

The Impact of the Earned Income Tax Credit and Social Policy Reforms On Work, Marriage, and Living Arrangements

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Abstract

This article examines the impact of the recent dramatic changes in the social policies, particularly the expansion of the EITC and welfare reform on labor supply, marriage, and cohabitation. Altered policies have increased incentives to work or marry for some, diminished incentives for others. The results strongly indicate expanded work by single mothers and reductions of work by married mothers in accordance with their changed incentives. By contrast, estimated impacts on marriage are small and ambiguous, though modest changes in cohabitation in the predicted direction suggest that impact on family structure might become more apparent in the future.

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The Impact of the Earned Income Tax Credit On Work, Marriage, and Living Arrangements in the United States

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All social policies create incentives, and most create at least some that are undesirable in the eyes of policymakers. The Earned Income Tax Credit (EITC) is unusual in that it creates sharply different incentives for different individuals. For some it serves as a strong work incentive; for others, it is a work disincentive. Similarly, the EITC rewards marriage among some and penalizes it among others. In contrast, traditional means tested benefits usually create unambiguous work disincentives and marriage penalties.

In this paper, I exploit the fact that work and marriage incentives have changed differentially for various groups in order to test the ramifications of these changed incentives. I use an intuitively straightforward methodology to allow both graphical and statistical "difference in difference" estimators to track work and marriage behavior of different sub-groups over time. Significantly, the paper is one of the first to examine the impact of changing economic incentives on marriage and to look specifically at their impact on marriage versus non-married cohabitation.

The results suggest that the EITC, welfare reforms, and the strong economy have had a strong positive effect on work by single parents and a somewhat more modest negative effect on the work of some married mothers. They also suggest that

marriage and cohabitation have not changed dramatically, but there is at least a hint of some changes, though these effects are far more tentative and sensitive.

Altered Economic and Social Policy

Several dramatic policy changes occurred during the late 1980s and early 1990s that profoundly shifted the incentives for work and marriage for low and moderate income parents. Welfare reform began in earnest at the state level in the late 1980s and early 1990s, with many states receiving "waivers" of federal rules that allowed them to experiment with alternative reforms. In 1996, in the midst of already sharp falls in the caseload since 1993, the Personal Responsibility and Work Opportunity Reconciliation Act was passed. Temporary Assistance for Needy Families (TANF) replaced Aid to Families with Dependent Children (AFDC), changing what had been an open-ended federal matching grant into a block grant to states. It added requirements that a sizable share of recipients be working (or that state caseloads be reduced equivalently), and imposed a 5-year lifetime limit on benefits for most recipients. States were given the option of adding whatever other restraints they chose, including setting even shorter time limits.

States have responded in a myriad of ways. Some have imposed strict time limits. Others require work immediately. Some have reduced benefits. Many have altered the effective tax rate on earnings when people go to work. But perhaps the most dramatic changes have been administrative. States have used a variety of mechanisms to push people off of welfare and towards work. Some states have

experienced caseload reductions as large as 70 or 80 percent, something completely unprecedented in the 60 plus year history of the program.

Yet as dramatic as the shift has been in welfare, expansions in supports for low-income working families are perhaps even more remarkable. Whereas low-income working families were eligible for about \$5 billion (1998 dollars) annually in federal aid in the late 1980s, by the late 1990s the total expenditures were above \$50 billion.¹ About half of this growth can be traced to expansions in the Earned Income Tax Credit (EITC). By 1996, inflation adjusted federal expenditures on the EITC alone exceeded the combined real state and federal benefit expenditures on AFDC benefits in any year. And starting in 1998, a non-refundable child tax credit has been in place (\$400 per child maximum in 1998, \$500 thereafter). As a family's income pushes them into the range where they owe taxes, this credit can be used to offset them.

Most of the rest of the growth is traced to expansions in medical assistance programs for the low income working families. Whereas Medicaid once was limited primarily to people receiving means tested cash assistance such as AFDC, states are now required to provide coverage for all children born after October 1983 with family income at or below the poverty line. Many states have chosen to cover children who are older and whose families are considerably above the poverty line either through Medicaid or the newly adopted Children's Health Insurance Program (CHIP) which

¹ See Ellwood (2000).

offers still more money for covering children. Some expansion in federal support for childcare has also occurred.

The New Work and Marriage Incentives

Figure 1 shows how EITC benefits vary with earnings. At first, each new dollar of earnings brings added benefits since each dollar of earnings generates up to 40% in refundable tax benefits up to a maximum benefit of \$3,756 in 1998. But when earnings exceed a cut off (\$12,260 in 1998), benefits are reduced as earnings rise. The phase-in, where benefits rise along with earnings, creates an incentive to work, while the phase-out, where benefits decrease as earnings grow above a certain level, creates disincentives. Moreover, the nature of the incentives also varies depending on whether a family has only one potential earner or two.

The overall incentive effects are a reflection of taxes, means tested benefits, work expenses and the like. To understand the changes let us begin with the situation as it stood in 1986 when a small EITC existed. Consider a low skilled woman with two children who might earn \$10,000 annually (1998 dollars) if she chose to work. She might also marry a man who earns \$15,000.² Table 1 shows what her family's disposable income would have been in 1986 under different combinations of work and marriage. In deriving this table and subsequent ones, I ignore any housing benefits and assume people collect benefits to which they are entitled.

Suppose the single parent is considering whether or not to work. If she did not work, she would have received \$8,804 in means tested aid and she and her children

would have been covered by government health insurance (Medicaid). If she went to work at a job paying \$10,000, her AFDC and food stamp benefits would fall dramatically, she would incur child care costs, and she would be expected to pay taxes. Her overall income would grow by only \$1,860 to \$10,664. In effect her tax rate was 81%. Plus her family would lose Medicaid, which would easily be worth over \$2,000, so the effective tax rate may well exceed 100%.

Table 2 shows how dramatically things had changed by 1998. A large EITC had been instituted, means tested benefits had been cut, medical care was usually offered to children of poor working families, and child care aid was often available. As a result the income of a similar single parent going to work in 1998 would rise from \$7,717 to \$14,593. The EITC and other benefits helped overcome the negative incentives of means tested benefits. And her children under age 15 would retain their Medicaid, and in many states, even older children would be covered.³

Next consider a husband-wife family with a single \$15,000 wage earner. In 1986, the family would have had \$15,613 and no medical benefits. By 1998 this working poor family would have gained an additional \$3,281 primarily due to the EITC, and Medicaid would cover the children. The EITC and other targeted benefits for working families clearly reward working poor and near-poor families.

² In fact, the median woman in the bottom quarter of the predicted wage distribution earned roughly \$10,000 in 1998 if she worked at least 26 weeks, and if a woman in the same bottom quarter was married, her husband earned a median of \$18,000.

³ Note, however, that although EITC use is very high, use of food stamps and Medicaid is hardly universal among working families eligible for such aid. This is especially true of Medicaid enrollment. See Guyer, Broaddus and Cochran (1999). Thus the table gives a sense of what people could qualify for, not necessarily the amounts that any individual actually collects.

Yet the EITC has also changed work incentives in the opposite direction for a mother married to a low-income working man. If her husband earns \$15,000 per year, a mother who enters work and earns \$10,000 would push up the family income well into the phase-out range of the EITC so that the returns reward to work are reduced by the falling EITC benefits. The impact of the EITC and other programs on work incentives is illustrated in Table 3. The last columns in the table illustrate:

- For a low wage, single parent, the payoff to taking a \$10,000 per year job has risen from roughly \$2,000, coupled with the loss of Medicaid, to almost \$7,000 in 1998 with only the adult losing Medicaid coverage. Even the adult can get coverage in some circumstances.
- By contrast, for a low wage mother married to a low-income man, rewards to working have been cut sharply, with the net increase in income from a \$10,000 job falling from \$4,700 in 1986 to \$3,300 in 1998. Married mothers in 1998 faced an effective tax rate of 67%. The situation was even worse in the previous year before a new child tax credit offset a large share of federal taxes for this family. And the family also faces a potential loss of medical coverage. Note that there is also a large income effect created by the EITC. Tables 1 and 2 show that the disposable income of a married couple with one \$15,000 earner would rise from \$15,600 in 1986 to \$18,900 in 1998. Thus both the marginal tax rate (substitution) effects and the income effects would reduce incentives for work by low wage married mothers. Of course, one could just as easily say that if the mother works, the father faces strong work disincentives.

Is it likely that people will even be aware of these work incentives and therefore respond to them? Welfare recipients often go to work for brief periods then return to welfare and then return to work. They should notice how their income changes. Moreover, the EITC typically arrives in a very visible lump sum after as a tax refund/credit. Indeed work by Smeeding et al. (2000) and Romich and Weisner (2000) indicates considerable knowledge of the program among potential participants. And some newspaper accounts suggest that while low-income families don't really

understand the EITC in detail, they do realize that if they go to work, they will get a big refund, in part because tax preparers are offering large immediate refunds.⁴ So for single parents at least, it is reasonable to suppose that they might be influenced by the work incentives.

The situation is more complicated for married mothers. The combination of multiple incomes and the fact that the family may get a tax credit that will mainly just offset other taxes owed, rather than producing a big refund check, seems more likely to obscure the overall impact of the EITC. Still, if a family experiments by sending a second earner to work or keeping them at home, they should notice just how much their living circumstances change. Perhaps more importantly, in the years when the EITC was being increased, the after-tax income of families with one low-wage worker would be rising rapidly and this dramatic difference in income would surely be noticed and might well diminish work by the second earner.

The EITC creates some bipolar incentives for or against marriage as well.

Table 4 illustrates the marriage penalties. Looking at the last columns, we see:

- In all years and in all circumstances, there are clear marriage penalties created, largely as a result of the means tested programs, though regular taxes play some role as well. There will, of course, be some compensating economic advantages, including economies to scale achieved by having to support only one household (though these could be achieved by living together outside of marriage).
- The size of the marriage penalties varies greatly by type of situation and year. In 1986, a *non-employed* single mother faced a \$5,815 penalty by marrying a man earning \$15,000. By 1998, the penalty was sharply reduced to just under \$1,500. The reduction in penalty is partially caused by the decline in means tested benefits available to unmarried parents. But

⁴ See for example DeParle (1999).

most of the improvement can be traced to the nearly \$3,000 increase in the EITC since 1986. In this case, the EITC serves as a marriage bonus.

- A *working* single mother who considers marrying a working man faces a much larger marriage penalty in 1998 than in 1986. For as the combined income of the husband and wife push people into the phase-out range, the EITC now serves as a marriage penalty. Whereas a working mother marrying a working man suffered a roughly \$3,000 marriage penalty in 1986, by 1998 that penalty had grown to over \$5,000—virtually all due to the loss of EITC and childcare aid to lower income working families. Indeed this situation was worse in 1997 before the child tax credit offered some additional tax relief.

Note that so-called marriage penalties could influence both decisions to marry and decisions to end a marriage.

This analysis of marginal marriage incentives misses a couple of critical issues, including income effects and the feedback from altered work behavior. Consider the situation facing a struggling low-income married couple where both parents are working. The expansion of the EITC does create a marriage penalty--the mother could potentially get a larger EITC by divorcing. But that same EITC also provides extra income to the household and reduces work incentives for the second earner. The added income may reduce stress in the household and increase the likelihood the family will stay together. The altered work incentives may lead the mother to spend less time in the workforce, which, according to a Becker (1981) type model of marriage, might increase the comparative advantages of marriage.

This analysis also misses altered incentives for childless couples. In particular, suppose a couple considers marriage with the expectation that they will have a child at some point and that they have no intention of having a child outside of marriage. For this couple, the EITC also serves as a marriage inducement. At worst they can get

nothing; at best, they will receive some money from the tax credit once they start raising a family.

Moreover, couples who face a marriage reward or penalty may at a later point face the opposite incentives if their economic status or parental status changes, given the duration of marriage. Thus short-term incentives may not reflect couples' expectations regarding the effects of social policy over the life of the marriage. Finally, the EITC might also influence fertility, encouraging some married and unmarried women to have children who might otherwise have been reluctant to do so. This too could influence the stock of married and unmarried mothers.

Once again it is fair to ask if people would notice the marriage incentives. It is very clear that people would notice the incentive effects of means tested programs. Few who marry expect to continue receiving welfare, so its loss is quite clear. The EITC incentives may be more obscure. Since marriages tend to last for extended periods, there is almost no way to implicitly learn about the tax consequences of being married or not by repeated marriages and divorces. Still one group might be unusually sensitive to marriage incentives—couples with children who are cohabiting, but unmarried. EITC marriage penalties or rewards might be particularly important in influencing decisions about marriage among these couples.

THE IMPACT OF THE POLICY CHANGES ON WORK

The remainder of the paper is broken into two main parts. The first section explores the impact of the EITC and other changes on work by single and married mothers. The remaining section examines marriage and living arrangements.

Methodology

There are three broad methods for estimating the effect of the altered work incentives. The first involves the estimation of structural models where the kinks and other features of the budget constraints created by the EITC and other programs are explicitly modeled. One can either use estimates of labor supply elasticities and/or estimate new ones based on actual work behavior to determine the structural parameters. This is essentially the methodology used by MaCurdy, Green and Paarsch (1990) and Moffitt (1986). Dickert, Houser and Scholz (1995) estimate 1990 cross sectional elasticities of labor supply response to net income after accounting for multiple program effects, and then use these to simulate the marginal effects of the expanded EITC. Attanasio and MaCurdy (1997) seek to estimate the effect of the EITC on the entire life-cycle of labor supply. One difficult problem is modeling the complex combination of means tested, EITC, and tax rates facing individuals. And, as we will see shortly, the changes in welfare policy are particularly difficult to model.

A second strategy is to quantify the various changed incentives facing potential workers and to use these to estimate a fairly straightforward reduced form labor supply model. The most sophisticated use of this strategy is Meyer and Rosenbaum (1999). These authors go to considerable effort to parameterize altered incentives created by

both state and federal EITC policies and by changes in AFDC including benefit rule changes, time limits, and some state Medicaid extensions, childcare and training benefits. Eissa and Hoynes (1999) estimate a reduced form model of labor supply for married women relying on variation in tax treatment (primarily the EITC) to judge the influence of the EITC on labor supply.

Though such methods hold the hope of fairly precise behavioral estimates, they suffer limitations. First the models usually explicitly assume that the response to a \$1 increase in the EITC (or any other financial benefit) will be the same as a \$1 increase in earnings. Indeed, the model is estimated by assuming that sample individuals fully understand the true multiple incentives they face. Yet the complexities of such incentives suggest that recipients may not fully understand them or respond appropriately. And to model the incentive effects properly, one really should take account of the wide range of kinks and slopes in the budget set as the structural models attempt to do, though the complexity of the incentives makes this a daunting task.

But perhaps more importantly, the recent changes in the incentives facing those who would previously have gotten means tested benefits (welfare) and changes in Medicaid policies are virtually impossible to characterize quantitatively. Some states have dramatically increased pressure to move off of welfare. Their methods have generally not involved dramatically lowering benefits. Rather they have sought other means to divert people from getting aid or to move people off of welfare quickly.

Consider two examples. In Georgia, before she can even begin the application process, a woman seeking aid is required to get a form signed by 6 employers saying

that she applied in good faith for a minimum wage job and was turned down.⁵ Once enrolled, if she is penalized twice for failure to meet some key administrative/work requirement, she is barred for life from seeking aid in the state. In Wisconsin, no aid is provided *unless the person is already working*. When applicants claim they really cannot find a job, the providers of TANF will, in some cases, provide a subsidized job for a limited duration, but aid remains tied to working. In both states, caseloads have dropped dramatically--nearly 80% in Wisconsin.

Meyer and Rosenbaum have examined a host of measures ranging from benefit levels to elements in waivers. They have done about as well as can be imagined. Yet none can really capture the crucial administrative elements in Georgia or Wisconsin or most other states. Indeed in the measures used by those authors in their published work, Georgia shows up as essentially unchanged. Administrative changes such as a greater emphasis on sanctioning or increasing the difficulty of getting aid by making it more unpleasant or stigmatizing are probably impossible to measure. And even if we could capture them somehow, modeling would still be a bit of a mystery.

Administrative changes interact with the economy and the availability of other benefits. States appear far more willing to sanction people or refuse them aid if jobs are perceived to be relatively plentiful.

This discussion suggests that state-to-state variation in AFDC/TANF structures are measured with considerable error, downwardly biasing the apparent impact of welfare "reforms." Since these changes occurred almost simultaneously with EITC

⁵ LaDonna Pavetti, personal communication.

expansions, the mismeasurement will likely create an upward bias in the apparent impact of EITC/tax changes.

And few authors have tackled the hardest issue of all--the changed availability of Medicaid for children.⁶ I previously noted that the federal cost of growing health coverage for children of the working poor and some adults has been almost as great as the cost of the EITC. But should such benefits be treated as the insurance value of Medicaid? Should it vary by the health of family members? What value do potential beneficiaries of such aid place on it when they are healthy?

The alternative methodology is to exploit the natural experiments created by the timing of the rapid expansion in the EITC and changes in social policy and the fact that they did not affect all persons equally. This method was used by Eissa and Liebman (1996), Schoeni and Blank (2000), and underlies much of the analysis in Meyer and Rosenbaum (1999) and Meyer and Rosenbaum (2000). For each group, subjected to sharply different incentives, the authors seek a control group that faces fewer changes, and this can be problematic. Eissa and Liebman (1996), Meyer and Rosenbaum (1999), Meyer and Rosenbaum (2000), and Eissa and Hoynes (1999) rely heavily on childless women as controls, which has some appeal, but also has some obvious problems. A much larger fraction of childless single women already worked even before EITC expansions, so their employment would not be expected to grow as much. Moreover, the temporal trends in labor force participation of the mothers with and without children are often different before the enactment of the EITC, so drawing

⁶ Ham and Shore-Sheppard (1999) is one of the few.

inferences from differential trends afterwards is troubling. An alternative, also used by Eissa and Liebman (1996), is to use mothers at higher education levels as controls for less skilled ones, but they too start at much higher levels of work. Probably the best strategy is to explore a variety of potential control groups.

While one would be unwilling to treat such “difference in differences” estimates as highly precise, they can offer powerful and straightforward evidence of the behavioral impacts. In the past, such methods made it difficult to disentangle the separate impact of the multitude of policy changes from each other or the economy. One could mostly test the overall impact of the combination of policies in a strong economy. While important in its own right, such a conclusion is distinctly frustrating to economists and policymakers interested in looking at the margin. Moreover, the remarkable variation in state welfare policies recently seems like it ought to provide some leverage for decomposing things.

This paper seeks to extend and exploit the difference in difference method to determine behavioral impacts of the changed policies for both unmarried and married mothers at various skill/potential wage levels. Moreover, it explores ways to measure welfare reform aggressiveness in hopes of comparing changes in work in states with more and less aggressiveness welfare reform policies. Hypothetically, if there were a group of states that had done very little in the way of serious reform, one might look to difference in difference estimates of work behavior in those states as a measure of the impact of the EITC alone. The additional changes in the more aggressive states could indicate the impact of welfare reforms.

Using the Current Population Survey from March 1999⁷, I ran a basic wage equation based on average hourly earnings in 1998 for women age 18-44 who worked at least 26 weeks, using characteristics including age, education, race, and number of children as independent variables. The results of that equation are provided in Appendix Table 1.⁸ I then take women aged 18-44 in each March CPS survey from 1975 to 1999 and use the wage equation to predict a potential 1998 wage for them, whether or not they worked. Finally I use that predicted wage to place the women into predicted wage/skill quartiles for their survey year.⁹ Note there are 25 percent of all women in each predicted wage quartile for each year.

Thus I have created a consistent set of four equal skill/wage groups in each year based on characteristics highly correlated with pay such as education and age. Since I use this same equation to create wage/skill quartiles each year, I can track what happens to quartiles of similar women over time. And if incentives changed differentially for women in the bottom versus the top skill/wage quartile over time, by tracking the behavior of women in each quartile, I have a natural experiment. I can compare, for example, the work of the single parents in the lowest wage quartile to work by women in the next highest quartile. And I can compare what happens to the

⁷ This is the CPS Survey conducted in March of 1999 that asked about income in the previous calendar year.

⁸ The preferred way to estimate potential wages is to use a Heckman correction for selection bias due to the fact that some women do not work because their wages are lower. I tried such a correction in several years and predicted wages again. Though the levels differed as a result of the correction, the rank order changed little. The correlation between the corrected and uncorrected wage predictions exceeded .95. Since we only seek to group people into quartiles, and since I want to estimate equations for well over 350,000 women over 18 years, I relied on the simpler OLS regressions.

⁹ One could also create new wage equations in every year. I prefer using the 1998 model since it guarantees I will be tracking very similar people over time. However, I have done all the estimates in the paper using separate year wage equations as well and the results are virtually identical.

lowest wage/skill single women with children to the lowest wage/skill women without children.

One might have chosen, as Eissa and Liebman (1996) did, to track people over time based on education levels. But the mix of education changes over time. In 1975, my first sample year, the fraction of women age 18-44 who were high school dropouts was 23%. By 1998, that figure had fallen to 13%. Thus I would be comparing the behavior of the bottom 23% of women in 1975 to the behavior of the bottom 13% in 1998. My method also largely obviates the need to do regression-corrected estimates. I have already grouped people according to their measured characteristics.

Measuring Welfare Reform Aggressiveness

Finding a legitimate measure of welfare reform aggressiveness is remarkably difficult given the nature of the program changes and the extremely close linkage between work and welfare. Yet the obvious desire to disentangle the impact of welfare reform from the economy and the EITC push toward the development of mechanisms to at least establish some reasonable bounds . In this paper I examine two different measures, one programmatic and one statistical.

Programmatic Measures--Meyer and Rosenbaum have compiled a considerable list of measures that might be used to determine the particular programmatic changes that could influence caseload. Ultimately they select a few measures for inclusion in their analysis. Based on their work and supplemented with my own experiments, I determined that 4 measures seemed particularly good candidates as indications of the states' aggressiveness: whether the state had a real

benefit decline of more than 25% between 1986 and 1997, whether the state had imposed a time limit of any sort under a waiver by 1996, whether the state used full family sanctions for AFDC recipients who did not comply with JOBS requirements (an earlier welfare reform), and whether any persons were terminated for failure to meet a requirement created under an AFDC waiver. All of these are for the period prior to the passage of national welfare reform since state data are not yet available after that period.

The benefit level is the most obvious and powerful financial incentive; the other measures are all indications of administrative or time limited measures that are not otherwise captured. I experimented with different weights for each of these based on their impacts on caseload changes, but ultimately concluded that a simple sum was the cleanest method of classifying. States that made none of these changes are least aggressive; states that did at least 3 out of 4 of them were the most aggressive.

Statistical Measures -- A state that aggressively pursues welfare reform through economic and administrative actions will likely have two possible observable effects: it will increase work among single parents thereby raising their earnings, and it will reduce the odds that someone with a given level of earnings receives aid. The incentives built into the EITC will also pull people into higher earnings categories, but it should have no impact on the odds that someone with a given level of earnings receives benefits. The distinction suggests that a measure that captures the changing odds that people of given earnings receive aid in a state would be a plausible measure of administrative aggressiveness that is not automatically correlated with EITC and

other incentive changes. And it is also likely to be closely linked to other features of welfare reform since administrative and non-administrative methods of discouraging welfare and encouraging work seem likely to go hand in hand.

Using CPS data for each state over the period 1984-1992, I estimated 51 state probit models of AFDC participation among single parents, conditional on age categories, education categories, racial categories, the state unemployment rate, 4 earnings categories, and a time trend.¹⁰ I use this period since it was not a time of particularly dramatic changes in policy at the national level. Thus this model simulates the eligibility/participation structure for each state in the late 80s and early 90s.

Then, using the actual earnings and education of single mothers and the state unemployment rates in 1997 and 1998, I predict the fraction of sample participants one would have expected to collect AFDC in 1997 and 1998 had the eligibility and participation structures been the same as during the 1984-1992 base period. Since earnings had risen and unemployment rates fallen, the model predicts declines in welfare use in virtually all states. But the actual declines in many states were even greater, presumably because they had become more aggressive in deterring welfare participation among persons of a given level of earnings. The difference between the actual and the predicted change in participation between 1991-1992 and 1997-1998 is

¹⁰ The age, race, and education categories are the same as those used on Appendix Table 1. The annual earnings categories are: \$0, \$7500, \$15000, greater than \$15000. State unemployment rates were taken from the Bureau of Labor Statistics. Earnings do not include EITC or similar benefits as these were not counted for as income in calculating AFDC benefits or eligibility.

thus an indicator of how aggressively states sought to reduce the roles through changes in eligibility.¹¹

As expected, in virtually every state, the model predicts less of a decline in participation than was actually observed even after accounting for the rise in work and earnings of single parents. Welfare reform had changed the rules. The states that had pushed caseloads down even further than one would have expected given the rise in work by single parents are presumably the ones that are acting most aggressively to move people off of welfare.

One might be concerned that this is simply a measure of the decline in participation in AFDC in the state over time. That is precisely why we are conditioning on the level of work and earnings of people in the later period. If non-welfare factors were pushing up employment and earnings and those rises were in turn reducing caseloads in a welfare environment that was unchanged, our model should accurately predict the change in participation. Only if the structure of welfare had changed *for single parents of given earnings* should the prediction deviate. The more aggressively the state has reduced participation for a given level of income, the greater the deviation from the prediction. Indeed the correlation between changes in the actual and predicted participation is "just" .36 revealing that this measure is not simply a measure of caseload change. The state by state differences in actual versus predicted caseload declines and their standard errors are shown on Appendix Table 2. I do not report the full probit results for each state in the interest of space.

¹¹ In making these projections I set the trend value at 1992, effectively simulating eligibility patterns for

Inevitably, this measure suffers from some potential weaknesses. There are dangers that deviation from predicted participation is capturing unmeasured changes in the state environment that might be affecting both caseloads and employment patterns simultaneously. Although the functional form is rather loose for each state, it does impose some structure. If local economic conditions that are not captured by the state unemployment rate altered the likelihood that people of a given earnings would apply for welfare, the measure might be biased. It is unclear what the direction of the bias would be, however. On the one hand people might be more optimistic and thus less likely to seek out aid (meaning the prediction will include a positive correlation with unmeasured economic conditions). On the other hand the newly working group has previous exposure with welfare and thus might be more likely to apply for welfare than previous low to moderate earning workers (creating a negative correlation between the trend and unmeasured economic conditions).

Another potential source of concern involves any correlation between AFDC aggressiveness and state EITCs. If states that are unusually aggressive are also more likely to have state EITCs, then aggressiveness may artificially capture some EITC effects. In fact, the reverse appears to be true. There were 7 states that had an EITC by 1996. Three of these were in states that are classified as least aggressive, three classified as intermediate, and only one, Wisconsin, was both aggressive in AFDC/TANF policy and had a state EITC.

1992.

The most serious concern may be that unmeasured individual characteristics may influence both employment and welfare participation. Thus if our sample for a particular state in 1997-1998 had a disproportionate share of single parents who were more inclined to work and were disinclined to accept welfare *for any given level of their earnings*, work would be higher and welfare use lower than we might have predicted--even given the higher level of work. This potential bias can be avoided by using one sample to generate the statistical aggressiveness measure and another to examine its link to earnings.

Inevitably this measure of aggressiveness is subject to error. Thus it would be a mistake to use this aggressiveness measure in any precise way. Instead I use it to break states into three categories. There seem to be a couple of natural breakpoints at -.04 and -.10. In other words, the least aggressive states show less than a 4 percent difference between actual and predicted participation and the most aggressive show a 10 percent difference. Using these breaks, roughly 1/4 of single parents were in the less aggressive states, 40 percent were in moderately aggressive states, and 1/3 were in the more aggressive ones.

Incentive Effects by Predicted Wage Quartile

Table 5 illustrates how different the work incentives are in 1986 versus 1998 for people in different quarters of the wage distribution. The first rows show the median level of earnings for all women (regardless of marital or family status) in the quartile who actually worked at least 26 weeks based on CPS data. These rows provide a rough estimate of what a woman in that quartile would earn if she went to work. The

second set of rows show the median amount husbands earn in cases where a woman in this wage quartile is married. These are not perfect estimates of what people might actually earn if they went to work. I use these figures only to illustrate roughly how different the incentives were across the groups and over time.

Look first at the situation facing single parents in the lowest potential wage quartile.

- Reinforcing the earlier finding, the table shows that a low skill single mother going to work could hope to earn just \$2,800 more (not counting the lost Medicaid) in 1986. Her effective tax rate was 76%. But by 1998, the gains to work had risen to nearly \$7,600 and her effective tax rate had fallen to 31%.
- Incentives for women in the next quartile also improved dramatically with the tax rate falling from 70% to 35%. Still this is not as great a change as for the lowest skill group.
- And the incentives for women in the highest wage/skill group changed the least. There was still an increased payoff, but the tax rate “only” fell from 55% to 37%.

Thus one should expect to see employment rates rising for unmarried mothers in all groups, but one should expect to see far greater increases at the bottom than the top. I will also compare the work patterns of low skilled unmarried mothers to low skilled unmarried women without children.

Next consider what happened to work incentives of married women.

- As we have already seen, incentives to work were sharply reduced for low-income women. By contrast, effective tax rates actually fell slightly for women in the other quartiles (mostly due to other tax changes). Women in the second quartile were affected the least. Thus I have a particularly good natural experiment. I can compare whether married mothers in the lowest quartile alter their work behavior relative to married mothers in the second and other quartiles.

In Table 5, I have only compared two years. The EITC has risen in several increments. Originally instituted in 1975, there was a modest jump in benefits in 1987, followed by sizable annual increases throughout the 1990s. Given the gradual ramping up of benefits and the potentially delayed response as people learn of the incentives, one would expect the behavioral responses to show up most dramatically in the 1990s.

Empirical Estimates of Work Effects for Single Mothers

The figures below examine the fraction of persons in each group who were working as of March in each year from 1980-1999. Results for work among single persons with and without children are presented in Figures 2 through 5.

- Figure 2 shows very large changes in work by the least skilled/lowest wage group of unmarried mothers. After virtually no change in employment patterns from 1980 to the early 1990s, suddenly employment rates have shot upwards, rising from roughly 34% in 1992 to 55% in early 1999. This truly unprecedented rise, which has been noted by numerous others including Liebman (1998), Dickert, Houser and Scholz (1995), Meyer and Rosenbaum (2000), and Blank, Card and Robbins (forthcoming), seems to offer powerful evidence that incentives can play a major role in work.
- As predicted, the levels of work also rose for women in the second quartile, though less than for women in the first, and so on up to the highest quartile.
- Work patterns changed little for one potential comparison group--unmarried women without children--as shown on Figure 3.

I perform a more formal test of the proposition that behavior really did change in a statistically meaningful way. I compare March 1986 with March 1999, based on the theory that the former was just prior to the beginning of the big growth in the EITC and the economy was stronger than in the preceding couple of years. The first four rows of Table 6 show what the figure also revealed: employment rose for all quartiles,

but it rose more for the lower quartiles. One sees no growth in employment among unmarried low wage childless women.

I then perform several treatment/control comparisons.

- I compare behavior of women in the lowest and highest wage quartile, the lowest and the third quartile (which would give a lower bound since both groups are affected by the incentives), and single women with and without children. Each of these comparisons yields an estimated effect of between 13 and 23 points. All are statistically significant.

Next I turn to the question of how significant a role welfare policy played in influencing this expansion in work among single parents. Figure 4 shows what happened to work (in three year moving averages) by single mothers in the lowest predicted wage quartile in states ranked by my statistical measures reform aggressiveness. Moving averages are used because the annual samples become rather thin. Similarly, I pool years in determining aggressiveness because sample sizes make most estimates of single year aggressiveness subject to considerable error. Pooling and using moving averages does pose a problem, however. The period from 1997 to 1999 was a time of most rapid change in welfare policies. While some states had begun reform years earlier through the waiver process, others were just becoming aggressive in the last year. Thus it is probably best to examine the impact of state aggressiveness through March 1998, particularly since I am using three-year moving averages¹². If, as seems plausible, some states which were previously less aggressive become more so over the 1998/1999 period, we would expect to see some convergence in employment patterns after 1998.

Prior to the late 1980s, patterns of work are relatively similar across the states, but then they diverge. Employment rates (three year moving averages) for the most aggressive states rise from .32 in 1986 to .53 in 1998. Employment starts at .34 in the least aggressive states, but rises only to .47.¹³ Table 7 indicates that this eight point difference in differences was statistically significant.

One concern with our measure of aggressiveness is that it may be biased because of sampling errors due to correlations in unmeasured propensities to work and to collect welfare at a given level of earnings. One can test for this problem by using the welfare aggressiveness measure on an alternative data source. Using data from outgoing rotation groups of the CPS for Jan-Feb and July-December from 1985 onward, one can explore employment patterns for single mothers who were never present in the March surveys--and thus represent a completely independent sample.¹⁴ I broke single parents into groups using the same wage model and examined the differences in employment gains in more and less aggressive states using the state aggressiveness measure generated using the March data. Table 7 indicates that the

¹² For 1999, where it is impossible to create a three year moving average, the value represents 2/3* the 1999 value and 1/3* the 1998 value.

¹³ As a test of whether these aggressiveness measures were somehow capturing other aspects of the state economic environment, I tabulated work patterns of unmarried women without children and of married mothers. As expected, I found no significant differences in work patterns by welfare reform aggressiveness in these groups.

¹⁴ The CPS sample includes a given household for four consecutive months, then skips them for eight months, and then includes them for another four. In effect they are interviewed for the same four months, two years in a row. At the end of both of these four month interview periods, they are members of the "outgoing rotation group" and special information is collected that, at least after 1984, allow one to determine the potential wage quartile and whether they were a female household head. Because of the nature of this sampling design, outgoing rotation groups from March to June includes members who were included in the monthly March CPS data. But those in outgoing rotations in other months were never a part of March samples.

outgoing rotation group shows a nine point difference in differences--virtually identical to the eight point gain found earlier.

Still, a closer inspection of Figure 4 suggests the results are not as robust as they might at first appear. If, for example, one used 1991 as a base year, one finds far less divergence in employment between the more and less aggressive states. Other years can heighten the effect. Moreover, there is some convergence in employment rates between more and less aggressive states in 1999 in both CPS and outgoing rotation group data. While this convergence in 1999 can quite plausibly be attributed to simple sampling variation or to formerly less aggressive states finally acting on reform, it may indicate that aggressiveness is changing rapidly and thus an average measure for the six year period selected may be subject to very considerable error.

I tried using these measures to conduct various decompositions of the relative magnitude of the economic strength (low unemployment), state aggressiveness, and the EITC and other work incentives. These proved to be quite sensitive to the comparison groups used and the time periods chosen.

I also explored other measures of aggressiveness--programmatic measures in a manner similar to Meyer and Rosenbaum (1999). The programmatic aggressiveness measures used to create Figure 5 show differences between more and less aggressive states, but the patterns over time show considerable variation, a finding that is consistent with the view that it is difficult to accurately measure the real changes in AFDC based on programmatic measures. Moreover, in the period prior to 1989, the trends in these states differed greatly. This pattern reveals that the measured impact

depends a great deal on which year is taken as the base. Since this measure mostly captures state changes up to 1996, one should not necessarily expect it to predict well after that period, but even in 1996, the moderately aggressive states seem to have had more impact on employment than the most aggressive ones.

State policies are changing rapidly over time. The methodology proposed here for determining statistical aggressiveness could, in principle, be used to create separate aggressiveness estimates for each state and year, but given the sample sizes, these would be subject to sizable measurement error. Indeed any measure of state reform--whether it be statistical or programmatic is certain to be subject to considerable measurement error, given the current state of data available for each. I suspect this renders the attempt to decompose the relative impacts of welfare reform, the economy, and the EITC perilous to impossible. One should be particularly cautious about attempts which begin by estimating the impacts of measured state variation in welfare and measured economic conditions, and then attributing the residual impacts to the EITC. Given the inherent difficulties in measuring state policy, such methods seem likely to understate the role of welfare changes.

Frankly, the whole exercise seems problematic in any case. State and federal policymakers would almost certainly have adopted a different set of policies in different economic conditions. The impact of the EITC will surely be different at a time when low paying jobs are plentiful than it would be if jobs were scarce. Recipients may be far more sensitive to welfare reform policies designed to get them working when the economy is strong and the rewards to work have grown so

significantly. Finally, the whole tenor of welfare reform, the changing social climate, and the strong economy may have dramatically increased the stigma associated with welfare receipt and failure to be working outside the home in all states--leading to changed behavior of recipients and caseworkers alike, irregardless of any actual changes in state policy. Thus the question of the marginal impact of particular policies may not even be meaningful--at least not in a sense that can be determined from existing behavior. Indeed, it is quite logical that the combination of welfare sticks, EITC carrots, and a remarkably strong economy had a multiplicative effect that is far greater than any one or two of these policies would have had on their own.

Empirical Estimates of Work Effects for Married Mothers

Figure 6 examines the work behavior of married mothers. Once again the responses are remarkably consistent with the altered incentives.

- Up until roughly 1988, the employment rates of married mothers at all potential wage levels were rising pretty much in tandem. Then abruptly the rises ceased for the low wage group only—the only group whose incentives were sharply altered by the EITC.

This change is particularly surprising in light of two other facts. The earnings of husbands in the lowest wage group were falling somewhat as the wage of less skilled men fell. That decline would ordinarily have been expected to lead to a disproportionate rise in the work of the low wage women. Second, the strong economy might have been expected to disproportionately benefit low skill married mothers, just as it seemed to help work by single mothers. Indeed, as shown in Figure 7, work among *childless* married women the bottom quartile rose relative to work of childless wives in higher quartiles. Thus change seems likely to have been caused by

changes in the EITC, especially since changes in the welfare system are not much of a factor. Married mothers virtually never would qualify for benefits before or after this period.

There is an alternative and even more powerful way to test the significance of the EITC. Some married women with low predicted wages are married to men with high enough earnings that the family does not qualify for the EITC whether or not the woman works. In other cases, the man's income is so low that work by the mother would actually increase the EITC benefits. One can exploit this natural experiment by comparing the employment patterns over time of low wage women whose incentives are adversely affected by the EITC to comparably skilled women who have neutral or positive work incentives. I simulated for all married women with children in my sample whether or not their EITC payments would rise or fall or be unchanged if they earned \$10,000 and had been facing the EITC rules as they existed in 1998. Thus I track over time the low wage women where the expanding EITC discourages work and compare them to other low wage women.

Throughout the sample period roughly 54 percent of married women in the bottom quartile would have faced 1998 EITC earnings penalties averaging \$1,288 (had they earned \$10,000 and faced the 1998 EITC provisions). These are typically women with working husbands who earn less than the EITC maximum. Another 28 percent would have had no work incentives from the EITC because their husbands' income was above the maximum. Finally, 18 percent would have had work incentives averaging \$2,678 because their husbands had low earnings.

Figure 8 shows what happened to work by women in each group over the period. Because married women in the three categories start with somewhat different levels of work in 1986, I have normalized the patterns relative to what they were for each group in that year to make visual interpretation easier. These results again appear to confirm strongly the earlier findings.

- As compared to 1986, work by married mothers in the bottom quarter where the EITC had a positive incentive on work rose the most, those for whom the EITC was neutral rose considerably less, and work rose least for those wives where the EITC penalized their work.

I should note that this result is somewhat more sensitive to estimation methods than others in this paper and one can see higher and lower estimates depending on the method used. And the timing is somewhat odd. There were some divergences in the late 1980s before the EITC was very large. And the biggest changes were in the past several years, somewhat after the time when EITC expansions occurred. Still it seems reasonable to presume that the effects of the dramatically expanded EITC in the mid 1990s would take some time to become more fully understood and experienced for married women for whom the impacts were less obvious. And the divergence in work by those with incentives to work less and others is striking.

In Table 8, I once again perform more rigorous statistical tests of what these graphs show. Here the tests are strong, but not quite as conclusive as before. All except the comparison with childless married women (a comparison that seems highly questionable given the already high rates of work in this latter group) show a depression of work from 3 to 7 percentage points. The 7 point estimate reflects the impact of the negative incentives only, while the 3 to 5 point estimate is for the low

wage group as a whole, including women facing positive and negative incentives (though negative incentives vastly outnumber positive in this group).

One estimate goes in the wrong direction: the comparison between low wage married women with and without children. A comparison of Figures 6 and 7 suggest this is likely to be a highly problematic comparison group. Employment rates were vastly higher for the group without children, and there has been essentially no growth in work for any of the wage quartiles since the mid 1980s, in sharp contrast to the pattern for married mothers. Thus I am inclined to accept the 3-5 point estimated range for low wage married women.

These results are consistent in direction with the projections of Dickert, Houser and Scholz (1995) and the findings of Eissa and Hoynes (1999). But the magnitudes are larger than the estimates of the latter who estimate that the labor force participation of married high school dropouts would have declined by somewhat more than one percentage point. This may reflect the fact that Eissa and Hoynes used married women without children as the control group with its attendant concerns.

One obvious question is whether the EITC on net, increased or decreased work by women when one combines the positive work effects for single mothers and the negative ones for married mothers. Based on the number of mothers in each group affected, the EITC still results in a net increase in work by women.

THE IMPACT OF THE POLICY CHANGES ON MARRIAGE AND COHABITATION ARRANGEMENTS

I now turn to an examination of how policy changes altered marriage patterns and cohabitation among single parents. My basic methodology is quite similar to that used for work. I begin by examining how incentives change for mothers in different quartiles of the potential wage distribution and then examine how those patterns changed.

Marriage Penalties and the EITC

The EITC creates a strong marriage bonus for low wage, non-working single parents. It creates an equally large penalty for a working single parent. An obvious question is whether the marriage rewards outnumber marriage penalties.

Bull et al. (1999) point out that any attempt to fully parameterize marriage penalties requires a comparison of how an existing or potential couple would behave if they were married or unmarried. Holtzblatt and Rebelein (forthcoming), Feenberg and Rosen (1995), Whittington and Alm (1997) and Congressional Budget Office (1997) all estimate the size of the marriage penalty based on the observed work earnings of men and women who are married with the assumption that these would be unchanged if the couple were not married. They then make further assumptions about how the children, deductions, and unearned income of the couple would be divided after marriage. Dickert-Conlin (1999) simulate marriage among low income unmarried women and men and separation among married couples and compare their tax liabilities before and after under the assumption that their work behavior would not change.

Still as an empirical matter, people's behavior clearly does change with marriage and some couples have children only after they marry. Thus in determining how many people might actually face penalties or bonuses, I find a variant on a method by Alm and Whittington (1997) particularly helpful. One can use data from the Panel Study of Income Dynamics (PSID) that allows one to observe reported income for each person in the year prior to marriage and after the marriage. In a sample of people who married before the EITC expansions had taken place, one can explore how many would have been rewarded or penalized by the EITC that was in place in 1996 based on their actual observed work behavior and family patterns before and after marriage. Had they been married in 1996, would their 1996 EITC have grown or diminished in the year after marriage? Since the 1996 EITC was not yet in place in the years we examine, it cannot have yet influenced behavior, and thus we can ask about the impact of the EITC absent behavioral change.

I observed 1,671 marriages (first or later) for women in the PSID between 1983 and 1991 that could be used in this analysis¹⁵. I limited the sample to marriages prior to 1992 to minimize the danger that their behavior had already been altered by the EITC changes that came later. I used information on income from the last full year prior to the year of marriage and the first full year after it to determine whether the couples would have been EITC winners or losers had the 1996 EITC provisions been in place when they married. I calculated what their combined EITCs would have been prior to marriage and compared that with their joint EITC after it.

¹⁵ A small portion of women married more than once in our sample period—all marriages are included.

One important feature of this method is that changes in the earnings or parental status of the partners following marriage may also affect the EITC. I would argue that the right question regarding the EITC for a couple contemplating marriage is whether or not their combined benefits will change after marriage after taking account of their likely choices regarding children and work if they did marry, and this method allows for behavioral changes that occurred after marriage absent the high EITC of the mid to late 1990s. But by not looking only one year forward, the method surely understates eventual winners because many people will wait a year or more to have children. Moreover, behavioral changes induced by the EITC would also tend to increase rewards and reduce penalties in actuality.

Table 9 is drawn from the PSID¹⁶. The results are rather striking.

- Marrying couples facing EITC marriage penalties outnumber couples getting EITC marriage rewards. The reason is simple enough. In 29% of the marriages in the sample, one or the other partner was living with a child prior to the observed marriage. In the large majority of those cases, both partners worked in the year prior to marriage. And in the bulk of those cases, marriage led to a decline in EITC benefits as the spouse's income reduced the benefits. Among partners where at least one partner is living with a child, losers outnumber winners, 16% to 5%
- There is another group that benefits from the EITC after marriage, however. Childless couples who marry and have a child in the year after marriage are often EITC winners. Thus in roughly 6% of marriages, the arrival of a child in the first year leads them to benefit from the EITC in ways they otherwise would have missed.
- Overall, 16% of marriages would have been EITC losers and 11% would have been winners.

¹⁶ In deriving this table, I ignored the very small EITC available to low earning, childless individuals.

- The size of the potential EITC penalties and benefits is not trivial, averaging almost \$1,400 in gains for the winners and \$1,500 in losses for the losers.

Holtzblatt and Rebelein (forthcoming) also find that among existing married couples, the presence of the EITC creates many more filers with marriage penalties in the tax system and fewer receiving marriage bonuses. On net, they report that the EITC increases marriage penalties by \$3.6 billion, though as noted, their method assumes no behavioral change among existing married couples.

The important question is whether these penalties and bonuses have had any behavioral impact. It has proven remarkably difficult for social scientists to reach a definitive consensus about the influence of social policies on marriage and family formation. In his fine review of the literature, Moffitt (1998) concludes that the current literature on the impact of AFDC on family structure is at best mixed, with somewhat inconsistent cross-sectional and time series patterns. He believes that the evidence hints at some very modest impacts of social policy on family structure, but the findings remain scattered and often contradictory. The only randomized experiment that found an impact of financial incentives was the Negative Income Tax, and even its findings remain highly controversial.

Unlike the case of labor supply, relatively little work has been done on the impact of the EITC on marriage and separation. In perhaps the most important work to date, Dickert-Conlin (1999) examines the impact of taxes and transfers on the decision to end a marriage. Using longitudinal data, she tracks divorce patterns, exploring whether persons with high marriage penalties are more likely to divorce.

She finds that divorces are indeed slightly more common in the penalized group. With this methodology, she can only examine behavior of those already married, not whether people marry more or less in the first place. And with this sort of longitudinal work, since marriage penalties chiefly arise when husband and wife incomes are similar, it may be hard to correctly model the impact of tax penalties versus labor supply patterns of the couple, though Dickert-Conlin attempts to do so using instrumental variables.

Dickert-Conlin and Houser (1999) seek to examine the overall female headship decision. They seek to parameterize financial incentives in AFDC and the EITC and examine the connection between these changes and female headship using a rather limited set of independent variables. They use aggregate measures of AFDC and EITC generosity. They find little impact of the EITC on female headship of either whites or blacks. They use a reduced form specification and the results appear to be somewhat sensitive to specification.

Most recently Schoeni and Blank (2000) compared the changes in marriage and female headship rates between waiver and non-waiver states among low education women and found evidence that early welfare reforms influenced marriage. On the other hand, they found very little evidence that TANF welfare reforms had any impact on family formation.

Here I once again use the natural experiment created by EITC expansion and AFDC contraction to look for behavioral effects in CPS data. Indeed, the 1990s seems to have been a time of changed marriage incentives as well as work incentives. The

EITC expansion was accompanied by a dramatic change in welfare policy. Thus, for very low income women on welfare who are not working, there is far more incentive to marry than before; welfare is less available and the EITC rewards marriage between a non-working parent and another working childless individual. Meanwhile, somewhat higher skill women who would likely work if they were single, face suddenly increased marriage penalties.

Table 10 illustrates marriage penalties and rewards for people of different wage percentiles under different conditions in the CPS. For the purposes of developing this table, I again assume that women who work earn the median for their wage group and that if they marry, they will find a man who will earn the median level of husbands of currently married women. In reality, currently married men are undoubtedly a select group and their wages may be somewhat higher than what an unmarried woman might expect from the remaining men, but for simple illustrative purposes, these estimates seem adequate.

The first and second major rows show marriage penalties under different conditions. Two striking features emerge immediately.

- In every case, whether the mother was working or not, regardless of whether the mother is in the top or bottom of wages, there is a financial penalty to marriage. Of course these are not all the result of the EITC. The impacts of means tested transfer programs at the bottom and tax policies at the top are an important part of the story.
- Incentives for working and non-working mothers changed dramatically over time and in opposite directions. After 1986, one sees a significant reduction in marriage penalties for non-working mother and a modest rise in them for working ones.

What is the overall impact of policy changes on women in each group? That depends on the probability a woman in each category would be working if she were a single parent. Very low skill women are less likely to work if they are single parents. For such women, the expanded EITC creates, on net, an incentive to marry. Higher skill women are more likely to work. For them the penalty is dominant.

The bottom line on marriage incentives by wage group in 1986 and 1998 is shown on the bolded bottom rows of Table 10.

- For the lowest quarter of women, on average marriage penalties have been reduced over time. In 1986 marriage meant enduring a marriage penalty equal to 20% of combined net income. By 1998, the penalty was down to 14%. Note this reduction in penalties would be even greater if the potential mates had lower incomes than those of current married men.
- For all other quartiles, the marriage penalties increased since 1986. In absolute terms, these increases were pretty large. But as a percent of combined income, the changes were relatively modest—1-3% of net income.
- Since marriage incentives grew by 4% of combined income (or nearly \$1000) for the bottom quartile and fell by 3% for the next one, and since welfare reform was sharply reducing opportunities for low wage women to support themselves via welfare, it appears that marriage incentives for the bottom quarter increased relative to those in higher quartiles. We should thus expect marriage to grow in the bottom group relative to the others.

These incentives should operate both on marriage and divorce. The fraction of persons who are married and living with a spouse reflect the impact of both these flows. If these incentives are influencing marriage, one should see the fraction married in the very bottom group to grow relative to the others.

It is important to recall the earlier discussion which suggests the true influence of the EITC and other policies is likely to be far more complex than the simple calculated marriage penalties shown here. The added income and reduced stress from an EITC might actually stabilize a low income married couple, even if it creates an apparent marriage “penalty.”

Still, it seems natural to compare the marriage patterns of mothers at the bottom with those of other mothers to see if there is any evidence of changing marital behavior in the face of the rather monumental changes in social policy. Figure 9 shows the fraction “married-spouse present” in each of the wage groupings.

- This graph shows little evidence that marriage patterns are changing in response to the new incentives. Instead of rising relative to the other marriage rates, the marriage rates for those in the bottom wage category seem to be still falling. There is some possibility that the trend has slowed somewhat in the past few years, but certainly no dramatic changes have been seen.
- One striking feature of Figure 9 is that marriage rates among women with children are rapidly becoming more unequal. The marriage patterns at the top have changed little, but at the bottom they are in sharp decline.

It is also possible that the incentives have slowed what would have been an even faster decline. The only way to test that is to find an adequate control group. Figure 10 shows the marriage patterns for childless women. The pattern is less consistent there, but one sees a spreading as well. For a variety of reasons, including the fact that marriage proportions among childless women are vastly smaller, I do not think this is a very good control group for this purpose.

One can also look to see if the aggressiveness of welfare reform has influenced marriage patterns. If welfare is powerfully influencing marriage, then the sharp cutbacks in aggressive states would be expected to influence family structure.

- I also find no relevant difference in the change in the odds of marriage or cohabitation between states with more aggressive welfare reforms and less aggressive ones, as can be seen in Table 12 below.

Table 11 confirms statistically what is evident already from the graphs.

Generally one finds insignificant results when one tries to compare the marriage patterns of the lowest wage group with other wage groups or with childless women. The results all have the opposite sign of what was expected.

But in interpreting these results, one should keep in mind that age is used in determining predicted wages, so that women in the lower wage quartiles tend to be younger than those in higher quartiles. Thus in comparing lower and higher wage women, one is partly comparing younger and older women.¹⁷ If younger women were postponing marriage for reasons unrelated to the EITC, the age differences might obscure EITC patterns. To test this hypothesis I generated Figure 11, limiting the sample to women age 24 to 44.¹⁸ Here a somewhat more interesting pattern emerges. Although there is year to year fluctuation, after falling until the early 1990s, the fraction married among the lowest wage quartile does seem to have flattened out in the past 6 or 7 years. By contrast, marriage percentages continued falling for the second and third quartiles. The differences in these trends are simply too tenuous to draw

¹⁷ Note that unlike marriage results, work results described earlier are essentially unaffected by limiting the sample to particular age groups.

¹⁸ Further segregating the sample into 25-34 and 35-44 reveals patterns that are similar to the one shown here.

strong conclusions. Yet there is at least some possibility that the decline in marriage among the lowest skill women has been slowed among women over 24 by the social policies of recent years. Note that confining the sample to this group of older women does not affect the finding that welfare aggressiveness seems unrelated to marriage patterns.

The spreading of marriage patterns shown in Figure 9, and the differences in patterns for the young and old, strongly suggests that our model is incomplete and that other factors are influencing marriage and may be obscuring the results. For example, if the declining fortunes of low skill men, especially younger men, have reduced the appeal of marriage, then trends in male earnings may obscure the impact of tax policy. It may also be that the combination of marriage rewards and penalties are confounding our estimation. Fortunately there is another more powerful way to test the power of the EITC and AFDC incentives to affect marriage.

The Influence of the EITC on Marriage Versus Unmarried Cohabitation

Couples often cohabit without being formally married and are doing so in increasing numbers. In general, if they live together without being married, they will be treated by the AFDC/TANF system and the tax system as though they were single.¹⁹ This offers a wonderful experiment. With CPS data it is possible to observe

¹⁹ Not surprisingly the story can get complicated. Alm et al. (1999) and Moffitt, Reville and Winkler (1998) note that in theory both the AFDC/TANF system and the tax system distinguish between cohabitation with the father of the children or an unrelated male, though my understanding of actual AFDC practice differs from their interpretation of the actual rules. Rather than include the father as part of the filing unit when the couple is not married, states may often elect to establish paternity and collect child support. States rarely look carefully at the contributions of other relatives in the home for AFDC cases, but benefits could be reduced somewhat if the family reported that a cohabiting adult was contributing money toward the rent. As Alm et al. properly note, the IRS has rules about who can claim the EITC when a cohabiting child is living with two adults. If the man is the father and lives with the

couples living with children who are living together--either married or as unmarried cohabiters. Since this latter group has already taken the step of living together, many factors influencing choices of adults to live together have already been accounted for. But a question often remains: whether to formally marry or not. This group seems the one most likely to be sensitive to economic incentives for marriage.

I begin by creating a consistently defined series on cohabitation from the CPS. (The rules I used for cohabitation are described in the appendix.) I calculate for each couple with children (whether married or unmarried cohabiters) whether the couple would have faced a marriage penalty or reward under the 1998 EITC rules. In this work I assume that each person's earnings would be the same regardless of whether the couple was formally married, that only the mother is the legal guardian of the children, and that she is the one claiming the EITC if they are not legally married.²⁰

If the EITC was influencing behavior, one would expect marriage to decline and unmarried cohabitation to rise amongst couples living together who begin to face a

child for at least 6 months, or if he is "caring for the child as his own" and lives with the child for a full 12 months, he can claim an EITC on the child if the mother has no earnings. If the mother has earnings, the person with the high earnings must claim the child for purposes of the EITC. Note that unlike the married case, however, the other partner's income is not counted in determining the level of the EITC--thus there remains a marriage penalty in cases where both adults have income. I know of no information on the number of cases where an unmarried father is legally claiming the EITC of a child who is legally in the custody of the mother or men who claim it for children they "care for as their own". Paternity has been legally established in a minority of cases of single parents. Moreover, I suspect these rules are rarely applied in practice and they still leave EITC marriage rewards and penalties in place for unmarried couples. Finally, I know of no reliable data on the fraction of cohabiters who are fathers of the children among low-income couples, much less a breakdown by earnings of each partner.

²⁰ Using the table from Alm et al. (1999), one can see that this assumption will roughly lead to the correct measure of marriage versus cohabitation rewards in every case except when the cohabiting man is the father of the child and the mother is not working and the father goes ahead and claims the EITC for the child. While I have no evidence on the incidence, I would suspect such cases are quite rare. I experimented with assuming that the high earner always claimed the EITC if a child was living with

large EITC penalty in the later years. Conversely, one would expect marriage to rise or at least fall less sharply among couples who face an EITC marriage reward. Figure 12 and Table 12 show the results:

- Among couples living together with children, marriage (as opposed to unmarried cohabitation) fell just as much in settings where the EITC rewarded marriage as where the EITC penalized it until 1996. Then, rather dramatically, marriage turned up among cohabiting couples that have a marriage reward.

The trend on Figure 12 among those getting rewards only really becomes evident with the addition of the last year's data. The rise relative to those with penalties is statistically significant. The timing is slightly odd, since most of the EITC changes had peaked by 1996, but this may be another case of people learning the incentives only gradually. Still these data do seem to suggest that EITC incentives may influence cohabitation versus marriage decisions.

These results should be treated cautiously. The percent married among cohabiters rose from 89 to 93 percent in these data, certainly not a massive change. The change is only seen in the last two years of data and it mostly represents greater marriage among the group of single mothers who are cohabiting and not working. This may simply be the result of changing work patterns of unmarried women--fewer non-working women were single, so fewer non-working cohabiters were single.

There is another way to look at this same question: compare the cohabitation rates of working and non-working low skill unmarried mothers. A working single parent now faces a larger marriage penalty than in previous years, thus if she would

two adults of the opposite sex and recalculating penalties and rewards. The results were similar to those reported.

like to live with a man, she would often be financially better off cohabiting outside of marriage. By contrast, a non-working single mother faces far less marriage penalty than before and thus would be better off marrying rather than cohabiting. Thus we might expect the fraction of single mothers who were cohabiting to diverge between the working and non-working mothers.

Figure 13 shows this pattern. Interestingly, after running together until the early 80s, the rates of cohabitation diverge sharply in the mid 1980s (when Reagan era welfare cuts and tightening rules regarding eligibility may have played a role), then they rise in parallel, and finally they diverge again in the past few years. A more convincing test will await longitudinal data to examine how marriage patterns of individuals changed over time as the incentives they faced changed. Nonetheless, the changes in apparent cohabitation patterns are intriguing. It is possible that marriage and cohabitation patterns are changing slowly.

In contrast to the changes in work, changes in marriage and living arrangements are more subtle. So far, there has been no dramatic change in marriage patterns, especially among the young. Nonetheless, there is the hint of possible family change beginning to occur in these results, at least for older women and women who are already cohabiting. The large EITC, perhaps coupled with welfare reform, may yet lead to somewhat greater marriage rates.

Conclusion

This examination of incentives and behavioral responses points to several findings regarding the EITC and welfare reform.

- The combination of the higher EITC, welfare reform and a strong economy has led to a truly unprecedented increase in labor market activity by low-income single parents.
- Since the late 1980s, labor market work by low wage married mothers has not increased in the way that work of other groups of married mothers has. Neither social policy changes nor the economy should have produced reduced work, so the income effects and adverse work incentives of EITC seem the most likely cause.
- Though the EITC sharply reduced marriage penalties and welfare reform has pushed many people off welfare, there is no dramatic increase in marriage or decrease in cohabitation among the lowest skill single mothers. But there is at least some possibility that marriage and cohabitation patterns have been changed slightly, especially among older women.

The interpretation of whether these results are reassuring or somewhat troubling depends on one's attitudes toward work and marriage among mothers. The fact that the EITC really does help working poor and near poor families is consistent with recent public opinion in the U.S. that the working poor are among the most deserving. And in the U.S. context where the nation seems to have concluded almost unequivocally that single mothers should work outside the home, the first finding in the list above should be welcome news. After years of being employed in lower proportions, single mothers are employed at the same rate as married mothers are. However, some observers may rightly worry about whether children are being helped or harmed by this rapid move into the labor market.

Whether the apparent reduction in work among married mothers is seen as good news or bad is likely to vary. Some believe that social policy ought to do more

to enable *married* mothers to remain home and nurture their children. Others will be troubled by the distortions in behavior and the danger that married mothers may lose out in the long run.

The possible results on marriage coupled with work reductions of married women seems to echo a very recent finding of the Minnesota Family Investment Program (MFIP) as evaluated by Manpower Demonstration Research Corporation and reported by Knox et al. (2000). In that experiment, a program of strong encouragement to work coupled with higher benefits to working two parent families lead to somewhat reduced work by wives and to lower levels of divorce and separation as compared to a randomized control group.

I did not find any real evidence that the EITC marriage penalties were reducing marriage. Nonetheless social policymakers may still want to remove penalties to marriage based as much on symbolism and fairness as on any observed behavioral response.

Some might use the existence of marriage penalties as an argument for cutting back the EITC. Such a plan would have significant consequences. The EITC supports low income working families with children. In an era of welfare reform, such aid may be especially important. The EITC clearly creates a marriage reward for the poorest single parents. The other alternative for fixing marriage penalties is to work on modifications in the design of the EITC.

The EITC's positive work and marriage incentives result from the fact that people with no earnings get nothing, while those with modest earnings get significant

benefits. One needs to work or be married to someone who works in order to qualify for the credit. The negative incentives mostly come as a result of the phase-out, specifically the fact that the income from one spouse can lead to a reduction in the EITC for the other spouse. Exploring alternative designs is beyond the scope of this paper. At a time when removing marriage penalties in tax policy affecting higher income families is a popular notion, it seems strange to ignore marriage penalties for those at the bottom of the income range.

Note that the incentives reported for the EITC apply to a host of other programs designed to provide aid to *low income working* families where low income is based on the *combined income of the family*. Any targeted program of this sort will show incentives like the EITC. Incentives for a first parent to find employment, incentives for a second parent to remain at home, incentives for a non-working parent to marry, and incentives for a working single parent to remain single will all be present. Since social policy in the U.S. is rapidly moving out of the traditional “welfare” type programs and into low wage worker supports, it behooves policymakers to look closely at this mixed group of incentives when designing new programs.

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Appendix

Rules for Creating Single Parents and Cohabitors

Single mothers with children can in principle be classified as primary family heads, related subfamily heads, and unrelated secondary family (now called unrelated sub-family) heads. Unfortunately, prior to 1982, there were errors in the way related subfamilies were formed due to the fact that the relation to the head variable created ambiguous situations. (See U. S. Bureau of the Census (1985)). A mother, two daughters, and a grandchild, would be classified as a "head," 2 "child(ren) of the head," and an "other relative of the head" (who was under 18). Thus it was unclear which of the two daughters was the grandchild's mother or even if some other non-resident was the parent. For many years, it appears that coders faced with this ambiguity often did not form subfamilies even when they should have. The situation was corrected in 1982/1983 when more detailed information on relationships was collected. As a result, there is a sudden jump in subfamilies after 1981 in the data. In time series work, this creates potential inconsistencies.

Thus as a starting point, I assigned related persons under 18 (who were not children of the head and not already in a related subfamily) to potential parents within the household. Children were assigned first to daughters of the head who were at least 14 years older than the child and no older than 44 themselves. When more than one daughter was a possible mother, the oldest qualifying daughter was chosen. If no daughters were found, the procedure next looked for sons and then to other relatives who still met the age criterion above. This procedure undoubtedly creates some

additional related subfamilies that would not be formed after 1983 when full data was available. And since we are interested in tracking trends, for consistency, I created additional "subfamilies" in the post 1983 period using this procedure as well.

The procedure seems to have worked well. In the years just prior to 1982, the procedure increased the number of subfamilies by close to 50%. In the post 1984 period, the procedure added just 7% to the total and time series trends now look sensible.

Various definitions have been used to define "Persons of the Opposite Sex in Shared Living Quarters" (POSSLQs) over the years. After the mid 1990s, more information was collected on the nature of relationships within the household. But prior to that time, POSSLQs had to be inferred. The standard Census procedure calls for forming POSSLQs when there were 2 and only two unrelated adults in a household. The reason for this restriction was to limit cases where groups of roommates were living together. But one weakness is that any family that included related adults (over 15) could not have a POSSLQ. Thus a mother and her 17 year old son could never be assigned a POSSLQ. Since adult children are living at home in greater and greater numbers, this restriction seems problematic. Moreover, it is quite possible for two POSSLQ couples to share an apartment. Thus I prefer a methodology similar to Moffitt, Reville and Winkler (1998) which does not limit POSSLQs to cases where there are only two adults or to families with only one unrelated adult as in Casper, Cohen and Simmons (1999).

Moreover, since I am seeking to create a consistent series over time, I use the same procedure to form POSSLQs after 1995 even though more complete data is available after that time.

A POSSLQ is formed if and only if two or more unrelated adults of the opposite sex separated in age by no more than 20 years are living in the same household. In cases where there were more than one possible POSSLQ, the pair closest in age were designated. POSSLQs may be formed with unmarried family heads, primary individuals, related subfamily heads, unrelated individuals, and unrelated secondary heads. The classification specifically allows for POSSLQs to be formed in cases where an individual listed as the head of household is living with an unrelated secondary family. Thus a man and his girlfriend and her daughter living in "his" house could be designated as a POSSLQs. Similarly, in rare cases a related subfamily can have a POSSLQ if there is an appropriate unrelated secondary individual or family of the right age and sex in the family and a POSSLQ cannot be formed with the household head.

In later years, I can examine the self-reported status of persons who were designated as POSSLQs using this procedure. Roughly 94% of those who classified themselves as a "partner" in 1999 were classified as POSSLQs using this procedure. Of those classified as POSSLQs, 64% report themselves as "partners", 24% report the somewhat more ambiguous designation as "roommate", 8% report the non-specific "non-relative of the head", and 4% report themselves as boarders. Not surprisingly then, some designated POSSLQs are simply "roommates," though one cannot be

certain whether some of the "roommates" and "non-relatives" preferred not to report themselves as "partners." I know of no reason to believe this upward bias varies over time. Note that limiting POSSLQs to cases where there is only one unrelated adult in the household excludes roughly the same number of self-designated "partners" (who one would want to include) as "roommates" (who one would like to exclude) and among families with children it mostly excludes persons who were self-designated "partners."

Figure 1
 Earned Income Tax Credit Payments in 1998 By Level of Earnings
 and Number of Children
 (Assuming No Other Income)

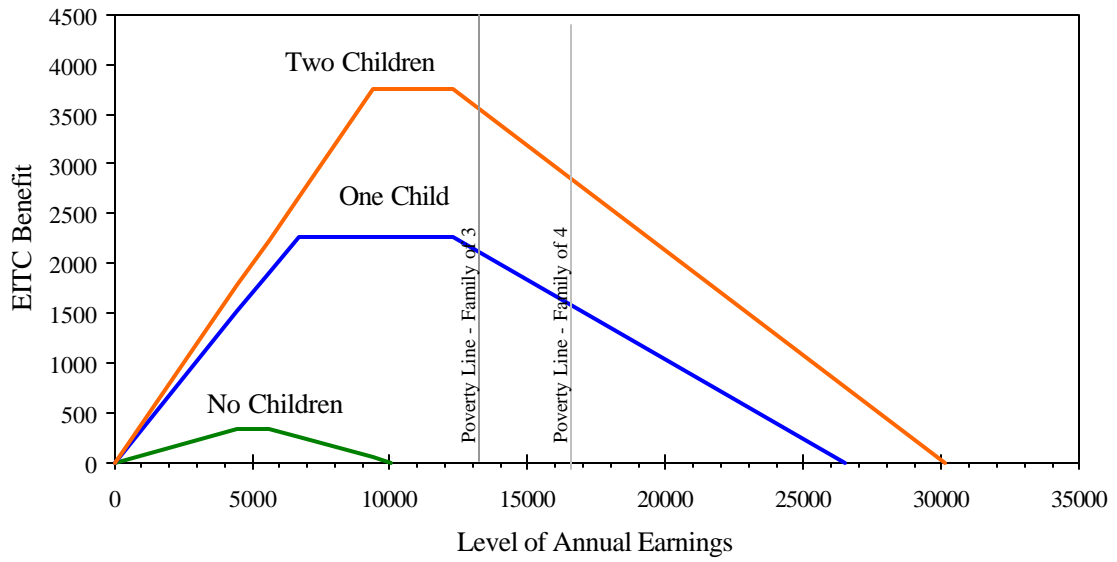


Figure 2
Employment Rates of Unmarried Mothers With Children
By Predicted Wage Quartile

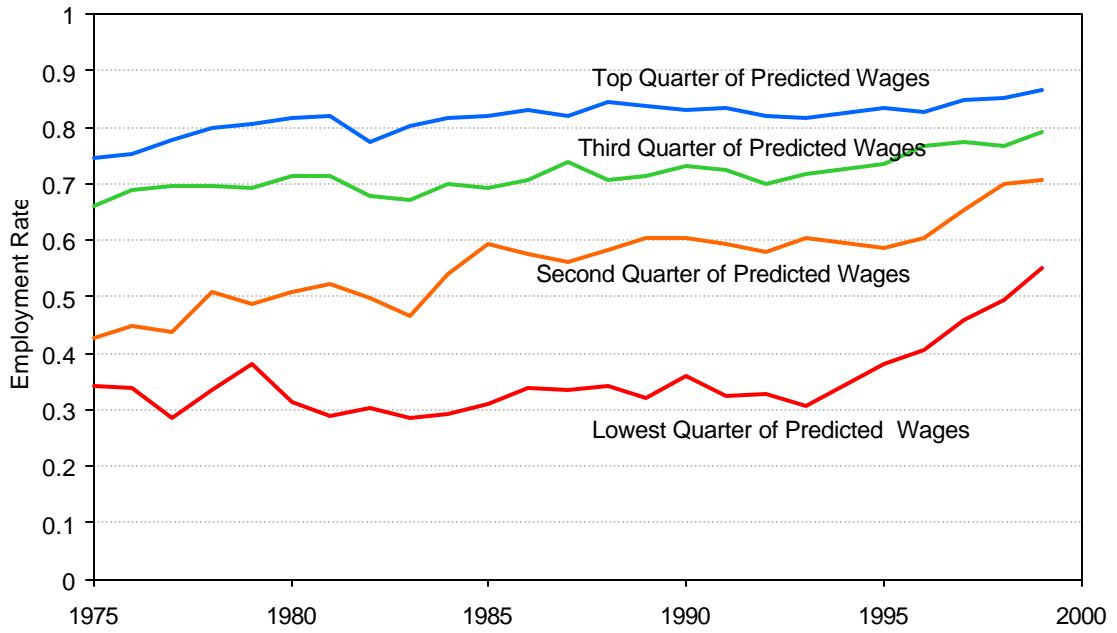


Figure 3
Employment Rates of Unmarried Women Without Children
By Predicted Wage Quartile

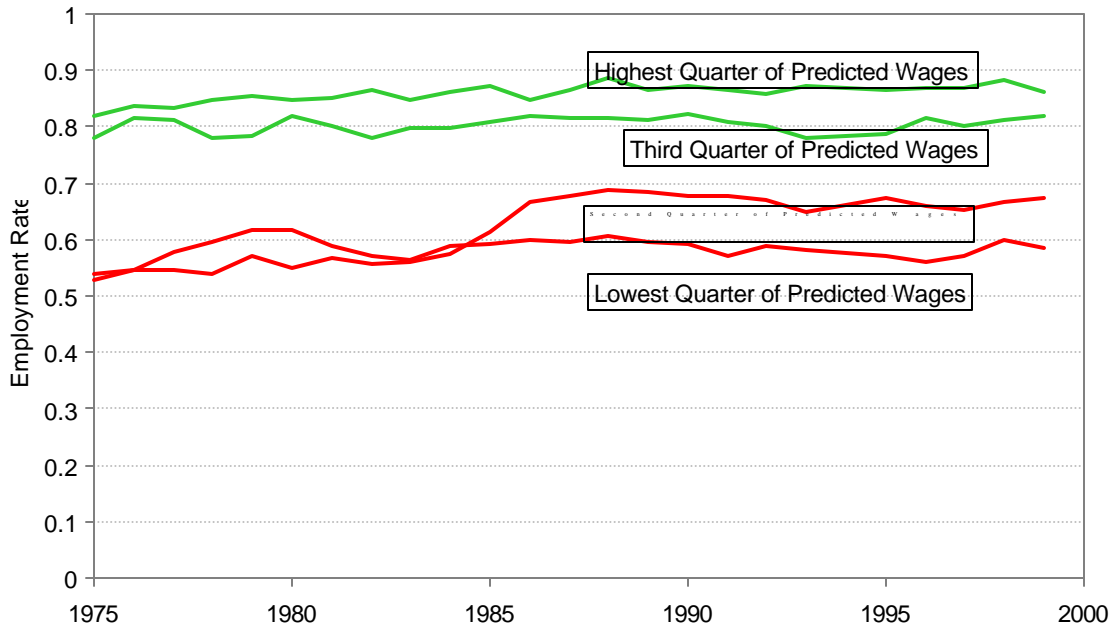
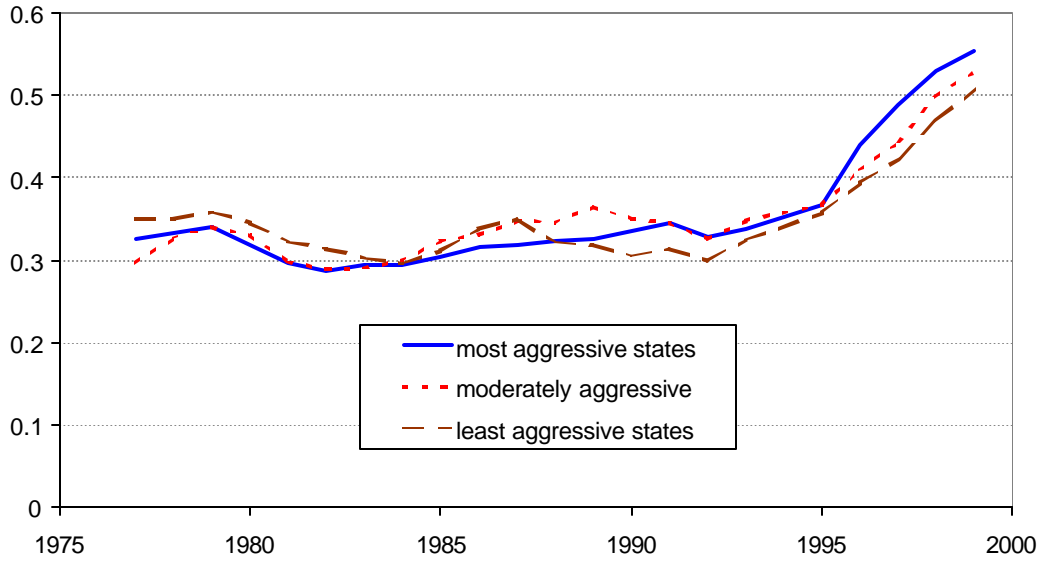


Figure 4
 Employment Rate of Single Mothers in the Lowest Predicted Wage Quartile by
 State Welfare Reform Aggressiveness-- **Statistical Estimation Method**
March CPS Data (3 year moving averages)



Note beginning and end years represent two year averages with the beginning or end year weighted double.

Figure 5
Employment Rate of Single Mothers in the Lowest
Predicted Wage Quartile by State Welfare Reform Aggressiveness
Program Parameter Method (3 year moving averages)

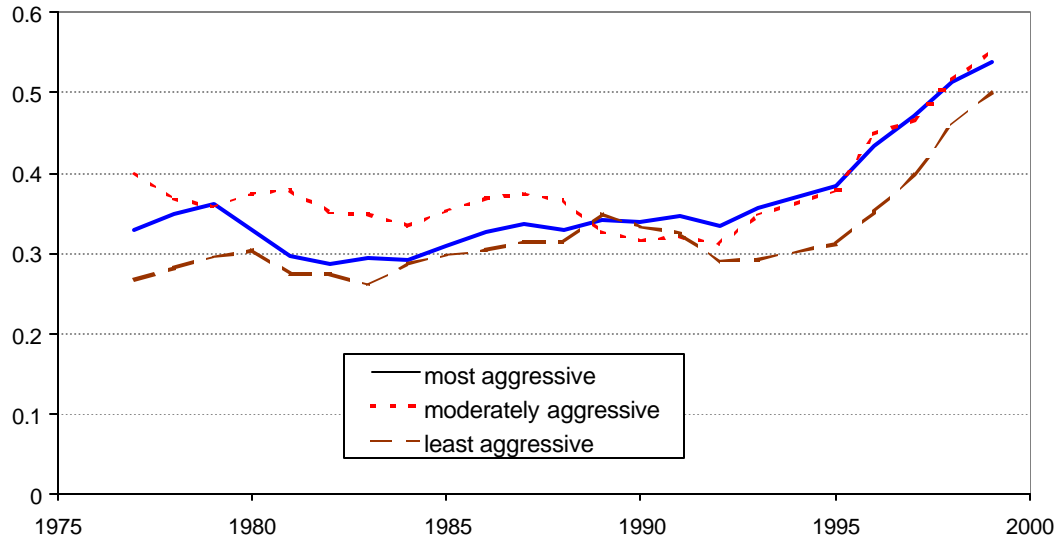


Figure 6
Employment Rates of Married Mothers
By Predicted Wage Quartile

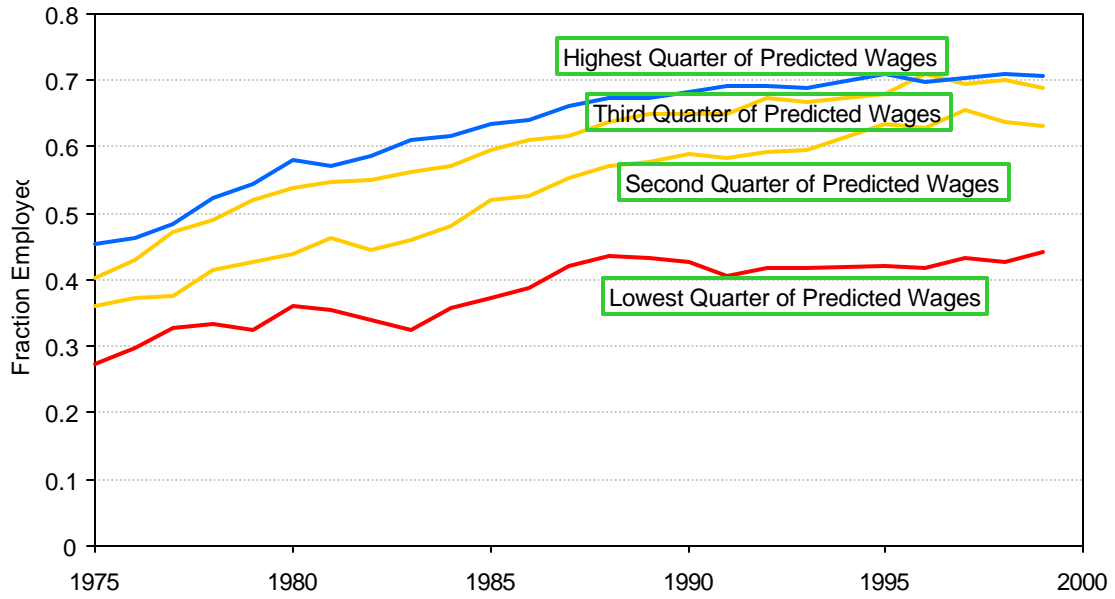


Figure 7
Employment Rates of Married Women Without Children
By Predicted Wage Quartile

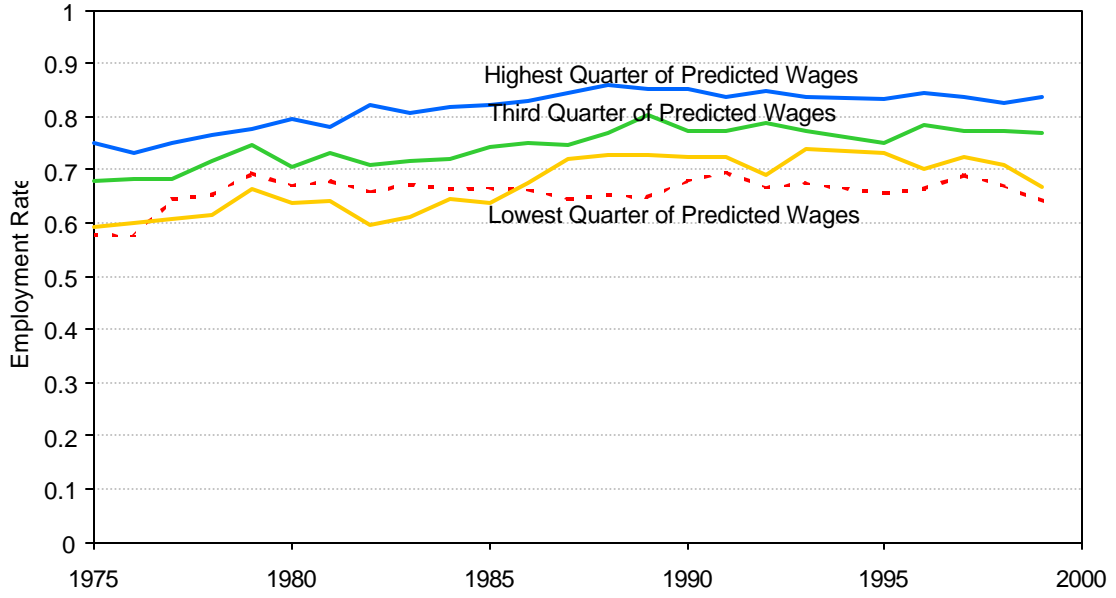
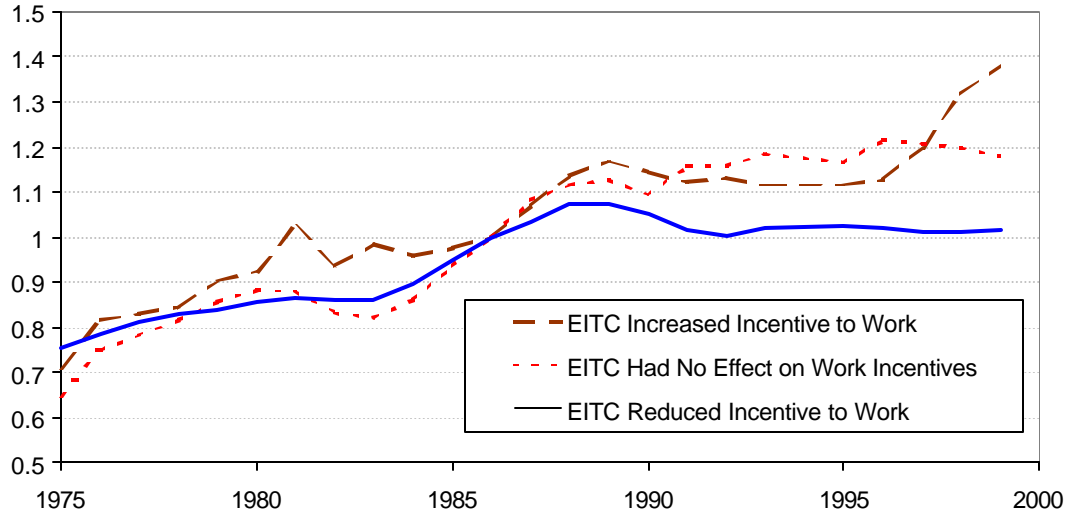


Figure 8
 Employment Rates Relative to 1986 for Married Mothers
 With and Without Disincentives to Work Due to EITC
 for Women in the Lowest Quarter of Predicted Wages
 (Three Year Moving Averages, 1986==1)



Note beginning and end years represent two year averages with the beginning or end year weighted double.

Figure 9
Fraction Married-Spouse Present Among Women Aged 18-44
With Children By Predicted Wage Position

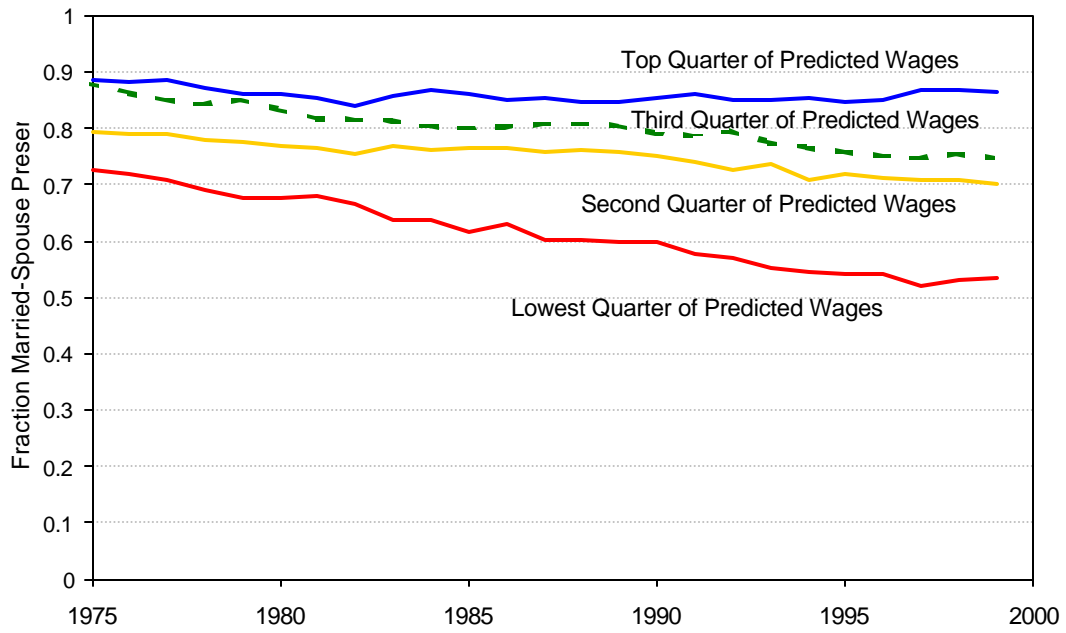


Figure 10
Fraction Married-Spouse Present Among Childless Women
By Predicted Wage Position

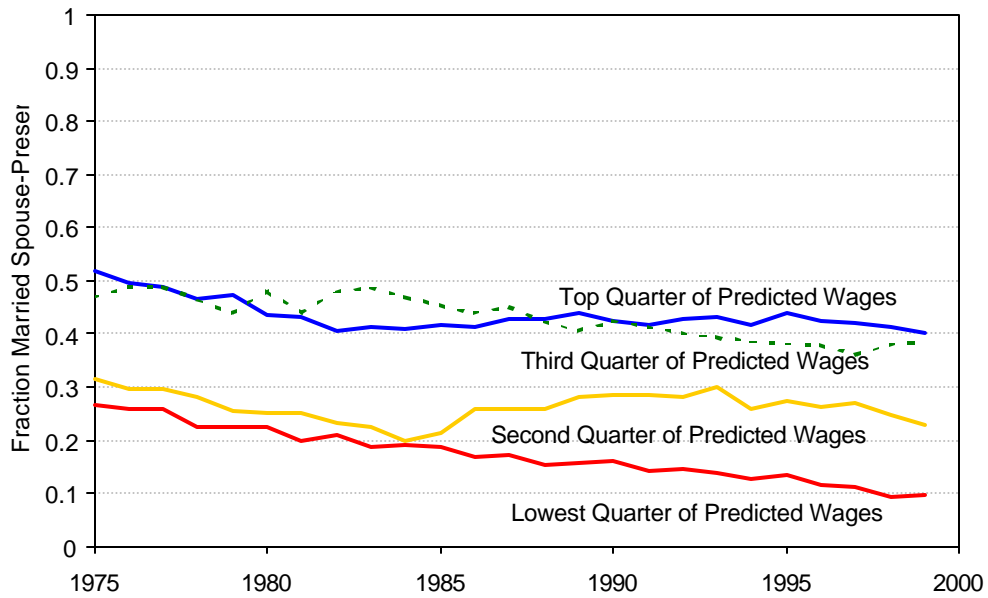


Figure 11
Fraction Married-Spouse Present Among Women Aged 25-44
With Children By Predicted Wage Position

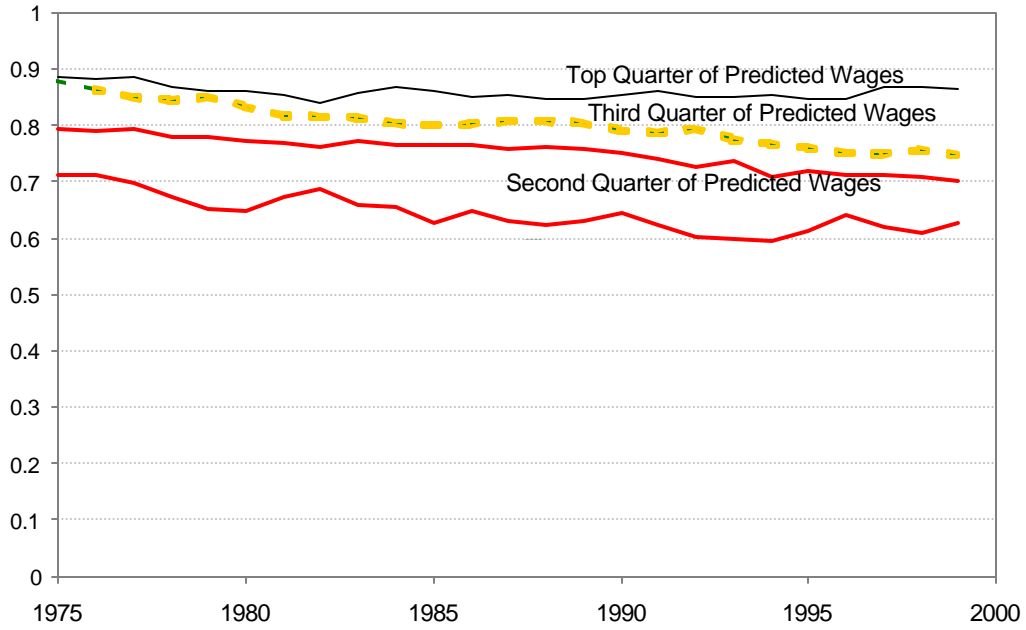


Figure 12
 Percentage of Cohabiting Mothers Who Are Married
 by Whether EITC Penalizes, Rewards, or Is Neutral With Respect to Marriage
Mothers in Lowest Quarter of Predicted Wages (3 year moving averages)

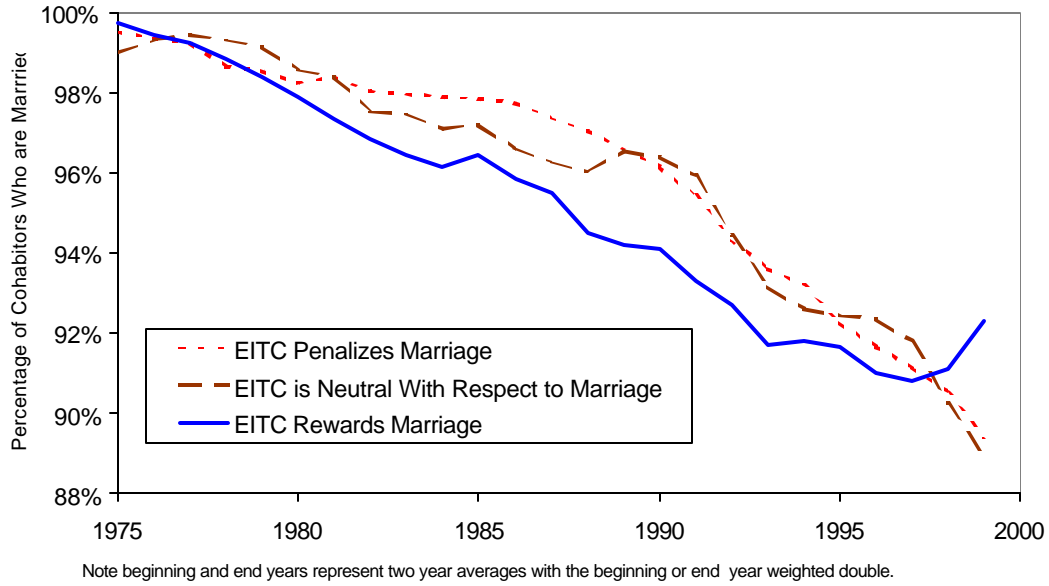


Figure 13
Percentage of Unmarried Mothers in the Lowest Wage Quartile
Who Are Cohabiting By Work Status
Three Year Moving Averages

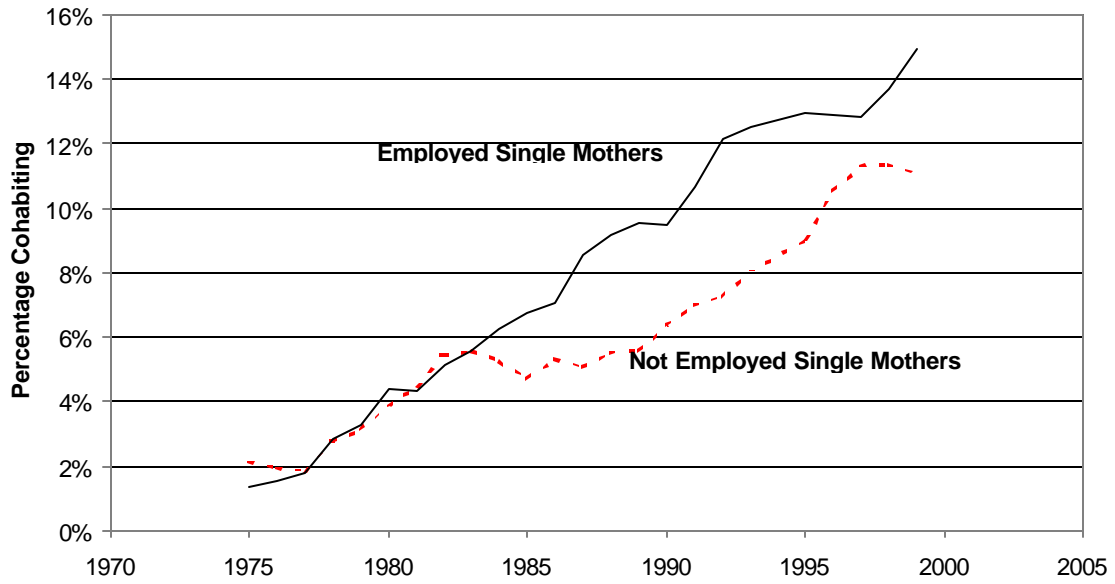


Table 1
Earnings, Taxes, and Benefits Under Differing Scenarios For Work and Marriage in 1986
(All figures in 1998 dollars)

Marriage and Work Scenario	Total Earnings	Federal Taxes: Social Security, Medicare and Income Taxes Other Than EITC	Means Tested Benefits: AFDC and Food Stamps	Child Care Expense (If All Parent(s) Work)	Earned Income Tax Credit	Child Care Support (Dependent Care Tax Credit)	Total "Disposable" Income	Government Paid Health Insurance? (Medicaid)
Unmarried Woman With Children, Woman Does Not Work	0	0	8,804	0	0	0	\$8,804	Yes
Unmarried Woman With Children, Woman Earns \$10,000	10,000	-879	2,602	-2000	777	164	\$10,664	No
Married Couple With Children, Man Earns \$15,000 Woman Does Not Work	15,000	-1,415	1,862	0	166	0	\$15,613	No
Married Couple With Children, Man Earns \$15,000 Woman Earns \$10,000	25,000	-3,316	62	-2000	0	532	\$20,279	No
Unmarried Man, No Children Man Earns \$15,000	15,000	-2,376	0	0	0	0	\$12,624	No

Table 2
Earnings, Taxes, and Benefits Under Differing Scenarios For Work and Marriage in 1998
(All figures in 1998 dollars)

Marriage and Work Scenario	Total Earnings	Federal Taxes: Social Security, Medicare and Income Taxes Other Than EITC	Means Tested Benefits: TANF and Food Stamps	Child Care Expense (If All Parent(s) Work)	Child Tax Credit	Earned Income Tax Credit	Child Care Support (Dependent Care Tax Credit plus other aid)	Total "Disposable" Income	Government Paid Health Insurance? (Medicaid)
Unmarried Woman With Children, Woman Does Not Work	0	0	7,717	0	0	0	0	\$7,717	Yes
Unmarried Woman With Children, Woman Earns \$10,000	10,000	-765	2,602	-2000	0	3,756	1000	\$14,593	At least children under 15, older in some states
Married Couple With Children, Man Earns \$15,000 Woman Does Not Work	15,000	-1,148	1,862	0	0	3,179	0	\$18,894	At least children under 15, older in some states
Married Couple With Children, Man Earns \$15,000 Woman Earns \$10,000	25,000	-2,978	62	-2000	800	1,073	265	\$22,223	Children covered in some states
Unmarried Man, No Children Man Earns \$15,000	15,000	-2,355	0	0	0	0	0	\$12,645	No

Table 3
 Work Incentives for Married and Unmarried Mothers for 1986 and 1998
 (All figures in 1998 dollars)

Year	Change in Earnings	Change in Federal Taxes	Change in Means Tested Benefits	Change in Child Care Expenses	Change in Earned Income Tax Credit	Change in Child Tax Credit	Change in Child Care Subsidies	TOTAL excluding medical benefits	Effective tax rate on earnings	Change in Government Medical Coverage
Unmarried Mother With Two Children Begins Working and Earns \$10,000										
1986	10,000	-879	-6,202	-2000	777	0	164	1,861	81%	loses all coverage
1998	10,000	-765	-5,115	-2000	3,756	0	1000	6,876	31%	children <16 remain covered
Mother w/ Two Children, Married to a Man Earning \$15,000, Begins Working and Earns \$10,000										
1986	10,000	-1,900	-1,800	-2,000	-166	0	532	4,665	53%	no effect
1998	10,000	-1,830	-1,800	-2,000	-2,106	800	265	3,329	67%	Children <16 may lose coverage

Table 4
 Marriage Penalties and Rewards Employed and Non-employed Mothers in 1986 and 1998
 (All figures in 1998 dollars)

Year	Combined Disposable Income if Couple Does Not Marry	Change in Federal Taxes	Change in Means Tested Benefits	Change in Child Care Expenses	Change in Earned Income Tax Credit	Change in Child Tax Credit	Change in Child Care Subsidies	Total marriage penalty excluding medical benefits	Marriage Penalty as a % of Combined Disposable Income if Unmarried	Change in Government Medical Coverage
Non-employed Mother With Two Children Marries Childless Man With \$15,000 in Earnings										
1986	21,428	961	-6,941	0	166	0	0	-5,815	-27%	loses all coverage
1998	20,362	1,208	-5,855	0	3,179	0	0	-1,468	-7%	children <16 remain covered
Employed Mother w/ Two Children, \$10,000 in Earnings Marries Childless Man w/ \$15,000 in Earnings										
1986	23,288	-60	-2,540	0	-777	0	368	-3,010	-13%	no effect
1998	27,238	143	-2,540	0	-2,683	800	-735	-5,015	-18%	may lose coverage for children <16

Table 5
Work Incentives For Women in Different Family Situations and Potential Wage Levels for 1986 and 1998

Year	Women With Characteristics That Would Place Them in the <i>Lowest Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Second Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Third Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Highest Quarter</i> of Potential Wages in 1998
Median Earnings of Women Who Work More than 26 weeks (Based on Annual CPS data)				
1986	11,600	14,872	19,334	27,346
1998	11,000	15,000	20,000	30,100
Median Earnings of Husbands for Mothers Who are Married (Based on Annual CPS data)				
1986	19,334	29,745	36,392	44,617
1998	18,720	30,000	35,000	45,000
Work Incentives if Single Parent Goes to Work				
Net Earnings				
1986	2,767	4,489	6,716	12,341
1998	7,559	9,716	12,081	19,080
Effective tax rate				
1986	76%	70%	65%	55%
1998	31%	35%	40%	37%
Work Incentives if Married Mother Goes to Work				
Net Earnings				
1986	6,894	9,676	12,310	16,732
1998	4,300	10,383	14,270	20,307
Effective tax rate				
1986	41%	35%	36%	39%
1998	61%	31%	29%	33%

Table 6
Changes in Work By Unmarried Mothers and
Estimated Impact of Social Policy between March 1986 and March 1999

	Fraction Working		
	1986	1999	Difference
Unmarried Women With Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages in 1998	0.34 (0.01)	0.55 (0.01)	0.21 (0.02)
--Second Quartile of Potential Wages	0.57 (0.01)	0.71 (0.01)	0.13 (0.02)
--Third Quartile of Potential Wages	0.71 (0.01)	0.79 (0.02)	0.08 (0.02)
--Highest Quartile of Potential Wages	0.83 (0.02)	0.87 (0.01)	0.04 (0.02)
Unmarried Women Without Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages in 1998	0.60 (0.01)	0.59 (0.01)	-0.01 (0.01)
Differences Between Groups			
	1986	1999	Difference in Differences (Estimated Impact of Social Policies)
Bottom As Compared to Highest Quartile Among Unmarried Mothers	-0.49 (0.01)	-0.32 (0.02)	0.18 (0.02)
Bottom As Compared to Third Quartile Among Unmarried Mothers	-0.37 (0.02)	-0.24 (0.02)	0.13 (0.03)
Bottom Wage Quartile Unmarried Women With Children As Compared to Unmarried Women Without Children	-0.26 (0.01)	-0.04 (0.02)	0.23 (0.02)

Table 7
 Work By Unmarried Women With Children With Characteristics That Would Place Them In
 the Lowest Quartile of Potential Wages in 1998 between March 1985-7 and March 1997-9:
 March Data and Outgoing Rotation Group Data Excluding Persons Present in March

	Fraction Working March Data		Difference
	1985-1987	1997-1999	
--Living in Least Aggressive States	0.34 (0.01)	0.47 (0.01)	0.13 (0.02)
--Living in Moderately Aggressive States	0.33 (0.01)	0.50 (0.01)	0.17 (0.02)
--Living in Most Aggressive States	0.32 (0.01)	0.53 (0.01)	0.21 (0.02)
	Fraction Working Outgoing Rotation Groups Not Present in March Samples		
	1985-1987	1997-1999	Difference
--Living in Least Aggressive States	0.39 (0.01)	0.50 (0.01)	0.11 (0.02)
--Living in Moderately Aggressive States	0.33 (0.01)	0.51 (0.01)	0.18 (0.02)
--Living in Most Aggressive States	0.33 (0.01)	0.53 (0.01)	0.21 (0.02)
Differences Between Groups	1985-1987	1997-1999	Difference in Differences
Most Versus Least Aggressive --March Data	0.02 (0.02)	-0.06 (0.02)	-0.08 (0.03)
Most Versus Least Aggressive --Outgoing Rotation Group Data	0.06 (0.02)	-0.03 (0.02)	-0.09 (0.03)

Table 8
Changes in Work By Married Mothers and
Estimated Impact of Social Policy between March 1986 and March 1999

	Fraction Working		
	1986	1999	Difference
Married Women With Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages in 1998	0.39 (0.01)	0.44 (0.01)	0.05 (0.01)
--Second Quartile of Potential Wages	0.53 (0.01)	0.63 (0.01)	0.11 (0.01)
--Third Quartile of Potential Wages	0.61 (0.01)	0.69 (0.01)	0.08 (0.01)
--Highest Quartile of Potential Wages	0.64 (0.01)	0.70 (0.00)	0.06 (0.01)
Married Women With Children in the Lowest Potential Wage Quartile			
-- Women For Whom the 1998 EITC Created Work Disincentives	0.42 (0.01)	0.43 (0.02)	0.01 (0.02)
--Women For Whom the 1998 EITC Had No Effect on Incentives	0.39 (0.02)	0.47 (0.03)	0.08 (0.03)
--Women For Whom the 1998 EITC Created Positive Work Incentives	0.28 (0.02)	0.43 (0.03)	0.15 (0.04)
Married Women Without Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages in 1998	0.66 (0.02)	0.64 (0.02)	-0.02 (0.03)
Differences Between Groups		Fraction Working	
	1986	1999	Difference in Differences (Estimated Impact)
Bottom As Compared to Third Quartile Among Married Mothers	-0.22 (0.01)	-0.25 (0.01)	-0.03 (0.02)
Bottom As Compared to Second Quartile Among Married Mothers	-0.14 (0.01)	-0.19 (0.01)	-0.05 (0.02)
Married Mothers in Bottom With Disincentives Versus Married Mothers With No Effect on Incentives	0.11 (0.02)	0.04 (0.03)	-0.07 (0.04)
Bottom Married Women With Children As Compared to Bottom Married Women Without Children	-0.28 (0.02)	-0.20 (0.02)	0.08 (0.03)

Table 9
 Percent of All Marriages Between 1983 and 1991 Where the Couple Would Have Had a Higher, Lower, or the Same EITC Before and After Marriage Had the EITC of 1996 Been In Place At the Time

Work and Family Situation Prior to Marriage	EITC benefits would have been LOWER in year after marriage	EITC benefits would have been UNCHANGED in year after marriage	EITC benefits would have been HIGHER in year after marriage	TOTAL
At least one partner was living with a child in year prior to marriage				
At least one partner with child and did not work in year prior to marriage	0%	3%	2%	6%
both partners worked in year prior to marriage	16%	4%	3%	23%
Neither partner was living with a child in the year prior to marriage	0%	65%	6%*	71%
TOTAL	16%	72%	11%	100%
Mean Amount of Gain or (Loss)	(\$1,505)	\$0	\$1,367	(\$92)

This table is based on author's tabulations of 1671 marriages in the Panel Study of Income Dynamics. It compares the sum of what the partners could have individually received from the 1996 EITC based on their earnings and child status in the last full survey year prior to their marriage with the 1996 EITC the couples could have received based on the couples' earnings and child status in the first full survey year after marriage. The table does not include the impact of the very small EITC available in 1996 for persons without children.

*These are cases where the couple had a child in the year after marriage and thus became newly eligible for the EITC.

Table 10
Marriage Penalties For Women in Different Family Situations and Potential Wage Levels for 1986 and 1998

Year	Women With Characteristics That Would Place Them in the <i>Lowest Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Second Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Third Quarter</i> of Potential Wages in 1998	Women With Characteristics That Would Place Them in the <i>Highest Quarter</i> of Potential Wages in 1998
Marriage Penalty for Non-employed Single Parent With Two Children Who Marries an Employed Childless Man				
1986	-6,854	-7075	-6527	-5888
1998	-2,710	-6055	-5724	-4424
Marriage Penalty for Employed Single Parent With Two Children Who Married an Employed Childless Man				
1986	-2,727	-1888	-934	-1497
1998	-5,846	-4988	-3134	-2797
<i>Probability Women Will Work If They Are Single Parents</i>				
1986	0.34	0.57	0.71	0.83
1998	0.49	0.70	0.77	0.85
Average/Expected Marriage Penalty (Penalty for non-employed*probability non-employed +penalty for employed*probability employed)				
1986	-5461	-4096	-2565	-2242
1998	-4258	-5309	-3734	-3040
Average/Expected Marriage Penalty as a Percent of Combined Male-Female Disposable Income if Remain Unmarried				
1986	-20%	-11%	-6%	-4%
1998	-14%	-13%	-8%	-5%

Table 11
Levels and Changes in Fraction Married-Spouse Present Among Women Aged 18-44 in Various Situations Between March 1986 and March 1999

	Fraction Married- Spouse Present		Difference
	1986	1999	
Women With Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages	0.63 (0.01)	0.53 (0.01)	-0.10 (0.01)
--Second Quartile of Potential Wages	0.76 (0.01)	0.70 (0.01)	-0.06 (0.01)
--Third Quartile of Potential Wages	0.80 (0.01)	0.75 (0.01)	-0.06 (0.01)
--Highest Quartile of Potential Wages	0.85 (0.01)	0.86 (0.00)	0.02 (0.01)
Women with Children in the Lowest Potential Wage Quartile			
--Women in States with the Most Aggressive Welfare Reform Policies	0.59 (0.01)	0.49 (0.01)	-0.10 (0.01)
--Women in States with the Least Aggressive Welfare Reform Policies	0.66 (0.01)	0.60 (0.01)	-0.06 (0.01)
Women Without Children:			
--Women With Characteristics That Would Place Them In the Lowest Quartile of Potential Wages	0.17 (0.01)	0.10 (0.01)	-0.07 (0.01)
Differences Between Groups	Fraction Married- Spouse Present		Difference in Differences (Estimated Impact of Social Policies)
	1986	1999	
Bottom Quartile As Compared to Third Quartile Among Women With Children	-0.17 (0.01)	-0.21 (0.01)	-0.04 (0.01)
Bottom Quartile As Compared to Second Quartile Among Women With Children	-0.13 (0.01)	-0.17 (0.01)	-0.04 (0.01)
Bottom Quartile Women in Most Aggressive Compared to Least Aggressive Welfare Reform States	-0.06 (0.01)	-0.10 (0.01)	-0.04 (0.02)
Bottom Women With Children As Compared to Bottom Women Without Children	0.46 (0.01)	0.44 (0.01)	-0.03 (0.02)

Table 12
 Levels and Changes in Fraction of Cohabiting Couples Who Are Married
 Among Couples Where the Woman is in the Lowest Predicted Wage Quartile
 March 1985-1987 and March 1997-March1999

	Fraction of Cohabitors Who Are Married		Difference
	1985-1987	1997-1999	
Couples With Children and Woman is in Lowest Predicted Wage Quartile:			
--Couples For Whom the 1998 EITC Creates a Marriage Penalty	0.959 (0.004)	0.911 (0.006)	-0.047 (0.007)
--Couples For Whom the 1998 EITC is Neutral With Respect to Marriage	0.966 (0.004)	0.903 (0.009)	-0.064 (0.010)
--Couples For Whom the 1998 EITC Creates a Marriage Reward	0.978 (0.003)	0.905 (0.006)	-0.072 (0.007)
Differences Between Groups	Fraction of Cohabitors Who Are Married		Difference in Differences (Estimated Impact of EITC)
	1985-1987	1997-1999	
Couples Facing EITC Marriage Penalty As Compared to Couples Facing a Marriage Reward Bottom Quartile As Compared to Third Quartile Among Women With Children	-0.019 (0.005)	0.006 (0.009)	0.025 (0.010)

Appendix Table 1
 Regression Coefficients of Wages of Women Who Worked At Least 26 Weeks in 1998
 And Model Used for Creating Predicted Wages in All Years

Variable	Coefficient (Standard Error)
Age 25-34	0.332 (0.012)
Age 35-44	0.445 (0.012)
Blacks	-0.048 (0.012)
Other	-0.017 (0.019)
High School Graduate	0.263 (0.016)
Some Education Beyond High School	0.413 (0.016)
College Graduate	0.736 (0.017)
More than College	0.900 (0.021)
Number of Children Under 18	-0.025 (0.004)
Constant	1.593 (0.016)
R Squared	.251
Number of Observations	17402
Standard Error of the Estimate	.533

Source: Based on March 1999 CPS Data. Includes all women regardless of marital status. Wage is defined as total earnings divided by annual hours worked (weeks worked times usual hours worked). Excludes persons with calculated wages of less than \$1 per hour or greater than \$75 per hour.

Appendix Table 2
Measures of Welfare Reform Aggressiveness

State	Probability that a Single Mother Received AFDC in 1991/1992	Actual Change in Probability 1991/1992 to 1997/1998	Predicted Change in Probability 1991/1992 to 1997/1998	Difference	Standard Error of Difference	Statistical Aggressiveness (1=less aggressive)	Program Aggressiveness Derived from Meyer and Rosenbaum (1=less aggressive)
Alabama	0.28	-0.14	-0.01	-0.14	0.06	3	1
Alaska	0.34	0.00	0.01	-0.01	0.06	1	1
Arizona	0.30	-0.12	-0.09	-0.02	0.05	1	3
Arkansas	0.35	-0.19	-0.19	0.00	0.05	1	2
California	0.37	-0.10	-0.09	-0.02	0.02	1	3
Colorado	0.36	-0.24	-0.06	-0.18	0.05	3	2
Connecticut	0.46	-0.35	-0.25	-0.09	0.05	2	3
Delaware	0.33	-0.16	0.00	-0.16	0.07	3	3
District of Columbia	0.34	-0.02	0.02	-0.04	0.05	1	1
Florida	0.30	-0.14	-0.10	-0.04	0.02	2	1
Georgia	0.30	-0.14	0.01	-0.15	0.04	3	2
Hawaii	0.31	0.08	0.23	-0.15	0.06	3	2
Idaho	0.17	-0.09	0.03	-0.12	0.05	3	3
Illinois	0.39	-0.18	-0.08	-0.10	0.02	3	3
Indiana	0.32	-0.22	-0.09	-0.13	0.04	3	3
Iowa	0.32	-0.10	-0.03	-0.08	0.06	2	3
Kansas	0.34	-0.20	0.04	-0.24	0.05	3	3
Kentucky	0.43	-0.18	-0.15	-0.03	0.06	1	1
Louisiana	0.43	-0.21	-0.10	-0.11	0.05	3	3
Maine	0.42	-0.18	-0.06	-0.11	0.07	3	2
Maryland	0.30	-0.16	-0.07	-0.08	0.06	2	2
Massachusetts	0.43	-0.16	-0.02	-0.14	0.03	3	3
Michigan	0.44	-0.18	-0.08	-0.10	0.03	2	3
Minnesota	0.49	-0.13	-0.15	0.02	0.06	1	2
Mississippi	0.36	-0.27	-0.10	-0.17	0.04	3	2
Missouri	0.37	-0.10	0.00	-0.10	0.06	3	3
Montana	0.32	-0.18	-0.02	-0.16	0.05	3	3
Nebraska	0.49	-0.23	-0.09	-0.13	0.08	3	3
Nevada	0.13	-0.05	-0.04	-0.01	0.04	1	1
New Hampshire	0.38	-0.18	-0.25	0.06	0.07	1	3
New Jersey	0.32	-0.10	-0.10	0.00	0.03	1	3
New Mexico	0.48	-0.23	-0.06	-0.18	0.06	3	1
New York	0.48	-0.15	-0.07	-0.07	0.02	2	1
North Carolina	0.29	-0.16	-0.07	-0.08	0.03	2	3
North Dakota	0.41	-0.23	-0.13	-0.10	0.05	3	1
Ohio	0.36	-0.09	-0.05	-0.05	0.03	2	3
Oklahoma	0.31	-0.14	-0.18	0.04	0.06	1	2
Oregon	0.41	-0.19	-0.02	-0.17	0.06	3	3

State	Probability that a Single Mother Received AFDC in 1991/1992	Actual Change in Probability 1991/1992 to 1997/1998	Predicted Change in Probability 1991/1992 to 1997/1998	Difference	Standard Error of Difference	Statistical Aggressiveness (1=less aggressive)	Program Aggressiveness Derived from Meyer and Rosenbaum (1=less aggressive)
Pennsylvania	0.40	-0.16	-0.12	-0.04	0.03	2	1
Rhode Island	0.65	-0.15	-0.22	0.08	0.09	1	1
South Carolina	0.30	-0.23	-0.19	-0.03	0.04	1	2
South Dakota	0.25	-0.09	0.02	-0.12	0.06	3	3
Tennessee	0.41	-0.29	-0.11	-0.18	0.05	3	3
Texas	0.25	-0.12	-0.05	-0.07	0.02	2	3
Utah	0.27	-0.10	-0.03	-0.07	0.06	2	2
Vermont	0.45	-0.24	-0.22	-0.02	0.07	1	3
Virginia	0.35	-0.13	-0.04	-0.10	0.08	2	3
Washington	0.42	-0.19	-0.08	-0.10	0.05	3	1
West Virginia	0.52	-0.31	-0.16	-0.15	0.06	3	3
Wisconsin	0.55	-0.36	-0.14	-0.22	0.08	3	3

Source: Authors calculations.