Hours}_{s_1 y_2} - \text{Hours}_{s_1 y_1}) - (\text{Hours}_{s_2 y_2} - \text{Hours}_{s_2 y_1})$$

Table 4 shows these calculations for all three state comparisons and for six different variables, four focused on labor force participation (probability of working, probability of working full-time, probability of working part-time<sup>23</sup>, and hours of work) and two focused on income (annual income and annual welfare income.) We show these calculations using data from all three data sets to test robustness across data sources. We report Huber-White standard errors.

The top left-hand cell on Table 4 indicates that the ORG data reveals high-change states showed a 3.6 point greater increase in the probability of working among low-skilled single mothers between 2001-03 and 1991-93, than did middle-change states. The March CPS data show a 4.2 point greater increase in work in high-change states, while the increase is 3.5 percent in the SIPP data. Although all are positive, none are significant at the 5 percent level. As discussed above, the comparison between high- and middle-change states is the most persuasive, since these states look most alike in their work incentives prior to welfare reform. Columns 4 through 6 indicate that there is more evidence of significant increases in work over this period in high-change states versus low-change states, but these states were also more different to start with; this comparison may reflect those preexisting differences rather than the effect of welfare reform. Columns 7 through 9 indicate there are no significant differences in the change in work probabilities between middle- and low-change states.

---

<sup>23</sup> Full-time work is 35 or more hours per week; part-time work is less than 34 hours per week.

The labor market results in Table 4 show some evidence that work increased faster in the high-change states versus the other states. This increase was focused in part-time work relative to middle-change states, and in full-time work relative to low-change states. These results are not significant in all data sets, however, and are generally small. Compared to the much greater incentives to work more in these high-change states (see Figure 3b), these results seem quite weak.

The bottom rows in Table 4 look at comparisons in total income and in public assistance income. Higher earnings disregards should mean that single mothers who leave welfare are receiving greater payments as they go to work, hence there should be less loss of welfare in high incentive states and more overall income. There is no evidence of more on-going public assistance receipt or greater income supplementation in states with high earnings disregards in these difference-in-difference calculations.<sup>24</sup>

To summarize, while one might conclude that there is some evidence to suggest small effects of larger earnings disregards on labor supply based on the contrast between high-change and other states over the 1990s, this evidence is quite sensitive to the time period used in the analysis and to the comparison group of states. In our opinion, these simple comparative results suggest that recent earnings disregard changes had little effect on labor supply and are consistent with the earlier literature indicating few effects of earnings disregards.

---

<sup>24</sup> We have also done the same calculations, comparing the years 1998-2000 with the same base years 1991-1993. We prefer the results in Table 4, as they provide a slightly more long-term comparison and because the macroeconomy is more comparable in the early 2000s to the early 1990s. The results from this alternative comparison are even weaker than those in Table 4, since the ORG data show no labor force effects in high-change states when using 1998-2000 as the comparison years.

## V. Regression-adjusted Estimates of the Effects of Earnings Disregards

The difference-in-difference calculations in the previous section suggest that earnings disregards changes played a negligible role in the large increases in work among low-income women over the 1990s. This inference rests on the assumption that the high-change states would have experienced similar changes in work behavior but for the differences in their respective earnings disregard policies. In fact, however, the economic, demographic, and policy environments may have changed in different ways in these three state groups over time so as to offset and obscure the effect of earnings disregards. For instance, unemployment appears to be more cyclical in the high-change states, rising more in the early 1990s and falling more over the expansion of the late 1990s. In this section we control for other differences in these states, using panel data to estimate the effects of earnings disregards on labor force behavior among single mothers over time within states.

### A. Methodology

We first discuss our panel data estimation strategies, and then discuss the individual fixed effect estimates we implement with the SIPP data. All three of our datasets can be used to create a state-by-year panel of data on the key variables we want to estimate. We use our sample of single mothers with less than a high school degree, using data from either the ORG or the March CPS from 1984 through 2004, or from the SIPP from 1986 through 2003.<sup>25</sup>

We estimate regressions of the following form:

---

<sup>25</sup> Our SIPP data is only available through 2003 (the last year of the 2001 panel). We stop in 2004 with the ORG and March CPS because several policy variables were not yet available past this date.

$$(1) \quad H_{ist} = \beta_1 ED_{st} + \beta_2 U_{st} + \beta_3 P_{st} + \beta_4 X_{ist} + \gamma_1 SFE_s + \gamma_2 YFE_t + \varepsilon_{ist}$$

where  $i$  indexes the individual,  $s$  indexes the state, and  $t$  indexes the year. SFE represents a vector of state fixed effects, controlling for any state-specific differences that are unchanged over time. YFE represents a vector of year fixed effects, controlling for any year-specific changes that affect all single mothers. For instance, changes in the federal EITC are implemented everywhere in the same year and YFE would control for these effects. The inclusion of state and year effects means that identification of the coefficients relies on variation in these variables within a state over time. Our reported standard errors are clustered at the state level to allow for arbitrary correlations of  $\varepsilon$ , the error term, within states over time.

H is one of five dependent variables. We focus on two measures of labor force involvement. We use a 0/1 dummy variable that indexes whether or not the woman is working at the time of the survey,<sup>26</sup> and we use a continuous hours variable indicating how many hours the woman worked during the week of the survey, where nonworkers have zero hours.<sup>27</sup> We use three different dependent variables to measure income supplementation (recall we only have income data from the March CPS and the SIPP.) We look at total income<sup>28</sup>, at cash welfare income from either AFDC or TANF, and at a 0/1 dummy variable that indexes whether or not the woman is receiving cash welfare assistance. All income data is deflated by the price index for Personal Consumption Expenditures (\$2000).

---

<sup>26</sup> For the ORG and the March CPS, this refers to work last week; for the SIPP this refers to work during the interview month.

<sup>27</sup> We estimate linear probability models for hours and for work probabilities.

<sup>28</sup> For the March CPS, this refers to income last year; for the SIPP it refers to the interview month (multiplied by 12 to make it comparable).

Our key variables of interest (ED) parameterize the earnings disregard rules over time. We implement two separate specifications to assess the effects of these policies on work behavior. In the first specification, we simply include measures of the disregard rates that are applied to earnings in both the first and sixth months of an employment spell that follows welfare receipt. (This is  $1-t$  as defined in Figure 1, the marginal disregard on the next dollar of earnings.) As Table 1 indicates, the earnings disregard rate was set by federal law at 33 percent in the first four months of work and at zero afterwards (that is, a 100 percent tax rate on earnings) until the mid-1990s.<sup>29</sup> After welfare reform in 1996, we use the state-specific earnings disregard rates that in many cases change according to the number of months a woman has been working.

In the second specification, we use a state-specific measure of the average disregard rate (rather than the marginal rate) at 40 hours of work. This is the total dollars a woman would receive from welfare during the first 12 months after going to work and working 40 hours per week at \$7/hour, divided by the total dollars she would receive during these 12 months if she did not work and received the maximum benefit amount (this ratio is expressed as a percentage, so multiplied by 100). If she receives no welfare income after going to work, this measure would be 0; if she has a 100 percent earnings disregard, so benefits are unaffected by increases in earnings, this number would be 100. This measure is calculated using detailed information on state benefit calculation rules over time.<sup>30</sup>

---

<sup>29</sup> Note that a few states have effective disregard rates that differ from 33 percent due to the peculiar method by which they calculate benefits – for example, in the late 1980s Mississippi calculated benefits as 0.6 times the difference between the need standard and countable income. Since the 33 percent disregard applies to countable income, the effective disregard is  $0.6 \times 0.33$ , or 19.8 percent.

<sup>30</sup> We also tested specifications using this same calculation at 30 hours of work per week, to see if our results were sensitive to the incentives at different hours of work. The 30-hour variable never showed a different effect than the 40-hour variable for any dependent variable or data set.

To control for the effects of different economic conditions, policies, and demographic changes we include as control variables  $U$ , the unemployment rate in each state and year;  $P$ , a vector of state welfare policy parameters in each state and year; and  $X$ , a vector of individual and family characteristics for each sample observation.  $X$  includes race, ethnicity, marital status, the number of children under age 19 in the household, the number of adults in the household, and the age of the mother, defined as four dummy variables for ages 18-22, 23-30, 31-40 and 41-54 (allowing for non-linear differences between these age groups.)

Our vector of state welfare program parameters,  $P$ , controls for policies other than earnings disregards that may affect work behavior. All of our regressions include a control for (inflation-adjusted) maximum benefit levels. As Figure 1 indicated, the maximum benefit ( $G$ ) and the various disregard parameters are key determinants of work behavior. We use two alternative specifications to control for other program parameters. First, we control for when major welfare reforms are implemented. This includes a dummy variable that turns “on” when a major state-wide waiver is implemented in the mid-1990s, and a dummy variable that turns “on” when the state’s TANF plan is implemented after 1996.<sup>31</sup> These variables have been widely used in the literature. Different states elect different “bundles” of policy change and these dummy variables control for the overall effect of changes without distinguishing between them.

Our second approach is to parameterize the specific policies within states, including controls for time limits, for exemptions from work requirements, for family

---

<sup>31</sup> If the waiver or the TANF plan is only in effect for part of the year, the dummy variable is replaced with the fraction of the year in which the plan was in effect. The waiver variable turns “off” when a TANF plan is implemented. We have duplicated our analysis using a single variable that turns on when *either* a waiver or a TANF plan is enacted (whichever happens first) and then stays on. The results are very similar.

caps, and for state sanction policy. These are dummy variables, equal to one in states where more stringent policies are implemented. This second specification provides potentially better controls for specific policy design differences across states, but it has its limitations as well. It is hard to parameterize these policies except in very aggregate ways (i.e., characterizing sanctions as ‘stringent’, ‘moderate’ or ‘lenient’), and there may be interactions across these policies that are important. In this case, the former approach of simply controlling for a separate state TANF effect in each state could be more effective than trying to control for the effects of separate policy elements.

The Data Appendix lists all of the variables used in the various specifications and gives detailed definitions and sources. Appendix Table 1 shows the means of these variables for each data source, across all years and separately in the years before and after welfare reform is implemented.

Unlike the ORG and the March CPS, the SIPP data provides longitudinal data on individual single mothers over time. The estimation approach outlined above is the only one that we can utilize with the cross-sectional ORG or March CPS data, but it does not take account of multiple observations on the same person that exist within the SIPP data. Hence, using SIPP data, we can estimate a regression that takes account of individual omitted variables by controlling for an individual fixed effect. This should provide a better identified estimate of the earnings disregard effect. This is the regression

$$(2) \quad H_{ist} = \beta_1 ED_{st} + \beta_2 U_{st} + \beta_3 P_{st} + \beta_4 X_{ist} + \gamma_1 YFE_t + \gamma_2 IFE_i + \mu_{ist}$$

where  $IFE_i$  is a vector of individual fixed effects, estimated for each person  $i$  who has more than one observation in the data.<sup>32</sup>

---

<sup>32</sup> There are no state fixed effects in equation (2), since these would be identified only if individuals moved across state lines over time. The race and ethnicity variables are also not identified in equation (2).

As noted above, we extract one observation per year for each single mother in the SIPP panels, using the last interview month in which she has data for that year. For women who are single mothers throughout the SIPP panel, this will give us from 2 to 4 observations per person, depending on the length of each SIPP panel (this estimation drops all the women who are observed as single mothers only once.) The regression identifies the effects of earnings disregards from changes in the behavior of women over time within states where earnings disregard parameters change.<sup>33</sup>

When we discuss the SIPP estimates below, we present estimates based on both equations (1) and (2) using the SIPP data, referring to the latter as SIPP Fixed Effect regressions. Because these regressions allow us to control for differences between states and among individuals, we believe that these regressions should provide our best estimate of the impact of the earnings disregards on individual behavior.

#### *B. Effects of earnings disregards on labor force participation*

To give a flavor of the full regression results, Table 5 shows the results from our estimates of the probability of work, using our first specification of disregard rates which controls for the state disregard rate at one and six months. The first four columns show how the results compare across our three data sets in a regression that includes only the disregard rates, the maximum state welfare benefit, state fixed effects, and year fixed effects. Columns (1) through (3) utilize equation 1 to estimate a panel data regression with each of our three data sets, while column (4) utilizes equation 2 to estimate an individual fixed effects regression with the SIPP data. Columns (5) through (8) also

---

<sup>33</sup> As before, standard errors are clustered at the state level.

include the dummy variables for TANF and state waiver program implementation, as well as the unemployment and demographic control variables. The last four columns replace the TANF and state waiver program dummy variables with controls for specific state welfare policies.

The disregard rate in month 1 is insignificant in all specifications and all data sets. The disregard rate in month six, however, appears to have a positive and moderately significant effect on hours worked in the ORG and CPS data, but the effect in the SIPP data is much smaller and insignificant. Interpreted causally, the ORG results in column 9 (specification 3) imply that a change from no disregard (as existed under AFDC) to a 50 percent disregard (the modal value in 2000) would lead to a 3.1 percentage point ( $50 \times 0.063$ ) increase in the fraction of single mothers without a high school degree who work. This represents about one-fifth of the total increase in work among this population between 1995 and 2000, and is relatively large. The SIPP data, however, show smaller and insignificant coefficients on the earnings disregard parameters. We find the SIPP fixed effect results most convincing, which would suggest a zero effect. We conclude that while some data sets and specifications suggest a positive effect of greater earnings disregards on labor supply, the best evidence indicates that these effects are not significant.

The coefficients on the remainder of the variables are not surprising. Because of the inclusion of state fixed effects, only variables that change significantly over time within states have robustly estimated effects. The effects of recent policy changes are quite weak. Higher benefit guarantees are not associated with changes in labor market

involvement.<sup>34</sup> Both the implementation of TANF and of waivers have small and insignificant effects. This is perhaps surprising since previous research has found significant effects of waivers and TANF on labor market involvement. Two previous studies that investigate the effects of welfare reform on labor market involvement using a similar methodological approach are Moffitt (1999) and Schoeni and Blank (2000). Both of these papers find significant effects of TANF and waiver implementation on both labor force participation and hours of work among high school dropouts, but results are based on different data, aggregated into educational categories and the years included in the regressions are different.<sup>35</sup> When these two variables are replaced with four variables controlling for specific policy choices, there is little evidence that these other policies have large effects either, although fewer work exemptions generally seem to increase labor supply and family caps appear to reduce it (an unexpected effect).<sup>36</sup>

The coefficients on the other included variables appear consistent with the findings of previous work. A 1-point rise in the unemployment rate is associated with more than a 1 percentage point decline in labor force participation, and this effect is even bigger in the fixed effect specification.<sup>37</sup> Younger single mothers with more total children, with more preschoolers, and who are never-married are less involved with the labor market. Both black and Hispanic women are also less likely to be in the labor market.

---

<sup>34</sup> This is a common result in panel data estimates with state and year fixed effects, since benefit levels largely trend downward with inflation and do not change markedly within states over time.

<sup>35</sup> Both of these earlier studies use March CPS data, aggregated into cells by demographic characteristics. Moffitt (1999) looks only at waiver effects, while Schoeni and Blank (2000) also look at TANF effects.

<sup>36</sup> Selecting the best way to specify these specific policies is difficult, and different researchers have utilized a variety of approaches. Our regressions include a relatively simple specification, which should nonetheless control for the major differences in these policies across states. The important result is that the inclusion of these variables does not affect the estimated disregard coefficients.

<sup>37</sup> This high estimate is consistent with other research showing women's greater labor market responsiveness to unemployment (Hoynes, 2000).

We show Table 5 to provide a sense of the entire set of regression results. From this point on, we look at the coefficients on the disregard variables only, across our two specifications and for each of the three data sets.<sup>38</sup> We show a variety of estimates because we want to assure the readers that our conclusions are robust to alternative data analysis.<sup>39</sup> Table 6 shows these coefficients for two labor market variables. Part 1 of Table 6 shows the coefficients for the probability of working, while Part 2 show the coefficients for estimates of hours worked. As in Table 5, Specification 1 includes only the disregard rates for months 1 and 6, the state maximum benefit level, along with state and year fixed effects. Specification 2 also includes the dummy variables for the implementation of TANF and Waiver programs, as well as the demographic variables. Specification 3 replaces the two implementation variables with the four specific policy parameters for time limits, work exemptions, family caps and sanctions.

Each coefficient reported in Table 6 comes from a different regression. The first four columns show the coefficients on the six month earnings disregard rate from regressions run on each data set and each specification. (These rows duplicate the results shown in Table 5. Although these regressions also include the 1 month disregard rate, we do not report those coefficients which are insignificant in all specifications for all variables.<sup>40</sup>) The second four columns show the coefficient on the average earnings

---

<sup>38</sup> Full regression results for all of the regressions reported in Tables 6-8 are available from the authors upon request.

<sup>39</sup> In addition to the results shown here for single mothers with less than a high school degree, we also looked at results for single mothers with a high school degree or less. The conclusions were not different.

<sup>40</sup> We are not surprised by this result. During the AFDC period of our data (1983 through 1996) first year earnings disregard rates are relatively high (at 33 percent), yet they end within four months, creating little long-term incentive to enter work. A number of states retain this type of structure under TANF. Several states provide very generous disregards in the first few months, but then eliminate them entirely within a few months. Such short-term subsidies may not provide much additional incentive to encourage women to enter the labor market. In contrast, states with earnings disregards that last six months tend to have earnings disregards in place for a year or more. Hence, the six month variable is highly correlated with the

disregard at 40 hours of work (calculated over the first 12 months of work), an alternative way of specifying the effect of the earnings disregard. It is not clear which of these two specifications of earnings disregards are superior. On the one hand, it is appealing to use the program parameters rather than a composite index, since these involve less discretion in construction. On the other hand, the precise parameter shows the incentives at only one point in time, while women who leave welfare and go to work face a changing set of incentives over time in most states. Our average disregard variable provides a better sense of the average incentives women face in each state as they leave welfare.

We have already discussed the results on the probability of working when the earnings disregards are specified as the disregard rate at six months. The results in columns 5-8 of part 1 using the alternative specification of earnings disregards do not change our conclusions from the discussion of Table 5. In fact, the coefficient on the average earnings disregard is never significant. We conclude that earnings disregards have few effects on the probability of working.

Part 2 provides a similar set of estimates for hours of work. The results are markedly similar to those in Part 1. There is some evidence in the CPS and ORG data of relatively small and marginally significant effects (although the significant coefficient for the March CPS in specification 1 for the average earnings disregard is of the wrong sign!) The SIPP results show no effect of earnings disregards on hours of work.

Our overall conclusion regarding the labor force effects of large earnings disregard changes in state welfare policies is that these policies had remarkably little effect. This is perhaps surprising given how large these changes were. We discuss

---

longer-term subsidy to work within a state, which may be the relevant factor determining whether earnings disregards have additional incentive effects on employment.

reasons for this result in the next section, after investigating the income effects of earnings disregards.

### *C. Effects of earnings disregards on income*

As noted before, we expected expanded earnings disregards to increase income, since the dominant movement in the post-reform era was to reduce welfare and increase work. States with large increases in earnings disregards should provide greater subsidies to low-wage women and improve their economic situation. In this section, we investigate these effects across our various specifications, data sets, and estimation approaches.

Table 7 is set up in a way similar to Table 6, showing the coefficients on the earnings disregard variables from a variety of specifications. Part 1 looks at the estimated effects of earnings disregards on total income; Part 2 uses public assistance income as the dependent variable; and Part 3 looks at the probability of public assistance receipt. We look at results across the same three specifications that were also presented in Tables 5 and 6. As before, the first three columns show the coefficients on the six month disregard rate, while the last three columns replace this with the average earnings disregard at 40 hours. Row 1 uses a panel data estimation technique with March CPS data; row 2 shows similar estimates from the SIPP; and row 3 estimates individual fixed effect regressions in the SIPP.

The results in Part 1 indicate that there are no noticeable effects of earnings disregards on total reported income, regardless of how earnings disregards are specified, the other variables in the regression, or the data and estimation technique. Indeed, in a number of cases we even get negative estimates, particularly with the individual fixed

effect estimators in the SIPP. For instance, the SIPP fixed effect estimates using specification 3 suggests that a change of 50 points in the disregard at month 6 would reduce income by \$936 per year.

Because we are surprised at the lack of income effects, in Parts 2 and 3, we investigate the effect of earnings disregards on public assistance usage. Realize that the ability to work a substantial number of hours and still receive public assistance should provide incentives for women to remain on public assistance longer, even while going to work. Hence, one should expect to see a relative increase (i.e., less of a decline) in the probability of public assistance receipt in higher earnings disregard states. This is particularly true if there are no labor supply effects. If women's work behavior changes similarly in high and low disregard states, then the primary effect of earnings disregards should be to increase the receipt of public assistance income at higher levels of work in high disregard states. Part 2 asks whether there is evidence that women are more likely to stay on public assistance in high disregard states. The answer is clearly 'no'. The coefficients in all specifications, data, and estimation approaches are small and insignificant.

Finally, in part 3, we also look at public assistance income. The effect of earnings disregards on cash assistance is uncertain. On the one hand, among women who go to work, public assistance should be higher among women who can continue to receive benefits in high-disregard states. On the other hand, if earnings disregards induce greater labor supply, then women getting a reduced benefit might receive less than women in a low-disregard state who do not go to work at all. Of course, if there are no labor supply effects, then public assistance receipt should unambiguously go up. Part 3 shows no

evidence for positive effects of earnings disregards on public assistance income received by less-skilled single mothers.

As the difference-in-difference estimates suggested in the previous section, we conclude that there is no evidence that earnings disregards are helping to subsidize the incomes of women in high disregard states. In fact, this entire section suggests that earnings disregards had few effects at all, on labor supply or on income, among single mothers. The next section discusses why this might be true.

## **VI. Understanding these results**

Our estimation results are both surprising and somewhat troubling from a policy perspective. Many states implemented large earnings disregards in their TANF programs in order to stimulate greater labor market involvement, and we show little evidence that this occurred. While previous research indicated few effects from earnings disregards on labor supply, the changes implemented by high incentive states were far greater than those studied in much of the previous research literature, and one might have expected these larger policy changes would show greater effects. Furthermore, as discussed above, earlier literature suggested that increases in work among nonworkers were offset by declines in work among already-working women (arrows 3 and 4 in Figure 1). This offsetting effect is very unlikely to have occurred following welfare reform, when states were pushing to move more and more women into work and off welfare. Almost every state halved the number of families receiving welfare in the five years after TANF reforms were implemented, suggesting that very few women entered cash assistance in order to collect some benefits while working. Yet, work effort by single mothers in states

that implemented high disregards does not appear to have increased relative to states that did not.

Even if there were no labor supply effects, one might expect that these earnings disregards would supplement incomes for low-wage workers leaving welfare. Indeed, the MFIP results discussed above suggested exactly this outcome, namely, that earnings disregards had anti-poverty effects but no labor supply effects. Based on this evidence, analysts have supported earnings disregards as a way to help provide additional income as women leave welfare.<sup>41</sup> Hence, the lack of evidence in this paper on income supplementation is also surprising.

In this section we suggest a clear reason for the lack of effects: very few women who go to work at 30 hours or more are taking advantage of earnings disregards over an extended period of time, even in high incentive states. We document this fact below and then discuss reasons for it.

In the analyses above, we show that there are no employment effects of higher earnings disregards; high incentive states and low incentive states had essentially the same increases in work after accounting for differences in economic conditions. Women working in states that increased their disregards, i.e. high-incentive states, remain eligible for cash assistance even if their earnings increase substantially. For women in low-incentive states, however, increased earnings take them past the “break-even point” more quickly and they lose welfare eligibility. Thus, to the extent that earnings disregards are utilized, the number of women who report both earnings and welfare income should rise in high-incentive states relative to low-incentive states.

---

<sup>41</sup> For instance, see Bloom and Michalopoulos (2001) or Blank (2002).

We can test this prediction in the SIPP data, which has monthly reports on all income sources.<sup>42</sup> Table 8 explores the probability that single women combine work and welfare, comparing states with higher and lower earnings disregards. We also differentiate between women working full time (35 hours or more) and part-time. While many states allowed women to remain eligible for cash assistance with moderate earnings levels, only high incentive states allowed women working full time to continue to receive welfare payments. Differences across state groups should therefore be more pronounced for the fraction of women working full-time while on welfare.

Table 8 shows regression results similar to those shown in Tables 6 and 7, where we test the effects of earnings disregards on the probability of working and receiving welfare, using different control variables, different specifications of earnings disregards, and two different estimation techniques (with and without controlling for individual fixed effects within our SIPP data.) We show the coefficients on the earnings disregard variable for regressions that estimate the probability of both working and receiving welfare, for working full-time and receiving welfare, and for working part-time and receiving welfare.

The coefficients suggest that there are no effects of earnings disregards on the probability of combining work and welfare receipt. All estimated coefficients are small and insignificant. This is true for the overall probability of both working and receiving welfare, as well as for the probability of working full time or part time and receiving welfare. In short, once we control for state and year effects, as well as other economic, demographic and policy variables, single mothers in higher disregard states show no

---

<sup>42</sup> The March CPS allows us to see if women have earnings and welfare income over the past year, but we cannot determine if they are receiving both income sources at the same time.

increased probability of receiving welfare while employed. The implication is that women are not taking advantage of the higher disregards in the high-incentive states, and are no more likely to collect welfare while working than in lower disregard states.

To double-check this result with one other data set, we requested administrative data from the Department of Health and Human Services on state reports of working families on TANF. We have this data for several years in the early 2000s. If we compare the share of working families on TANF within our three different state groupings, we find no difference between states with higher versus lower disregards. In 2001, high disregard states report 25 percent of the families who received TANF were also working; versus 26 percent in our middle incentive states. Low incentive states report a slightly lower percent (21 percent) of welfare recipients who work.<sup>43</sup> These administrative data are consistent with the conclusion that women in states with substantial earnings disregards are no more likely to receive welfare while working than women in states with more modest disregards.

These results indicate that women do not utilize the earnings disregards that would allow them to continue to receive public assistance checks even when working a substantial number of hours in high-change states. There are at least three reasons why this might occur. First, there are costs to participating in welfare and these costs may be greater once working. Mothers of children who are working a substantial number of hours may find it harder to find the time to check in with the welfare office on a regular basis. Such costs may include not only time and inconvenience, but stigma effects, as

---

<sup>43</sup> We do not want to make too much of these data, since the base of who is counted in TANF differs somewhat across states and is not entirely comparable. Some states created separate state programs for harder-to-employ women and moved them out of their TANF program.

well as the psychological cost of dealing with welfare office demands and bureaucrats.<sup>44</sup> Once women enter work and have greater earnings, they may decide that they are better off leaving welfare entirely. In fact, women on welfare typically indicate that they believe work requirements are a good idea (Cherlin, et. al., 2000).

Second, women may leave welfare once they are working substantial hours, in order to preserve their remaining welfare eligibility. In a world of time limits, receiving welfare today will reduce future welfare availability. Grogger (2004) has shown that time limits cause women to leave welfare faster when they have an incentive (such as small children) to preserve future eligibility.

Third, many welfare offices made major changes in the ways they dealt with clients following welfare reform. Caseworkers were trained to send a 'tough' message about the need for women to find a job and leave welfare (Gais, Nathan, Lurie, and Kaplan, 2001). It is quite possible that caseworkers did not encourage women to stay on welfare once they started to work, even if the program design allowed it. In some situations, caseworkers themselves may have lacked information about the extensive availability of ongoing benefits to workers. Or, even if they knew this information, they may not have communicated it to clients, preferring to push them off welfare if possible. Hence some women who entered work may simply have assumed they had no more access to public assistance support.

## **VII. Conclusions**

We have found little evidence in this paper that the large earnings disregards enacted by some states post-welfare reform have had significant effects on labor supply

---

<sup>44</sup> Moffitt (1983) shows that such costs can reduce welfare participation.

or that they provided income supplementation to low-skilled single mothers. Estimates from a variety of data sets, specifications and estimation approaches all support this conclusion.

The lack of labor supply effects is consistent with much of the earlier literature on earnings disregards; this is surprising, however, since the changes in earnings disregards the mid-1990s in a subset of states were far greater than those measured in earlier research. Furthermore, given the large reductions in welfare rolls, the lack of labor supply effects is not because greater work among nonworkers was offset by less work and more welfare among those just past the previous welfare eligibility level. It appears that women simply did not take-up the higher earnings disregards in states that offered them. We cannot tell, however, if this was a conscious choice on the part of these women, or if it was the result of misinformation. We expect that this is due to a combination of factors, including reluctance on the part of women to remain in the public assistance system, lack of information about availability of ongoing benefits, and efforts by case managers to move women off welfare sooner rather than later. The complexity of state earnings disregard rules may make them less effective as incentives, particularly for states that have enacted changes in disregard rates that occur as an employment spell lengthens beyond the first few months.

If women are not utilizing these earnings disregards, there is no income supplementation provided by this policy. This is in contrast to some of the waiver experiments, such as MFIP, which shows earnings disregards having a significant anti-poverty effect. The environment in which these waivers were implemented was different, however. Minnesota worked hard to implement this program, encouraging women to

take advantage of the income supplementation that the earnings disregards provided. It is much less likely that caseworkers following welfare reform encouraged women to remain on welfare, even in states with high disregards.

Our conclusion from this research is that state earnings disregards enacted as part of welfare reform, while very large in some cases, have had remarkably little effect on the behavior or the well-being of single mother families. Women who were successful in finding work left welfare participation entirely. Hence, states that enacted very generous disregards show similar changes in labor supply and income to states that enacted more modest changes. Of course, this also means that states with high earnings disregards are paying few benefits to workers, so while they are getting few benefits from these high disregard policies, neither are they paying any costs.

From the point of view of researchers, these results confirm the take-up literature that indicates the gap between on-the-books policies and their actual receipt.<sup>45</sup> Anyone simulating welfare receipt in high-disregard states might assume a substantial number of working low-wage single mother families would receive some ongoing welfare benefits, but this would be inaccurate. In fact, many working women who are eligible for ongoing benefits do not receive them.

---

<sup>45</sup> Currie (2006) provides a recent review of this literature.

## References

- Auerbach, Alan J. and Joel Slemrod. 1997. "The Economics Effects of the Tax Reform Act of 1986." Journal of Economic Literature. Vol 35(2): 589-632. June.
- Blank, Rebecca M. 2002. "Evaluating Welfare Reform in the United States." Journal of Economic Literature. Vol 40(4): 1105-66. December.
- Blank, Rebecca M. 2002. "Can Equity and Efficiency Complement Each Other?" Labour Economics. Vol 9(4):451-468.
- Bloom, Dan and Charles Michalopoulos. 2001. How Welfare and Work Policies Affect Employment and Income: A Synthesis of Research. New York, NY: MDRC. May.
- Blundell, Richard and Thomas MaCurdy. 1999. "Labor Supply: A Review of Alternative Approaches." In Handbook in Labor Economics, Volume 3A, Orley Ashenfelter and David Card, eds. Amsterdam: Elsevier.
- Burtless, Gary. 1986. "The Work Response to a Guaranteed Income: A Survey of Experimental Evidence." In Lessons from the Income Maintenance Experiments, Alicia H. Munnell, ed. Conference Series No. 30. Boston, MA: Federal Reserve Bank of Boston.
- Cherlin, Andrew, Pamela Winston, Ronald Angel, Linda Burton, P. Linsay Chase-Lansdale, Robert Moffitt, William Julius Wilson, Rebekah Levin Coley, and James Quane. "What welfare recipients know about the new rules and what they have to say about them." Welfare, Children & Families: A Three City Study, Policy Brief 00-1. Baltimore, MD: Johns Hopkins University.
- Council of Economic Advisers. 2000. Economic Report of the President. Washington, D.C.: Government Printing Office. February.
- Currie, Janet. 2006. "The Take-up of Social Benefits." In Public Policy and the Income Distribution, Alan J Auerbach, David Card, and John M. Quigley, eds. New York: Russell Sage Foundation.
- Danziger, Sheldon, Robert Haveman and Robert Plotnick. 1981. "How Income Transfers Affect Work, Savings and the Income Distribution." Journal of Economic Literature. Vol 19(3): 975-1028. September.
- Eissa, Nada and Hilary W. Hoynes. 1998. "The Earned Income Tax Credit and the Labor Supply of Married Couples." National Bureau of Economic Research Working Paper 6856. Cambridge, MA: NBER. December.

- Eissa, Nada and Jeffrey B. Liebman. 1996. "Labor Supply Response to the Earned Income Tax Credit." Quarterly Journal of Economics 111(2): 605-37.
- Fang, Hanming, and Michael P. Keane. 2004. "Assessing the Impact of Welfare Reform on Single Mothers." Manuscript draft. Yale University.
- Gais, Thomas L., Richard P. Nathan, Irene Lurie, and Thomas Kaplan. "Implementation of the Personal Responsibility Act of 1996." In The New World of Welfare, Rebecca Blank and Ron Haskins, eds. Washington, D.C.: Brookings Institution Press.
- Grogger, Jeffrey. 2004. "Time Limits and Welfare Use." Journal of Human Resources. Vol 39(2):405-424. Spring.
- Grogger, Jeffrey, and Lynn A. Karoly. 2005. Welfare Reform: Effects of a Decade of Change. Cambridge, MA: Harvard University Press.
- Hanushek, Eric A. 1986. "Non-Labor-Supply Responses to the Income Maintenance Experiments." In Lessons from the Income Maintenance Experiments, Alicia H. Munnell, ed. Conference Series No. 30. Boston, MA: Federal Reserve Bank of Boston.
- Heckman, James J. 1993. "What Has Been Learned About Labor Supply in the Past Twenty Years." American Economic Association, Papers and Proceedings. Vol 83(2): 116-121. May.
- Hotz, V. Joseph and John Karl Scholz. 2001. "The Earned Income Tax Credit." National Bureau of Economic Research Working Paper #8078. Cambridge, MA: NBER. January.
- Hoynes, Hilary W. 2000. "The Employment, Earnings, and income of Less-Skilled Workers Over the Business Cycle," in Finding Jobs: Work and Welfare Reform, David E. Card and Rebecca M. Blank, eds. New York: Russell Sage Foundation.
- Meyer, Bruce D. 2002. "Labor Supply at the Extensive and Intensive Margins: The EITC, Welfare, and Hours Worked." American Economic Review, Papers and Proceedings. Voln 92(2): 373-79.
- Meyer, Bruce D. and Dan T. Rosenbaum. 2001. "Welfare, The Earned Income Tax Credit, and the Labor Supply of Single Mothers." Quarterly Journal of Economics. Vol 116(3): 1063-1114. August.
- Michalopoulos, Charles, Doug Tattric, Cynthia Miller, Phillip K. Robins, Pamela Morris, David Gyarmati, Cindy Redcross, Kelly Foley, and Reuben Ford. 2002. Making Work Pay: Final Report on the Self-Sufficiency Project for Long-Term Welfare Recipients. Ottawa, CA: Social Research and Demonstration Corporation. July.

Miller, Cynthia, Virginia Knox, Lisa A. Gennetian, Martey Dodoo, Jo Anna Hunter, and Cindy Redcross. 2000. Reforming Welfare and Rewarding Work: Final Report on the Minnesota Family Investment Program. Vol 1: Effects on Adults. New York: MDRC. September.

Moffitt, Robert. 1983. "An Economic Model of Welfare Stigma." American Economic Review. Vol 73(5):1023-35.

Moffitt, Robert. 1992. "Incentive Effects of the U.S. Welfare System: A Review." Journal of Economic Literature. Vol 30(1):1-61. March.

Moffitt, Robert. 1999. "The Effects of Pre-PRWORA Waivers on Welfare Caseloads and Female Earnings, Income and Labor Force Behavior." In Economic Conditions and Welfare Reform. Sheldon H. Danziger, ed. Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Moffitt, Robert. 2002. "Welfare Programs and Labor Supply." In Handbook of Public Economics, Volume 4, Alan J. Auerbach and Martin Feldstein, eds. Amsterdam: Elsevier

Schoeni, Robert F. and Rebecca M. Blank. 2000. "What Has Welfare Reform Accomplished: Impacts on Welfare Participation, Employment, Income, Poverty, and Family Structure." National Bureau of Economic Research Working Paper #7627. Cambridge, MA: NBER.

U.S. House of Representatives. 1996. 1996 Green Book. Committee on Ways and Means. Washington, D.C.: U.S. Government Printing Office.

Table 1  
**Federal Earnings Disregard Rules in the AFDC Program**  
 1979-1996

Year	Months Working	Initial Earnings Disregard				
		Standard Disregard	Cap on State Child Care Disregards	Cap on State Work Expense Disregards	Benefit Reduction Rate	Gross Income Cap on Eligibility
1979-80	all	\$30	none	none	0.66	no cap
1981-84	1 to 4	\$30	\$160	\$75	0.66	150% of the need standard
	5 on	\$0	\$160	\$75	1.00	
1985-89	1 to 4	\$30	\$160	\$75	0.66	185% of the need standard
	5 to 12	\$30	\$160	\$75	1.00	
	13 on	\$0	\$160	\$75	1.00	
1990-96	1 to 4	\$30	\$175	\$90	0.66	185% of the need standard
	5 to 12	\$30	\$175	\$90	1.00	
	13 on	\$0	\$175	\$90	1.00	

Source: U.S. House of Representatives (1996)

Table 2  
**Distribution of State Earnings Disregard Rates**  
 1990-2000

Part 1: In First Month of Employment

% Disregard in Month 1 of Employment	1990	1995	2000
0	0	0	3 LA,TN,WY
16-20	1 MS	2 MS , MI	3 MI, NE, SD
21-25	0	1 VT	2 MT, VT
26-30	0	0	1 AZ
31-35	5 0	4 5	7 AK,CO,DE,GA,IN,MD, VA
36-40	0	0	3 ID, KS, MN
41-50	0	0	1 7 CA,DC,FL,HI,ME,MA,NH,NM,NY,OH,OK,OR,PA,RI,SC, UT,WA
51-60	0	1 IA	2 IA, WV
61-70	0	1 IL	3 IL, MO, ND
71-90	0	0	1 TX
100	0	1 CT	9 AL,AR,CT,KY,MS,NV,NJ,NC,WI

Part 2: In Sixth Month of Employment

% Disregard in Month 6 of Employment	1990	1995	2000
0	5 1	4 5	1 0 CO,DE,GA,IN,LA,SC,TN,TX,VA,WY
16-20	0	1 MI	4 AL,MI,NE,SD
21-25	0	1 VT	2 MT,VT
26-30	0	0	2 AZ,NC
31-35	0	1 CA	3 AK,KY,MD
36-40	0	0	3 MN,ID,KS
41-50	0	0	1 8 CA,DC,FL,HI,ME,MA,NV,NH, NJ,NM,NY,OH,OK,OR,PA,RI,UT,WA
51-60	0	1 IA	2 IA,WV
61-70	0	1 IL	3 ND,IL,MO
71-90	0	0	0
100	0	1 CT	4 AR,CT,MI,WI

Table 3

**Cash Welfare Payments Available to a Welfare Recipient with a Preschooler Who Begins to Work at \$7.00/hour, 30 hours/week** (numbers in 2000\$)

State	1990			2000			Difference in Expected Income Gain from Work <sup>1</sup> (12 month average @ 30 hour/wk) 2000-1990
	1st Month of Work	6th Month of Work	13th Month of Work	1st Month of Work	6th Month of Work	13th Month of Work	
Alabama	0	0	0	164	0	0	288
Alaska	784	543	506	672	672	621	2120
Arizona	97	0	0	0	0	0	-184
Arkansas	0	0	0	0	0	0	588
California	595	354	317	301	301	301	1232
Colorado	217	12	0	0	0	0	68
Connecticut	455	214	176	543	543	543	5132
Delaware	0	0	0	269	144	129	2588
D.C.	241	0	0	167	167	167	3140
Florida	98	0	0	141	141	141	2044
Georgia	72	0	0	0	0	0	420
Hawaii	518	277	240	487	487	487	4136
Idaho	124	0	0	0	0	0	680
Illinois	189	0	0	85	85	85	1212
Indiana	0	0	0	0	0	0	840
Iowa	262	21	0	251	251	251	3032
Kansas	209	0	0	133	133	133	1324
Kentucky	212	80	60	262	95	0	-332
Louisiana	0	0	0	240	240	0	1392
Maine	543	302	265	388	388	388	1292
Maryland	237	0	0	23	23	23	372
Massachusetts	452	211	174	416	416	416	2708
Michigan	311	70	32	94	94	94	752
Minnesota	394	153	116	244	244	244	1676
Mississippi	114	0	0	170	170	0	312

Missouri	96	0	0	268	268	0	3684
Montana	193	0	0	171	171	171	1076
Nebraska	0	0	0	10	10	10	1176
Nevada	143	0	0	348	86	0	1990
New Hampshire	374	133	96	338	338	338	1988
New Jersey	260	19	0	424	162	162	2250
New Mexico	0	0	0	0	0	0	-648
New York	450	209	171	153	153	153	44
North Carolina	71	0	0	272	0	0	1324
North Dakota	231	0	0	0	342	342	2850
Ohio	148	0	0	181	181	181	2084
Oklahoma	137	0	0	90	90	90	1876
Oregon	284	43	6	0	0	0	-892
Pennsylvania	234	0	0	0	0	0	240
Rhode Island	407	166	129	377	377	377	3020
South Carolina	125	6	0	141	0	0	652
South Dakota	211	0	0	0	0	0	-268
Tennessee	242	4	0	185	185	185	1904
Texas	0	0	0	201	0	0	1140
Utah	281	99	71	239	239	239	1312
Vermont	555	314	277	253	253	253	704
Virginia	0	0	0	0	0	0	492
Washington	355	114	77	284	284	284	1988
West Virginia	42	0	0	178	178	178	1440
Wisconsin	376	135	98	628	628	628	5132
Wyoming	180	0	0	0	0	0	564
Column Mean	226	68	55	192	167	149	1450

Source: Authors' Calculations. We assume this woman has two children, one a preschooler, so she is eligible for the state child care disregard in the AFDC program in 1990 and receives the state work expense disregard (if working.)

<sup>1</sup>This is the average, across the first 12 months of work, of the expected income gain between 1990 and 2000 at 30 hours of work, assuming a constant wage (\$7/hour) and applying the state earnings disregard rules. The expected income gain is the difference between expected income if working 30 hours/week versus expected income if not working and on welfare.

Table 4  
**Difference-in-Difference Comparisons Across State Groups, (defined by post-welfare reform earnings disregard [ED] levels) and Pre- and Post-welfare reform (2001-2003 versus 1991-1993)**

	High ED states vs. Middle ED states			High ED states vs. Low ED states			Middle ED states vs Low ED states		
	Data source:			Data source:			Data source:		
	<u>ORG</u>	March <u>CPS</u>	<u>SIPP</u>	<u>ORG</u>	March <u>CPS</u>	<u>SIPP</u>	<u>ORG</u>	March <u>CPS</u>	<u>SIPP</u>
Percent working	0.036 (0.019)	0.042 (0.026)	0.035 (0.026)	0.062** (0.021)	0.019 (0.028)	0.065* (0.028)	0.026 (0.017)	-0.022 (0.023)	0.03 (0.020)
Percent full-time	0.007 (0.017)	0.034 (0.024)	-0.006 (0.024)	0.044* (0.018)	-0.002 (0.025)	0.031 (0.026)	0.037* (0.015)	-0.036 (0.022)	0.037* (0.018)
Percent part-time	0.028* (0.014)	0.008 (0.018)	0.041 (0.021)	0.018 (0.015)	0.021 (0.019)	0.034 (0.023)	-0.010 (0.012)	0.013 (0.016)	(0.007) (0.015)
Hours of work	1.300 (0.720)	1.810 (0.983)	0.087 (1.009)	2.451** (0.778)	0.629 (1.071)	2.359* (1.084)	1.151 (0.646)	-1.181 (0.912)	1.486 (0.778)
Total income	na	-44 (646)	682 (510)	na	1232 (634)	1351* (553)	na	1276 (701)	670 (397)
Welfare income	na	-171 (127)	36 (122)	na	156 (132)	-579** (122)	na	327.3** (99)	-615 ** (87)

Standard errors in parentheses. See text for definition of state groups and detail on data sources.

\* Significant at 5% level; \*\* Significant at 1% level.

