Evaluating Welfare Reform

in the United States

Rebecca M. Blank
University of Michigan and NBER

May 2002 (revised)

Contact information:
Gerald R. Ford School of Public Policy
440 Lorch Hall
611 Tappan Street
University of Michigan
Ann Arbor, MI 48109-1220
734-763-2258
734-763-9181 FAX
blank@umich.edu

This paper was commissioned by the *Journal of Economic Literature*. Thanks are due to Lucie Schmidt and to Elizabeth Scott for excellent research assistance, and to Jeffrey Grogger, Charles Michalopoulos, Robert Moffitt and an anonymous referee for comments and advice.

Abstract

This paper reviews the economics literature on welfare reform over the 1990s. A brief summary of the policy changes over this period is followed by a discussion of the methodological techniques utilized to analyze the effects of these changes on outcomes. The paper them critically reviews the econometric and experimental literature on caseload changes, labor force changes, poverty and income changes, and family formation changes. A growing body of evidence suggests that the recent policy changes have influenced economic behavior and well-being in a variety of ways. One particular set of "new-style" welfare programs seems to show especially promising results, with significantly increased work and earnings and reduced poverty.

Over the 1990s the United States fundamentally changed the structure of its public assistance programs to low-income families. These policy changes have, in turn, generated a growing body of economic research that has evaluated their effects. This article reviews the major changes in U.S. welfare programs over the 1990s and critiques some of the key methodological approaches and results in areas where a substantial economic research literature has accumulated. I particularly focus on areas where the new research contributes to long-standing debates.

It is worth noting that the U.S. policy changes have been much discussed in other countries and the evaluation literature from the U.S. may be increasingly relevant to policy debates elsewhere. For instance, in 1996, Canada gave provinces greater discretion over their social assistance programs, similar to changes the U.S. As we shall discuss below, Canada enacted a very interesting demonstration program in the 1990s (the Self Sufficiency Project), designed to move women on welfare into work. In 1999, Great Britain enacted the Working Families Tax Credit, a generous tax credit for low-income working families, similar to the U.S. Earned Income Tax Credit program. Some communities in Germany are imposing time limits on the receipt of public assistance (Feist and Schöb, 1998). In contrast to earlier decades, when the different design and lower generosity of U.S. social welfare programs led U.S. policies to be dismissed as irrelevant or aberrant by other westernized nations, during the 1990s many of these countries watched the U.S. welfare experiments with great interest.[1]

[1] Not discussed here are social insurance programs such as Social Security or Unemployment Insurance, around which there has also been a great deal of trans-Atlantic conversation.

I. Federal Changes in U.S. Welfare Programs Over the 1990s

The U.S. enacted major welfare reform legislation in August, 1996. The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) passed with a relatively high degree of bipartisan support. President Bill Clinton had, however, vetoed two earlier versions of this bill and it remained controversial. Several of his senior advisors resigned in protest when he signed PRWORA into law.[2]

The major provisions of PRWORA included:

- *Devolution of greater program authority to the states*. PRWORA replaced the federal Aid to Families with Dependent Children Program (AFDC) – the primary cash assistance program for low-income families -- with the Temporary Assistance for Needy Families (TANF) block grant. This essentially removed almost all federal eligibility and payment rules, giving states much greater discretion in designing their own cash public assistance programs. This also eliminated a federal entitlement to cash assistance. States could choose which families they supported.

- *Changes in financing*. TANF replaced a matching fund arrangement under AFDC, in which federal funding moved up or down with state funding. The TANF block grant was fixed and the contribution for each state was determined by the federal AFDC matching grant contribution in the years prior to PRWORA. States were required to maintain at least 75 percent of their previous state spending levels on AFDC in order to receive the full block grant.[3]

- *Ongoing work requirements*. By 2002, at least 50 percent of all recipient families and 90 percent of two-parent families were required to be working or in work preparation programs, although states were given great discretion to design and implement these programs. The law treated caseload reductions as similar to work, however. Thus, a state which reduced its caseload by 50 percent would meet its work requirement, regardless of how many current or former recipients were actually employed.

- *Incentives to reduce non-marital births*. There was more rhetoric than program in the legislation in this area, but three of the four stated goals of PRWORA involved reducing non-marital births and encouraging marriage. States that reduced out-of-wedlock child bearing without raising abortion rates qualified for special bonuses.

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[2] For a detailed description of the events leading up to this legislation, see Weaver (2000). For further discussion about the provisions of PRWORA see Blank (1997b) or Blank and Ellwood (2002). Moffitt (1999b) discusses the factors behind PRWORA's passage. Moffitt (forthcoming) provides a more detailed summary of the changes from AFDC to TANF.

[3] Not included in this paper is any discussion of the public finance literature that investigates the potential impact of block grants on welfare funding. For a good overview of these issues, see Chernick (1998).

- *Five year maximum time limit*. PRWORA set a lifetime limit of 60 months on the receipt of TANF-funded aid. States could exempt up to 20 percent of their caseload from this limit, could set shorter time limits if they chose, or could continue funding assistance to families entirely out of state funds after 60 months.

Although this paper will focus less on these issues, PRWORA also imposed additional limits on eligibility for Food Stamps and Supplemental Security Income (SSI, the cash assistance program to low-income aged and disabled individuals) among certain populations. Legal immigrants who arrived after August 1996 were largely denied access to TANF and to these other programs; the impact of this policy change will grow over time as an increasing share of U.S. immigrants will have arrived post-PRWORA. Finally, PRWORA made changes designed to encourage greater paternity establishment and more payment of child support by absent parents.

While the 1996 legislation has received the most public attention, it was preceded by a variety of earlier and significant changes. Growing dissatisfaction with AFDC had led an increasing number of states to seek waivers from the AFDC rules. These waivers were mostly designed to allow states to more stringently enforce work requirements for welfare recipients. Such waivers had started under President Ronald Reagan, but the Clinton Administration actively encouraged more expansive state-wide waiver programs. As a result, by the time PRWORA passed, 27 states had major state-wide waivers in place. Most of these states designed new TANF-funded welfare programs that were closely based on their waiver experiments, although virtually all waiver states used their new discretion under PRWORA to make additional program changes.

All of these waiver programs had to be seriously evaluated by the states that implemented them. The Department of Health and Human Services (HHS), which

approved and administered the waivers, typically required some form of random-assignment evaluation. Over time, this generated a body of literature about welfare-to-work programs that was crucial in convincing people that such programs could have positive effects on earnings and labor supply and negative effects on welfare spending. Along with reform of traditional cash welfare programs, there were also major changes in federal legislation affecting low wage jobs and workers over the 1990s. The minimum wage rose from $3.35 at the end of 1989 to $5.15 in 1997. By 2000, this left real minimum wages 10.8 percent above their levels in 1989.

Even more important, one of the first legislative proposals from the Clinton administration to receive Congressional approval in 1993 was a major expansion of the Earned Income Tax Credit (EITC). The EITC operates as a refundable tax credit through the federal tax system to subsidize low wage workers in low income families. Figure 1 describes the EITC subsidy as of 2000. Nonworkers receive no subsidy. Low income low wage workers with one child (two or more children) are initially subsidized at a rate of 34 percent (40 percent). Over some income range they receive a flat subsidy of $2353 ($3888), and as their income increases further this is taxed away at a rate of 15.98 percent (21.06 percent). This subsidy offsets federal income tax obligations (including taxes that fund the Social Security and Medicaid programs) and results in subsidies (checks from the government) for workers whose EITC subsidy is greater than their tax obligations.[4]

The combination of increased minimum wages and increased EITC subsidies meant that the real earnings plus wage subsidy (in 2000 dollars) received by a woman with one child working full time at the minimum wage rose from $10,568 in 1989 to

[4] More detail on the EITC is available in U.S. House of Representatives (2000, p808-813). For a history of the EITC, see Ventry (2000).

$12,653 in 2000, a 19.7 percent increase. For a similar woman with two or more children, real earnings and subsidies rose from $10,568 in 1989 to $14,188 in 2000, a 34.3 percent increase. These changes should have greatly increased the work incentives for low-wage single mothers with children.

Two other federal legislative changes also deserve mention. First, from the mid-1980s on, access to public health insurance became increasingly delinked from participation in cash public assistance programs. By 1999, all children in families whose income was below 100 percent of the poverty line were eligible for Medicaid, the publicly-funded health insurance program for low-income persons.[5] In addition, women who left welfare for work were eligible for one year of transitional Medicaid coverage.[6] Because many eligible children did not appear to be accessing Medicaid, in 1997 Congress funded a $24 billion, 5-year program known as the Children's Health Insurance Program (CHIP), providing incentives and funding to states to expand health care usage and health insurance access among low-income children.

There were also substantial changes in subsidies for child care assistance. PRWORA abolished a plethora of older programs and created a single Child Care and Development Block Grant. States were also allowed to use a certain share of their TANF funds for child care. In addition, there were expansions in the Child Care Tax Credit for lower middle income families.[7]

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[5] Since 1983, all pregnant women and children age 5 or less in families with incomes below 133 percent of the Federal poverty line have access to Medicaid; 23 states use a higher cutoff point. Older children, born after September 1983, in families below 100 percent of the poverty line are also covered by Medicaid; 26 states set a higher cutoff point for eligibility for these older children (Ku, Ullman and Almeida, 1999). For more detail on Medicaid and how it operates, see Gruber (forthcoming).

[6] Thirteen states extend this for more than one year.

[7] Loprest, Schmidt and Witte (2000) discuss these changes in more detail. For a summary of the research on the impacts of child care subsidies, see Blau (forthcoming) and Anderson and Levine (2000).

Combined, these changes constitute a revolution in public assistance programs within the United States over this past decade. Federal dollars available to support *working* low income families increased from $11.0 billion in 1988 to $66.7 billion in 1999.[8] Dollars paid in cash welfare support to (largely nonworking) families headed by non-elderly, non-disabled adults rose from $24 billion in 1988 to $27 billion in 1992, then fell to $13 billion by 1999 (all numbers in 2000 dollars). This suggests that the work incentives imbedded in the public assistance system should have increased markedly over this period: cash assistance became far less available, welfare recipients were pushed much harder to find employment and leave the rolls, the returns to low wage work rose, and the availability of work supports (child care and health insurance) increased to low income families.

Not unimportant, these changes took place at the same time as a major economic boom. The U. S. unemployment rate fell to 5 percent in April, 1997, and remained at or below this level until October 2001. Most places experienced worker shortages in the years following the passage of the 1996 legislation, making employers more willing to hire ex-welfare recipients. Wages among less skilled workers started to rise in 1995, for the first time since the late 1970s (Blank and Schmidt, 2001). This meant that the macroeconomy reinforced and supported the direction of legislative change over the 1990s. In many ways, the late 1990s were the best time imaginable to enact and implement work-oriented welfare reform.

[8] Blank and Ellwood (2002, Figure 1). This includes dollars spent on the EITC, child care assistance to poor and near-poor families, and Medicaid and CHIP expenditures on low-income children and adults who are not receiving cash assistance. It does not include money spent on job training or job placement assistance, or cash benefits paid to working families.

II. The State Response

Describing the Federal changes provides only half of the picture. After the passage of PRWORA each state began to design and enact its own TANF-funded program.[9]

Historically, analysis of public assistance programs has focused on two parameters: benefit levels and benefit reduction rates (BRRs). Figure 2 shows the income available to a low-wage family under a typical welfare program. A maximum benefit level, G, is available to non-workers. Workers earn an hourly wage rate w. As hours of work (and earnings) increase, benefits are taxed away at a rate t (the BRR). Ongoing historical discussion has focused on the trade-offs of higher benefits (raising G provides a stronger safety net but discourages work and raises program costs) and higher BRRs (raising t reduces the return to low levels of work but leads to lower program costs). The Negative Income Tax experiments of the 1970s were largely experiments involving different levels of G and t (Burtless, 1986; Ashenfelter and Plant, 1990).

Frustration with the work disincentives imbedded in traditional welfare programs led President Reagan to promote welfare-to-work programs in the early 1980s. Mandatory job search or job placement programs would replace the endless effort to tinker with the contradictory incentives imbedded in a given level of G and t, by forcing welfare recipients to work regardless of the resulting loss in benefit income. A strong version of work requirements is a so-called "workfare" program, which mandates a certain level of work in a publicly provided job as a condition of ongoing welfare receipt. A less extreme requirement might mandate participation in a job preparation or job

[9] For a description of the structure of means-tested programs prior to 1996, see Blank (1997a).

search program. A wide variety of states have experimented with different versions of work requirements over the past 20 years.

Initially using waivers and later using their authority under PRWORA, states have transformed the nature of public assistance programs. While benefit levels and BRRs remain important parameters, states are increasingly using a wide variety of additional program design components to promote work and to reduce caseloads. What follows is a brief description of these changes. What will be clear is that both the *number* of possible program parameters available to states has increased markedly, and that different states are choosing very different *combinations* of these parameters. Hence, the variance across states in their TANF-funded programs is enormous and still growing.

Benefit Levels. States have always been able to choose their own maximum benefit levels for non-workers. This part of the system has changed little. Most states over the 1990s made only small legislative changes in their benefit levels, even after the passage of PRWORA. In fact, the overwhelming trend in benefit levels in the 1990s has been inflation erosion in benefits (a trend visible since the early 1970s). Table 1 indicates that the median benefit level (in $2000) fell from $480/month for a family of three to $379 between 1990 and 2000. Most of this decline was due to inflation erosion. Similar changes occurred across the distribution of benefit levels, as Table 1 indicates.[10]

As cash assistance becomes less broadly available, benefit levels are of decreasing importance. The steady decline in benefit levels, however, should increase work incentives over this period.

Benefit Reduction Rates. Under AFDC, BRRs were set by federal law (although a few states received waivers to experiment with alternative BRRs in the early 1990s).

BRRs had been raised significantly in the early 1980s, and many AFDC recipients faced almost 100 percent tax rates on their earnings.

A major change post-PRWORA is that many states have chosen lower BRRs, in order to encourage work (and to a lesser extent, as a way of supplementing income among low-wage workers). Free to set their own rules, many states have also chosen to have BRRs rise at some point after a woman goes to work, so her public assistance subsidy is reduced over time even if her earnings do not increase.

Table 2 is based on calculations of the cumulative cash welfare benefits available over the first 24 months of work by a welfare recipient with two children whose earnings are $6/hour (slightly above the minimum wage of $5.15 per hour) and who works part-time (30 hours/week) or full-time (40 hours/week). The first two columns show the cumulative cash benefits that a welfare recipient family would have expected to receive in each state in January 1996 (all numbers adjusted to 2000 dollars) if the mother went to work under the old AFDC program. The second two columns show the cumulative cash benefits that a family would have expected to receive in 2000 if the mother went to work under each state's TANF program.

The AFDC program provided little cash support to workers. Almost half the states in 1995 would have paid no cash benefits to a part-time worker.[11] Only 13 states would have provided any support to a full-time worker.

By 2000, BRRs had fallen in almost all states, dramatically changing these results. Almost all states provide some support to the mother who enters part-time work in 2000; in 28 states this support exceeds $1000 over the first 24 months of work. Half

[10] The standard deviation in benefits across states changed little over these 10 years.

the states also provide some cash supplement to the woman who enters full-time work,[12] with the median state paying $299 in cumulative cash benefits over the first 24 months. Sixteen states pay more than $1000 in benefits over these first 24 months.[13]

A change in the BRR is equivalent to a change in the effective wage rate (see Figure 2). Because this imbeds both income and substitution effects, it is theoretically ambiguous whether work incentives should rise or fall. Most labor economists assume that substitution effects dominate income effects for low wage workers. This suggests that lower BRRs should increase work incentives. Moffitt (1992) notes the remarkable historical inelasticity of responsiveness among welfare recipients to changes in BRRs, however, which suggests that the work incentive effect of lowering BRRs in the mid-1990s might not be large. On the other hand, the changes in BRRs implemented in the 1990s were often made in conjunction with strong work requirements. As the discussion of financial incentive programs in Section VIII below indicates, the combination of lower BRRs and work mandates may have a quite powerful combined effect.

Note that lower BRRs may have other effects as well. In the presence of time limits, lower BRRs keep welfare recipients on welfare longer and encourage families to "use up" their time. If clients are aware of time limits and worried about using up their public assistance eligibility, this will further increase their incentives to work and may lead them to leave welfare even while still eligible for some benefits in order to preserve future months' welfare eligibility.

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[11] Differences across the states in columns 1 and 2 of Table 2 are entirely due to differences in AFDC benefit maximums across states; all states are subject to identical (federally determined) BRRs.

[12] Cumulative benefits equal 0 for a woman earning $6/hour if BRRs are very high (and in some states they are 100 percent, so benefits are reduced $1 for $1 of earnings) and/or if benefit levels are very low (so that one "works one's way off welfare" more quickly).

[13] More detailed information on these earnings disregard calculations are available from the author upon request.

Welfare-to-Work Programs. Virtually all states have tried to expand their welfare-to-work programs starting in the early 1980s. Since the passage of PRWORA, states are mandating participation in job search assistance and work preparation among a much higher share of their caseload. By 1999 states reported that 38.3 percent of their caseload was engaged in work or job activities, up from 20.4 percent in 1994.[14]

The exact meaning of "welfare-to-work" varies substantially across states. In the early 1990s, many states ran both job placement and job training programs. By the late 1990s, the focus of most state programs was "work first", aimed at getting recipients into a job as soon as possible. Hence, most programs focus on narrow job preparation skills (interviewing, getting along on the job, organizing child care) and job search assistance. Relatively little money is currently being spent on longer-term training, a somewhat controversial fact in many states.[15]

These work programs should increase work incentives, both by improving employment-related skills and by establishing job search as an expected activity for welfare recipients. Indeed, a number of states have focused on changing the "culture" of their public assistance offices, retraining and reorganizing staff so that their primary goal is to encourage work rather than to provide monthly assistance (Gais, et. al., 2001).

Sanctions. To enforce job search and work requirements, states have implemented a variety of sanction policies aimed primarily at penalizing individuals who do not respond to work requirements (most commonly, these are individuals who miss

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[14] The 1994 data is from U.S. House of Representatives (2000, Table 7-25). The 1999 data is from U.S. DHHS (2000, Table 3:1).

[15] See discussion of this issue by Strawn, Greenberg, and Savner (2001). Job training or education among adults can be counted as a work activity, but cannot count toward the first 20 hours/week of required work participation.
An exception is teen mothers under age 18, who are required to participate in education activities unless they hold a high school degree.

required job preparation or job search sessions). Sanctions involve a reduction in welfare benefits, but states vary in how much they reduce benefits and for how long. Pavetti and Bloom (2001) classify 25 states as "strict", including a number of states that impose permanent full benefit losses on the families of noncompliant individuals. They classify 13 states as "lenient," imposing only temporary and partial reductions.

If low BRRs are the "carrot" for participating in welfare-to-work programs (providing ongoing subsidies to those who can only find low wage jobs), then sanctions are the "stick." All states have some form of sanctioning policy, which is to say that no state relies only on positive work incentives to get people employed.

Time Limits. While all states are subject to the 60 month federal time limit for individuals using TANF-related funds, they can also set shorter time limits, or can provide state funding beyond 60 months.[16] Seventeen states have time limits of less than 60 months for some families, 26 states use the 60 month federal time limit, and 8 states have not imposed time limits which mandatorily end all benefits.[17] For instance, several of these states impose time limits on adult recipients but continue benefits for children (Pavetti and Bloom, 2001).

Time limits should have two work-inducing effects. First, they should provide incentives for recipients who might need welfare in the future to leave welfare as rapidly as possible, in order to preserve future eligibility.[18] This requires a thorough understanding of the fact that "the clock is ticking" and some states have been better at reminding recipients of this. There is some evidence that many recipients misunderstand

[16] States are allowed to exempt up to 20 percent of their caseload from the 60-month time limit.
[17] As in Tables 1 and 2, Washington, D.C.c is included as the 51st "state".
[18] For example, Swann (2000) develops a model indicating that time limits will have larger effects when welfare recipients are forward looking.

where they are on their time clock (Bloom, 1999). Second, once time limits are imposed, ex-recipients can no longer use cash assistance as a back-up to work.

Time limits have not yet been widely imposed; the first recipients did not begin to hit the 60 month limit in most states until late 2001 or early 2002. As noted before, there are somewhat perverse interactions between time limits and lower BRRs. In addition, there is also evidence that time limits and sanctions interact in interesting ways. Sanctions tend to affect the same less responsive and often more disadvantaged population that is likely to hit time limits. This suggests that time limits may not have a very large effect if many individuals will have already been removed from eligibility through sanctions (Pavetti and Bloom, 2001).

Diversion. With no national entitlement to public assistance, states can deny assistance to individuals. Many states have implemented eligibility determination processes that encourage some applicants to be diverted from cash public assistance. Ten states impose work search requirements on applicants prior to eligibility (i.e., applicants must show that they've applied for a certain number of jobs as a condition of eligibility). Twelve states provide short-term cash payments as an alternative to public assistance eligibility, designed to meet some immediate need of the applicant which will then allow her to return to work. Nine states use both techniques in order to divert applicants from welfare, while 20 states make no effort at diversion.[19]

Work Support Subsidies. With more attention to moving welfare recipients into work, states have also recognized the need to help families with work-related expenses. States have greatly increased their expenditures on work support programs, primarily child care subsidies. Between 1993 and 2000, federal funds available to the states for

child care subsidies rose from $9.5 billion to $18 billion, an 89 percent increase.[20] States are also helping to fund work transportation expenses, or job search expenses. Indeed, more money is currently going into work support, including child care and transportation subsidies, wage subsidies, as well as cash payments to working families, than into cash assistance to non-working families (Gais, et. al., 2001).

While this review focuses primarily on the changes outlined above to cash assistance and work-related programs, it is worth noting that these changes have had a substantial impact on the utilization of other non-cash public assistance programs as well. AFDC was historically the gateway program through which families were also certified for Food Stamps or Medicaid. As access to cash assistance has fallen, Food Stamp usage has fallen as well.[21] Working poor families seem to find it particularly difficult to access Food Stamps. Offices are often open only during daytime hours and persons must regularly report to the office in person to maintain eligibility. The complexity of calculating Food Stamp amounts for working individuals, whose Food Stamp benefit level will change from month to month as their earnings vary, often creates incentives for caseworkers to try and get workers off the Food Stamp rolls. Arcane rules about the resale value of a car and other assets limits can also restrict eligibility. The net result is a program with very low participation among eligible families with a working adult head, despite the declared goal of Food Stamps to provide assistance to all low-income families. For instance, in 1999 only 43 percent of eligible persons with earnings were receiving Food Stamps, while 70 percent of eligible nonearners were participating

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[19] For one of the few discussions of state diversion strategies, see Maloy, et. al. (1999).
[20] Provided by Ron Haskins, based on calculations with data from the Congressional Research Service.
[21] For further discussion of the problems with Food Stamp access post-PRWORA, see Greenstein and Guyer (2001) and Zedlewski (2001).

(USDA, 2000). Of course, this may merely reflect an effort to structure the program so that only the most needy among the eligible will actually participate, consistent with the argument in Nichols and Zeckhauser (1982).[22] Efforts are currently underway to reduce these barriers to Food Stamp participation among working families.

III. Changes in Behavior and Well-Being Over the 1990s

At the same time as major changes in program structure occurred during the 1990s, there were also stunning changes in behavior. Strong adjectives are appropriate to describe these behavioral changes. Nobody – of any political persuasion – predicted or would have believed possible the magnitude of change that occurred in the behavior of low-income single-parent families over this decade.

Caseload Changes. The most-discussed change over the 1990s was a remarkably rapid decline in caseloads between 1994 and 2000, illustrated in Figure 3. The vertical line indicates passage of the 1996 legislation. Between 1994 and 2000, caseloads declined by 56.5 percent. Furthermore, these declines occurred everywhere in the nation, with every state experiencing strong reductions in their welfare rolls.

Three things should be noted about the data underlying Figure 3. First, the rapid caseload decline after 1994 was preceded by an unexpectedly strong increase in caseloads in the early 1990s. Despite a relatively mild economic slowdown, caseloads rose 27 percent between 1990 and 1994. This rise in caseloads was one of the driving forces behind the desire of state governors to implement more radical welfare reform. Ideally, any theory that explains the caseload decline of the late 1990s should also explain the

[22] As these numbers indicate, takeup rates among eligibles in means-tested programs are typically far below one. For a discussion of takeup in the Food Stamp and the AFDC program, see Blank and Ruggles (1996).

caseload rise of the early 1990s. As discussed below, most researchers have focused on the decline in caseloads without paying attention to the earlier rise.

Second, caseloads start to decline well before the enactment of the 1996 legislation, suggesting that legislation was not solely responsible for the caseload decline. Third, the caseload decline in the late 1990s far exceeds anything in previous decades. Despite relatively strong economic growth from 1983 to 1989, there is little evidence of any change in caseload levels over that time period. This suggests that the economy alone cannot explain caseload changes in the 1990s. The strong economic growth of the 1960s is actually correlated with a rise in caseloads. Most observers ascribe this to increased take-up of welfare programs among the eligible following the launch of President Lyndon Johnson's War on Poverty (Moffitt, 1992). This at least suggests that take-up changes might be important in the 1990s as well.

Labor Force Participation Changes. Changes in caseloads by themselves are not very informative, and immediately lead to questions about the behavior and income of those who are no longer receiving welfare. In particular, one of the major goals of the 1996 legislation and the policy changes that preceded it was to increase work effort among welfare recipients. As it turns out, work effort soared over this time period among single mothers with children.

Figure 4 presents labor force participation rates among women by marital status and presence of children from 1989 through 2000. Unmarried women without children work at a high and unchanged level throughout this time period. Married women, both with and without children, show steady increases in labor force participation over the 1990s, at a slightly slower rate than in earlier decades.

In sharp contrast, single mothers with children show little change in their labor force participation rates through the 1980s and into the mid-1990s. But between 1994 and 1999 their labor force participation rises by 10 percentage points. Among single mothers with children under the age of six, labor force participation rates rise by 5 percentage points. In short, at exactly the same time as caseloads start to fall, work effort rises substantially among exactly the population most affected by the caseload declines. This provides at least *prima facae* evidence that the caseload declines were associated with increases in work. Other available data supports the idea that women are moving from welfare to work at a high rate. Among those who report receiving public assistance income in the previous year, the share reporting themselves employed in March of the following year rises from 19.8 percent in 1990 to 44.3 percent in 2000.[23]

Even among women who remain on welfare, work effort rises strongly. This may reflect both the greater effort to move welfare recipients into work, as well as the lower benefit reduction rates that continue subsidies to working women as their earnings rise. Among women on welfare, the share with earnings rises from 6.7 percent in 1990 to 28.1 percent by 1999.[24]

Increases in work not only increase earnings and add to immediate family income, but may also build labor force experience that leads to higher wages over time. Gladden and Taber (2000) indicate that even among low-skilled women wages increase with experience.

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[23] Calculated by the author from March Current Population Survey data. Bishop (1998) also provides detailed information on labor supply increases among never-married women in the 1990s.

[24] This is all the more remarkable in light of the amazing inelasticity of work effort among welfare recipients in previous decades, discussed by Moffitt (1992). The 1990 data is from U.S. House of Representatives (1998, Table 7-19); The 1999 data is from U.S. DHHS (2000, Table 3:3.c).

Changes in Income and Poverty. Declines in public assistance usage and increases in labor market involvement may or may not signal income increases, since reductions in welfare benefits will offset increases in earnings. Hence, there has been a great deal of interest in whether incomes have risen among less-skilled single mothers.

The official U.S. poverty data suggest unambiguous improvements in poverty among single-mother families, as Table 3 indicates. The share of all families in poverty declines from 11.9 in 1992 (the end of the recession in the early 1990s) to 8.6 percent in 2000, below the previous historic low of 8.8 percent in 1974.[25] Given the strong economy, this is perhaps a disappointingly small decline in poverty. Poverty among single-mother families declines more rapidly however, from 35.4 percent in 1992 to 24.7 percent in 2000, a new historic low. Poverty rates among single mother families headed by blacks and Hispanics were also at historic lows in 2000. This suggests that, at least in the short run, changes in social policy did not worsen the economic situation of poor households. The decline in poverty is far less, however, than the reduction in public assistance caseloads. As a result, the share of working poor in the U.S. population rose, as some women left public assistance for employment but remained poor.

Unfortunately, U.S. poverty rates provide only partial information on well-being (Citro and Michael, 1995).[26] Table 4 provides information on poverty gaps among families with children between 1993 and 1999, showing how far average family income is below the poverty line among poor families. Row one shows the poverty gap based only on pre-transfer income among families. Row two includes social insurance benefits

[25] All official poverty rate data can be found at www.census.gov.
[26] Alternative poverty calculations can be found in U.S. Department of Commerce (1999), with updated numbers for 1998 at www.census.gov/poverty/povmeas/expov.tabe.html. These calculations also show a strong decline in poverty among female-headed families over the 1990s.

(Social Security, disability and workers compensation), row three adds means-tested benefits (cash and in-kind), and row four calculates poverty gaps based on total income net of taxes. The bottom part of the table shows the percentage reduction in the poverty gap as the definition of income is sequentially expanded.

Between 1993 and 1999 substantial increases in earnings resulted in a declining poverty gap when looking only at pre-transfer cash income. With increases in earnings come reductions in means-tested benefits, however. Social Insurance reduces the poverty gap by a relatively constant 16 to 18 percent over these years. Means-tested benefits, however, reduce the poverty gap 44 percent in 1993, but only 34 percent in 1999, reflecting the declining caseloads. Over time, the federal tax system expands to further reduce poverty gaps, largely because of the growth in the EITC. The net result is a slight rise in poverty gaps based on after-tax income over the 1990s, from $1447 to $1524. Of course, since fewer persons are in poverty by the end of this period, it is hard to state whether the net effect is to raise or lower well-being. A more disadvantaged group may remain poor over this period, resulting in a rising poverty gap.

Table 5 presents information on a set of tabulations recently completed by the Center on Budget and Policy Priorities,[27] which calculate the average income of female-headed households by quintile. The results in Table 5 indicate that incomes among women in the top 80 percent of the income distribution of female-headed families (quintiles 2 through 5) rose unambiguously over the 1990s, including increases post-1996. This is consistent with evidence from other surveys that do similar data tabulations (Primus, et. al., 1999; Haskins, 2001; Gabe, 2001, Jencks, Swingle, and Winship, 2001).

[27] These data are currently unpublished, but available upon request from Wendell Primus.

There is other evidence that some group of disadvantaged women lost income in the mid-1990s. Haskins (2001) discusses evidence of a rise in deep poverty (the number of persons at less than 50 percent of the poverty line) in the mid-1990s. The very poorest quintile of single-mother families experienced an increase in income in the first half of the 1990s, but little overall income growth post-1996. This is not surprising, as underlying calculations indicate that means-tested income among this population fell by more than $1500, while earnings rose by less than $1000. In higher quintiles, earnings gains were much stronger than the loss in means-tested income. Zedlewski, et. al. (2002) also document rises in deep poverty between 1996 and 1998 among families with children.

Somewhat contrasting evidence comes from data on consumer expenditures, which shows increases in consumption spending through the 1990s, even among very low-income single mothers with children (Haskins, 2001; Meyer and Sullivan, 2001). Jencks, et. al., (2001) indicate that food-related problems declined between 1995 and 1999 for single mothers as rapidly as among other poor groups. In short, the available evidence suggests that most single mothers gained ground in the 1990s, but there is a group of the poorest single mother families who have made only minimal gains over the 1990s and some at the very bottom who might have lost ground.

All of these income calculations should be viewed with some skepticism. First, a substantial minority of those leaving welfare appear to be unemployed at some later point (Brauner and Loprest, 1999). We have little evidence on how these women are surviving, but the best guess is that they are relying more upon boyfriends or other family members for income. While this may be a viable short-term strategy, over time such

arrangements may fall apart and are unlikely to provide long-term economic stability for either the women or their children.

Second, few of these studies actually measure disposable income. While the studies cited above take into account the EITC and some other non-cash transfers, they do not fully calculate tax rates on earners. They typically impute EITC receipt and their data on housing, Food Stamps or medical assistance is not complete. They provide little information on income sharing with other individuals or families. Furthermore, none of these calculations take account of increased expenses associated with work, particularly out-of-pocket child care expenses. There is a need for research that provides a more complete picture of the changes in the actual economic well-being of less-skilled single-mother families and their children over the 1990s, in the midst of major policy and behavioral changes.

IV. Research and Evaluation Challenges

Estimating the effects of the program changes described above creates real evaluation challenges. One must control effectively enough for all other environmental influences to produce a credible estimate of a policy effect. This is particularly difficult in a world where many things are changing at the same time, as happened in the 1990s.

Past work evaluating the AFDC program tended to describe the welfare environment for an individual by controlling for state benefit levels and (occasionally) for effective state BRRs. Since most eligibility rules were uniformly set by the federal government, state variation in benefit levels was the dominant feature describing welfare generosity and access across states.

Post-1996, it is much more difficult to characterize the policy environment for each state. State welfare policies vary along multiple program dimensions, and the precise nature of the bundle matters since different program components may interact with each other. For instance, one may need to control for the interaction of BRRs and sanctions, rather than just controlling for each separately. Not all of the program elements described above are easily coded, and there is little guidance in the research to date showing the most effective way to measure and code some of the newer policies like time limits, sanctions or diversion activities. In some cases only a few states have adopted particular policies or combinations of policies. For data sets with state level observations, this can make it difficult to estimate precise policy effects.

With individual-level data, it is much more difficult to identify the specific program rules facing any individual. Data sets like the Current Population Survey provide no information on whether an individual is required to participate in a welfare-to-work program, whether they have been sanctioned, how close they are to reaching their time limits, what type of subsidies for child care or other work supports they might be receiving from TANF dollars, or whether they receive EITC funds. In short, most of our data sets are designed to collect information on cash welfare assistance, appropriate for the old world of AFDC but not very useful in the new world of TANF where cash public assistance levels are less and less descriptive of state welfare programs.

For all of these reasons, it has become much harder to study the impact of welfare programs or their specific components on individual behavior. The complexity and diversity in state programs means that an increasing amount of analysis focuses on data from a single state, creating problems of comparability and generalizability. Closely

linked to this focus on single states, there has been a substantial increase in the use of administrative data to analyze welfare-related questions. Administrative data typically provide more detailed information on the parameters of the welfare system that impact any individual, including their use of multiple programs, their work requirements, their accumulated timing of welfare receipt, and so on. More and more researchers are linking information from multiple administrative data sets. For instance, welfare receipt records might be linked with unemployment insurance records to determine quarterly earnings after leaving welfare.

Most researchers have tried to measure the direct effects of the enactment of waivers and the implementation of TANF. This is complicated not only by the data problems mentioned above, but also by other evaluation difficulties.

First, waivers were not implemented by a random set of states. States with higher unemployment rates were more likely to request major welfare waivers (Schoeni and Blank, 2000). This means that waivers cannot be used as a simple "natural experiment" in which results in waiver states are compared with results in non-waiver states.

Second, the coincidence and the interaction of the economic expansion and the implementation of welfare reform creates problems. The strong economic boom and the passage of PRWORA occur simultaneously and it is difficult to separately identify their causal effects. This is even more true if the two events interact with each other. For instance, states may have been able to change their cash public assistance programs to work-oriented support programs more quickly and more thoroughly because they did not have to worry about job availability issues. Most people who could be placed in a job search assistance program were able to locate a job. Conversely, the strong push that

increased the supply of less skilled women into the labor market may have changed the demand side of the labor market in some places, for these women and for other less skilled workers (Bartik, 2000). For all of these reasons, separating economic effects from policy effects promises to be difficult in the mid-1990s.

Third, multiple policy changes were being implemented at the same time, and these policy changes almost surely interacted with each other in a reinforcing way. The large increases in the EITC subsidies occurred just before welfare reform was passed and at the same time as minimum wage increases in the mid-1990s. As noted above, child support subsidies were restructured at the same time as welfare was reformed. This makes it difficult to separately identify individual policy effects. For instance, Blank (2000) argues that it was the combined interaction between multiple policy changes and a booming economy that led to the unexpectedly large caseload declines and labor force participation increases.

Fourth, the implementation of state TANF programs is particularly difficult to evaluate because it occurred at about the same time in all states. Within a nine-month period from September 1996 through July 1997, all states began implementing their new TANF plans. This is in contrast to major state waivers, which were approved over a four year period in 27 states, allowing a researcher to identify the effects of these waivers from the differences in when they were implemented across states.

Finally, almost ignored in the economics literature, there is often a difference between enacted program rules and actual implementation practices. This may be particularly true for a major program change that is being implemented quickly. Because staff are not fully trained in the new systems or because staff may disagree with some of

the new program changes, what's actually done "on the ground" could differ substantially from the formal description of state programs (Meyers, Glaser, and MacDonald, 1998; Gais, et. al., 2001).

A multiplicity of empirical approaches have been used to study the welfare policy changes of the 1990s. Three of the most common are summarized here, with a brief discussion of their pros and cons.[28]

Random Assignment Experiments. For more than two decades, researchers have studied labor market interventions with random assignment experiments. In these cases, an experimental group is randomly chosen from among those eligible for a program and this group receives the services and program benefits. An alternative control group is refused entrance into the program and operates in an environment (presumably) unaffected by the program. If randomization is done correctly, the only difference between the two groups should be that one group receives the program treatment and one does not. That means that simple outcome differences between the groups can be used as a measure of program effects.

Experimental evaluation of welfare-to-work programs has occurred since the mid-1980s and became quite sophisticated by the 1990s. During the 1980s these experimental evaluations focused solely on welfare-to-work programs. The use of waivers in the 1990s allowed states to implement more extensive reforms involving other program changes beyond welfare-to-work efforts. Because the Department of Health and Human Services required experimental evaluations of waiver programs, a body of results are now available from pre-1996 state programs analyzing the impact of more complex welfare

[28] In addition to these three approaches, there is a great deal of more descriptive work, much of it involving the collection of new data, including a variety of ethnographic studies in particular neighborhoods.

reforms with multiple components – welfare-to-work training, time limits, sanctions, family caps, etc -- on AFDC receipt and earnings.[29] The federal mandate for experimental evaluations of waiver programs ended when TANF was implemented and there have been no experimental evaluations of TANF programs post-1996.

These experimental evaluations are viewed as highly credible, since they come as close to a controlled research environment as possible. When experiments are appropriately designed, there is no better methodology available. Indeed, the widespread acceptance of the positive results of welfare-to-work evaluations in the 1980s were a major reason why policymakers supported work-oriented welfare reform in the 1990s.

These experiments have limitations as a way to study the welfare reforms of the 1990s, however. First, when multiple program changes are occurring, it is difficult to study the separate effects of individual program changes in an experimental way. Hence, the policy implications of experimental results were more interpretable in narrowly implemented welfare-to-work programs (which changed only one or two program parameters) than in broad waiver programs (which typically involved multiple program changes). For instance, it is not possible to separately identify the effect of time limits from other welfare-to-work components in existing experimental evaluations from the early 1990s; the experimental results identify the aggregate effect of all program changes together rather than the specific effect of each individual program change. Similarly, it is not possible to use experimental evaluations to study the impact of a legislative change (such as TANF) where multiple changes are implemented throughout the state at the same time.

[29] See Gueron and Pauly (1991) and Friedlander and Burtless (1995) for a summary of much of the earlier research, and Bloom and Michalopoulos (2001) for a summary of some of the key 1990s research.

Second, experiments should be designed so that the program effects cannot influence the control group. With all of the simultaneous policy changes occurring in the 1990s, however, it was hard to prevent some contamination. Word circulated among welfare recipients – including the control group – about mandatory work requirements or upcoming time limits. This might lead the control group to respond to these rumors, even though they were formally unaffected by the changes.

Third, experimental studies are expensive and time-intensive. They almost always require collecting additional survey data, both at baseline (when the control and experimental groups are defined) and at multiple follow-up study points. Because their implementation takes skill and often requires some administrative reorganization within welfare programs to separate the control and experimental groups, they put additional demands on administrators and front-line workers. In the midst of all the other changes occurring in welfare offices, few states wanted to invest either the funds or the time necessary for experimental evaluations over the late 1990s.[30] The locations that did participate in earlier experimental evaluations during the 1990s are not a random sample of all states or welfare offices. For instance, we have a number of excellent evaluations done in north midwestern states and fewer evaluations done in the traditional "deep south" states. This can call into question the generalizability of the experimental evidence.

Fourth, these experimental studies are not well-designed to study "entry effects." An experimental program may not induce the same discouraging effect on welfare usage

[30] A variety of waiver evaluations were ongoing and continued into the post-1996 period. Post-TANF, many governors pointed to the declines in caseloads and the implementation of welfare reform as a major success. This could have made them more reluctant to authorize serious evaluations that might reduce their ability to garner political credit from these changes.

that might result from a permanent welfare reform, hence caseload change may be underestimated. Even if some families are discouraged by the experiment from ever applying for welfare at all, this effect is typically not measured in most experiments.[31]

For all of these reasons, the experimental evidence on the effects of welfare reform is highly useful where it looks at the impact of specific program components in particular types of welfare programs. But the experimental evidence tells us relatively little about the overall effects of TANF implementation in states in the mid-1990s.

Leavers' Studies. A substantial amount of research time and money has been devoted to following persons as they left welfare in recent years. A number of organizations and individuals launched so-called "leavers' studies" soon after the implementation of the 1996 legislation. The primary intent was to analyze the behavior and well-being of those who lose welfare benefits (either voluntarily or involuntarily) in the post-reform era.

Leavers' studies answer a very specific policy question, namely, "How are people faring who used to receive public assistance but are no longer on the rolls?" The interest in this question has been strong, particularly as caseloads have declined so precipitously in most states.

Most of these studies use a combination of administrative data and new survey data. Persons on welfare at a specific point in time are tracked and surveyed at some later point, to ask about their employment, family, and income situation. This survey information may be combined with other administrative data to investigate program receipt of Food Stamps, Medicaid, or other support programs, to measure recidivism

[31] An alternative version of entry effects occurs if a program induces additional welfare participation. This seems less relevant to the welfare reforms of the 1990s, which tended to focus on moving people directly

(return to cash public assistance), or to verify employment using Unemployment Insurance program data on earnings and employment. These studies can provide quite detailed information on the behavior of ex-welfare recipients.

Like experimental evaluations, leavers' studies can be complex, costly, and time-consuming. It is often difficult to locate and survey ex-welfare recipients, and a number of not-very-credible leavers' studies have low (and presumably quite selective) response rates. Working with administrative records, particularly records across multiple programs, requires matching individual identifiers and dealing with complex data problems. As a result, the quality of leavers' studies varies greatly. Since most leavers' studies are state-specific, it is often difficult to compare them as different researchers focus on different outcomes or use different methodologies in different states.

The biggest limitation to leavers' studies is that they provide very little information about policy effects. Unlike an experimental evaluation, it is impossible to separate those who would have left welfare even under AFDC from those additional leavers due to the new welfare program design. This means leavers' studies tell us almost nothing about the effects of new programs.[32]

Furthermore, leavers' studies by design focus on a limited population--those who were once on welfare. Some studies ignore those who remain on welfare longer, a group of some concern. None of these leavers' studies say anything about those who might have come onto AFDC pre-1996 but who chose not to come onto TANF post-1996. Evidence suggests that both entry into welfare has fallen and exits from welfare have

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into jobs; in contrast, the reforms of the 1980s often had substantial education and training components.

[32] An exception is Cancian, et. al. (2002), who compare pre-reform leavers with post-reform leavers in the state of Wisconsin. While this is a superior methodology, even these estimates are contaminated by other changes (such as the booming economy) which occur at the same time as reform.

risen (Mueser, et. al., 2000). To the extent that those who are diverted from receiving welfare are differently selected from those who come on but leave faster, the leavers' studies cannot be interpreted as evidence on the general well-being of persons affected by welfare reform. One might expect leavers to be somewhat less employable and more disadvantaged than those who have options that allow them to choose not to enter welfare in the first place.

In short, leavers' studies provide little information about the overall effects of welfare reform. At best they tell us something descriptive about how a specific population of ex-welfare recipients is faring, but it is difficult to interpret anything causal about policy (or any other explanatory variable) from these studies.

Econometric evaluations. A growing body of literature uses a combination of national and administrative data to study the impact of policies. Typically, these studies use data on a key dependent variable – such as caseloads or labor force participation -- from multiple years and regress it against controls for economic factors and policy factors. Some studies also include controls for demographics and political changes. Much of this work is based on state panel data. For instance, state caseload data might be regressed against state unemployment rates, state AFDC/TANF benefit levels, and dummy variables that signal the implementation of a state waiver. Alternatively, some of this research utilizes individual data on welfare participation or work behavior for multiple years.

A typical regression equation based on state panel data is as follows:

$$(1) \qquad Y_{st} = \alpha_s + \beta_t + \delta P_{st} + \gamma X_{st} + \varepsilon_{st}.$$

where Y_{st} is the dependent variable (say AFDC/TANF caseloads) in state s in year t. The vector α_s represents the set of estimated state fixed effects for all s states, β_t represents a set of year fixed effects for all t years (sometimes there are also state-specific time trends included), P_{st} is a set of policy-specific parameters and δ is its related coefficient vector, while X_{st} is a set of all other included variables with γ its related coefficient vector. X_{st} typically includes state unemployment rates and may include other state economic and demographic variables. Equation (1) is usually estimated with a weighted least squares estimation procedure, with weights based on state population.[33]

Policy variables are typically represented as dummy variables that equal zero prior to the implementation of a specific policy (a waiver or a TANF program), and equal 1 in each year thereafter.[34] Hence, the policy coefficients measure the average change in Y after the policy change, controlling for all other variables. The state effects remove long-term state-specific differences and allow one to interpret the coefficients as the effect of changes in the independent variables over time within a given state. The year effects remove any common changes occurring in all states in the same year (and hence remove the effects of policies that are implemented everywhere at once, such as an EITC change or a minimum wage change).

Identifying the true effects of policy on the dependent variable Y requires several things: First, policies must be accurately and completely coded; second, there must be a way to identify the policy effects separately from the other variables; and third, there must be no omitted variables correlated with the policy changes to bias the policy

[33] Even if individual level data is available some researchers aggregate this to the state level, arguing that the variables of interest (policy differences within and across states) vary only at the state level. In a few cases, authors interact policies with individual-level characteristics, in which case utilizing individual-level data is a necessity.

coefficients. Most researchers use relatively sparse specifications, hoping that state and year fixed effects and state-year time trends will control for the large number of omitted variables that inevitably haunt these econometric exercises.

As discussed below, most of this work has focused on AFDC/TANF receipt, looking at caseload changes over time as the dependent variable. Some papers look also at changes in labor force participation over time, and a few papers use earnings, income, poverty, fertility or marriage rates as the dependent variable.

These econometric studies confront a variety of problems. First, identifying the policy effects can be a problem. The effects of welfare waivers are reasonably well identified, since different states adopted these waivers at different points in time. The effects of TANF implementation are much less well-identified. As noted above, virtually all states implement TANF at about the same point in time. Most papers try to use the differences in timing over 1996-97 to identify an effect, but the standard errors of these estimates are high.[35] Some papers try to identify effects by combining waivers and TANF, coding a dummy variable that equals one if a state has a major waiver in effect *or* if it has adopted a TANF program. This has the odd effect of forcing waivers and TANF programs to have identical effects, almost surely not justified given how much more extensive were the changes involved with state TANF plans.

Even if identification were easier, this research merely estimates the aggregate effect of these changes, without differentiating between the very different set of waiver or TANF program components adopted by different states. Hence, some researchers have

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[34] In the year the policy is enacted, the dummy variable is typically equal to the fraction of months that the policy is in effect.

tried to code the adoption of specific program components rather than the adoption of a single policy change.[36] Unfortunately, the identification problems with this approach are severe. As noted above, it is not clear how to code some policy changes (and we have only limited information post-1996 on what specific states are doing in certain policy areas). Furthermore, some individual policies are adopted by so few states (and only in the few years post-1996) that there is not enough information to estimate a reliable coefficient. The result is that much of the econometric literature focusing on individual policy components finds insignificant or even perverse coefficients.

Finally, there have been substantial specification arguments in this literature. Most papers have chosen to utilize a simple panel data framework with fixed effects, perhaps including lags on a few key variables (like unemployment rates). A few papers, however, have chosen more complex specifications, including lagged dependent variables, a greater number of lags on key independent variables, and/or more extensive fixed effects. These choices matter because the more complex specifications typically find smaller or less significant policy effects. Those who like these latter papers tend to argue that the more complex specifications better mirror reality and are more reliable. Those who find these latter papers less persuasive (including myself) tend to argue that they are overspecified, with extensive lag structures that leave little scope for measuring

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[35] See the discussion of identification problems in Schoeni and Blank (2000). They try an alternative way to estimate TANF effects based on a difference-in-difference estimates, pre- and post-1996 and between more and less educated women.

[36] Rather than controlling for the implementation of waivers, for instance, this could mean controlling for the type of sanctions approved in the waiver, the presence and length of time limits, the implementation of a family cap, or the nature of the work mandates in the state.

policy effects based on simple dummy variables. In addition, the combination of a lagged dependent variable with state fixed effects produces inconsistent estimates.[37]

A seminal contribution to this specification argument was provided by Klerman and Haider (2001), who point out that there is no clear theoretical justification provided for any of the specifications used in earlier papers. They note that the stock of welfare cases is the result of flows into and out of welfare. They model the dynamic process of entering and leaving welfare and derive an estimable model of aggregate caseload change from this. They show that even if the entry rate and the continuation rate are functions only of contemporaneous economic conditions, per capita caseloads will be a non-linear function of lagged economic conditions equal to the longest period individuals are on aid. Hence, only particular lagged specifications are correct. They also indicate the conditions under which including a lagged dependent variable is appropriate.

Klerman and Haider's work provides a more believable and persuasive specification than earlier papers, and suggests that much of the other research estimating the determinants of per capita caseload levels has been misspecified. Klerman and Haider prefer a model which estimates welfare entry and exit flows, rather than net caseload levels. Unfortunately, estimating this specification requires flow data, which does not appear to be reliably available at the national level. (Klerman and Haider estimate their model on California data only, where they believe they have reliable data. This makes it hard to generalize their results and compare them to other work.) Klerman and Haider's results from this co-called "stock-flow model" are closer to those of the simpler specifications in the role that it ascribes to the economy over the 1990s.

[37] See the discussion of these specification issues in Grogger, Karoly and Klerman (2002) or Blank (2001b). For a more positive reading of these results, see Bell (2001) or Ziliak (2002). Moffitt

Ultimately, econometric models – however limited – will probably provide the best evidence we are likely to have available on the overall effects of welfare reform. Such models are almost surely less reliable in providing evidence on individual program components; when available, experimental evidence on specific program changes is probably more believable. Future research should focus on better ways of utilizing econometrics to identify individual welfare program components.

For instance, Grogger (2000, 2002, forthcoming) takes a clever approach with time limits. He notes that families with young children should be more affected by time limits than families with older children (since families with young children have a longer period of future potential welfare eligibility). Hence, he interacts state time limit information with information on the ages of children in a household, and finds substantially larger effects among families with younger children, as hypothesized. Similar creativity in teasing out the effects of other specific program components would be useful.

The remainder of this paper summarizes the research findings from papers that use the above methodological approaches. I organize this review by the dependent variable in the paper.

V. Caseloads

The most voluminous literature on welfare reform in the past decade has focused on caseload changes. Interestingly, prior to the mid-1990s, there was virtually no published literature in economics journals looking at movements in caseloads over time,[38]

(forthcoming) also provides a summary of this literature.

[38] An exception is Moffitt (1987).

but the number of more recent articles is growing rapidly. The primary interest in this research literature is to explore the steep caseload decline that started in the mid-1990s, with particular attention to separating out the effects of economy from policy.

Almost all of the literature on caseloads fits into the third methodology described above, and utilizes regression analysis on some sort of panel data over time. (Some evidence from experimental studies on the impact of specific program choices on welfare usage is discussed in Section VIII below.) Different papers focus on different variables, and the discussion below focuses sequentially on the effects of economic variables and of aggregate policy variables on caseloads. I briefly discuss the (few) papers which focus on caseload flows rather than caseload levels, followed by a discussion of research that distinquishes the effects of specific policy components on caseloads. I close this section with a short discussion of the literature on food stamp caseloads.

Table 6 provides a list of the papers to date that use regression analysis to investigate the effects of welfare reform during the 1990s, indicating the dependent variable, data source, primary included variables, and a few key conclusions. In most of these papers, caseloads are the dependent variable, although a few of them (discussed below) look at employment, income, and family structure changes as well. Part A of Table 6 lists the papers using data prior to the implementation of TANF, which primarily focus on the effects of state waivers. As we discuss below, some of these papers focus on the implementation of any waiver, while others try to differentiate between the policy components in different waivers. Part B lists the papers that utilize data post-TANF and

that estimate the effects of TANF as well as waivers. Part C lists the papers that use flow data on exits from or entries onto welfare, rather than stock data on caseloads.[39]

Aggregate Caseloads and the Economy. The majority of papers utilize annual state panel data, based on administrative records, to study movements in total AFDC/TANF caseloads, using some variant of equation (1). The typical economic variable is the state unemployment rate, although a few papers use state income or wage information as well. This is due to data convenience as much as anything else -- state unemployment rates are one of the few readily available annual state-level economic variables. Given the sensitivity of less-skilled workers to movements in unemployment (Hoynes, 2000), this is often assumed to be reasonable characterization of the economic environment.[40]

The majority of papers find relatively similar effects of unemployment on caseloads, not surprising since these papers tend to use similar methodologies and data sets. For instance, Council of Economic Advisers (1997, 1999), Levine and Whitmore (1998), Wallace and Blank (1999), and Blank (2001a) all find that a 1-point rise in unemployment tends to increase caseloads by about 5 to 7 percent. Blank (2001a) suggests that these effects are larger for the small part of the caseload composed of married couples than among single mothers.

Most of these papers attempt to see how much of the caseload change over the mid-1990s can be ascribed to economic changes. For instance, Council of Economic Advisers (1999) indicates that changes in unemployment explain 26 to 35 percent of the

[39] I do not include papers based on data from one state only in Table 6. A number of good state-specific research exists, such as MaCurdy, Mancuso, and O'Brien-Strain (2002) for California. I also omit studies that are based on a single cross-section rather than panel data (such as Mead, 2000.)

caseload change in 1993-96, but only 8 to 10 percent of the caseload change in 1996-98. These estimates range widely across studies, and are highly affected by the years over which they are estimated and by the specification. Specifications utilizing lagged dependent variables and/or first differences (Ziliak, et. al., 2000; Figlio and Ziliak, 1999) tend to find larger effects due to economic changes.[41]

It also appears that the responsiveness of caseloads to unemployment has increased over time (Moffitt, 1999a; Council of Economic Advisers, 1999). This means that studies based on more recent and shorter panels (such as Ziliak, et. al., 2000) are more likely to find larger unemployment effects. While the reasons for this change are unclear, it is consistent with a welfare system that is doing more and more to emphasize work. This reduces the dependence of welfare recipients on cash benefits, and increases their dependence upon job availability.[42]

Several of the papers that utilize data post-1996 and compare the TANF era with the AFDC era indicate that unemployment effects on caseloads are smaller post-1996 than pre-1996 (Council of Economic Advisers, 1999; Wallace and Blank, 1999; Schoeni and Blank, 2000; O'Neill and Hill, 2001). Post-1996, public assistance programs had a harder edge and put more attention into enforcing time limits, sanctions, and diversion policies. Given this policy environment, it may not be surprising that the economy mattered less to caseload changes during this time. Alternatively, the very strong economy of the late 1990s might have been expected to have a greater effect than the more sluggish economy of the early 1990s, in which case this is a surprising finding.

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[40] This assumption is typically made without real evidence. As discussed below, other economic variables also appear to be important over the past two decades in explaining caseloads.

[41] Klerman and Haider (2001) indicate that these specifications should bias the economic effects upward.

A few studies have gone beyond attention to state unemployment rates and utilized a richer set of economic variables. Several studies include information on state-specific minimum wages (Council of Economic Advisers, 1999; Grogger, 2000), on employment/population ratios (Ziliak, et. al, 2000) or on state income levels (Schoeni and Blank, 2000). Several studies have tried to characterize the shifts in relative wages and in industry mix that occur over the 1980s and 1990s. Blank (2001a) calculates state-specific average wage levels as well as 50^{th} percentile/10^{th} percentile wage level ratios within each state and year. Bartik and Eberts (1999) utilize data on the education requirements, wage premiums, and low-wage employment implied by the industry mix in each state and year. In most cases, these additional variables are significant and suggest that more than the unemployment rate matters in affecting caseloads. Blank (2001a), however, indicates that the estimated unemployment effect is relatively unchanged by the inclusion of demographic or political variables.

Virtually all research agrees that state economies had a significant effect on caseloads, and that caseloads rise in bad economic times and decline in good times. Most of the research suggests, however, that economic changes alone cannot explain the majority of caseload movements over the 1990s; policy variables also matter.

Aggregate Caseloads and Overall Policy Effects. Policy shifts are typically characterized by dummy variables that "turn on" when a policy is implemented in the state. Early papers focused particularly on the effects of state waivers. More recent papers have also tried to investigate the impact of TANF implementation. The

[42] Bartik and Eberts (1999) suggest this might be due to declining demand for less skilled labor, which could make joblessness increasingly difficult to escape for low-wage workers.

identification problems discussed in the previous section make the latter particularly difficult to estimate.

Most of the literature finds that state waivers implemented in the first half of the 1990s had a significant and negative effect on caseloads.[43] For instance, one of the earliest studies finds that the implementation of major statewide waivers explains about 13 to 31 percent of state caseload change between 1993 and 1996 (Council of Economic Advisers, 1997). While the magnitude of impact varies somewhat across studies, similar results are found in Levine and Whitmore (1998), Wallace and Blank (1999), and Blank (2001a). Moffitt (1999a) and Schoeni and Blank (2000) use somewhat different data than other studies, aggregating individual data into state-year observations on AFDC participation by women's age and education level. Both of these papers also find significant negative waiver effects on AFDC participation, with particularly strong effects among less educated women, consistent with the expected impact of welfare reform.

Fewer studies have looked at the impact of TANF implementation (Part B of Table 6). Council of Economic Advisers (1999) and Schoeni and Blank (2000) both indicate that TANF appears to have a larger effect on caseloads than did waivers.[44] This is consistent with the evidence cited above that shows that economic factors had smaller effects post-TANF, suggesting that the policy changes of the mid-1990s were a major cause of declining caseloads.

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[43] As discussed above, these waivers were not randomly implemented across states. Once one controls for differences in state unemployment rates, non-waiver states and waiver states show similar caseload trends prior to the implementation of waivers (Schoeni and Blank, 2000)

[44] Wallace and Blank (1999) and Grogger (2000) find TANF effects that are similar in magnitude to waivers, but both papers only have data through 1998. This is before the full effects of state TANF programs might have been felt.

A few studies find almost no effects of policy, although most of these are focusing on the waiver period and not the post-TANF period (Ziliak, et. al., 2000; Figlio and Ziliak, 1999). These studies, as discussed above, tend to use more complex specifications. The shorter time period in Ziliak, et. al. (2000) also makes it more difficult to separately identify policy and economy effects. Other specification issues matter as well, including the choice of fixed effects. For instance, Schiller (1999) includes no fixed effects and (not surprisingly) finds stronger state policy-related effects. O'Neill and Hill (2001) do not include year fixed effects and as a result their estimated coefficient on the TANF dummy variable over the late 1990s is much larger than in other studies.

Despite evaluation and methodological problems, the bulk of the research literature suggests that the policy changes over the 1990s were important to the rapid caseload decline. This evidence remains unsatisfying, however, for a variety of reasons. First, most of these papers focus on aggregate policy changes (the adoption of a major state waiver or the implementation of a TANF plan) rather than on specific policy components. For instance, some aspects of these waivers (time limits and stricter sanctions) might have been expected to reduce caseloads. Other aspects (lower benefit reduction rates) might have increased caseloads. I discuss the efforts to characterize and measure the impact of policy specifics below.

Second, it remains very difficult in any of this literature to separate economic and policy effects, given the extent to which these were interacting with each other over the late 1990s. The combination of extremely strong economic growth with rapid policy change means that both effects were reinforcing each other.

Third, while these papers focus on welfare-related policies, they are less useful in measuring the additional impacts of other policy changes over this time period. Because of the nature of the models (typically including year fixed effects) the impact of changes in the EITC or minimum wage on caseloads are difficult to evaluate. No one has yet put together the appropriate data to measure the impacts of changes in child support policy, child care subsidies, or health insurance. To the extent that these other changes are interacting with the measured policies over time, estimates of the measured policies may be biased up or down.

While there remains a very strong interest in the question "what has been the effect of the policy changes of the 1990s on caseloads (and other variables as well)?" it remains a difficult question to answer in an entirely credible manner. Future research in this area, with better data or better identification strategies, will be received with interest.

Caseload Entry and Exits. Aggregate caseload changes are the result of an underlying dynamic process in which individuals are choosing to enter welfare and choosing how long to stay on it. In other areas, looking at these underlying dynamic processes has produced new insights about net changes.[45] A few papers, listed in Table 6, part C, have tried to investigate the underlying flows in and out of public assistance.

This is hard to do because there does not exist good national flow data on AFDC/TANF entry and exit. While states have reported welfare entries and exits for many years, this data has severe problems. It is inconsistently reported across states (states define entries and exits differently) and some entries and exits appear not to be reported. For instance, if one aggregates entries and exits over the 1980s and 1990s in

[45] For instance, the literature on employment and unemployment has benefited by looking at flows in and out of the labor market and in and out of employment.

many states, there is little evidence of the significant rise in caseloads that is visible in the aggregate caseload data, suggesting either that entries are consistently undercounted or exits are overcounted.[46]

Klerman and Haider (2001) provide a model of dynamic flows in and out of welfare, and relate them to the net caseload. As discussed above, they also critique previous econometric estimates as misspecified, based on their model. They utilize county-level entry and exit data from the state of California only, which they believe to be accurate, to estimate the determinants of entry and exit. Their results on the role of unemployment in caseload changes based on flow models are quite similar to those estimated in much of the literature using stock models, and indicate that the economy accounts for less than half of the caseload change in the mid-1990s.

Mueser, et. al. (2000) take a somewhat similar approach. They use administrative data on welfare recipients from five major metropolitan areas to create their own entry and exit rate calculations. Using data over a number of years in the mid-1990s, they estimate that about two-thirds of the decline in welfare is due to higher exits from welfare, while about one-third is due to lower entry. Consistent with the literature on aggregate caseloads, both state economic conditions and policy changes affect welfare exits. Welfare entry appears less affected by the economy.

Finally, Hofferth, Stanhope and Harris (2001) estimate the probability of exiting welfare using longitudinal data from the PSID between 1989 and 1996. Although the time period is quite short (particularly to measure the impact of policy changes that

[46] Discussions with staffers at HHS suggest that the primary problem is likely to be that entries are undercounted. In particular, in many states a woman who has recently been on welfare and who returns to the rolls appears not to be counted among the new entrants.

largely occurred in 1994 and 1995) they find significant effects of waivers, particularly on the probability of exiting AFDC to work.[47]

Further work focusing on the underlying dynamics of caseload changes would be useful. This will require the creation of new and better flow data. In particular, it would be highly interesting to be able to estimate the impact of specific policy choices on entry and exit rates within states.

The Effects of Policy Components. A number of the econometric investigations of caseload changes attempt to measure the impact of specific policy components. That is, these studies go beyond simple dummy variables that code the implementation of any waiver or any TANF plan and try to code the specific nature of the policy change. Because the available data on state policies is limited, these studies typically limit themselves to looking at 6 or 7 specific policies.[48] For instance, one might code when a state enacted time limits (some states received waivers to do this, some states enacted them as part of their TANF plan, some states have no time limits but have declared they will continue funding families out of state funds after 60 months).

On the one hand, this literature goes inside the "black box" of policy changes and does not treat "welfare reform" as a single variable. On the other hand, efforts to analyze the effects of specific policies have at least two major problems. First, only a partial set of policies are typically analyzed. For instance, if states that implement lower benefit

[47] This paper utilizes relatively simple duration models. A companion paper (2002) looks at recidivism (returns to AFDC) among AFDC leavers. But given the short time frame in which to observe the effects of policies enacted in 1994-95 on recidivism, it is not surprising that there are few significant effects estimated for waivers.

[48] Most typically, policy coding includes the implementation of time limits, a family cap, benefit reduction rates, age-of-child work exemptions, the timing of work requirements, sanctions and benefit reduction rates. Not included in any of these studies is information on diversion policies, the nature of job-preparation activities, or the availability of child care subsidies or other work supports. Of course if the

reduction rates also provide more generous child support to those who go to work, then including one policy (BRRs) without the other (child support supplements) will produce a biased estimate. An example is found in Kaushal and Kaestner (2001) whose only policy variables are measures of time limits and family caps. This has the effect of attributing all of the effects of welfare reform to these two variables. Second, the number of states that have implemented specific policies is very limited in many of these studies. Many studies use data only through 1996, which allows them to code the policy specifics of waivers only. Few enough states received waivers for specific policies that there is a fundamental lack of identification in many of these studies. Evidence of this is seen in the regular occurance of perverse signs on some of these policy variables.[49]

Among the studies that look at policy specific effects are CEA (1997, 1999), Ziliak, et. al. (2000), Moffitt (1999), Schiller (1999), and Kaushal and Kaestner (2001). (For more information on these studies, see Table 6.) It is hard to produce a summary set of conclusions for this work. With different specifications and slightly different data periods, different studies get different results on quite similar variables. Not all of these results make sense, for instance, CEA (1999) finds that family caps increase caseloads; Ziliak, et. al. (2000) finds that strong work incentives increase caseloads. It is difficult to draw strong conclusions about the impact of specific policies from this literature.

Three papers are somewhat more persuasive. Grogger (2000) notes that families with younger children should be more affected by time limits, since they are more likely to be constrained by such limits. He interacts the adoption of state time limits with age of

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implementation of these other (nonincluded) policies are correlated with the implementation of some of the included policies, this can bias the measured effects.

[49] Most of the specific variables also end up being insignificant in most of the estimates. Of course, this is consistent with a lack of identification, but it is also consistent with a lack of policy effects.

youngest child in the family and finds consistently greater and more negative effects of time limits on families with younger children. He estimates that 12 percent of the decline in welfare caseloads in the mid 1990s might be due to time limits. Even this paper, however, should be read with some skepticism since there are omitted policies that might be correlated with time limits. Grogger (forthcoming) extends this work, showing that time limits also increased work more among women with young children and Grogger (2002) indicates the effect occurs in other data sets.

Hofferth, Stanhope, and Harris (2001) investigate the effects of specific waiver policies, using data from 1989-96 from the Panel Survey of Income Dynamics to look at welfare exits. While their results on the determinants of "any exit from welfare" result in largely insignificant coefficients on specific policies, their results on the determinants of "exit from welfare that results in work" are more conclusive. They find that stricter work requirements and fewer exemptions for mothers with young children are positively correlated with welfare-to-work exits, while lower benefit reduction rates are negatively correlated. The lack of a complete set of welfare-related policies causes omitted variable problems with this study as well.

There is still much room for further research that uncovers the effects of specific policies through controlled estimation techniques. It would be helpful to have a fuller specification of state policies, longer time periods to observe the full effect of these policies (especially in the post-TANF era), variation in policy within states over time to provide stronger identification of effects, and better ways to identify the effects of these policies on the groups which they most impact. Experimental evidence on the effect of a few specific policy choices is discussed in Section VIII below.

Food Stamp Caseloads. While most of the literature has focused on AFDC/TANF caseloads, one might also be concerned about access to other means-tested programs as well. As women leave cash public assistance programs for work, they should maintain their Food Stamp eligibility if their overall incomes remain low. Hence, rapid declines in Food Stamp caseloads in the mid 1990s were viewed with concern by many.

Food Stamp caseloads have tracked AFDC caseloads quite closely since the late 1970s. This is not surprisingly, since over 80 percent of AFDC recipients also received Food Stamps (U.S. House of Representatives, 2000, Table 15-3). Although Food Stamps have been historically available to all low-income families and individuals, take-up rates have been quite low among the non-elderly, non-AFDC population.[50] This suggests the historical importance of AFDC as a "gateway" program into Food Stamps.

Like AFDC, Food Stamp caseloads increased from 1990-94, peaked in 1994, and then began to decline rapidly. They fell 37.5 percent from 1994-2000. While their rate of decline was similar to the decline in AFDC/TANF in the 1994-98 period, it began to slow and Food Stamps fell much less in 1998-2000 than did TANF caseloads.

Despite the expectation that many families should maintain Food Stamp eligibility, Zedlewski (2001) indicates that almost two-thirds of welfare leavers leave Food Stamps as well. Even among very low income families leaving welfare (i.e., those whose income is below 50 percent of the poverty line), only about half continue to receive Food Stamps although all of these families should still be eligible. Schirm (2001)

[50] Ohls and Beebout (1993, Table III.1).

indicates that there has been a clear decline in Food Stamp take-up rates among eligibles, from 71 percent in 1994 to 59 percent in 1998.[51]

There have been few econometric studies of Food Stamp declines. Wallace and Blank (1999) estimate Food Stamp caseload determinants, using a model identical to that which they use with AFDC/TANF determinants. They find Food Stamp caseloads are slightly more responsive to economic cycles (a one-point rise in the unemployment rate results in a 6-7 percent increase in Food Stamp caseloads). Waivers appear to have a significant negative effect on Food Stamp caseloads, although there are typically few provisions in these waivers directly related to Food Stamps. Similar results are found in Wilde, et. al. (2000), who look at transitions on and off Food Stamps in the early 1990s and find strong economic responsiveness and an effect of state welfare waivers. Currie and Grogger (2001) use post-1996 data and indicate that waivers and TANF both decreased the food stamp caseload. Figlio, Gundersen, and Ziliak (2000) find significant cyclicality in the Food Stamp caseload. As with related work on cash benefits, once they include a lagged dependent variable and various lags in other variables, they find much smaller effects of policy changes.

More work indicating how Food Stamp caseloads are changing within different populations can be important. The 1996 legislation limited Food Stamp eligibility among able-bodied persons without children and the effect of these restrictions would be interesting to study. Similarly, there has been a sharp decline in Food Stamp use among

[51] This raises a variety of policy-related issues about how families learn of their Food Stamp eligibility and how they maintain benefits while working. See Greenstein and Guyer (2001) for a review of these issues.

legal immigrant families and their children since the 1996 legislation eliminated their eligibility. The effects of this change could also be studied.[52]

VI. Labor Force Participation

Declining caseloads are a mixed indicator. They indicate less receipt of cash assistance, which is good for those concerned with government budgets but may be bad from the viewpoint of individual well-being. The thrust of the 1990s reforms was to increase work and it is important to explore not just whether policies caused women to leave welfare, but also whether these same policies helped women enter the labor force and replace their public assistance income with earnings.

The leavers' studies focus on employment behavior among ex-welfare recipients. As noted above, this is only a subgroup of those affected by welfare reform (others choose never to come onto welfare as a result of the reforms). But labor market opportunities for this group have received substantial public attention. The evidence suggests that close to two-thirds of welfare leavers are working at a future point in time.[53] An even higher share held at least one job since leaving welfare. Martinson (2000) notes that only 20 percent of leavers appear to never work in a four year follow-up of work programs in six sites. In short, most leavers find jobs, although jobs and jobholding may be unstable, so that leavers may not work continuously.[54]

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[52] Borjas (2001b) has a recent paper on this.

[53] Cancian, et. al. (1999) uses the two-thirds number to summarize existing leavers studies. Loprest (2001) indicates 61 to 64 percent are working at a survey point within two years of leaving welfare. Moffitt and Roff (2000) cite 63 percent working in studies of three major metropolitan areas. Devere (2001) indicates 55 to 64 percent are working within 3 months of leaving welfare, based on a review of state leavers studies.

[54] A growing body of research has focussed on the effects of personal barriers – such as health problems, substance abuse problems, low skills, or domestic violence – on work, indicating that mothers with multiple barriers work less (Danziger, et. al., 2000; Zedlewski and Loprest, 2001).

Wages among leavers vary by study, but typically range between $5.50 and $8.50 per hour among those who are working.[55] Most leavers appear to have no more than two spells of employment inside 4 years (Martinson, 2000), which does suggest job churning is not extensive. Holzer, Stoll, and Wissoker (2001) indicate that employers rate welfare recipients as performing as well or better than other employees.

While descriptively interesting, these studies raise a variety of questions. First, how much did policy influence labor market changes? Second, how did the labor market respond to this influx of low-wage workers? I summarize the research on each of these questions next.

Did Policy Influence Work Behavior? Figure 4 indicates that work among single mothers started to rise in the mid-1990s, strongly suggesting that policy was important to this change. Other more controlled evidence supports this conclusion.

Moffitt (1999a) and Schoeni and Blank (2000) investigate the effects of waivers on labor force participation and weeks and hours of work. Both conclude that waivers had a significant and positive impact on work behavior among less-skilled women. In contrast, Schoeni and Blank (2000) find few effects of TANF implementation on increasing labor market participation; once they control for state unemployment and income changes, the post-1996 rise in labor force participation among less-skilled women is fully explained. Using a slightly different data set and empirical techniques O'Neill and Hill (2001) find that both economy and policy matter in the post-TANF era.[56]

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[55] Devere (2001) cites these numbers in summarizing a large number of state leaver studies. Loprest's (2001) national survey of leavers finds average wages of $7.15 among those who left welfare between 1997 and 1999. Moffitt and Roff (2000) cite average wages of $7.50. Cancian, et. al. (1999) cite $6.50-$7.50 as the typical wage range of leavers.

[56] The lack of time fixed effects in the O'Neill and Hill study almost surely results in a larger coefficient on the TANF dummy variable than in other studies.

In short, these regression studies conclude that policy changes appear to have mattered in the mid-1990s, although the strong labor market unambiguously helped increase work among less skilled women as well. These studies focus only on welfare reform, however, and are largely unable to separately identify the effects of larger national changes in the EITC or the minimum wage.

There is unanimous agreement that the growing EITC increased labor force participation among single parents. A number of studies, including Eissa and Liebman (1996), Meyer and Rosenbaum (2001), Blank, Card, and Robins (2000), Ellwood (2000), and Hotz, Mullin and Scholz (2001), all find that a sizable component of the growth in labor force participation among single women can be linked to the EITC expansions.[57] The lack of studies that effectively include both welfare reforms and EITC changes makes it difficult to talk about the comparative impact of these two policy changes. Grogger (forthcoming) is unique in trying to explicitly control for both EITC parameters and welfare reform effects. He finds significant effects of the EITC on welfare usage and work behavior as well as significant policy reform effects. Ellwood (2000) argues the independent effects of the policies cannot be accurately separated because many of the welfare reforms were administrative in nature and interacted with the strong economy and the growth of EITC supports.

The minimum wage also rose in both the early and the mid 1990s. While minimum wage increases should raise the returns to work among low skilled workers, they might also result in a loss of jobs. Despite a great deal of debate about minimum wage effects on teenagers, we have far less evidence of minimum wage effects on single

[57] Eissa and Hoynes (1998) indicate that the EITC may have small negative effects on labor force participation among married women.

mothers. Card and Krueger (1995) summarize the studies on minimum wage effects; almost all focus on teenage employment. Bernstein and Schmitt (1998) investigate the minimum wage increases of 1996 and 1997 and conclude there is little evidence of job loss among adults due to these minimum wage changes. Neumark (2001) finds the changes affected employment primarily among unskilled teen workers.

Only one paper has addressed the effect of child care subsidy policies under welfare reform on mothers' work behavior. Lemke, Witt, and Witte (2001) use data from Massachusetts to demonstrate that the availability of subsidies and the attributes of the local child care market both affect the probability a welfare recipient moves into work.

Ideally, one would like to measure the combined effects of all policy changes, as well as their individual (and potentially interactive) effects. Research that does this is not available. It does appear, however, that both the welfare reforms and the growing work support programs of the 1990s contributed to the sharp rise in work among single parents.

A final set of evidence on the impact of welfare reform on labor market participation comes from the randomized experiments that measure the impact of specific welfare-to-work efforts on the behavior of the experimental group (the group that receives the program) versus the control group (the group that does not receive the program). This evidence is voluminous, with two decades of experiments that analyze various welfare-to-work efforts. Almost unanimously, these studies indicate a significant positive effect of welfare-to-work efforts on labor market participation, although the size of that impact varies across studies and programs (Gueron and Pauly, 1991; Bloom and Michalopoulos, 2001). I summarize some more recent contributions to this experimental literature in more detail in Section VIII below.

How Did the Labor Market Respond? The large influx of less-skilled women into the labor market constitutes a substantial increase in labor supply. A best estimate is that welfare reform will increase labor supply among less-skilled women by a little over 1 million workers between 1996 and 2002 (Bartik, 2000, Table 2.1). This is a labor supply shock equal to 3.1 percent of employment among all women with less than a college degree.

An outward shift in the labor supply curve of one group of less-skilled workers would be expected to lower wages for all less-skilled workers, if the new group is a close substitute for existing workers. An observer might be particularly worried about how these labor force participation increases have affected other previously-working less-skilled women. Although the less-skilled male labor force tends to work in a somewhat different set of occupations and industries (Blank and Schmidt, 2001), there is enough overlap in jobs that this group should also be affected.[58]

The raw data suggests that any supply shifts (which would have lowered wages) were swamped by the overall demand increase due to a booming economy. Wages among less skilled women rose throughout the 1990s and unemployment rates fell. In fact, female unemployment rates were at their lowest point in several decades, while wages among less skilled women were at their highest point in several decades by the end of the 1990s. As a result, there was little public discussion of displacement issues. Of course, it is always possible that wages would have risen even faster in the absence of this supply shift.

[58] Black, McKinnish, and Sanders (forthcoming) show that shocks to wages of low-skilled men affect levels of welfare receipt.

There is evidence that the rapid labor supply increase among single moms might have affected other groups. As Figure 4 indicates, labor force participation increases among married women with children slowed noticeably in the latter part of the 1990s, at just the time when work among single women with children expanded. Even more striking, among less-skilled men labor force participation over the 1990s continued to fall (Holzer and Offner, 2001). This surprised labor economists, who would have predicted that the strong economy of the 1990s would pull more of these men into the labor force.[59] On the other hand, it is hard to believe that men (or women) were deterred from entering the labor market by increased employment among ex-welfare recipients when overall unemployment rates were hovering around 4 percent throughout the country and employers complained constantly about a lack of workers.

Several papers have tried to document who hired ex-welfare recipients. Holzer and Stoll (2001) analyze survey data from employers in four major cities in 1998 and 1999 and find that three percent of all job openings went to ex-welfare recipients. Lane and Stevens (2001) use administrative data from Maryland to investigate whether certain employer characteristics are more likely to result in long-term employment by newly-hired welfare recipients. They find that welfare leavers appear to have significantly different experiences in different industries, among smaller versus larger employers, and among shrinking versus growing emplowers.

Bartik (2000, 2001) takes on the displacement issue. He indicates that welfare reform is unlikely to have large effects on the overall labor market, since the labor supply shock is too small. He estimates the effect on wages and employment among less-skilled

[59] For instance, Freeman and Rodgers (1999) indicate that falling unemployment strongly increased labor force participation among young men in metropolitan areas in the mid-1990s, although they could find no

labor market groups using a variety of models and indicates that different estimation approaches can result in quite different numbers. For instance, estimates of the elasticity of labor demand to changes in labor supply vary widely across models. Bartik's preferred estimates suggest that welfare reform should reduce wages among female high school dropouts by between 5 and 15 percent.[60] Wage effects among other groups are lower. He forecasts that the effects of welfare reform on wages and employment among less skilled women will be larger and more visible in the near future.

It is difficult to untangle the effect of changing labor supply when labor demand is also changing rapidly. Bartik's estimates suggest that the answer depends very much upon the model specified. This is a ripe area for further investigation, including a closer look at why labor supply among less-skilled men did not grow over the 1990s, better estimates of the labor demand elasticities for less-skilled workers, and attention to the effects of any future economic slow-down on (more recently hired) ex-welfare recipients versus other labor market participants.

VII. Income, Poverty and Well-Being

Many would claim that the overall effects of welfare reform should be evaluated in terms of their net income-increasing or poverty-reducing impacts.[61] As we will see, this is a much less-well understood area of analysis than the analysis of caseload change or labor force participation.

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effects for adult men.

[60] Others who have simulated these effects with simpler models include Bernstein (1997) and Solow (1998).

[61] The 1996 legislation itself made no mention of poverty reduction.

In part, this reflects the fundamental problem of appropriately measuring economic well-being. Cash income is not an adequate concept if many low-income families receive in-kind benefits. There are serious problems with underreported income among the low-income population (Edin and Lein, 1997). After-tax income may be a better measure than before-tax income, particularly in a period when tax rules (especially the EITC) for low-income families are changing.[62] One might want to measure disposable income and include an accounting for unavoidable transportation or child care expenses associated with entering the work force. Even better, one might want to look at consumption rather than income data. Unfortunately, data are not readily available that provide reliable and consistent information on all of these measures. Even if one focuses only on changes in income-based poverty or poverty gaps (the two poverty-based measures that are most often used) there are difficulties in measurement and interpretation, as discussed in Section III above.

Finally, it is worth noting that many other measures of well-being than income, poverty, or even consumption might seem useful. One might want to know about quality of housing, food intake and nutrition, crime victimization, mental health, health insurance access, or access to good public education. Welfare reforms may change child well-being by affecting parental oversight as well as family income.[63] Consistent data on these measures are even less available than are credible income measures.

In this section I summarize what is known about the interaction between welfare reform and changes in income, poverty, and other measures of well-being. The existing

[62] The Bureau of the Census makes available special Current Population Survey tapes, based on the March supplement (which includes income and income components for the previous year), that include imputed in-kind income and taxes.

information described in this section is quite limited. There is a great need for more research, better data and more comprehensive measurement of well-being changes and their relationship to the 1996 legislative reforms.

A major premise of the 1996 reform was the claim that work would make people better off. Those who testified in favor of this legislation presented charts at Congressional hearings showing that women who worked would be economically better off than women who stayed on welfare, particularly with the minimum wage and EITC changes enacted by earlier Congresses (Haskins, 2001). Indeed, a series of simulation exercises done in the late 1990s showed unambiguously that steady work, even at a relatively low wage, when combined with the EITC, with child care assistance, with access to Medicaid, with Food Stamp and Child Support assistance, would leave a woman substantially better off (Acs, et. al., 1998). Critics claimed that these simulations were unreliable since many women did not have access to all of these work supports and many women were not able to find or maintain steady employment. The evidence on income and poverty (Tables 3 through 5) suggest that most single mothers had higher incomes by the end of the 1990s, despite a loss in government assistance.

Leavers' Studies. Few of the studies of ex-welfare recipients contain any information on overall economic well-being of leavers; most focus only on welfare use and labor force participation. Haskins (2001) indicates that income among welfare leavers in these studies must be around $11,000, based on earnings and work information. Among those studies that calculate poverty rates, most find quite high poverty among leavers, although the numbers vary widely. For instance, Loprest (2001) uses a national

[63] Grogger, Karoly and Klerman (2002) summarize various studies (mostly state-specific) that present survey data measuring material well-being among children.

survey of leavers to suggest that 48 percent remain poor. Danziger, et. al. (2001) report about 50 percent of welfare leavers were poor (based on annual incomes two years after welfare receipt.) Moffitt and Roff (2000) look at data in three cities and estimate that 74 percent of leavers are poor. Cancian, et. al. (1999) uses data from the NLSY to indicate that 55 percent of women are poor in the year following an exit from welfare, but this falls to 42 percent five years later. Bavier (2002) looks at monthly panel data on welfare leavers from the SIPP and concludes that income rises over time among leavers, in part because earnings of other family members rise.

Two studies provide some comparisons over time. Loprest (2001) finds evidence of slight improvements in the percent working and the monthly earnings of women who left welfare between 1997-99 versus leavers from 1995-97. This should mean that later leavers had higher incomes, and indeed poverty rates are lower in this group. Cancian, et. al. (2002), in a study based on Wisconsin data alone, find evidence of lower earnings among welfare leavers in 1997 versus 1995. Neither of these studies, however, can control for changes in the composition or selectivity of welfare leavers over time.[64]

Regression Analysis of Poverty and Income Changes. None of the evidence cited above attempts to separately identify the causal factors behind income and poverty changes over the 1990s. Three studies listed in Table 6 have looked at these variables, using the now-familiar state panel data methodology to estimate the determinants of changes in family income and poverty probabilities.

[64] I cite no experimental evidence in this section. Most randomized studies of welfare-to-work changes looked only at earnings and have no information on overall income changes. A few more recent studies of waivers involving multiple program changes are exceptions to this. Consistent with leaver study results, these tend to show little change in income (Bloom and Michalopoulos, 2001).

Schoeni and Blank (2000) find that the waivers enacted in the early 1990s appeared to increase income among low-income women. Moffitt (1999) finds an effect of waivers on earnings, but their estimated effect on total income is small and poorly determined.[65] Grogger (forthcoming) finds that waivers and TANF reforms increased both earnings and income. Schoeni and Blank investigate the impact of waivers and TANF on poverty rates as well as income. They find that the implementation of waivers is associated with a 2.4 point decline in the poverty rate among less-skilled women, holding the state economic climate constant. The implementation of TANF is associated with a 2.0-2.2 point decline in the poverty rate among less-skilled women. This evidence suggests that the welfare reform policies had income-improving effects for disadvantaged women.[66] Gunderson and Ziliak (2001) investigate the effects of welfare reform on poverty rates and poverty gaps among different subgroups among the poor and also find significant effects.

These results, based on national samples of all low income women, are stronger than those from studies that look only at welfare leavers. This suggests that income increases among those who don't enter welfare (that is, the non-entrants who would have gone on welfare in the pre-reform period but choose to remain off welfare post-reform) are large. This is precisely what one might expect if non-entrants were self-selected to be more employable. It is also worth noting that leaver studies typically do not ask about income or earnings among other family members, which several studies find to be a major reason why total incomes among single mothers are rising.

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[65] Moffitt looks only at single mothers, while Schoeni and Blank look at all women.

[66] Schoeni and Blank try to investigate the effect of these policies across the income distribution and find that waivers appear to have benefited all women with less than a high school degree. In contrast, TANF

Consistent with these results are estimates by Meyer and Sullivan (2001) who focus on consumption rather than income among single mothers. They find that total consumption of single mothers increases in the mid-1990s, both absolutely and relative to income among women without children and married mothers. (There are a few subgroups for which this is not true.) They interpret this as evidence that welfare reform did not harm women's well-being.[67]

The well-being of two specific groups deserves mention. Legal immigrants entering the country after August 1996 were made ineligible for virtually all forms of federal public assistance (TANF, Food Stamps, Medicaid, or SSI).[68] The impact of this provision is likely to grow over time as a growing share of new immigrants are covered by it. There has been remarkably little literature studying the impact of these provisions on the behavior and well-being of the immigrant population. Use of public assistance among immigrants has fallen much more rapidly than among non-immigrants. For instance, between 1994 and 1998, the share of immigrants receiving AFDC/TANF or Food Stamps fell almost in half (Borjas, 2001a, Table 14-2). Food Stamp receipt among eligible citizen children living with immigrant parents fell from 80 percent in 1994 to 46 percent in 1999.[69] Both Lofstrom and Bean (2001) and Haider, et. al. (2001) claim that the faster declines in immigrant welfare use are largely due to different local labor markets in high-immigrant versus low-immigrant regions. Kaestner and Kaushal (2001) suggest that welfare changes have increased employment among more recently-arrived

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appears to have benefited only female high school dropouts above the 20th percentile of the income distribution of all female high school dropouts.

[67] Another closely-related topic is the effect of welfare reform on household savings. Hurst and Ziliak (2001) provide an analysis of this issue.

[68] At their option, states were allowed to run state-funded programs serving immigrants and a number of states have taken this option (Borjas, 2001b). California, with the largest immigrant population, is particularly generous.

immigrants. Further studies looking at the connections between these policy changes and the changing economic and employment status of immigrants would be very useful.

A second group of concern is children. Duncan and Chase-Lansdale (2001a, 2001b) provide the best available discussion of how child well-being might interact with welfare reform. Below, I discuss evidence on the impact of financial incentive welfare-to-work programs on child well-being. Evidence is limited that relates welfare reform more broadly with child outcomes. Bennett, Lu and Song (2001) argue that welfare reform has led to an income decline among families where the parent has less than a high school degree. Paxson and Waldfogel (forthcoming) indicate that higher rates of out-of-home placement appear linked with some aspects of welfare reform. Haider, Jacknowitz, and Schoeni (2001b) show that breastfeeding rates appear lower due to greater work behavior. This is another area where further research would be highly useful.

VIII. Experimental Evaluations of Specific Policy Choices

While experimental evaluations of welfare reform programs have limitations, as discussed above, they provide some of the most credible evidence about the effects of specific reforms. I focus here on a few topics which have been evaluated by random assignment methodology and that particularly add to our theoretical and empirical knowledge of the effects of redesigned welfare programs. This section reviews the evidence on three specific policy choices: Mandatory employment programs, earnings disregards and financial incentive programs, and time limits.

Mandatory Employment Programs. Since the mid-1980s, states have experimented with a wide variety of welfare-to-work employment programs. Most of

[69] Underlying data provided by USDA for tables published in USDA (2000).

these have been mandatory, meaning that welfare recipients who are ruled to be "work eligible" are required to participate if they want to continue receiving welfare benefits. Summaries of evaluations of earlier welfare-to-work efforts, both mandatory and voluntary, can be found in Gueron and Pauly (1991) and Friedlander and Burtless (1995).

States that received waivers from AFDC program rules to run strong mandatory welfare-to-work programs in the early 1990s were also required to fund a serious evaluation of their new program, typically a random assignment evaluation. As a result of this, a wide variety of mandatory welfare-to-work programs were evaluated in the early 1990s. The results from these programs are nicely summarized in Bloom and Michalopoulos (2001) and in Michalopoulos and Schwartz (2001), both of which review randomized evaluations of 20 mandatory welfare-to-work programs in specific states.[70]

Virtually all of these programs produced some significant increases in employment and reductions in welfare usage and payments. Earnings per year increased between $200 and $600 in most programs.[71] Michalopoulos and Schwartz (2001) break these results down for different groups of participants. They indicate little difference in the employment gains experienced by the most versus the least disadvantaged participants in these programs (which means that the ongoing earnings and employment differentials between these two groups remained about the same after recipients participated in mandatory welfare-to-work programs, although both groups worked more and earned more). It is particularly interesting that employment outcomes did not seem significantly worse among less skilled participants, or participants with identifiable

[70] See also Hamilton, et. al. (2001) which summarizes information on program effectiveness in a five year follow-up of 11 welfare-to-work programs.
[71] The largest earnings increase occurred in the Riverside County GAIN program, where annual earnings gains exceeded $1000/year.

barriers to work, such as child care problems. The one group that appears not to benefit from these programs are recipients at a high risk of depression.

A key comparison in these studies is between those programs that focused on job search or labor force attachment (LFA) through some sort of work-first program that pushed recipients into jobs as rapidly as possible, and those programs that focused on human capital development (HCD) and provided more training and educational opportunities to recipients. Three programs included side-by-side evaluations of LFA versus HCD welfare-to-work programs within the same location (Atlanta, GA, Grand Rapids, MI, and Riverside County, CA). Other programs had somewhat mixed models, including work-first efforts for some recipients and education and training for others.

While labor economists are particularly likely to predict that education and training will make people better off in the long run, the existing evaluations challenge that assessment. Work-first and LFA programs increased earnings and decreased welfare usage more quickly, while HCD programs cost more, particularly in the first year when women were in training rather than working. But even three years out, after women from HCD programs had been in the labor market for up to two years, HCD participants did not outperform LFA participants. This may suggest that the gains to experience among women who have been out of the labor market may be larger than the gains to education and training, at least initially. While the HCD programs significantly increased the number of participants holding a GED degree, there was little evidence that this resulted in higher earnings or more work hours (Freedman, et. al., 2000).

These evaluations had three year follow-up surveys, at the most. Using data from the California GAIN program, Hotz, Imbens, and Klerman (2000) match control and

treatment group members from earlier welfare-to-work programs in California with earnings records, allowing them to follow welfare-to-work participants (and their control groups) for up to nine years afterwards. They find that in years 7-9, those who received more education or training are doing as well or better than those who were put into work-first programs. Their conclusion is that the HCD programs look better with longer-term evaluations. This study uses data from one state only, and verifying these results for other programs would be useful.

Interestingly, the best results from these studies occur in programs with mixed activities, suggesting that a combination of work-first for some women and education for others might be optimal. For instance, the Portland, OR, program and the GAIN program in Butte, CA, for single women showed $1200/year earnings increases, while GAIN Riverside (CA) showed $1400/year increases (Bloom and Michalopoulos, 2001). Further experimentation with mixed activity programs would be useful, particularly evaluations of retention-based programs that provide education, training, or job retention services to women after they have worked for a period. Anecdotal evidence suggests that many women who participate in work-first programs appear to recognize the need for further training at a later date. Some women lose their jobs and have difficulty finding another job. Second-chance programs would aim at assisting job retention or providing training after a woman has acquired some labor market experience. No programs of this sort for women moving from welfare to work have been evaluated with randomized experimental evaluations.

A disappointing aspect of the mandatory work programs is that they provide little evidence of increased income. In fact, increases in earnings appear to be entirely offset

by losses in public assistance income.[72] Hence, these programs have no anti-poverty effects. This has led to a great deal of interest in the use of earnings disregards in conjunction with mandatory work, to create work incentives and subsidize earnings at the same time.

Earnings Disregards and Financial Incentive Programs. Several studies have tested the effects of strong earnings supplements as a way to incentivize work and reduce welfare usage. These so-called "financial incentive programs" include the Minnesota Family Investment Program (MFIP) (Miller, et. al., 2000, and Gennetian and Miller, 2000), the New York Child Assistance (CAP) program (DeMarco and Mills, 2000), the New Hope program in Milwaukee (Bos, et. al., 1999), the Work Restructuring Project (WRP) in Vermont (Bloom, et. al., 1998), and the Self-Sufficiency Project (SSP) in Canada (Michalopoulos, et. al., 2000). Two of these programs – MFIP and SSP -- have explicitly tested the combined versus separate effects of earnings disregards and other employment service programs. Three other programs have been evaluated that included financial incentives along with other policies: the Connecticut Jobs First program (Bloom, et. al., 2000b), the Florida Family Transition Program (FTP) (Bloom, et. al., 2000a), and the Iowa Family Investment Program (FIP) (Fraker and Jacobson, 2000). In these three evaluations it is difficult to make statements about the effect of financial incentives only, but they do show the combined effects of financial incentives along with a mix of other mandatory and voluntarily-available job search services. Table 7 shows

[72] These results are typically based on administrative data on earnings and benefits. Cancian, et. al. (1999) indicates that this is likely to undercount true family income since it does not include income from partners or other family members.

the effects of these programs on a set of key variables, as measured by random assignment evaluations.[73]

Because of their strong evaluation results, both MFIP and SSP have garnered a great deal of interest and I describe them briefly to the reader. MFIP was implemented in Minnesota in 1994, and provided a strong earnings disregard that allowed women to receive some cash assistance until their earnings were about 140 percent of the poverty line.[74] Participants were also required to participate in mandatory job search programs. A subset of the treatment group was provided with the financial incentives from earnings disregards, but not subject to mandatory job search requirements. A control group continued to receive AFDC. Participants were randomized into both treatment groups and the control group, so that the separate effects of mandatory job search and earnings disregards could be explored.

The SSP program was implemented in 1992 in two provinces in Canada (New Brunswick and British Columbia).[75] It offered a randomly chosen group of women on welfare (known as Income Assistance in Canada) an alternative: If they agreed to leave welfare, they could receive an earnings supplement in every month when they averaged 30 hours of work per week or more. The supplement was large and roughly doubled the earnings of most participants. It was calculated as half the difference between a participant's earnings and an "earnings benchmark", set at $30,000 in New Brunswick and $37,000 in British Columbia (Canadian dollars). The supplement was reduced by 50

[73] For a more thorough discussion of the results of financial incentive programs see Blank, Card, and Robbins (2000) or Michalopoulos and Berlin (2001).
[74] For more detailed description of the MFIP program, see Miller, et. al. (2000).
[75] For more information on the SSP project, see Michalopoulos, et. al. (2000) or Quets, et. al. (1999). Note that the SSP experiment offered women a choice, hence some of the treatment group chose to stay in Income Assistance. In contrast, those in MFIP's treatment group had to participate in the alternative program and were no longer eligible for AFDC.

cents for every dollar of earnings. Some of these women were also randomly assigned to an SSP-Plus program, which combined the earnings supplement with job-related services such as resume writing assistance, job search assistance, and self-esteem workshops. Participation in these services was not mandatory, so the evaluation compares the effect of earnings supplements with and without voluntary employment services.

The results of the earnings disregard experiments suggest that positive work incentives coming through larger earnings disregards can increase employment. The top part of Table 7 indicates that employment increased by 3.6 percent among those receiving MFIP financial incentives only, and by 3.3 percent in the New York CAP program. The relatively small financial incentives in the Vermont WRP program led to almost no employment effects.

As discussed above, mandatory employment programs alone appear to have little effect on income or poverty, but Table 7 indicates that the MFIP incentive-based program had significant positive effects on monthly income and significant negative effects on poverty (although WRP and CAP did not.) In the MFIP incentives program, this was entirely due to the earnings benefit supplements; earnings actually decreased slightly. This highlights the fact that earnings disregard programs cost more, since they pay benefits to supplement earnings of workers, phasing out more slowly than less-generous welfare programs. Hence, these programs tended not to reduce the cost or the usage of welfare assistance. They significantly increased the amount of assistance that went to *workers*, however, rather than non-workers.

Most promising of all were the programs that combined employment programs with earnings disregards, shown in the bottom half of Table 7. The SSP earnings

supplement required working 30 hours per week and provided the largest financial incentive to work among these programs. The result was a 7.2 percent employment gain among those offered the SPP program, significant income increases, and a greater than 9 percent decline in poverty rates.[76] The full MFIP program that included both generous disregards *and* mandatory employment programs had large positive effects on employment and income, and significant negative effects on poverty. This was due to both significant earnings increases combined with significant ongoing cash transfers to the workers.[77] The MFIP evaluation allows us to measure the separate impact of disregards versus mandatory employment; the results indicate that the significant employment effects depended upon the mandatory employment program, while the anti-poverty effects depended upon the high earnings disregard.

Other programs that combined both financial incentives and strong work requirements show similar gains in both employment and in income. As Table 7 indicates, the Connecticut Jobs First program and the Vermont Welfare Restructuring Program resulted in strong earnings and employment gains, and (smaller) income gains. The Florida FTP program and the Iowa FIP programs have limited earnings disregards, but these are large enough that the loss of cash transfers is less than the earnings gains, hence women in these programs also increased employment and increased earnings. Again, these results are a sharp contrast to the mandatory employment programs alone, where earnings gains are typically entirely offset by cash transfer losses.

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[76] Card, Michalopoulos, and Robins (2001) also describe earnings gains among SSP recipients of around 2.4 to 3 percent per year over the period of the experiment. It is also worth noting that a substantial number of persons who were offered the SSP supplement did not take it up. Most of these non-participants said that they would have liked to receive the supplement but could not find full-time work or could not overcome various barriers to work.

[77] The 30 hour work requirement in SPP resulted in a substantial rise in full-time work. In contrast, MFIP had more of an effect on part-time work.

The evidence in Table 7 is striking and worth emphasis. In traditional welfare programs (see Figure 2), maximum benefits are paid to non-workers. Hence, traditional welfare programs typically increase income but reduce labor supply at the same time, creating inefficiencies. Burtless (1986) notes that in the Negative Income Tax experiments, the government had to spend almost $2 to increase incomes by $1, largely because of labor supply reductions. In contrast, financial incentives programs (especially those with mandatory employment programs) provided strong work incentives at the same time they supplemented incomes. In these programs, *both* labor supply *and* income increased.

Part I of Table 8 presents calculations of income gains per dollar spent for some of the financial incentive programs that included both incentives and work requirements. Column 1 shows income gains to program participants and column 2 shows transfer payments. Column 3 indicates the income gains per dollar transferred. An alternative calculation occurs in column 6 which shows total program benefits (column 4, including such things as the imputed value of additional health care) per dollar of total program costs (column 5, transfers plus the provision of other services and administration). In looking at Table 8, one might be tempted to think that programs with a benefit/cost ratio of less than one should not be run. I caution against this interpretation. In many cases programs with lower benefit/cost ratios may offer important and necessary services. Programs with higher benefit/cost ratios are relatively more appealing, but the data in Table 8 do not allow one to drawing any absolute conclusions about the value of these programs.

In the SSP program, $1 in government transfers increased income by almost $2 among long-term welfare recipients since the earnings supplements were reinforced by increased labor supply. In MFIP (among long-term recipients) the gain is even larger and $1 in government transfers increased income by almost $2.50; New Hope also shows substantial gains. Only two-parent families in MFIP show relatively small gains for each dollar transferred. This suggests that these financial incentive programs are relatively efficient in redistributing income. Even in column 6, where the calculation is based on total program costs, the ratios are better than the 0.50 number cited by Burtless for the negative income tax (again the exception is among two-parent families in MFIP.)[78]

In contrast, the mandatory employment programs summarized in part II of Table 8 show much smaller benefit gains in absolute terms (column 4) and generally lower income gains per dollar of government spending (column 6). In some cases, there are actual income losses among recipients, resulting in negative ratios. As redistributive programs, these are simply less effective. These mandatory employment programs were generally not focused on income gains, so it may be unfair to judge them on this basis. In fact, a primary goal of most of these programs was to reduce government spending, and many of them were quite effective at that. Some of these programs, such as Riverside GAIN, even saved the government substantial amounts of money per recipient; while income gains were small in Riverside, the government savings suggests a very efficient transfer program.

In addition to their income-enhancing effects for the family and their employment-enhancing effects for the parent, financial incentive programs also appeared

[78] The calculations in columns 4, 5, and 6 require more imputations and assumptions and hence may be regarded as slightly less reliable than those in columns 1, 2, and 3.

to have positive effects on children's outcomes. A number of these studies have collected data on child outcomes as well as on employment and income. Morris, et. al. (2001) summarize this research and indicate that earnings supplement programs (alone or combined with mandatory employment programs) appear to have significant and positive effects on school achievement and child behaviors among elementary-school-age children.[79] Mandatory employment programs alone do not appear to produce these gains. These effects are strongest among the children of long-term welfare recipients. Duncan and Chase-Lansdale (2001a) suggest that these changes appear to be operating through changes in the utilization of child care or after-school programs, rather than through changes in home environments.

Adolescent effects are based on smaller samples and are less positive. Two studies find reductions in achievement and increases in behavioral problems among teens whose mothers participate in work incentive programs.

The evidence that financial incentive programs can reduce poverty, increase earnings, and may even improve child outcomes at the same time, particularly when combined with other job-related services, suggest that these financial incentive programs are the most promising avenue of policy change to come out of the U.S. (and Canadian) welfare reforms of the mid-1990s. Several caveats about these results should be noted, however. First, these financial incentive programs have not been run in a random set of states. The strongest results come from Minnesota and Canada, and might not be generalizable to other locations. Second, the wide variance in results shown in Table 8 suggests that financial incentive programs are more effective with some populations.

[79] Samples in these studies are too small to produce reliable conclusions regarding the effects on children under age 3. For an earlier summary of the literature see Hamilton, et. al. (2000).

Time Limits. The effects of time limits have been studied both by randomized experiments and through larger econometric studies of national data. It is fair to say, however, that we lack a great deal of information about the effect of time limits, largely because not enough time has passed to evaluate them. By mid-2000, only about 60,000 families had hit time limits of one sort or another. Only in late 2001 and early 2002 will the first group of women who were continuously on welfare since TANF was enacted begin to hit the 60-month time limit set in many states. As more and more families hit the five-year time limits over the near future, there will be more opportunities to study the effects of these time limits.

Six state programs that implemented time limits earlier (through state waiver programs) have been studied through random assignment experiments (Arizona, Connecticut, Delaware, Florida, Indiana and Virginia). In none of these states do time limits show large effects on employment, though there is some evidence that women are leaving welfare early in order to preserve welfare eligibility in the future.[80]

These results are somewhat difficult to interpret, however. These experiments were conducted on the very first group of women anywhere in the United States to hit time limits. This group may be more prone to misunderstand the implications of time limits or to disbelieve that they would actually be enforced. Furthermore, all of these experiments are studying of a larger set of changes, which include time limits as one component. In none of these studies can the independent effects of time limits be deduced. Conclusions about the effect of time limits on employment are largely based on comparisons between the results in these states to experimental results in other states

without time limits. In addition, none of these evaluations can investigate the entry effects of time limits, that is, the extent to which they discourage families from receiving welfare in the first place.

The effects of time limits on families are an issue of great interest and scrutiny. Some evidence indicates that a high share of clients who are subject to future time limits do not entirely understand this provision. The evidence from Florida suggests that the most disadvantaged were the least affected by Florida's time-limited program; it had few effects on their employment, earnings or income (Bloom, et. al., 2000a). Those who hit the time limit in Florida were clearly struggling financially, but were not notably worse off than many other families who left welfare for other reasons. Time limits in Florida did appear to reduce welfare use the most among families with younger children (Grogger and Michalopoulos, 1999). Those who are "sanctioned off" welfare may be similar to those who hit time limits. Leavers' studies indicate that those who leave welfare due to sanctions are less likely to be employed after leaving welfare and are also more disadvantaged than other leavers across a range of attributes (U.S. General Accounting Office, 2000). Lack of long follow-up periods in many of these studies makes it difficult to evaluate the long-term effects of time limits or sanctions.

IX. Marriage and Fertility Changes

A major impetus for welfare reform within the U.S. has been ongoing concern about rises in out-of-wedlock births and declining marriage rates, especially among low

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[80] See Bloom (1999), Bloom and Michalopoulos (2001) and Pavetti and Bloom (2001) for summaries of CN, DE, FL and VA studies and references to the original experimental results. The AZ and IN experiments are described in Fein, et. al. (1998) and Kornfeld, et. al. (1999), respectively.

income women and men. Indeed, the major stated goals of the 1996 legislation included reducing out-of-wedlock births and increasing marriage.

There are several mechanisms by which welfare reform might accomplish this. A simple economic model would suggest that means-tested transfer programs make it easier for lower-income couples to afford a child. Furthermore, the fact that most of this support has historically been focused on single mothers and not married couples means that the incentives to marry are limited. While changes in cash assistance programs made assistance to two-parent couples more available in the early 1990s[81], growth in the EITC increased the marriage penalty for low-income workers (Ellwood, 2000).

In addition to standard economic incentive-based models, there are also a host of more cultural/behavioral models that claim the growth in out-of-wedlock childbearing is due to decreased social disapproval of out-of-wedlock childbearing and a shift in behavioral norms away from marriage. Increases in means-tested support would make it easier for such shifts in marital and fertility norms to occur.[82]

Both of these theoretical approaches imply that recent U.S. changes in public assistance should have reduced the incentives to become a single mother and increased the incentives to marry. Time limits, sanctions, diversion activities, and work incentives all make it harder to receive public assistance as a single mother without also engaging in work-related activities.

Some states have gone beyond these welfare reforms to enact programs that are explicitly directed at fertility and marriage decision-making. For instance, some state

[81] The Family Support Act of 1988 required all states to make two-parent married couple families eligible for AFDC (although in most cases these families faced more stringent eligibility requirements).

waivers in the early 1990s allowed states to mandate that teen mothers must stay in school and live at home with their parents (or in another supervised setting) if they are to receive cash assistance, the so-called "minor parent provision." Other states were given authority to enact "family caps", which refuse benefit increases to welfare recipients who have further children outside of marriage. After states received greater discretion under TANF, more states implemented these programs.[83]

Trends in the data. Is there evidence of changes in fertility or marital behavior in the mid-1990s, similar to the labor force participation changes discussed above? Figure 5 shows the trends in three key variables.[84] Marriage rates have fallen steadily for many decades, with no noticeable break in trend over the 1990s. Similarly, divorce rates are also slowly falling, with no change in the 1990s. Birth rates to unmarried women did change noticeably around 1990, ending a steep increase to flatten out and even fall slightly over the 1990s. This change in the nonmarital birth rate is evident among both black and white women, and among teens and older women, but occurs well before major welfare reforms were implemented.

Further evidence on trends in key family structural variables can be seen by looking at the share of families with children that are headed by never-married females.[85] This number has increased steadily from 3 percent in 1976 to over 10 percent in 2000, the highest level ever recorded. When one looks at the trends in headship among never-married mothers who have lower incomes or are less educated, however, there is a bit more evidence that the increases of the past two decades slowed somewhat in the mid-to-

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[82] For instance, Murray (1994) argues that the growth in means-tested benefits in the late 1960s/early 1970s led to long-term increases in out-of-wedlock childbearing. For a discussion of these behavioral arguments, see Murray (2001).

[83] Maynard, et. al. (1998) discuss these state policies in more detail.

late 1990s. Other recent evidence suggests that the share of children living with single mothers (particularly in African American families) declined significantly in the late 1990s (Dupree and Primus, 2001). This is at least consistent with the theory that welfare reform might be producing some behavioral changes among less skilled women.

It might be unreasonable to expect any marriage or fertility effects from welfare reform to show up within a few years. Only a relatively small share of the population gets pregnant or becomes married in any given year; changes in these decisions will affect aggregate numbers only slowly over time. Furthermore, marriage and fertility patterns may be much more sluggish and resistant to change than is work behavior. All of this suggests that identifying effects on family structure due to welfare reform is likely to be difficult, and any measured effects are likely to be small.

Causal analysis of these changes. The literature that relates the policy changes of the 1990s to changes in marital or fertility behavior is still quite limited and much good research remains to be done. A large body of earlier work has analyzed the relationship between AFDC benefits and fertility behavior. I do not review this literature here, in part because it has been well reviewed elsewhere (Moffitt, 1992; Moffitt, 1998b). The primary conclusion is that studies tend to show either no effects or small effects. To cite Moffitt (1998a, p5) in the introduction to a book analyzing the relationship between welfare and fertility behavior, "[I]t is also fair to note that if there were a sizable effect of welfare on demographic behavior, it would probably be more evident with the available statistical methods than appears to be the case in the research literature. The findings

[84] For a discussion of related trends see Murray (2001) or Bachrach (1998).
[85] Author's tabulations on the March Current Population Survey data from 1976 to 2000.

reported in the chapters are…consistent with the existence of a small, real effect but one that is difficult to detect and sensitive to the methodology used[.]"

Evidence of small effects in the existing literature do not imply that recent welfare reform effects must also be small, for at least two reasons. First, this previous literature focuses on the effect of differences in welfare benefit levels on fertility patterns. As discussed above, welfare benefit levels have not changed radically; rather, a host of other behavioral incentives and mandates have been imposed on welfare recipients. These might have different and stronger effects, particularly if these changes seriously limit welfare benefit availability. Second, programs like family caps and minor parent provisions are directly aimed at changing fertility behavior and might have larger and more direct effects than changes in benefits or availability.[86]

Table 9 summarizes the literature that attempts to link welfare reform with changes in family structure. Part A lists the econometric studies. Horvath-Rose and Peters (2001) use state panel data to investigate out-of-wedlock birth trends from 1984 to 1996, including controls for the implementation of family caps or minor parent provisions. They find a significant and negative effect of family caps, similar to the New Jersey study. The minor parent provision appears to have positive effects on nonmarital fertility in their study, however, which is not the expected sign and suggests that there may be omitted variable problems with the entire study. Clearly, the mixed evidence on family caps suggests that further investigation might be useful. Schoeni and Blank (2000) use panel data to investigate the effects of waivers and of TANF implementation on the percent married and the probability of being a female household head. They find

evidence that the policy reforms had small but significant effects on these family formation variables. Fitzgerald and Ribar (2001) use data from the early 1990s and find state waivers had mixed results on female headship.

Part B of Table 9 summarizes econometric studies that have tried to investigate the expected effect of family caps using data from the AFDC program. Fairlee and London (1997) look at whether differences in AFDC benefit levels change the likelihood that single mothers would have additional births. They find little evidence of this. Acs (1996) and Grogger and Bronars (2001) have similar results. Argys, Averett and Rees (2000) investigate how AFDC benefits might affect pregnancy and abortion behavior. They find small effects that are highly sensitive to specification and methodology.

Experimental evaluations of the effects of employment programs on marriage and fertility behavior (summarized in Part C of Table 9) show quite mixed results, from what are admittedly quite a different group of programs.[87] Three studies directly analyze actual family cap programs, each using quite different methodologies. Camasso, et. al. (1998a, 1998b) report on a pre-post data comparison and an experimental comparison in New Jersey after family caps were implemented and indicate that the birth rate was significantly lower among those subject to family caps. Some have raised concerns about the interpretability of these evaluations, however (Maynard, et. al., 1998). The A Better Chance (ABC) program in Delaware ran a strong mandatory work activities program and included a family cap. A randomized study of the program showed few effects on marriage or fertility (Fein, 1999).

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[86] This might be especially true if one takes the behavioral norms model seriously. The implementation of family caps in most states occurred simultaneously with a lot of media attention to the fact that the state wanted to actively discourage additional out-of-wedlock births.

The New Chance experiment, which provided educational and job assistance to teen welfare mothers (along with family planning counseling) found pregnancy rates in the experimental group actually increased in a randomized evaluation (Quint, Bos, and Polit, 1997). The Teen Parent Demonstration (TPD) Project in Ohio, which required teen welfare mothers to participate in a rich set of education and work support programs, had no effect on second pregnancies (Kisker, Rangarajan and Boller, 1998). The California Work Pays Demonstration (Hu, 2000) was a work-oriented welfare reform that increased marriage rates in some populations.

The largest and most positive effect of welfare reform on family structure is found in the MFIP demonstration, discussed above as a particularly effective financial incentive program (Miller, et. al., 2000). Single mothers who were in the MFIP experimental group married at a significantly higher rate than those in the control group, while two-parent families in MFIP stayed married at a higher rate.[88]

Overall, the recent literature on the effects of policy on family structure has not provided clear guidance as to what states should do if they want to influence fertility and marriage through their welfare reform efforts. While the MFIP results are cited as evidence that welfare reform can influence marriage, it would be useful to have additional experimental evidence that showed similarly strong and positive results on marriage or fertility from other state reform efforts. Even some programs that explicitly focused on fertility issues as one component of welfare reform (such as New Chance or the TDP program) did not show the desired effects. In addition, further econometric

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[87] Murray (2001) also reviews the studies cited here, as well as several others that are not based on recent welfare reform efforts.

exploration of the determinants of marriage and fertility trends, particularly focusing on the changes in the 1990s, would be highly interesting.

X. Conclusion

The U.S. welfare reforms of the 1990s have generated extensive interest. Both the federal changes in work support programs like the EITC and the revolution in the design of state public assistance programs have drawn research attention. While it is far too early to draw any final conclusions about the long-term effects of these program changes, the research literature to date has produced several important results.

More significant caseload declines and larger increases in labor force participation among less-skilled mothers were possible than many observers would have predicted. Entry into welfare fell and exits from welfare rose. There remains debate as to how much these results were due to a strong economy, to program reform, or to their interactive effects. While some of this change in behavior is due to traditional labor supply responses to growing wages and increased financial incentives to work, the changes were greater than historical experience would lead one to expect. State welfare programs were substantially different post-1996, including such elements as time limits, sanctions, and diversion efforts. Tracking down the exact relationship between these program differences and specific behavioral changes remains difficult, but there is a growing body of evidence indicating that these new program elements mattered.

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[88] Another financial incentive program discussed above, SSP (implemented in Canada), had more mixed results on marriage. Positive marriage effects occurred in one of the two sites, while the other site showed negative marriage effects. Harknett and Gennetian (2001) discuss possible reasons for this.

At least over the late 1990s, these changes in behavior occurred along with moderate increases in cash income and moderate declines in poverty among less-skilled single mother families, those most affected by the policy changes. This is in contrast to the mandatory employment programs of an earlier era, which increased labor supply but seemed to have few positive effects on income (earnings gains offset benefits losses). These positive outcomes were best demonstrated in a set of experimental evaluations of so-called "financial incentive programs" which both provided financial incentives to work while also mandating strong work efforts. These programs, enacted in only a limited number of states, seem to have been particularly effective in increasing employment and reducing poverty, and they provide perhaps the best model of "new-style" welfare programs. Their results are markedly different from those of the older negative income tax experiments.

The literature evaluating these welfare reforms is likely to continue to grow. Let me mention three key areas where future research may be particularly important. First, some important questions can only be answered after more time has passed. We have only very preliminary evidence on whether these reforms have had any long-term impacts on marriage or fertility behavior. Similarly, we are just at the beginning of observing the impact of actually imposing time limits on larger numbers of welfare recipients. Most important, perhaps, is the question of how much the remarkable U.S. economy in the late 1990s was fueling the declines in caseloads, and increases in work and income among low-wage single mothers. Only as we experience economic cycles will we be able to effectively separate the economic effects from the policy effects of welfare reform. Tracking the changes in a less-robust economy will be important, as will investigating

whether certain states have packages of programs that make their low-income citizens more or less vulnerable in an economic downturn.

Second, the well-being effects of these policy changes should be better understood. We need to know more about families' disposable income changes after leaving public assistance; about their long-term opportunities for wage and income growth as their labor market experience grows; and how families whose access to public assistance has become much more limited cope with the combined demands of work, parenting, and economic survival. For instance, there is a growing interest in how such things as chronic child health problems (such as asthma) or parental mental health (particularly problems of depression) might interact negatively with efforts to become economically self-sufficient.

Third, there is a need to develop adequate research experience in methodologies that these new research questions require. This means a better understanding of how to effectively utilize administrative data sources; a better way to identify and code specific policy components within welfare reform; a better way to interpret and generalize program impacts from an increasingly diverse set of state programs; and more credible ways to identify policy effects and analyze their impacts on long-term behavioral changes.

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Table 1

Maximum Benefit Levels Across States

(2000 Dollars)

Selected Points In Benefit Distribution	1990	1995	2000	Percent Change 1995-2000
20th Percentile State	$358 (NC)	$319 (IA)	$288 (IN)	-19.60%
Median State	$480 (NE)	$428 (IL)	$379 (DC)	-21.00%
80th Percentile State	$680 (MI)	$607 (MD)	$546 (WA)	-19.70%

Source: State Policy Documentation Policy (www.spdp.org) and The Urban Institute (www.urban.org/).

Note: Maximum benefit levels for family of three. 51 states (including D.C.) used in analysis.

Table 2

Cumulative Cash Welfare Available During First 24 Months of Work for a Welfare Recipient with 2 Children

	Jan 1996 AFDC Program ($2000) Hourly Wage = $6/hour		2000 TANF Program Hourly Wage = $6/hour	
State	30 hr workweek	40 hr workweek	30 hr workweek	40 hr workweek
Alabama	$0	$0	$492	$492
Alaska	$11887	$6175	$13478	$9794
Arizona	$0	$0	$0	$0
Arkansas	$0	$0	$0	$0
California	$4198	$788	$8724	$5700
Colorado	$131	$0	$3	$0
Connecticut	$7862	$2150	$13032	$13032
Delaware	$0	$0	$3096	$560
District of Columbia	$126	$0	$1296	$0
Florida	$0	$0	$672	$0
Georgia	$144	$0	$16	$0
Hawaii	$3733	$710	$7882	$4785
Idaho	$0	$0	$0	$0
Illinois	$0	$0	$3108	$1112
Indiana	$0	$0	$0	$0
Iowa	$152	$0	$3024	$605
Kansas	$165	$0	$168	$0
Kentucky	$582	$0	$757	$524
Louisiana	$0	$0	$1140	$1140
Maine	$698	$26	$5568	$2544
Maryland	$0	$0	$0	$0
Massachusetts	$750	$78	$6000	$2976
Michigan	$294	$0	$456	$0
Minnesota	$608	$0	$7776	$4026
Mississippi	$0	$0	$1020	$1020
Missouri	$0	$0	$1614	$616
Montana	$647	$0	$1332	$0
Nebraska	$0	$0	$0	$0
Nevada	$0	$0	$1044	$1044
New Hampshire	$685	$13	$5400	$2376

Table 2-continued

	Jan 1996 AFDC Program ($2000) Hourly Wage = $6/hour		2000 TANF Program Hourly Wage = $6/hour	
State	30 hr workweek	40 hr workweek	30 hr workweek	40 hr workweek
New Jersey	$225	$0	$1551	$424
New Mexico	$0	$0	$3336	$312
New York	$802	$130	$5294	$2028
North Carolina	$660	$0	$816	$816
North Dakota	$174	$0	$3399	$299
Ohio	$0	$0	$2952	$0
Oklahoma	$0	$0	$0	$0
Oregon	$298	$0	$0	$0
Pennsylvania	$131	$0	$672	$0
Rhode Island	$703	$31	$6336	$3312
South Carolina	$0	$0	$261	$82
South Dakota	$501	$0	$0	$0
Tennessee	$827	$155	$1848	$0
Texas	$0	$0	$536	$435
Utah	$763	$91	$3024	$0
Vermont	$2133	$444	$4128	$0
Virginia	$0	$0	$6984	$5924
Washington	$668	$0	$4104	$1080
West Virginia	$0	$0	$0	$0
Wisconsin	$544	$0	$0	$0
Wyoming	$892	$185	$0	$0
Median State	$152	$0	$1140	$299

Source: Author's calculations from program parameters found in the State Policy Documentation Project (www.spdp.org) and U.S. House of Representatives (1996).
Note: All numbers in 2000 dollars. Ignores any waivers that affected BRRs in 1995, and assumes all states are subject to the federally-mandated BRR.

Table 3

U.S. Poverty Rates

Percent Poor	1979	1989	1992	2000
All Families	9.2%	10.3%	11.9%	8.6%
Families with Single Female Householder	30.4%	32.2%	35.4%	24.7%
Black Families, Single Female Householder	49.4%	46.5%	50.2%	34.6%
Hispanic Families, Single Female Householder	49.2%	47.5%	49.3%	34.2%

Source: The Census Bureau (http://www.census.gov/)
Note: 1979, 1989 and 2000 represent the end of extended periods of economic growth. 1992 is the end of the recession of the early 1990s.

Table 4

Impact of the Safety Net on Poverty Gaps per Person (All persons in families with children, 1999 dollars)

	Year			
	1993	**1995**	**1997**	**1999**
Poverty Gap				
Based on:				
Pretransfer Income	$2,737	$2,562	$2,562	$2,370
Plus Social Insurance[a]	$2,559	$2,394	$2,338	$2,185
Plus Means-tested Benefits[b]	$1,488	$1,419	$1,529	$1,547
Plus Federal Taxes (including EITC)	$1,447	$1,386	$1,514	$1,524
Percent Reduction in Poverty Gap due to:				
Social Insurance	16.1	15.8	18.4	17.7
Means-Tested Benefits	43.8	44.7	37.2	34.0
Federal Taxes	1.1	4.4	5.7	6.8

a. Includes Social Security, disability, and worker's compensation.
b. Includes cash benefits, food stamps, housing subsidies and school lunch.

Source: Center on Budget and Policy Priorities (2001).

Table 5

Average Income of Female-Headed Families by Quintile
(1999 Dollars)

Average Disposable Income	1993	1995	1997	1999	% Change 93-95	% Change 97-99
Quintile 1	7,714	8,532	8,292	7,835	10.6	-5.5
Quintile 2	12,929	14,438	14,403	15,494	11.7	7.6
Quintile 3	16,216	18,971	18,850	19,984	17.0	6.0
Quintile 4	22,568	24,698	25,130	27,204	9.4	8.3
Quintile 5	42,718	47,057	50,801	59,858	10.2	17.8

Source: Tabulations by Center on Budget and Policy Priorities.

Table 6

Research on Welfare Reform Impacts

Part A: Econometric Research on Welfare Reform Impacts Using Data Prior to 1996 Welfare Reform

Study	Dependent Variable(s)	Key Independent Variables	Results on Key Variables
Bartik & Eberts (1999)	Log(AFDC caseloads per capita) Based on state administrative data, 1984-96	Multiple economic variables (including unemploymt rates, and local labor market demand information) Dummy variables for state waivers	• Local labor demand information is important in explaining caseload changes and including these variables reduces the unemployment coefficient. • 1% increase in employment growth leads to 4% decline in caseloads, similar to the effect of a 1% decline in unemployment. ▪ Gross job flows (high job turnover) is positively correlated with higher caseloads.
Includes state and year fixed effects. Some estimates include lagged dependent variable and first difference models.			
Blank (2001a)	Log(AFDC caseloads per capita) Based on state administrative data, 1977-96	Multiple economic variables (including unemploymt & wage information) Dummy variables for state waivers	• Share of caseload change due to economic factors: 29% in 1990-94 59% in 1994-96 • Share of caseload change due to waivers: -22% in 1990-94 28% in 1994-96 • 5% estimated change in AFDC caseloads due to 1-point increase in unemployment.
Also includes extensive controls for demographic, program and political variables, along with state and year effects.			

Table 6-continued

Council of Economic Advisers (1997)	Log(AFDC caseloads per capita) Based on state administrative data, 1976-96	Unemploymt rates Dummy variables for state waivers (looks at overall waiver effects & policy components)	• Share of caseload change due to economic factors: 24% to 31% in 1989-93 31% to 45% in 1993-96 • Share of caseload change due to waivers: 13% to 31% in 1993-96 • 3% to 5% estimated change in AFDC caseloads due to 1-point increase in unemployment.
Also includes state effects, year effects and state time trends.			
Figlio & Ziliak (1999)	log (AFDC caseloads per capita) Based on state administrative data, 1976-96	Unemploymt rates Dummy variables for state waivers	*In static models*: • Share of caseload change due to economic effects: -10% to 36% in 1993-96 • Share of caseload change due to waivers: 0% ro 24% in 1993-96 *In dynamic models*: • Share of caseload change due to economic effects: 18% to 76% in 1993-96 • Share of caseload change due to waivers: -7% to 1% in 1993-96 • 6% to 9% long-run rise in caseloads due to 1-point rise in unemployment rate.
Also includes state effects, year effects, and state time trends. *Dynamic models* include first-difference and lagged dependent variables.			
Levine & Whitmore (1998)	Log(AFDC caseloads per capita) Based on state administrative data, 1976-96	Unemploymt rates Dummy variables for state waivers	• Economic effects of same size as CEA (1997) study. • States with waivers have almost twice the caseload reduction but no difference in unemployment rates.
Also includes state effects, year effects and state time trends.			

Table 6-continued

Moffitt (1999a)	Log (AFDC participants/ female population, aged 16-54) Weeks and Hours of work Earnings Income Based on March CPS data, aggregated into education & age cells by state, 1977-95	Unemploymt rates Dummy variables for state waivers (looks at overall waiver effects & policy components)	• Reduction in participation due to waivers: -1.7 percentage pts among women high school dropouts. -0.8 to –1.0 percentage points among all women. ▪ Among high school dropouts also find significant effects of waivers on weeks and hours of work; no significant effects on earnings or income. • 0 to 0.3 percentage point rise in participation due to 1-point rise in unemployment rates
Also includes state effects, year effects and state time trends, along with demographic controls.			
Schiller (1999)	AFDC caseload growth by state Based on state administrative data, 1991-96.	Multiple economic variables (including unemployment rates and per capita income) Dummy variable for "soft" or "tough" state reforms State program operation variables (approval rates exemption rates, work assignment rates, etc.)	• Institutional program operation variables are highly significant. • Programs defined as "tough" produce greater caseload reductions.
Does not include state or year fixed effects.			

Table 6-continued

Wallace & Blank (1999)	Log(AFDC caseloads per capita) Based on state administrative data, 1980-96	Multiple economic variables (including unemploymt and wage information) Dummy variables for state waivers	• Share of caseload change due to economic effects: 50% for 1990-94 47% for 1994-96 • Share of caseload change due to waivers: -13% for 1990-94 22% for 1994-96 • 5% to 6% rise in caseloads due to 1-point rise in unemployment rate.
Also includes extensive controls for demographic, program and political variables, along with state and year effects.			
Ziliak, Figlio, Davis, & Connelly (2000)	Log(AFDC caseloads/ Female population, aged 14-55) Based on state administrative data, 1987-96	Unemploymt rates Dummy variables for individual policy components of state waivers	• No separate estimates of economic effects alone; 66% of change due to economic and seasonal factors in 1993-96. • Share of caseload change due to waivers: -9% in 1993-96. • 2% estimated change in AFDC caseloads due to 1-point increase in unemployment that lasts 5 months.
Also includes state effects, state time trends, time trends (t, t^2, t^3), and month effects. Estimated models include lagged dependent variables and first differences.			

Table 6-continued

Part B: Econometric research on Welfare Reform Impacts Including Data After the 1996 Welfare Reform

Study	Dependent Variable(s)	Key Independent Variables	Results on Key Variables
Council of Economic Advisers (1999)	log(AFDC caseloads per capita) Based on state administrative data, 1976-98	Unemploymt rates Dummy variables for state waivers and TANF implementn (looks at overall waiver effects & policy components)	▪ Share of caseload change due to economic factors: 26% to 36% in 1993-96 8% to 10% in 1996-98 ▪ Share of caseload change due to waivers: 12% to 15% in 1993-96 ▪ Share of caseload change due to TANF: 35% to 36% in 1996-98
Includes state effects, year effects, and state time trends.			
Grogger (2000)	AFDC/TANF participation Based on March CPS data, 1978-98	Unemploymt Rates Dummy variables for state reforms (waivers and TANF) and for time limits	▪ TANF and waivers have identical (negative) effects on participation, creating a 2.1 percentage point decline (exclusive of time limits). ▪ Time limits have significant negative effects on participation in families with younger children.
Includes state effects and year effects. Also includes demographic controls. Children's ages are interacted with time limit dummy variables.			
Grogger (forthcoming)	AFDC/TANF participation Employment measures Earnings & income Based on March CPS data, 1978-99	Unemploymt Rates Dummy variables for state reforms (waivers and TANF) and for time limits EITC parameters	▪ Time limits have significant negative effects on TANF participation and positive effects on earnings, especially among women with younger children. ▪ No effect of time limits on earnings or income. ▪ Significant effect of EITC on welfare use and participation.
Includes state effects and year effects. Also includes demographic controls. Children's ages are interacted with time limit dummy variables.			

Table 6-continued

Kaushal & Kaestner (2001)	Employment Hours Fertility Based on March CPS data, 1995-99	Unemployment Rates Dumy variables for family cap and time limits Also codes reforms by "intensity"	• States with both time limits and family caps show significant increases in employment and hours among married women with children. • Little significant effects of reform on fertility.
Difference in difference estimates for unmarried women with children, using married women with children and unmarried women without children as control groups. Includes state and year effects and demographic controls.			
O'Neill & Hill (2001)	AFDC/TANF participation Employmt last week Based on March CPS data, 1982-99.	Unemploymt rates Dummy variables for state waivers and TANF implementatn	▪ Share of caseload change due to economic factors: 30% in 1992-96; 17% in 1996-99 ▪ Share of caseload change due to welfare reforms: 12% in 1992-96 (waiver period) 49% in 1996-99 (TANF period) ▪ Similar effects on employment changes. ▪ Policy effects strongest among younger and more educated women.
Includes state effects, state time trends and national time trend. No year effects included. Demographic controls included. Separate groups of single mothers by age, education, and race are analyzed.			
Schoeni & Blank (2000)	AFDC/TANF participation Employment measures Earnings measures Income & poverty Family structure Based on March CPS data, 1977-99, aggregated into education & age cells by state	Unemployment or state income measures Dummy variables for state waivers and TANF implementatn (Also measures TANF effects as a pre-post 1996 effect)	▪ Waivers have a significant effect on AFDC participation, labor market participation, earnings, income, poverty rates, and marital status. ▪ TANF has significant negative effects on welfare participation, larger than the effects of waivers. ▪ TANF has relatively small but significant effects on earnings, poverty rates, and household structure. ▪ Economic factors fully explain labor market changes in the TANF period.
Includes state effects, year effects, state time trends. Also includes demographic controls.			

Table 6-continued

Wallace & Blank (1999)	log(AFDC caseloads per capita) Based on monthly state administrative data, 1980:1-1998:6	Multiple economic measures (including unemployment and wages) Dummy variables for state waivers and TANF implementatn	▪ Estimated caseload change due to economic factors: 20% to 36% in 1990-94 8% to 12% in 1994-98 ▪ Estimated caseload change due to waivers: -4% to –5% in 1990-94 26% to 31% in 1994-96
Also includes state-month effects. Models estimated in first differences with lagged dependent variables.			▪ Estimated caseload change due to TANF: 28% to 35% in 1997:1-98:6

Table 6-continued

Part C: Econometric Research on Welfare Reform Impacts Using Flow Data

Study	Dependent Variable(s)	Key Independent Variables	Results on Key Variables
Hofferth, Stanhope & Harris (2001)	Prob(exit welfare conditional on spell duration) Based on monthly PSID data on welfare spells, 1989-96	Unemployment and state income Dummy variables for individual policy components in state waivers	▪ Unemploymt not linked to welfare exits. ▪ Work exemptions for mothers with young children increase welfare exits; earnings disregards decrease exits; other policy components insignificant. ▪ Primary policy effects on exits from welfare to work. Non-work-related exits not affected by policy.
Uses event history analysis. Also includes demographic controls and state fixed effects			
Hofferth, Stanhope & Harris (2002)	Prob(Return to AFDC conditional on time since leaving AFDC) Based on monthly PSID data on post-welfare spells, 1989-96	Unemployment and state income Dummy variables for individual policy components in state waivers	▪ Higher unemployment correlated with welfare re-entry. ▪ Work exemptions for mothers with young children make women less likely to re-enter welfare; other policy components insignificant. ▪ Some income increases visible over time among welfare leavers.
Uses event history analysis. Also includes demographic controls and state fixed effects.			

Table 6-continued

Klerman & Haider (2000)	Log (Caseloads per capita) Welfare entry rate Welfare continuation rate Based on monthly administrative data from CA, 1989-98	Unemployment rates (No policy effects are estimable since data is from a single state)	▪ Standard estimates based on stock data may be biased. ▪ Simulated effects of economic factors on caseload changes between 1995-98 range from 12% to 47%.
Develop stock-flow model of caseload change. Estimates include county and time fixed effects.			
Mueser, Hotchkiss, King, Rokicki & Stevens (2000)	Welfare exit rate Welfare entry rate Quarterly adminis-trative data from 5 metro areas, early 1990s-97	Unemployment rates Dummy variables on significant state program changes (including waiver and TANF implementatn)	▪ Changes in exit rates explain about 2/3rds of the caseload decline in 4 of the 5 cities; changes in entry rates explain 1/3 of the decline. ▪ Economic conditions affect welfare exits, not entries. ▪ State waivers reduce welfare exits by 21%; TANF reduces welfare exits by 11%.
Includes site specific dummies, time trends, and quarter effects.			

Table 7

Effects of Financial Incentive Schemes Tested Using Random Assignment
Third Year Effects

I. Programs with Incentives Only

Outcome	MFIP Incentive Only	WRP Incentive Only	NY CAP
Employment (%)	3.6*	0.2	3.3*
Annual Earnings ($)	-191	-69	523*
Annual Cash Transfers ($)	1165***	-58	186
Annual Income ($)	973***	-106	200
Poverty (%)	-8.3***	n/a	n/a

II. Programs with Incentives and Work Requirements

Outcome	SSP	Full MFIP	Full WRP	CT Jobs First	FTP	Iowa FIP
Employment (%)	7.2***	11.5***	11.2***	6.4***	8.6***	2.4***
Annual Earnings ($)	649***	571*	575*	730***	915***	308***
Annual Cash Transfers ($)	800***	614***	-358***	-515***	-293	115***
Annual Income ($)	1449***	1185***	199	175	521**	--
Poverty (%)	-9.4***	-12.4***	n/a	n/a	n/a[a]	--

Sources: Employment, Earnings, Cash Transfers, and Annual Income for all programs except NY CAP and Iowa FIP come from Bloom and Michalopoulos (2001, Appendix Tables C.2 and C.3). Poverty data for MFIP comes from Miller et al (2000, Table 4.5). Poverty data for SSP comes from Michalopoulos et al (2000, Table ES.2). Data for NY CAP program come from DeMarco and Mills (2000) and are averages over the first five years after random assignment. Data for Iowa FIP program come from Fraker and Jacobsen (2000) and are averages over the third year after random assignment. All data in U.S. dollars.

[a] The FTP evaluation provides no information on poverty rates, but reports a 6.0 percentage point positive impact on the question "usually has enough money at the end of the month" (significant at the 1 percent level).

Table 8

Annual Benefits Received per Dollar Spent in Various Welfare Reform Evaluations

	Income gains per recipient (1)	Transfer payments per recipient (2)	Column 1 / Column 2 (3)	Total program benefits per recipient* (4)	Total program costs per recipient (5)	Column 4 / Column 5 (6)
I. Financial Incentives Programs (including strong work requirements)						
Self Sufficiency Project (SSP), Canada						
Long-term recipients	$1849	$948	1.95	n/a	n/a	n/a
Minnesota's Family Investment Plan (MFIP)						
Single parent urban long-term recipients	$1476	$596	2.48	$2044	$1693	1.21
Single parent urban recent applicants	$840	$868	0.97	$1193	$1624	0.73
Two parent applicants	$844	$2088	0.40	$104	$2552	0.04
New Hope Demonstration, Milwaukee, WI	$806	$483	1.67	$2306	$3616	0.64
II. Mandatory Employment Programs only						
National Evaluation of Welfare to Work Strategies (NEWWS, 7 sites)						
Maximum (Portland, OR)	n/a	n/a	n/a	$121	$208	0.58
Minimum (Riverside HCD, CA)	n/a	n/a	n/a	-$509	$798	-0.64
Greater Avenues for Independence Program (GAIN, 6 sites in California)						
Maximum (Riverside)	n/a	n/a	n/a	$380	-$587	large**
Minimum (Los Angeles)	n/a	n/a	n/a	-$312	$688	-0.45

* Includes the total imputed value of all program benefits and services to recipients. This includes the income gains represented in Column 1, as well as the imputed value of health insurance benefits, support services, and fringe benefits.

** It is difficult to calculate comparable cost-benefit ratios for the GAIN Riverside site, since the program increased income to recipients while generating savings to the government in reduced program costs. In some sense, with government savings the cost-benefit ratio is infinite. Of the 6 GAIN sites, 3 increased income to recipients at a cost savings to the government, 2 increased income to recipients while increasing government costs, and the Los Angeles program reduced income to recipients while increasing government costs.

Sources and notes:

1. Dollar amounts are calculated over different years for different programs; all in U.S. dollars. See sources for details.
2. SSP: Michalopoulos et al (2000). Column 1 and 2 figures come from table ES-1, page ES-7. Does not include taxes, and ignores costs of administering program. Effects are over 3 years.
3. MFIP: Miller et al (2000). Column 1 and 2 figures come from tables 4.1, 5.1, and 6.1, and present effects over 10 quarters. Column 4 and 5 figures come from Table 7.1, and present effects over 5 years. Both sets of numbers include the Earned Income Tax Credit (EITC).
4. New Hope: Bos et al (1999). Column 1 and 2 figures come from Table 5 of the Executive Summary. Column 4 and 5 figures come from Table 8.3. Effects are over 2 years.
5. NEWWS: Program benefits come from Freedman et al (2000), Tables 5.1, 6.1, and 6.2. Costs come from Bloom and Michalopoulos (2001), and are net of savings in transfer payments (AFDC and Food Stamps), which come from Tables 6.1 and 6.2 of Freedman et al (2000). Columns 1 and 2 are not applicable as no additional transfer payments were paid to experimentals. Does not include the EITC on either the cost or the benefit side. Effects are over 2 years. Riverside ran more than one program. The results presented above were from the Human Capital Development (HCD) program, which emphasized education first.
6. GAIN: Riccio, Friedlander and Friedman (1994). Columns 1 and 2 are not applicable as no additional transfer payments were paid to experimentals. Column 4 and 5 figures come from Tables 6b and 7 of Executive Summary. Effects are over 5 years.

Table 9

Research on the Effects of Welfare Reform on Family Structure and Fertility

Part A. Econometric Estimates of Waiver and TANF Effects

Study	Data	Dependent Var	Key Independent Var	Results
Fitzgerald and Ribar (2001)	SIPP panels 1990, 1992, 1993	Female headship *Levels *Exits from and entries to headship	Welfare waivers (Aggregate and by policy component)	• Waivers don't affect headship levels • Waivers increase exits from and reduce entries to headship • Estimated effects of waivers by policy component often show wrong sign
Horvath-Rose and Peters (2001)	State administrative panel data 1984-96	Nonmarital birth ratios	Welfare waivers (aggregate and by policy component)	• In aggregate, waivers reduce nonmarital births • Family caps have negative and significant effect • Minor parent provisions have significant positive effect • Few effects of time limits, AFDC-UP or work requiremts
Kaushal and Kaestner (2001)	CPS data 1995-99	Childbearing	Family caps and time limits	• No significant or credible effects of family caps or time limits on childbearing among single mothers with children.

Table 9-continued

Schoeni and Blank (2000)	CPS data, aggregated to state level, 1977 – 1999	Among all women: *Share who head households *Share married	Welfare waivers (aggregate effects) TANF implementation	• Waivers reduce headship and increase marriage • TANF reduces headship

Part B. Econometric Estimates of the Effects of Family Caps

Study	Data	Dependent Var	Key Independent Var	Results
Acs (1996)	NLSY Women w/ at least one child	Probability of higher order birth	Incremental AFDC benefits	• Negative effect, but insignificant
Argys, Averett and Rees (2000)	NLSY	Probability of pregnancy and abortion	Incremental AFDC benefits	• Positive significant effect on pregnancy, unless state fixed effects are included • No effect on abortion
Fairlee and London (1997)	SIPP 1990 Women w/ at least one child	Probability of higher order birth	Incremental AFDC benefits	• Few effects on births
Grogger and Bronars (2001)	Census 1980 PUMS	Time to next birth	Incremental AFDC benefits	• No significant effects

Table 9-continued

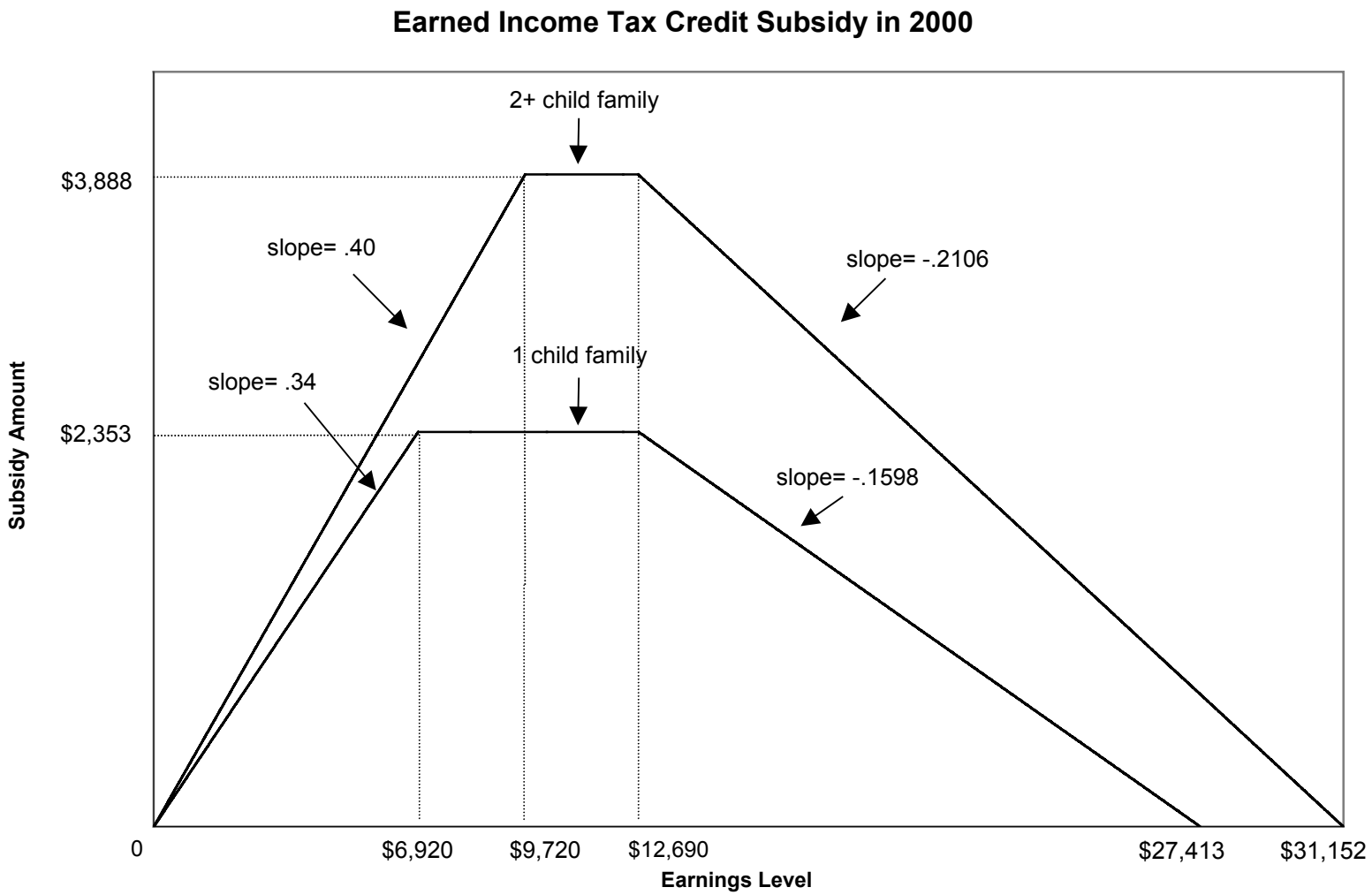
Part C. Experimental Evidence on the Effects of Welfare Reform on Family Structure and Fertility

Experiment	Location and Years	Nature of Intervention	Results
A Better Chance (ABC) Demonstratn Fein (1999)	Delaware 5 sites Experiment: 1995-96 Final evaluation: 18 months out	Mandatory job search 2-year time limits Earnings disregards Strong sanctions Family caps Work supports	• Few effects on marriage • Cohabitation increases among less skilled and younger mothers • No fertility effects • Reduced enthusiasm for childbearing
California Work Pays Demonstratn Hu (2000)	California 4 sites Experiment: 1992-96 Evaluation includes data thru 1997	Reduced benefit levels Increased BRRs	• Significantly more married couples stay together. • No significant effects on single women's marriage or cohabitation rates.
Family Developmt Program Camasso, et. al. (1998a and 1998b)	New Jersey 1994-95	Family cap program Earnings disregards Work supports Two-parent families given better access to AFDC	• Birth rates 9% lower • Increased family planning use • No change in abortions
Minnesota Family Investment Program Miller, et. al. (2000)	Minnesota 7 sites Experiment: 1994-96 Final evaluation: 3 years out	Work mandates Large earnings disregard Service coordination	• Increased marriage among single long-term recipients • Reduced separation among married two-parent recipients

Table 9-continued

New Chance Quint, Bos, and Polit (1997)	10 states 16 sites Experiment: 1989-92 Final evaluation: 3½ years out	Comprehensive services (education, employment and life planning) to AFDC recipients who were teen mothers and high school dropouts	• Increased instability in living arrangements • No effect on pregnancy, births, or abortions
Teen Parent Demonstration Project Kisker, Rangaranjan, and Boller (1998)	Illinois and NJ 3 sites Experiment: 1987-91 Final evaluation: 6½ years out	Mandatory participation in employment, education, and life planning assistance	• More births in experimental group (an unexpected effect)

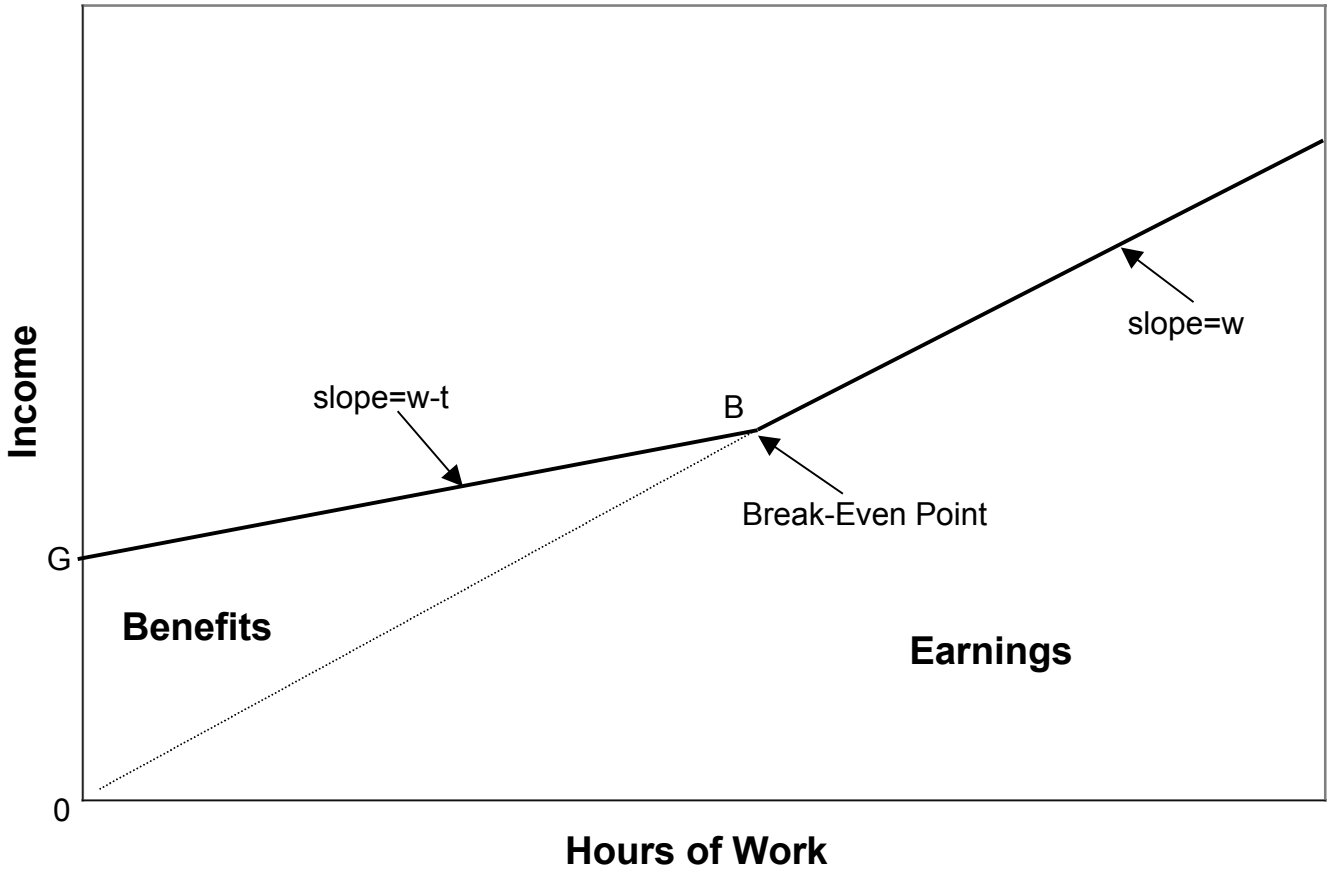
Figure 1



Source for EITC Parameters: "The EITC and the Taxation of Lower-Income Working Families." Joint Economic Committee Staff Report, March 2000. (http://www.seante.gov/~jec)

Figure 2

Income Constraint Resulting From A Typical Welfare Program

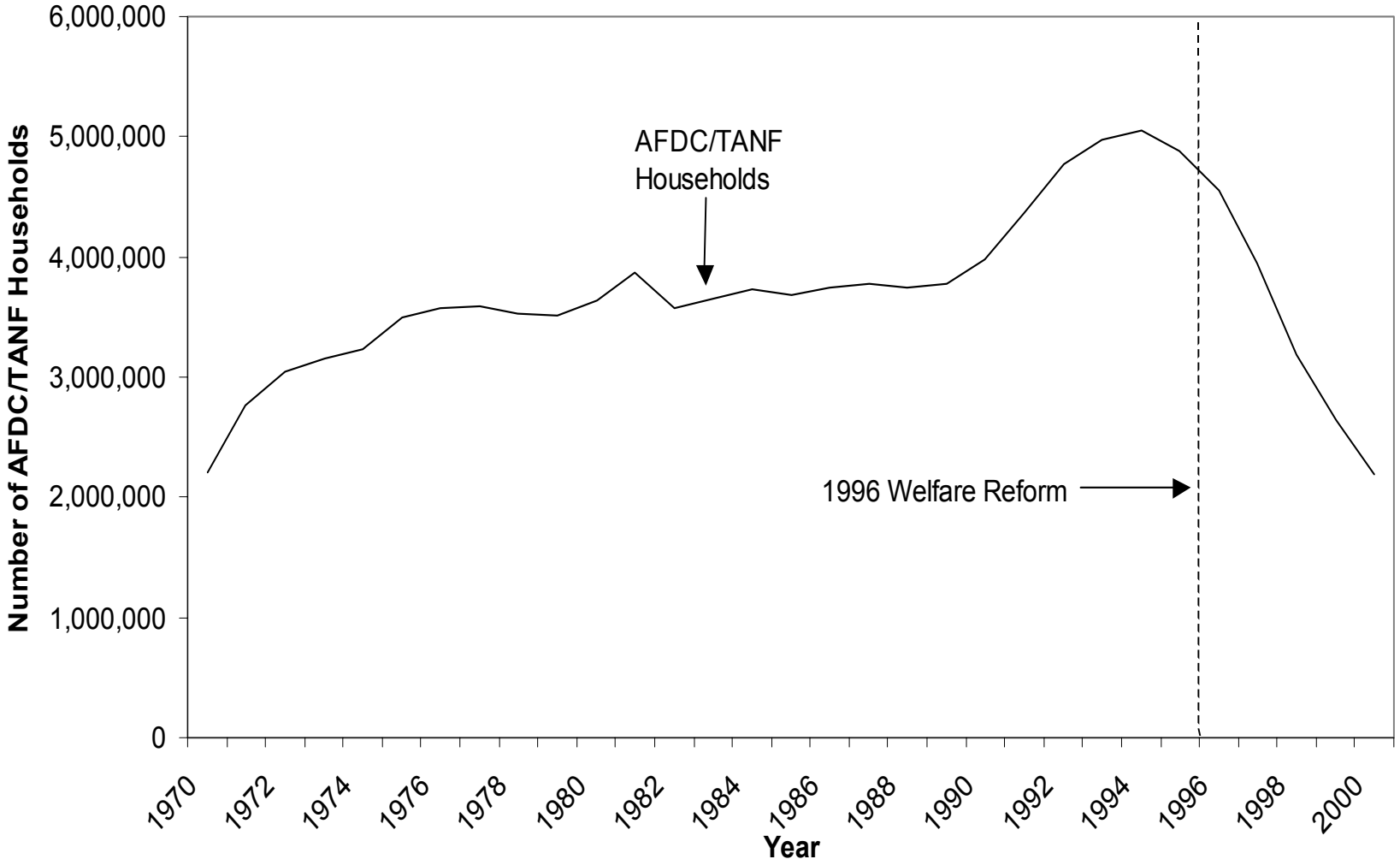


G: Maximum Benefit at 0 Earnings
w: Wage Rate
t: Benefit Reduction Rate

Note: Assumes no income other than welfare benefits and earnings.

Figure 3

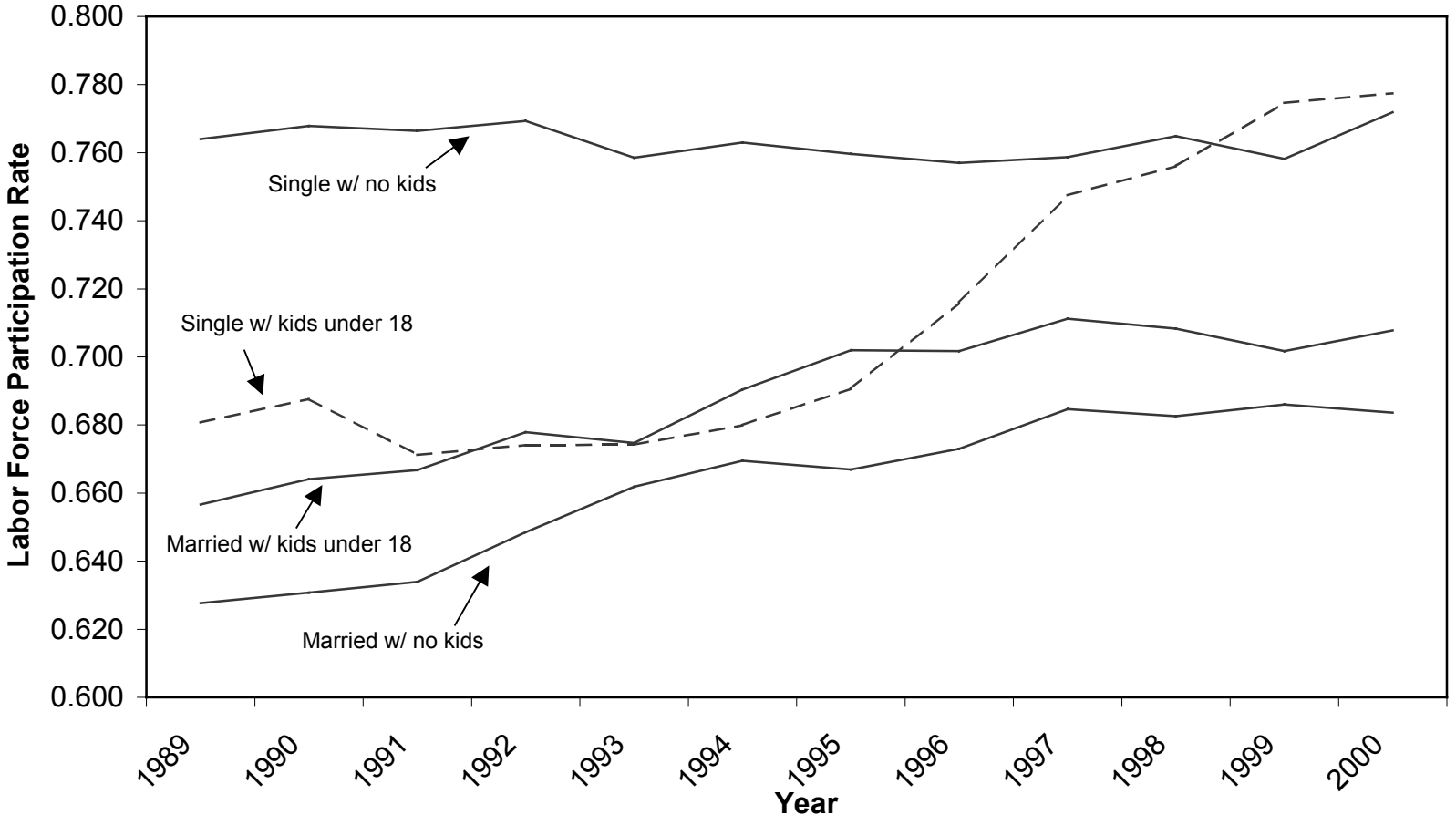
Total AFDC/TANF Caseloads



Source: Agency for Children and Families, Department of Health and Human Services (http://acf.dhhs.gov)

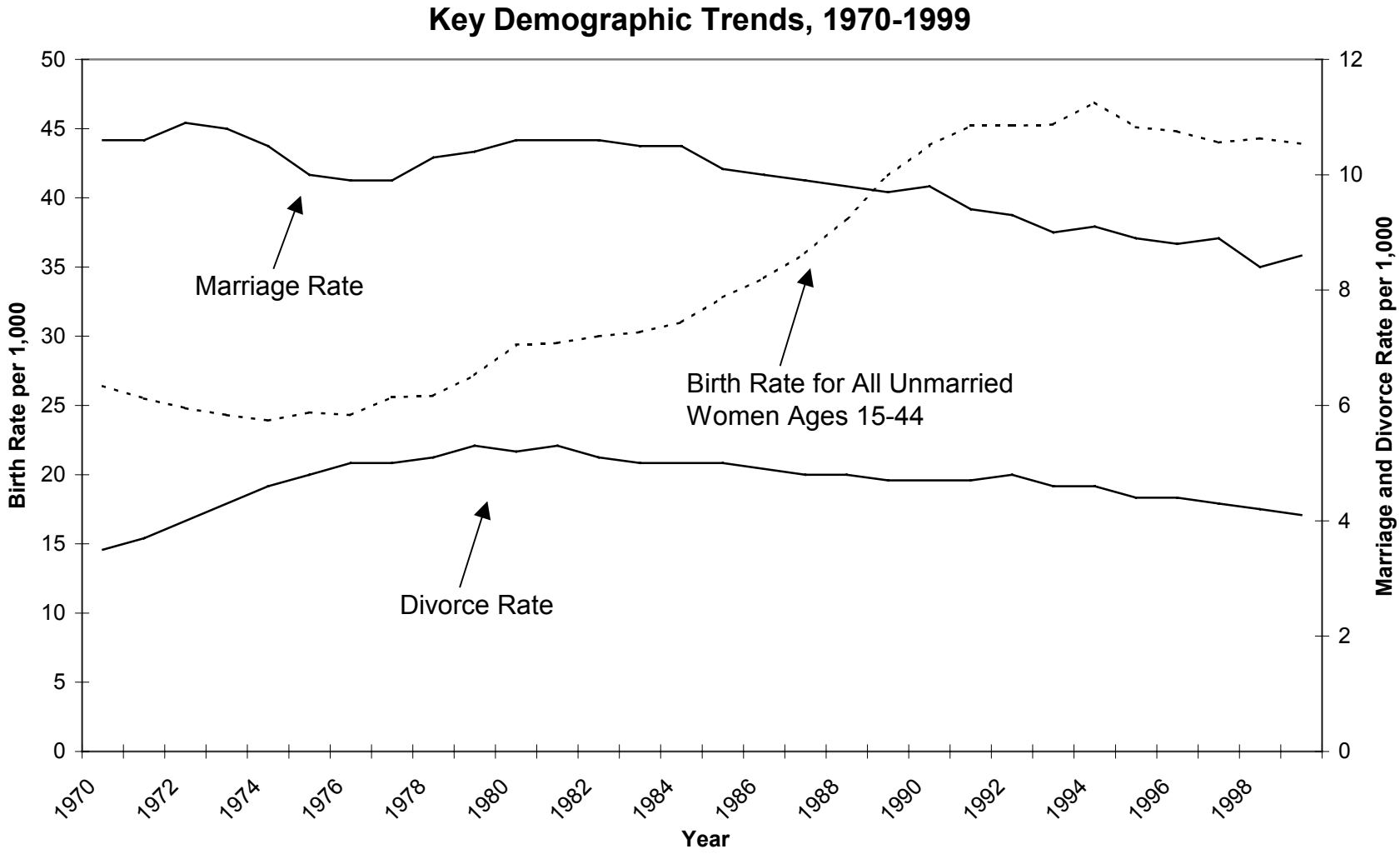
Figure 4

Labor Force Participation Rates for Women by Marital Status and Children (Ages 20-65)



Source: Tabulations of March Current Population Survey Data

Figure 5



Source: National Center for Health Statistics, National Vital Statistics Reports. (http://www.cdc.gov/nchs)