# Are In-Work Tax Credits Effective in the Presence of Generous Public Assistance? Evidence from the 1975 Earned Income Tax Credit

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**Abstract**

It is well known that the Earned Income Tax Credit (EITC) expansions in the 1980s and 1990s had a positive impact on female employment. In contrast, the original 1975 EITC has commonly been described as a modest program that did not keep up with inflation; furthermore, during this time public assistance was twice as generous as it would be two decades later. For these reasons, there has been a general assumption that the incentives of the 1975 EITC were insufficient to induce much of a female employment response. In this paper I find strong evidence that the original EITC did have a positive and permanent impact on female employment. Using samples from two different populations of women, I use difference in differences to show that EITC-eligible women increased their relative employment by 3 to 5 percentage points (or about 5% to 8%). These findings are robust to triple differences analysis, model choice, state-specific time trends, choice of sample years, reweighting, and potentially endogenous fertility responses to the EITC. Subgroup analysis shows that this employment response varied by education, cost of living, race, age, marital status, and spousal income in a manner consistent with a standard labor supply model. The largest responses came from subgroups of women more likely to be EITC-eligible and women eligible for more EITC benefits. As far as I am aware, this is the first paper to empirically examine the introduction of the EITC and its first decade of existence.

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The EITC literature has repeatedly shown that single women increased their labor supply in response to EITC expansions in the 1980s and 1990s (Dickert, Houser, Scholtz 1995; Eissa and Liebman 1996; Meyer and Rosenbaum 2000, 2001; Keane and Moffitt 1998; Ellwood 2000; Grogger 2003; Hotz, Mullin, and Scholtz 2006; Eissa, Kleven, Kreiner 2008). In contrast, little is known about the effects of the EITC introduction in 1975. Until now no empirical study had been done on the first decade of this anti-poverty program.[1] The 1975 EITC has often been dismissed as too small to have had an impact on female employment and is only ever mentioned as an aside. Meyer and Rosenbaum (2001) write that "Between its beginning in 1975 and the passage of the Tax Reform Act of 1986, the EITC was small, and the credit amounts did not keep up with inflation." Eissa and Liebman (1996) state that the EITC "began in 1975 as a modest program aimed at offsetting the social security payroll tax for low-income families with children. After major expansions in the tax acts of 1986, 1990, and 1993, the EITC has become a central part of the federal government's antipoverty strategy."

In this paper I use difference in differences (DD) to show that the original EITC led to a permanent 5% to 8% increase in the employment of EITC-eligible women. The 1975 EITC provided a 10% income subsidy to working parents with annual earnings below $18,000 (2013 dollars) and altered the budget constraints of EITC-eligible adults whether or not they actually received EITC benefits. The treatment and control groups in this paper consist of EITC-eligible and EITC-ineligible women. The DD results are calculated by comparing regression-adjusted employment rates between treatment and control groups, before and after the 1975 program introduction.

Unconditional 1969-1985 employment trends reveal a trend break after 1975 for the treatment group but not for the control group. Between 1969 and 1975 the employment rate of women with kids was consistently 24 percentage points (pp) lower than for women without kids. Between 1975 and 1979 this employment gap fell from 24 pp to 18 pp where it remained through 1985. Figure 1a illustrates

1 That I am aware of. Ventry (2000) provides a thorough political and intellectual history of the EITC.

these unconditional employment trends and Figure 1b shows the between-group employment gap over time. These figures show that something affected the treatment group in 1975 that did not impact the control group.

Although many papers have estimated the effect of the EITC on female employment, these focus on EITC expansions in the 1980s and 1990s when lawmakers had begun explicitly cutting public benefits and nudging low-income women into the labor force. During the mid-1970s, public assistance (e.g. Aid to Families with Dependent Children; Women, Infants, and Children) was twice as generous as it would be by the late-1990s[2]; in this environment the incentives of the 1975 EITC might have proven insufficient to induce much of a female employment response. This is the first study of the 1975 EITC and I find that it did result in a positive female employment response. This has policy lessons for countries with generous public assistance considering an in-work redistribution program.

Another reason to study the 1975 EITC is that the population of (primarily) low-income women is different than in later EITC expansions. On average these women had lower education, higher fertility, and more traditional social norms[3]. With this in mind, the results in this paper may have policy implications for developing countries contemplating how to increase the employment (and empowerment) of women.

A third reason to study the original EITC is that the stable policy environment of the 1970s[4] enables a precise estimate of the EITC's employment effect. Unlike EITC expansions in the 1980s and 1990s, there were no major contemporaneous policies nudging low-income women into the labor force. For example, at the time of the 1993 EITC expansion many states had begun cutting and setting strict time limits on welfare benefits.[5] Discussion in section VI and the parallel employment trends shown in

2 See Figure A5 and A7.
3 Cite GSS or Gallup here.
4 See section VII for a thorough discussion on the stability of the 1970s policy environment. I also show in this section that the nominal price swings of the 1970s do not jeopardize my results.
5 Cite some stats here about state-led welfare reform.

Figure 1a suggest an absence of other major policies or trends affecting the relative employment of the treatment group between 1969 and 1985.

I briefly describe the EITC in section I and the March CPS data, sample of women, and treatment and control groups in section II. In section III I describe the probit model, show that the necessary conditions hold that enable a causal interpretation of DD, and present DD estimates of the EITC employment effect. For each set of controls the DD results are between 3 pp and 4 pp (or about 5% to 8% since 60% of the treatment group was employed before 1975). Since the EITC was a 10% wage subsidy this maps to an extensive margin labor supply elasticity of about 0.5 to 0.8 (defined as the percent change in female employment divided by the percent change in the after tax wage). These results show that low-income women were sensitive to changes in work incentives and illustrate the impact that public policy can have.

In section IV I explore heterogeneous responses to the original EITC and find that the largest responses came from women more likely to be EITC-eligible and women eligible for more EITC benefits. Each additional $100 (1975 dollars) in EITC eligibility led to a 1 pp (or about 1.5%) increase in the EITC treatment effect. In subgroup analysis I show that the EITC had a larger employment effect on women more likely to have lower non-labor income and reservation wages; this included lower-educated, younger, non-white, and residing-in-lower-cost-of-living-areas women. Among married women, those with lower-earning husbands had less non-labor income; each additional $1,000 (1975 dollars) of spousal earnings decreased the average EITC treatment effect by 0.3 pp (or about 0.5%), peaking at an estimated 5.3 pp (or about 9%) for women with zero-earning spouses.

In section V I utilize an alternative sample (women with less than 12 years of education) and estimate a 4.6 pp (or about 9% since about 50% of the treatment group was employed before 1975) DD treatment effect. This is almost 25% larger than the DD estimate from the main sample. In section V I also show that the baseline DD results are robust to model choice, when the sample period ends, state-

specific time trends, reweighting, changes in how March CPS imputes missing values, and potentially endogenous fertility responses to the EITC. I also employ several control groups to show that triple differences consistently leads to estimates similar to (though slightly smaller than) those from DD.

In section VI I discuss a number of potentially confounding policies and macroeconomic events; I find no evidence of contemporaneous changes and therefore interpret the relative increase in female employment to be a direct result of the 1975 EITC. In section VII I conclude.

## I. EITC

The EITC is the largest anti-poverty program in the US. In 2013 the EITC paid out $66 billion to 28 million individuals and lifted 6.5 million people out of poverty including 3.3 million children (Center on Budget and Policy Priorities 2014). The EITC has led to a substantial increase in female employment as well as increases in child test scores (Dahl and Lochner 2012; Chetty, Friedman, Rockoff 2011), infant health (Hoynes, Miller, and Simon 2013; Strully, Rehkopf, and Xuan 2010), educational attainment (Michelmore 2013), and macroeconomic growth (Edwards 2004).[6]

The EITC was signed into law over forty years ago by President Gerald Ford on March 29, 1975. This refundable tax credit provided a 10% income subsidy to working parents with annual earnings up to $18,000 (2013 dollars). This resulted in a maximum benefit of $1,800 that phased out at a 10% rate for incomes over $18,000 and reached zero for income above $36,000. Figure 2 shows the budget constraint for EITC-eligible individuals[7].

6 The EITC has also had at least one unintended consequence, driving down the pre-tax equilibrium wages of low-skill workers (Leigh 2010; Rothstein 2010; Neumark and Wascher 2011).

7 To be EITC-eligible a working family needed to have at least one eligible child that lived in the home for more than half the year and was under 19, under 24 if the child is a full-time student, or any age if disabled. Until 1990 parents must also be able to pass the "support test" by demonstrating that they provided over half the costs of maintaining the household. This meant that cash and in-kind public assistance must total less than half the household budget (Holtzblatt 1991; Holtzblatt, McCubbin, and Gillette 1994). Parents were also required to pass the "residency test" and show that the child lived with them for at least half the year. This prohibited a child from being claimed by more than one parent. Married couples must also file joint tax returns to be EITC eligible.

Since the 1975 introduction, the EITC has been expanded and redesigned several times. In 1979 a plateau region was added to the EITC schedule (see Figure 2). In 1986 the phase-in rate was increased to 14% and the EITC parameters were indexed to inflation. In 1990 additional EITC benefits became available to parents with two or more children. The largest overhaul came in 1993: by 1996 the phase-in rate had been increased to 34% and 40% for women with at least one and two children; benefits were also extended to adults without children (though at a less-generous rate of 7.65%). Finally, in 2009 additional EITC benefits became available to parents with three or more children.[8]

## II. Data and Sample of Women

The data used in this paper are from the 1970-1986 March Current Population Surveys (CPS) and correspond to employment in 1969-1985. For most of the analysis I focus on 1971-1980. The sample includes single and married women 16-45. I exclude women not in the labor force due to illness, disability, retirement, or full-time school, as well as women with negative individual or household earnings, and women with positive earnings and zero weeks of work.[9] In all years, I exclude married women that would not have been eligible for the EITC due to a higher-earning spouse[10] (though adding these women to the control group does not change the DD results much; see Table 7). In summary, the sample comes from the population of single women and married women with lower-earning spouses.[11]

The sample can be divided into an EITC-eligible treatment group and an EITC-ineligible control group. The 1975 EITC altered the budget constraint of women in the treatment group but not in

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8 Mention changes to “marriage penalty” too.

9 I focus on the population of women in a position to be nudged into entering employment. If the threshold for being ill or disabled changes over time, this could bias my results (see Autor and Duggan 2003). However, Table A1 shows that my DD estimates in Table 2 do not change much when any or all of these excluded groups are included.

10 Families earning under $8,000 (nominal dollars) were eligible to receive the EITC. However, if a husband earned between $4,000 and $8,000 any additional earnings by a spouse would decrease household EITC eligibility. I consider these married women to be EITC-ineligible and exclude married women with spouses earning over $4,000 (over $5,000 beginning in 1979). See Figure 2.

11 A woman's decision to work and her husband's earnings are likely intertwined. Conditioning on something endogenous could be problematic. To address this concern I also run the analysis on a sample from a different population (less than 12 years of education). Table 12 shows that the DD estimates are very similar (slightly larger) for this population.

the control group. The treatment group consists of women with children after 1975; the control group is composed of all women before 1975 as well as women after 1975 without children.[12]

Table 1 shows summary statistics for the sample of 132,272 women. These women average 27 years old, 12 years of education, $15,750 income (2013 dollars), and 30 work hours a week. 28% are nonwhite, 83% have income below the EITC limit[13] (though only 58% do conditional on having positive income), and 75% of women are employed.[14] In Table 1 I also divide the sample into subgroups of women with or without kids, observed before or after 1975, and married or single. Figure 3 shows the income distribution of women in the sample: 25% have zero earnings, 58% earn within the EITC-eligible range, and about 17% earn above the original EITC limit.

## III. The Impact of the EITC on Female Employment

Unconditional employments trends shown in Figures 1a and 1b motivate and preview the regression-adjusted DD results: Between 1969 and 1975 the relative employment of women with and without kids was constant. Between 1975 and 1979 the relative employment of the treatment group increased by 25% (the difference in employment rates fell from 24 pp to 18 pp). After women with kids had fully responded to the EITC, relative employment was again constant between 1979 and 1985.

Figures 1a and 1b provide evidence that the treatment and control groups are appropriate comparison groups and that the parallel pre-trends necessary condition required for a causal interpretation of DD is satisfied.[15] They also suggest that during this period no other major policies

12 Since I do not observe tax filing I assume that single women file as head of household and that all married couples file joint taxes. I assume that any family member under the age of 19 (or 24 if a full-time student) is a dependent child for tax. I treat all subfamilies (both related and unrelated) within a household as separate tax-filing units and potentially EITC-eligible. Nor can I account for the "support test" or "residency test."

13 DD results in Table 2 are slightly larger if I exclude women earning above EITC limit.

14 Working is defined as having positive income. Another way to define working is by having a positive number of weeks worked. I use positive income in this paper but my results are essentially identical using positive weeks worked.

15 Other necessary conditions include no contemporaneous shocks (besides treatment) and relatively constant group composition. These are discussed in sections VI and V.

affected the relative employment of women with kids. These trends imply that if not for the EITC, the employment gap between these two groups of women might have remained at the pre-1975 level.

The 1975 EITC is a 10% wage subsidy for qualifying parents and should have a positive effect on the employment of the EITC-eligible treatment group and a null effect on the EITC-ineligible control group. I use the following probit to model the probability that a woman works.

$$(1) \qquad P\left(Employed_{i,s,t}\right) = \Phi\left(\beta_0 + \beta_1\left(Kids_{i,s,t}\right) + \beta_2\left(Post1975_{i,s}\right) + \beta_3\left(Kids\ x\ Post1975_{i,s}\right) + \beta_4 X_{i,s,t} + \gamma_s + \delta_t + \epsilon_{i,s,t}\right)$$

The DD EITC treatment effect is estimated by $\beta_3$ and is measured in percentage points (pp). $X$ is a set of controls described below, $\gamma_s$ and $\delta_t$ account for state[16] and year fixed effects, and $\epsilon_{i,s,t}$ is an idiosyncratic error term. Probit is a nonlinear model; all results shown in this paper are average marginal effects. Standard errors are robust to heteroskedasticity and clustered at the state-year level in order to account for any correlation of unobservable characteristics.[17] March CPS weights are used throughout.

Table 2 shows the main DD results from equation (1). Columns 1-5 show estimates with covariates added cumulatively. Column 1 uses no control variables. Column 2 adds state and year fixed effects. Column 3 adds demographic controls: an age cubic, an education quadratic[18], number of children, amount of AFDC received, and binary variables for nonwhite, married, and having a child under 5 years old. Column 4 adds state-year employment-to-population ratios and the national unemployment rate.[19] Column 5 adds the following interacted control variables: nonwhite and kid,

16 Prior to 1977 (working year 1976) March CPS did not uniquely identify all states of residence. Groups of smaller states were bunched together. In order to provide a balanced panel of consistently defined geographical units, states are merged into the smallest consistent unit. This leaves me with the following 21 state-groups that I will refer to as states: CA, CT, DC, FL, IL, IN, NY, NJ, OH, PA, TX, and AL-MS, AK-HI-OR-WA, AR-LA-OK, AZ-CO-ID-MT-NE-NM-NV-UT-WY, DE-MD-VA-WV, GA-NC-SC, KY-TN, IA-KS-MN-NE-ND-SD, ME-MA-NH-RI-VT, MI-WI .

17 I choose state-year since 21 states is too few to cluster at (Cameron, Gelbach, and Miller 2008, 2011; Angrist and Pischke 2008 p.319). All DD results remain significant at the 99% level if I cluster at only the state or year level.

18 Results are virtually identical whether I use years of education quadratic or a categorical education covariate.

19 National unemployment rate will be collinear with year fixed effects, but will matter for interactions in column 5.

nonwhite and years after 1975, age and having kids, married and years after 1975, unemployment rate and married, unemployment rate and having kids, and state-year employment ratio and having kids.

These controls are fairly standard and intuitive (Table A2 shows the impact of each individual covariate on the DD estimate). Unemployment rate and state-level employment-to-population ratios control for business cycles and local economic conditions. Controlling for age is important since women with kids are older on average. Interaction terms are important and allow for a more flexible model (e.g. allowing economic conditions to differentially impact married women, nonwhite women, or women with kids).

For each specification the DD estimate is fairly stable between 2.3 pp and 3.9 pp (or about 3.8% and 6.5% since about 60% of the treatment group was employed before 1975).[20] Table 2 also shows that on average the following are associated with a woman being less likely to work: married (22 pp), having an additional child (1.6 pp), having a child under 5 (10 pp), and an additional $1000 in AFDC (10 pp). The sign on *post1975* flips from positive to negative between columns 3 and 4 since state-year employment-to-population ratio is included and increasing over this time period.

Column 6 is identical to column 5 except that it substitutes *post1975 x kid* in equation (1) with coefficients for each year after 1975 interacted with kid (annual DD estimates). This is one way to show that the trend break is permanent and that the DD results are not just due to one or two outliers in the pooled years after 1975. These five DD estimates also reflect the pattern in Figures 1a and 1b: it takes three or four years for women to fully respond to the 1975 EITC. Between 1976 and 1980 the annual DD estimates grow larger and more statistically significant at 1.7 pp, 2.9 pp, 3.8 pp, 5.9 pp, and 4.6 pp (or about 2.8%, 4.8%, 6.3%, 9.9%, 7.6%).

Figure 4 extends the approach in column 6 and interacts year and having children for 1969-1985. In Figure 4 I show that the 1979-1985 annual DD estimates are consistently in the 4 pp to 6 pp

20 Using an alternate definition of working (having positive weeks worked) the point estimates are equally statistically significant with point estimates 0.2 to 0.4 pp smaller.

(7% to 10%) range. I interpret this to mean that it took women a few years to fully respond to the EITC[21] and that the relative employment of the treatment group had stopped increasing by 1979. These regression-adjusted estimates are what would be expected from the unconditional trends in Figure 1a.

## IV. Heterogeneous Treatment Effects

A standard labor supply model would predict that the EITC treatment effect should have been larger for subgroups of women more likely to be EITC-eligible and women eligible for more EITC benefits. Figure 5 illustrates a budget constraint in the presence of the EITC and non-labor income (e.g. spousal income or public benefits). Point B represents non-labor income; the higher B is the more likely it is that the highest indifference curve a woman can attain goes through point B and she will choose not to work. On the other hand, women with less non-labor income will be more likely to respond to the EITC by working, which would include younger women, women living in low-cost-of-living areas, non-white women, and less-educated women.[22] This also predicts that single women should respond more than married women, and that within married women those with lower-earning husbands should respond more.

Table 3 uses my preferred set of controls from Table 2 column 5 and estimates the treatment effect of the 1975 EITC for various subgroups. These subgroups are defined by education (less than high school, high school, or more than high school), marital status (married or single), race (black or white), region (Northeast, Midwest, South, or West), and age (16-25, 26-35, or 36-45). The findings are

21 Eissa and Liebman (1996) also finds that it takes a few years for the new equilibrium to be reached after the 1986 EITC expansion. Excluding 1987 they find annual DD estimates of 0.8 pp, 2.9 pp, and 2.8 pp in 1988-1990. I include the first year after the policy change (1976), excluding it would result in even larger DD estimates. Liebman (1998) suggests that this gradual ramp up might be due to the fact that the EITC does not pay off until the tax refund in the following year; therefore it would be at least a year before EITC recipients became aware of and responded to the EITC. Even if taxpayers do not fully understand the EITC, those that are on the margin of working and not working might try working for one year, discover that they ended up better off than they expected, and decide to remain employed. Though it is true that the EITC did offer an option of receiving EITC payments throughout the working year before taxes were filed, most did not utilize this option (see Liebman 1995 for more details).

22 This is a reason to suspect that less women would respond to the original EITC than in later expansions when public benefits were smaller.

consistent with Figure 5 and the discussion above: women with lower reservation wages, opportunity costs, and spousal earnings should and do respond more to the EITC.

**A. Treatment Effect Equal for Single Women and Married Women with Low-Earning Spouses**

In Table 3 columns 2-3, I cannot reject that the treatment effect on the sample of married women and all single women was identical. This may seem like a surprising result since the EITC literature has consistently found that single women respond positively to the EITC (Dickert, Houser, Scholtz 1995; Eissa and Liebman 1996; Meyer and Rosenbaum 2000, 2001; Keane and Moffitt 1998; Ellwood 2000; Grogger 2003; Hotz, Mullin, and Scholtz 2006; Eissa, Kleven, Kreiner 2008) but that married women do not (Dickert, Houser, Scholtz 1995, Ellwood 2000, Eissa and Hoynes 1998 and 2004). The sample of married women in this paper have spouses earning in the bottom fifth of all married males (Figure 6 shows the income distribution of these men).[23] For these married women, a standard labor supply model would predict that the labor supply of some secondary earners would increase – likely on the extensive margin – since a wage subsidy could bring her offered wage above her reservation wage. Eissa and Hoynes (2004) acknowledge that "For some secondary earners, however, the incentives may be positive. If the spouse is not working, the EITC (as in the case of single parents) encourages the wife to enter the labor force. If the primary earner has income in the subsidy region and the effect of the greater returns to work dominates the income effect, the EITC would encourage employment."

In Table 4 I explore this issue directly and add an interaction between the DD variable (*kid x post1975*) and spousal earnings to equation (1). For this analysis I drop all single women and include married women with higher earning spouses.[24] In column 2 I show that the EITC treatment effect for

[23] Figure 6 shows the income distribution of husbands of married women in this sample: 12% have zero earnings, 22% earn within the EITC-eligible range, and 67% earn above the original EITC limit. This shows that married women in the main sample have a selected group of spouses.

24 I continue to omit ill, disabled, retired women as well as the other categories outlined in section II. For DD results using all single and married women see Table 7.

women married to a spouse with zero earnings was 5.26 pp and this response declines on average by 0.32 pp with each $1,000 in earnings.[25] In column 1 I estimate equation (1) and show that the DD estimate for *all* married women is 0.53 pp and is statistically indistinguishable from zero. Though this is closer to the negative results generally found in the EITC literature for married women, the point estimate is still positive. Although the population of women is different during the 1970s than for later EITC expansions, this complicates the widespread belief that the EITC had a negative impact on the employment of married women.

**B. Largest Treatment Effect for Lower-Educated Women**

Lower-educated women are generally found to be the largest responders to the EITC (Eissa and Liebman 1996) and in Table 3 columns 4-6 I also find this. However, the relationship between education and the EITC response was not monotonic. I estimate an EITC treatment effect on women with less than 12, 12, and more than 12 years of education of 6.3 pp, 2.1 pp, and 3.8 pp. Each between-group difference is significant at the 95% level.

In Figure 7 I show the annual DD estimate for the three education subgroups and find suggestive evidence that not only did lower-educated women respond more, but that they also responded faster. Since lower-educated women are more likely to earn in the EITC range and benefit from the EITC, information about the EITC likely traveled by word of mouth within this group.[26] Chetty, Friedman, and Saez (2013) show that this local EITC knowledge leads to increased EITC benefits.

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25 Using real or nominal dollars does not change the results. These two estimates imply that at about $18,000 the response to the EITC falls to zero, which is about 225% of the original EITC limit.
A model consistent with these results is laid out in Eissa and Hoynes (2004) where the intra-household labor supply decision is treated as a two-person sequential game where the husband decides how much to work first and then the wife chooses her best response based on her spouse's decision. As a result women with higher earning spouses are less likely to decide to work. See discussion of Table 4.

26 That the highest-educated women did not respond suggests that EITC response does not primarily require an understanding of public policy, law changes and the tax code.

There is strong evidence that EITC responses vary by marital status and education. Table 5 looks at education subgroups by marital status and reveals that education seems to be *negatively* correlated with responding to the EITC for single women, but *positively* correlated for married women. The largest responses to the 1975 EITC apparently came from lower-educated single women and higher-educated married women with low-earning spouses. Eissa and Hoynes (1998) and (2004) also find that education was negatively correlated with responding to the 1993 EITC expansion among married women.[27] We both find a negative DD for married women with less than a high school education and positive treatment effects for women with 12 or more years of education. However, since Eissa and Hoynes (2004) limit their main analysis to lower-educated women they conclude that the EITC led to a *decrease* in the employment of married women. Although these married women were certainly impacted by the EITC, they yield an incomplete picture of the heterogeneous treatment effects of the EITC on married women.

**C. Treatment Effect Larger for Black Women, Young Women, Women Living in Low-Cost Areas**

Columns 7 and 8 in Table 3 shows that the DD estimate for black women is slightly larger than for white women (4.1 pp versus 3.8 pp), although this difference is not statistically significant.[28] On average black women have lower earning spouses, higher welfare income, and less education,[29] resulting in a theoretically ambiguous prediction concerning which group had lower reservation wages and lower opportunity costs of working. Race-education subgroup analysis (not shown) shows that for each race of women, lower-educated women respond the most to the EITC.

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27 Eissa and Hoynes find DD point estimates for married women with low, middle, and high education of -1.1, 1.75, and 2.25 pp; in my main sample I find -1.2, 1.2, and 6.0 pp. If I do not restrict my married sample to those with low-earning spouses and instead utilize all married women then the point estimates become -1.2, 1.2, and 0.8 pp.

28 Black and White do not exhaust the sample; small samples of Hispanic and Other women exist but are not well-documented by the CPS during the 1970s.

29 Conditional on being married the average (nominal) spousal earnings for black and white women is $8,370 and $11,677. 45% and 68% of black and white women are married in the sample, 33% and 4% receive welfare benefits, and the average years of education is 10.8 and 11.9.

Columns 9-11 in Table 3 reveal that younger women 16-35 respond more than older women 36-45. These DD estimates are 4.0 pp, 3.3 pp, and -0.7 pp for women aged 16-25, 26-35, and 36-46. On average younger women have lower reservation wages and opportunity costs, and are more flexible and able to respond to the incentives of the EITC. In Figure 8 I plot the DD estimate against the full age distribution and show that the treatment effect is monotonic and negatively correlated with age.

Cost of living differs greatly across and within states. The 1975 EITC was a national program[30] that did not account for this variance in cost of living and the actual value of the EITC varied across geographic areas. In columns 12-15 in Table 3 I show the response to the EITC across regions and was largest in the Midwest (5.2 pp), smallest in the West (2.3 pp), and somewhere in between for women in the Northeast and South (3.6 and 2.9 pp). Figure 9 shows the DD point estimates for each state.[31] The map reflects the regional results in Table 3 as well as adjacent-state clumping since the CPS only identifies 21 state-groups before 1977 (see footnote 16).

To check for within-state heterogeneous responses to the EITC I use information on metropolitan status and interact this with the DD coefficient of interest. Metropolitan status is a good proxy for cost of living. I assign one of three values to each woman that corresponds to living in a major city, smaller city, or rural area.[32] Table 6 column 1 shows the baseline DD estimate from Table 2 and column 5 shows that the treatment effect was 1.7 pp larger in the lowest cost of living areas within a state compared to the highest cost of living areas.

### D. Women Eligible for More EITC Benefits Responded More

Women eligible for more EITC benefits should respond more than women eligible for less (or none). In

30 The first state-level EITC would not exist in Rhode Island in 1986 (http://www.taxcreditsforworkingfamilies.org/earned-income-tax-credit/states-with-eitcs/).

31 In addition to cost of living, state-level responses to the EITC will depend on local labor markets, public benefit generosity, and a number of other things that would be difficult to disentangle.

32 March CPS data in the 1970s is not detailed enough to assign each woman a county of residence as done in Fitzpatrick and Thompson (2010).

Table 7 I test this directly by interacting the DD estimate with the amount of maximum potential EITC benefits (*maxEITC*). For married mothers with spouses earning zero and single mothers, *maxEITC* = $400 until 1978 and $500 beginning in 1979 (nominal dollars). For married women with a working spouse, maxEITC = 0.1*max[EITC kink cutoff[33] – spousal earnings, 0]. A model consistent with this setup is given in Eissa and Hoynes (2004) where the intra-household labor supply decision is treated as a two-person sequential game. First, the husband decides how much to work (and earn), then the wife chooses her labor supply.[34] As a result, women with higher earning spouses are less likely to decide to work.

In Table 7 I expand the sample and add married women ineligible for the EITC due to a high-earning spouse to the control group.[35] Using equation (1), in column 1 the DD estimate on this larger sample of women is 2.8 pp, which is smaller than the baseline DD estimate of 3.9 pp. Column 2 adds an interaction between the DD variable (*kid x post1975*) and *maxEITC*. As a result the DD estimate falls to a statistically insignificant 0.8 pp but the coefficient on *kid x post1975 x maxEITC* is 1.0 pp (about 1.5%) and statistically significant at the 1% level.[36] An additional $100 in potential EITC benefits is correlated with a woman being 1.0 pp more likely to work on average.[37] This implies that between 1976 and 1978 a woman eligible for the full $400 was 4 pp more likely to work relative to an EITC-ineligible woman and 5 pp more likely beginning in 1979.

## V. Robustness Checks

### A. Triple Differences Corroborates DD Treatment Effects

DD results shown thus far could be biased if an unaccounted for policy or trend affected the relative

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33 This kink was $4,000 until 1978 and $5,000 beginning in 1979.

34 Of course there is reason to believe that employment decisions are made jointly between spouses. See discussion in part E of section VI and analysis in Table 12.

35 I continue to omit ill, disabled, retired women as well as the other categories outlined in section II.

36 The results are similar (slightly smaller) if I eliminate women with very high-earning spouses.

37 Results are similar for nominal or real dollars.

employment of women with kids. For example, if for some reason it became easier for *all* women with kids to work after the mid-1970s, triple differences (DDD) is one way to net out this trend and check the validity of the DD results.[38] DDD implicitly estimates equation (1) for an EITC-ineligible control group (that includes women with and without kids, before and after 1975) and subtracts this DD estimate from the baseline DD estimate. If this new DD is near zero, the difference of the two DD estimates (and the resulting DDD) should resemble the original DD and further back up the claim that the EITC is the cause of the relative employment increase of the treatment group.[39]

Control and treatment groups differ in obvious ways. To ensure that the treatment effect is a result of the 1975 EITC and not simply differential trends, I use several different DDD control groups: EITC-ineligible married women with higher-earning husbands, men, and older women. If the employment of EITC-ineligible married women was increasing, it should not have been due to the EITC. Men were largely not in a position to respond to the EITC on the extensive margin since 95% of men already worked during the 1970s (according to the March CPS). Older women had higher barriers to enter employment and responded less to the EITC (as shown in Figure 8).[40] I also utilize subgroups within these three control groups (detailed below) to construct arguably better comparison groups.

I estimate the following saturated DDD probit model; $\beta_7$ is the DDD coefficient of interest.

$$(2) \qquad P\left(Employed_{i,s,t}\right) = \Phi\Big(\beta_0 + \beta_1\left(Kids_{i,s,t}\right) + \beta_2\left(Post\ 1975_{i,s}\right) + \beta_3\left(Treatment_{i,s,t}\right) + \beta_4\left(Kids\ x\ Post\ 1975_{i,s,t}\right) + \beta_5\left(Kids\ x\ Treatment_{i,s,t}\right) + \beta_6\left(Post\ 1975\ x\ Treatment_{i,s,t}\right) + \beta_7\left(Kids\ x\ Post\ 1975\ x\ Treatment_{i,s,t}\right) + \beta_8 X_{i,s,t} + \gamma_s + \delta_t + \epsilon_{i,s,t}\Big)$$

[38] This is one reason that Angrist and Pischke (2008: page 182) states that a DDD model "may generate a more convincing set of results than a traditional DD analysis."

[39] According to Gruber (1994), although the DDD point estimate should resemble the DD, it will generally be less precisely estimated.

[40] The main sample consists of women 16-45; clearly many women older than 45 will have a child under 19 (or under 24 and a full-time student) and be EITC eligible. This means that many women in the "control" group actually receive "treatment." In an attempt to minimize this contamination, I exclude women 45-54 and use women over 55 (with different upper bounds) as control groups.

In Table 10 I show the baseline DD estimate (3.9 pp) in column 1. In columns 2-3 I estimate equation (2) using EITC-ineligible married women as the control group; column 2 uses married women with spouses earning over 100% of the EITC limit and column 3 restricts this sample to women with spouses earning between 100% and 200% of the EITC limit. These two DDD point estimates are 2.2 pp and 2.5 pp, smaller than the DD estimate of 3.9 pp, and significant at the 95% level. Columns 4-6 use men as the DDD control group; column 4 uses all men, column 5 restricts this sample to all single men, and column 6 further restricts this to all single men 16-45. These estimates are 2.6 pp, 2.9 pp, and 2.3 pp and partly due to successively smaller samples are statistically significant at the 99%, 90%, and below 90% level. Columns 7-9 use older women 55-65, 55-75, and over 55 as the DDD control group. Each DDD estimate fall between 2.7 pp and 3.2 pp and is significant at the 95% or 99% level.

These DDD estimates all fall between 2.2 pp and 3.2 pp and may indicate that a portion (perhaps 25%-33%) of the baseline DD may reflect a general positive trend in the employment of women with kids. Although if any individuals in the control group actually received treatment (which is likely) then these DDD would be biased towards zero. Overall, the DDD estimates suggest that the DD EITC treatment effect largely reflects the causal impact of the EITC on female employment.

**B. DD Estimates Robust to Choice of Sample Period and Model Specification**

Figures 1a and 5 suggest that the DD results should be robust to the choice of sample period. Figure 10 shows how the probit DD estimate changes with the choice of when the sample period ends. If I end the sample period in 1976 such that the EITC has only been in place for one year[41] the DD estimate is just over 1 pp but not statistically different than zero. If I extend the sample to 1977, 1978, and 1979 the point estimate (and statistical significance) steadily grows from about 1 pp to 4 pp. If I end the sample period in any year between 1979 and 1985, the DD estimate is very stable around 4 pp (or about 7%). If

[41] For this reason I cannot estimate a treatment effect for years before 1976.

the DD estimate had continued to rise with time, the DD results could be dismissed as simply reflecting a positive trend in the relative employment of women with kids.

Throughout this paper I use a probit to model a binary dependent variable (employed). It would raise a red flag if a logit or linear probability model (LPM) yielded very different results. With a binary dependent variable LPM is consistent even when the error terms are heteroskedastic; however, the same is not true of probit and logit models where misspecification can lead to inconsistent estimates (Wooldridge 2012). The probit and logit DD treatment effect estimates are identical at 3.9 pp, while the LPM estimates are about 30% larger at around 5 pp (results omitted). Since a LPM is not constrained between 0% and 100%, it can suffer from over- or under-prediction, especially when many women in the sample have a predicted probability of working near 0 or 1. Post-estimation analysis of the LPM residuals yield evidence that the LPM treatment effect is inflated: 69% of residuals are positive (over-predicting the probability that a woman is employed) and 7% of women are predicted to work with a probability over 100% while less than 1% have a probability less than 0%. This is one reason that I chose to use a probit model and the more conservative DD estimates that it generates.

**C. More Than One Child**

The 1975 EITC was available to working adults with children, but did not differentiate between the number of children.[42] In Table 9 I estimate the following model which accounts for any additional impact of having two or more children.

$$(3) \qquad P\left(Employed_{i,s,t}\right) = \Phi\left(\beta_0 + \beta_1\left(Kids_{i,s,t}\right) + \beta_2\left(Post\ 1975_{i,s}\right) + \beta_3\left(\geq 2\ Kids_{i,s,t}\right) + \beta_4\left(\geq 1\ Kids\ x\ Post\ 1975_{i,s,t}\right) + \beta_5\left(\geq 2\ Kids\ x\ After_{i,s,t}\right) + \beta_6 X_{i,s,t} + \gamma_s + \delta_t + \epsilon_{i,s,t}\right)$$

Ex ante I expected the coefficient on $\beta_5$ to be near zero. Table 9 columns 1 and 2 reflect the regression from equations (1) and (3). The DD estimate on $\beta_4$ falls about a third from 3.9 pp to 2.6 pp and $\beta_5$ is

[42] Additional EITC benefits to parents with two or more children began in 1991.

estimated to be 2.0 pp and statistically significant at the 5% level. Eissa and Liebman (1996) also find this additional response from women with at least two children.[43] They suggest that this may be due to new 1986 tax exemptions given for each dependent, which benefited families with multiple children more and therefore may have given an extra incentive to such women to enter employment. In 1979 the tax exemption for each child was increased from $750 to $1,000 (Tax Foundation 2006). However, even when I exclude 1979 and 1980 I still find a similar estimate on $\beta_5$.

**D. Accounting for Potentially Endogenous Fertility and Changes in Group Composition**

The EITC is only available to working adults with children and thus might impact fertility choices. Although theory would predict a positive effect on the probability of having a child[44], existing evidence shows little to no responses on this margin.[45]

One simple way that I account for potentially endogenous fertility after 1975 is by restricting the sample to women with kids born before 1975 to see if the estimated treatment effect is affected. Table 10 shows that this eliminates about 8,000 observations and has almost no impact on the DD estimate, increasing the point estimate from 3.9 pp to 4.0 pp.

Another way that I account for endogenous fertility and changing group composition is by reweighting women observed after 1975 to look like women before 1975. Although regression controls should largely account for any changing composition of women over time, DiNardo (2002)

43 Using the 1986 EITC expansion – which also did not reward having a second child – their DD coefficient falls about a quarter from 2.8 to 2.2 and the coefficient on the second child DD is positive and statistically significant. Since the authors do not show probit marginal effects for this second child DD estimate it is unclear exactly how large the point estimate is (Ai and Norton 2003).

44 There are no extra EITC benefits to having more than one kid until 1993, so there should not be an incentive on the intensive margin of fertility.

45 Baughman and Dickert-Conlin (2003) and (2009) find small positive and negative fertility responses during the 1990s. Related, Ellwood (2000) finds no effect on marriage or cohabitation, Dickert-Conlin and Houser (2002) find no impact on single women and a very small negative effect on married women, and Herbst (2011) finds a small reduction in new marriages. Eissa and Hoynes (2004) summarize the empirical evidence as showing very small responses on these margins.

recommends reweighting as an additional robustness check.[46] In Table 11, I use two sets of weights: DFL weights, which are detailed in DiNardo, Fortin, and Lemieux (1996) and inverse propensity (IP) weights. I generate these weights in the following two step process: First I use the following logit[47] to estimate the probability than a given observation in the sample is from a year before 1975.

$$(4) \qquad P(Year \in \{1971, 1972, 1973, 1974, 1975\}) = \Lambda\big(\beta_0 + \beta_1\big(Kids_{i,s,t}\big) + \beta_2\big(Nonwhite_{i,s,t}\big) + \beta_3^j\big(Age_{i,s,t}^j\big) + \beta_4^k\big(Education_{i,s,t}^k\big) + \beta_5^l(State_{i,t}^l) + \beta_6(Married_{i,s,t}) + \epsilon\big)$$

I use a parsimonious set of observable characteristics $X$ that includes six age bins, three education bins, married and nonwhite dummy variables, and 21 state bins for a total of 1512 cells.[48] Each observation in the sample is assigned a probability of being from a year before 1975 conditional on $X$. This probability $p$ is then used to create DFL and IP weights by assigning each observation a weight of $p/(1-p)$ and $1/p$. Women before 1975 will be weighted 100% and women more likely to be observed after 1975 (based on $X$) will be weighted down while women less likely to be observed after 1975 will be weighted up. Rising education and falling fertility (see Bailey 2006 or Fernandez 2013 for more details) during the 1970s imply that lower-educated women and women with more kids have a higher probability of being observed in a year *before* 1975. Therefore such women observed *after* 1975 will be weighted up to account for the fact that less of these types of women exist after 1975.

In Table 11 I find that the baseline DD estimate of 3.9 pp is largely unchanged at 4.0 pp and 3.6 pp using DFL and IP weights. Since reweighting ensures that the composition of women after 1975 will be "identical" to the composition of women before 1975, this is further evidence that the relative

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46 Reweighting analysis requires that the characteristics of the different populations overlap sufficiently (Busso, DiNardo, and McCrary 2013) and that any differences between the two groups can be captured by these observable characteristics. The first condition is demonstrated in Figure 11, which shows common support exists between the two distributions of characteristics for women observed before and after 1975; the second condition will be assumed.

47 The logit has the advantage over a probit in that the sum of predicted values equals the sum of the empirically observed ones (Butcher and DiNardo 2002). Although the results are very similar if a probit is used instead of a logit.

48 DiNardo, Fortin, and Lemieux (1996) utilize a parsimonious X that contains only 32 education-experience-gender cells. Butcher and DiNardo (2002) utilize several covariates which yields an X with many more cells. The approach here is closer to the latter, although my weighted results do not change much with the choice of X and the number of cells therein.

increase in employment by women with kids was due to the EITC and not a change in characteristics.

### E. Accounting for State-Specific Time Trends

Angrist and Pischke (2008: page 178) recommend adding state-specific time trends as a robustness check on DD regressions. The DDD analysis above implicitly controls for common trends shared by states, but not individual state[49] time trends. Whether I include linear or quadratic state time trends, or flexible state-year fixed effects, the DD estimate remains constant at 3.9 pp (results not shown).

### F. Accounting for Endogenous Household Employment Decisions

The main sample includes married women with low-earning spouses. Clearly employment decisions between spouses are an intertwined, endogenous decision, which means that this could bias the results. To get around this issue, I utilize a different sample that does not depend on marital status or spousal income: women 16-45 with less than a high school education.[50] This is the sample used by many EITC papers and subgroup analysis in Table 3 columns 3-5 show that *within the original sample* this less-education group was the most responsive to the 1975 EITC. Table 12 mirrors the analysis in Table 2 and cumulatively adds covariates. The DD estimate is 2.9 pp with no controls and increases to 4.3 pp when the full set of controls are added; this is about 10% larger than the 3.9 pp DD estimated using the main sample. Finding a consistent positive response to the EITC from these two different (but overlapping) populations further supports the case that the EITC is responsible for the observed DD.

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49 Recall that the 1970s March CPS only has 21 state-groups. See footnote 17.

50 I am grateful to Jeff Smith for this suggestion. I exclude the same women as I did before: those not in the labor force due to illness, disability, retired, or full time school, as well as those with negative earnings, negative household earnings, and those with positive earnings and zero weeks of work. The populations represented by these two samples are clearly overlapping, but not identical. Of the 132,272 women in my main sample, 31% were in this low education sample. Of the 73,807 women in this less-education sample, 55% were in my main sample. 165,277 unique women are included in these two samples.

## G. March CPS Imputations

In 1975 the Census changed its *hot deck* procedure[51] for imputing missing earnings (Bound and Freeman 1992). This could affect the results since I define employment as having positive income. The percentage of imputed observations in the sample between 1975 and 1980 is 6.6%, 6.3%, 6.3%, 5.3%, 1.0%, and 1.0%.[52] In Table 13 I explore how robust the DD estimate is to various treatments of these imputed observations. Column 1 shows the baseline DD estimate of 3.9 pp from Table 2 column 5, column 2 drops all imputed observations and finds that the DD estimate rises to 4.0 pp. In columns 3-4 I use a logit to predict the probability than an observation is missing and account for selection (missing not at random) by reweighting each observation with DFL and IP weights (see discussion in part C of this section). These yield DD estimates of 3.4 pp and 4.5 pp. Columns 5 and 6 are for instructive purposes only and assume that none or all observations with imputed earnings are working. This yields DD estimates of 4.5 pp and 3.9 pp.

## H. Reconciling Constant EITC Recipients with Increasing Employment due to the EITC

Figures A1 and A2 show that the number of EITC *recipients* and the aggregate EITC *benefits* remained roughly constant between 1975 and 1985. However, Figures 1a and 6 show that there are numerous women entering the labor force due to the EITC. At first glance these appear to be a contradiction.[53] However, this appears to represent women already working before the EITC was introduced with earnings just below the EITC limit. Inflation was high in the 1970s, nominal income was quickly rising, and the EITC schedule was in nominal dollars[54]. These factors pushed the earnings of some

51 In this process people with missing information are matched with similar people (based on sex, race and ethnicity, household relationship, years of school completed, geographic area, age, disability status, presence of children, veteran status, work experience, occupation, class-of-worker status, earnings, and value of property or monthly rent) and assigned the same value for the missing variable (IPUMS: https://usa.ipums.org/usa/voliii/80editall.shtml#note1).

52 Imputations fall into three categories: where it is unclear whether the individual had positive earnings, or what the actual amount of earnings was, or both. Only in the third case is it unclear whether the individual worked or not. This category is what I refer to as imputations.

53 I am grateful to Bryan Stuart for point this out.

54 The EITC schedule was finally pegged to inflation in 1986.

working women beyond the EITC limit, akin to "bracket creep" in the tax literature (see Saez 2003 for more details).

To approximate how many working women became ineligible for the EITC due to rising nominal income, I use the 1975 female income distribution from the CPS, assume constant real earnings[55], and inflate these nominal earnings by the annual inflation rates of 1976-1980 (6.9%, 4.9%, 6.7%, 9.0%, and 13.3%). I then calculate the percentage of working women who were EITC-*eligible* in 1975 (earnings below $8,000) but EITC-*ineligible* in 1976, or in any other year between 1977 and 1980, due to rising nominal income. Figure 12 shows a zoomed-in view of this income distribution and the income cutoffs that would "bracket-creep" out of EITC-eligibility in 1976, 1977, and 1978. The percentage of women already working before the introduction of the EITC that would "bracket-creep" out of EITC eligibility was 1.9%, 2.9%, 5.2% 7.0%, and 11.0% for 1976-1980. Since about 66% of women were working in 1975, these five percentages correspond to percentage points of 1.3, 1.9, 3.4, 4.6, and 7.3. The first four line up with the annual DD estimates in Table 2 column 6 and Figure 4. This explains how constant EITC recipients and increasing employment due to the EITC can coexist.

## VI. Ruling out Possible Confounding Policies or Events

The estimation strategy in this paper requires that no other public policy, macroeconomic event, or structural change in the mid-1970s affected the relative employment of women with kids. In the following discussion I address a number of potential confounders and conclude that none threaten the conclusion that the increased employment of the treatment group was due to the 1975 EITC.

The 1970s were a period of inflation, oil and food price shocks, and two recessions. In August 1971 the Bretton Woods system ended when the US terminated convertibility of the dollar to gold. The first oil shock began in October 1973 when the Organization of Arab Petroleum Exporting Countries

55 Rising real wages would inflate these numbers even more and yield more "bracket creep" out of EITC eligibility.

proclaimed an oil embargo against the West in response to support for Israel in the Yom Kippur War against Egypt. This led to a quadrupling of oil prices by March 1974, double digit inflation and increases in food prices, and a US recession marked by *stagflation* lasting from November 1973 to March 1975. A few years later the second oil shock began when oil production sharply decreased in Iran due to the Iranian Revolution. This preceded the double-dip recession that occurred between January 1980 and July 1980 and between July 1981 and November 1982.

This first recession ended around the same time that the EITC was introduced. Could this have led to an increase in relative employment of women with kids? If the relative employment of women with kids is strongly pro-cyclical then perhaps so. But then this should also have occurred after the double-dip recession of the early 1980s and Figure 1a shows that it did not. This suggests that unless the 1973-1975 recession is somehow very unique, it is also likely not responsible for this change.

One reason that the 1975 EITC was enacted was to offset rising food and energy costs, as well as the Social Security payroll tax. The only way that any of these factors could confound the analysis would be with a permanent price jump around 1975. This could make women worse off and induce many to begin working through an income effect. However, the high fuel and food prices, and inflation of the 1970s were temporary and should not have led to a permanent change in the relative employment of women with kids. As for the payroll tax, a sharp and permanent change around 1975 could also have induced women to work more through an income effect. However, Figure A3 shows that the payroll tax rate was stable during the mid-1970s and should not represent a threat to the analysis.

One public policy that impacted women with and without kids differently is WIC (Women, Infants, and Children). Rollout begins at the county-level in 1972 and is made permanent in 1975. This program provides in-kind benefits to pregnant women and mothers with children up to age 5. Figure A4 (from Hoynes, Page, and Stevens 2011) shows that the percentage of counties with WIC in place rose from 0% in 1973, to 60% in 1975, to 100% in 1979. In Figure A5 I show trends for the number of

families receiving WIC, total WIC payments, and average benefits per recipient. Currie (2003) provides a summary of the program and cites Moffitt and Fraker (1988), Moffitt and Keane (1998), and Hagstrom (1996) as showing that WIC had a small but negative impact on labor supply. This empirical finding – and the trends shown in Figures A4 and A5 – show that if anything WIC had a negative impact on the relative employment of the treatment group, which would bias the estimated EITC treatment effects in this paper downward towards zero.

Aid to Families with Dependent Children (AFDC) and Food Stamps are two more programs that affect women with kids differently than women without kids. Food Stamps began rolling out in 1964 at the county level, and a 1973 amendment required that all counties adopt the program by 1975. During the 1970s the number of families on Food Stamps increases from about 13 million to about 20 million. Figures A6 and A7 show trends for Food Stamps and AFDC recipients, total payments, and average benefits per recipient. As with WIC, these trends are somewhat positive and likely resulted in a negative employment response (Hoynes and Schanzenbach 2012) from the treatment group. Again, this would bias the results found in this paper downward towards zero.

Abortion is legalized in 1973 with Roe v Wade. Could the increased employment of the treatment group be a lagged response to legalized abortion? It does not appear so. Four states (Alaska, Hawaii, New York, and Washington) legalized abortion in 1970. Figure A8 compares the employment trends for women with and without kids in these early-abortion states and for the other 46 states (plus DC). This figure suggests that legalized abortion did not result in any noticeable trend break in relative employment.

Another policy to consider is the rise of divorce and unilateral divorce laws. Divorce has been rising in the US since 1960 and was propelled higher with the introduction of unilateral divorce laws (Wolfers 2006). These laws began in 1970 in California and existed in all jurisdictions by 1985 (see Peters 1986 and Parkman 1992 for more details). Johnson and Skinner (1986) estimate that 2.6 pp of

the 15 pp increase in married women's employment between 1960 and 1980 can be attributed to increased divorce risk and future income risk. However, these policies had a gradual impact on women that began well before the 1975 EITC. Furthermore, isolating California shows a trend in relative employment that looks similar to other states (analysis not shown). I do not believe that unilateral divorce laws will confound the analysis.

During 1975-1979, 23 states passed laws that outlawed treating pregnancy differently than a similar medical condition. These state laws and the 1978 Pregnancy Discrimination Act prohibited discrimination against pregnant women for the first time. However, Gruber (1994) finds little effect on the total labor supply of this group of women, nor do I find a differential response to the EITC from these particular states.

Gruber and Kubik (1997) finds that a 30% increase in the disability insurance denial rate between 1977 and 1980 led to a fall in the labor force non-participation rate of 45-64 year olds of 1.4 pp. This should not be a major concern for the sample of 16-45 year old women. Table A1 column 5 shows that the results are robust to whether or not I include disabled women in the sample. Autor and Duggan (2003) show that disability insurance has become more positively correlated with the business cycle in recent decades. In the 1980s (and presumably in the 1970s as well) the number on disability insurance was uncorrelated with macroeconomic conditions.

The Child Care Tax Credit has been a part of the US tax system since 1976. This allowed working families to claim a credit against taxes owed for up to 20% of child care expenditures. The credit applied to the first $2,000 for the first child and the first $4,000 for two or more children.[56] Averett, Peters, and Waldman (1997) show that government subsidies to child care increases the labor supply of married women. However, since the tax credit is not refundable, low-income families that do not pay any (or much) taxes cannot benefit from the credit. It requires parents to pay out-of-pocket first

56 http://www.iff.org/resources/content/3/1/documents/century_of_caring.pdf

and then claim the credit at the end of the year (Cohen 1996). The credit does not actually cover the cost of child care and mainly benefit upper-middle class families (Tax Policy Center 2011).[57] This program likely did not have a large impact on women in the sample.

I conclude that there are no apparent macroeconomic or policy confounders that would cast doubt on the analysis. The 1970s provides a nice environment in which to study a natural experiment: the impact of the 1975 EITC. If anything the policies in place are pushing women in the treatment group out of the labor force, which would imply that the results should be interpreted as lower bound estimates of the EITC on female employment. This stands in contrast to studies of EITC expansions in the 1980s and 1990s when policymakers had begun explicitly cutting public benefits, setting time limits, and nudging low-income women into the labor force.

## VII. Discussion

There are a number of EITC-eligible families that do not claim that EITC as well as a number of EITC-ineligible families that do claim the credit (intentionally or not). This contamination imply that the estimated treatment effects are intent-to-treat effects and that the results should be scaled up by an amount proportional to the contamination. Liebman (1995) and Eissa and Liebman (1996) find that in the 1980s 89% and 95% of women allocated to the treatment group and control group filed taxes appropriately. Assuming that this misallocation occurs at random, the authors suggest that the employment effects of the EITC should be increased by 19%. This (along with the discussion in section VI) implies that the DD results estimated in this paper should be treated as lower bound estimates.

In this paper I show that the EITC led to a 3 to 5 percentage point (or about 5% to 8%) increase in relative female employment. Since the EITC was originally a 10% wage subsidy, this maps to an extensive margin labor supply elasticity of about 0.5 to 0.8 (defined as percent change in employment

57 http://www.taxpolicycenter.org/press/quickfacts_cdctc.cfm#two

divided by percent change in initial earnings). An alternative sample of lower-educated women yield even larger results (4.3 pp from a baseline of 50%) and imply an extensive margin labor supply elasticity closer to 0.85. This is quite large but aligns with earlier female labor supply estimates: Goldin (1990) finds that female labor elasticity increased over the first half of the twentieth century, peaked around 1950, and has been declining ever since. Mincer (1962) finds an elasticity of 1.5 for married women in 1950. LaLumia (2008) also finds a large elasticity for women around 1948. Blundell and MaCurdy (1999) show that empirical studies using data from the 1970s and 1980s produce a wide range of elasticity estimates, with an average of about 0.8. Blau and Kahn (2005) finds that the participation elasticity was about 0.6 in 1980 and has steadily decreased over time to about 0.3 in 2000. Mroz (1987) summarizes the details of many of these early studies. Evaluations of the early negative income tax experiments that took place between 1968 and 1982 also find large elasticities (e.g. Burtless and Hausman 1978; Robins 1985; Munnell 1986).

Figures 1a, 1b, and 5 show that employment trends were on similar trajectories before the 1975 EITC policy was in place, then illustrate a four year period when EITC-eligible women entered the labor force in greater relative numbers and close the gap by about 6 pp. After 1979 a new equilibrium is reached where women with kids are still relatively less likely to work but more likely than they were before 1975. This implies that if the EITC was never enacted in 1975 the employment of women with children likely would have remained permanently lower over the next decade (and beyond). This also implies that if the policy had been enacted sooner the employment (and possibly empowerment) of women would have been occurred sooner.

This is the first systematic study of the introduction of the EITC. The results show that this program had a substantial and permanent impact of female employment. The large treatment effects estimated here are an important reminder that incentives matter in the design of public program.

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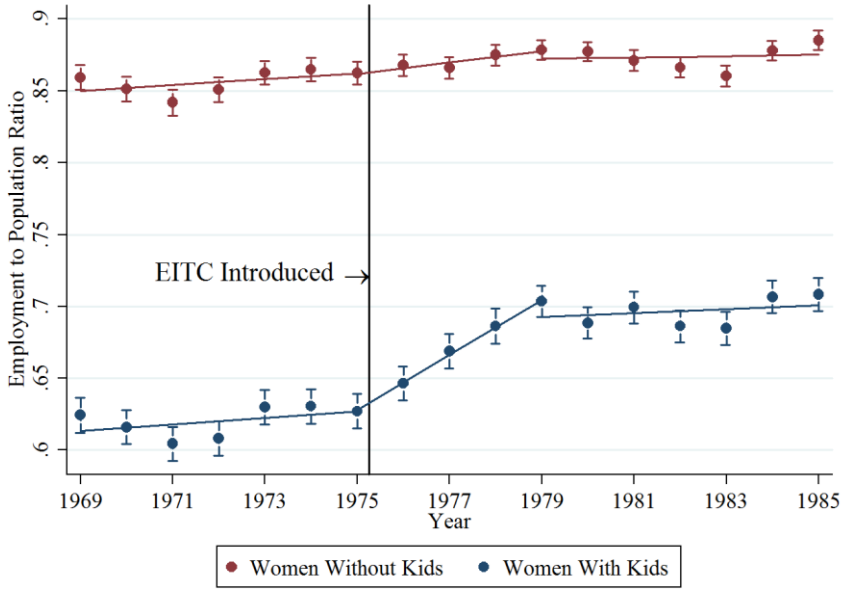


*Figure 1a: Annual Employment for Women with and without Kids*

See text for details.

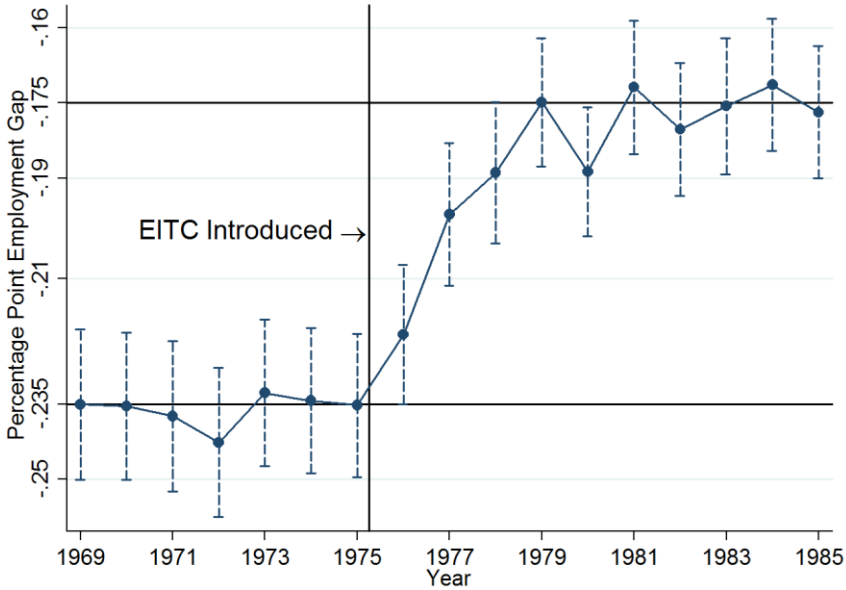


*Figure 1b: Gap in Employment for Women with Kids Compared to Women without Kids*

See text for details

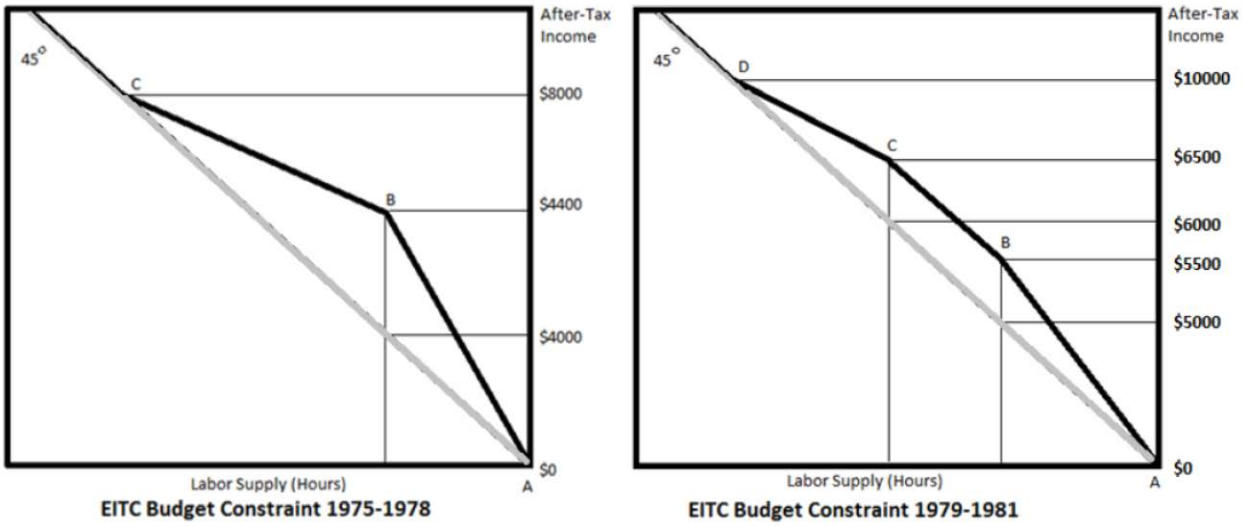


*Figure 2: Budget Constraints under the EITC*

See text for details

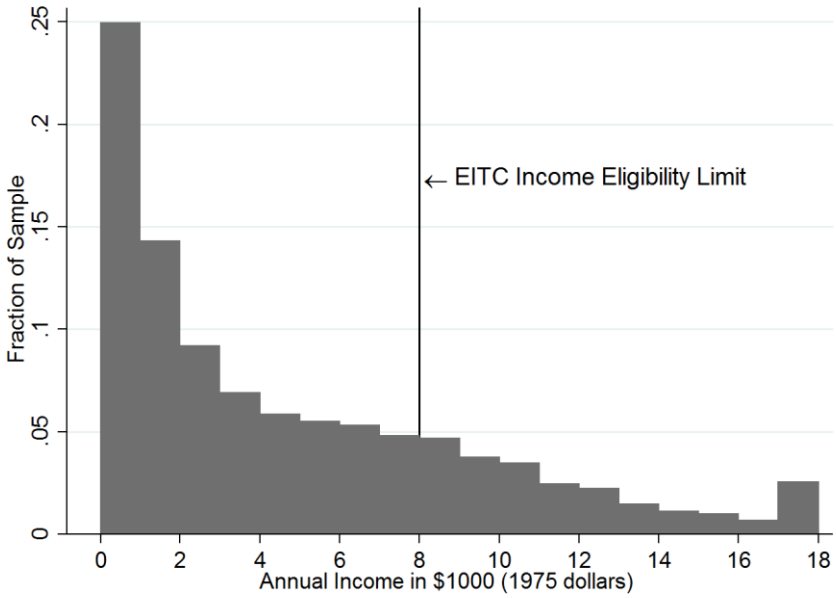


*Figure 3: Income Distribution for Women in my Sample (top-coded at $18,000)*

See text for details

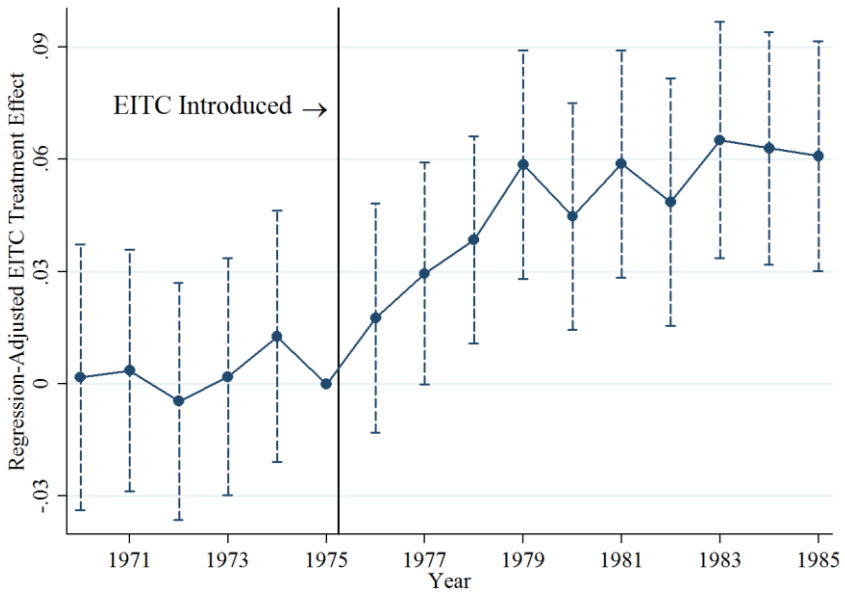


*Figure 4: Annual Treatment Effect on Employment*

See text for details

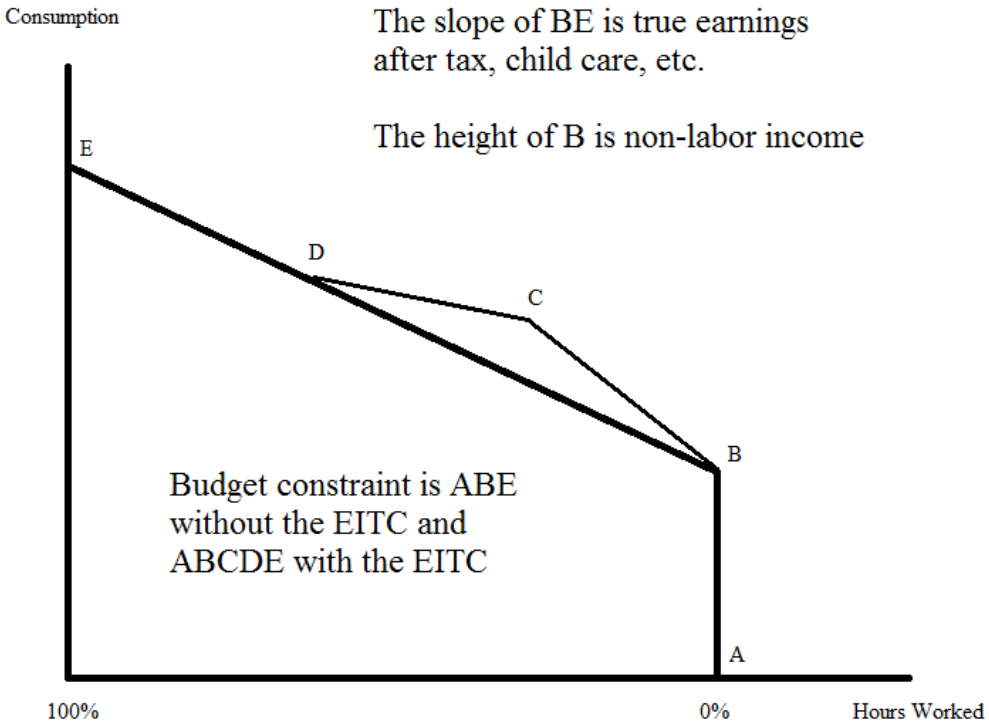


*Figure 5: Response to the EITC Depends on Non-Labor Income and True Earnings*

See text for details

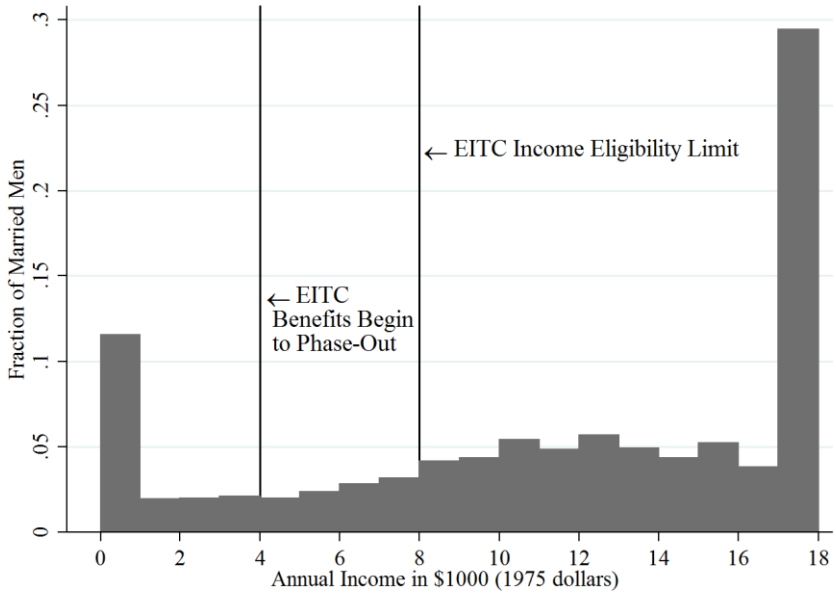


*Figure 6: Income Distribution for Spouses of Married Women in Sample (top-coded at $18,000)*

See text for details

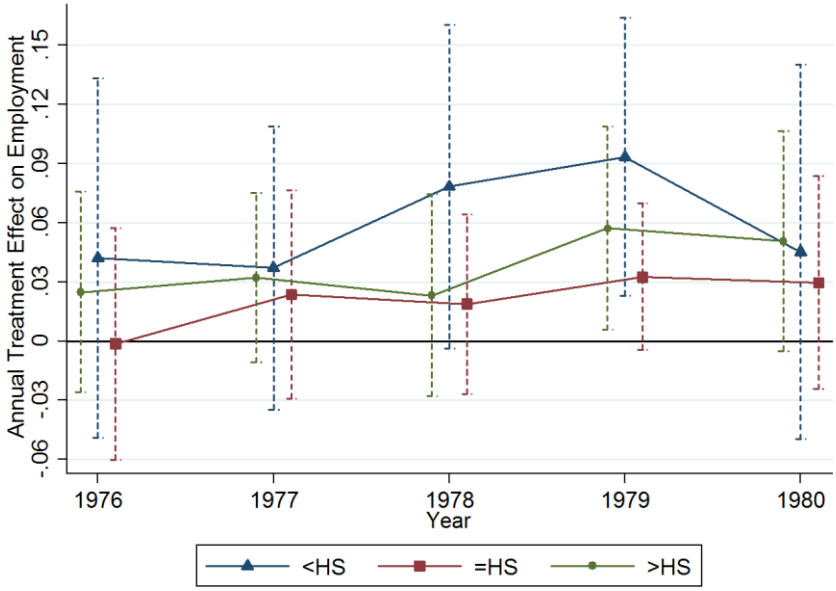


*Figure 7: Annual Treatment Effect on Employment by Education Subgroup*

See text for details. The slight variation within each year is for visual purposes only.

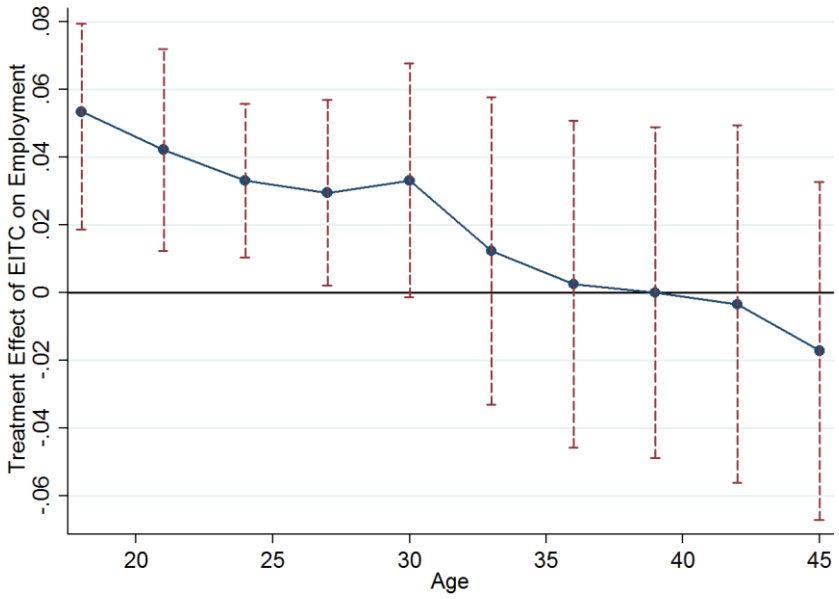


*Figure 8: Treatment Effect by Age*

See text for details

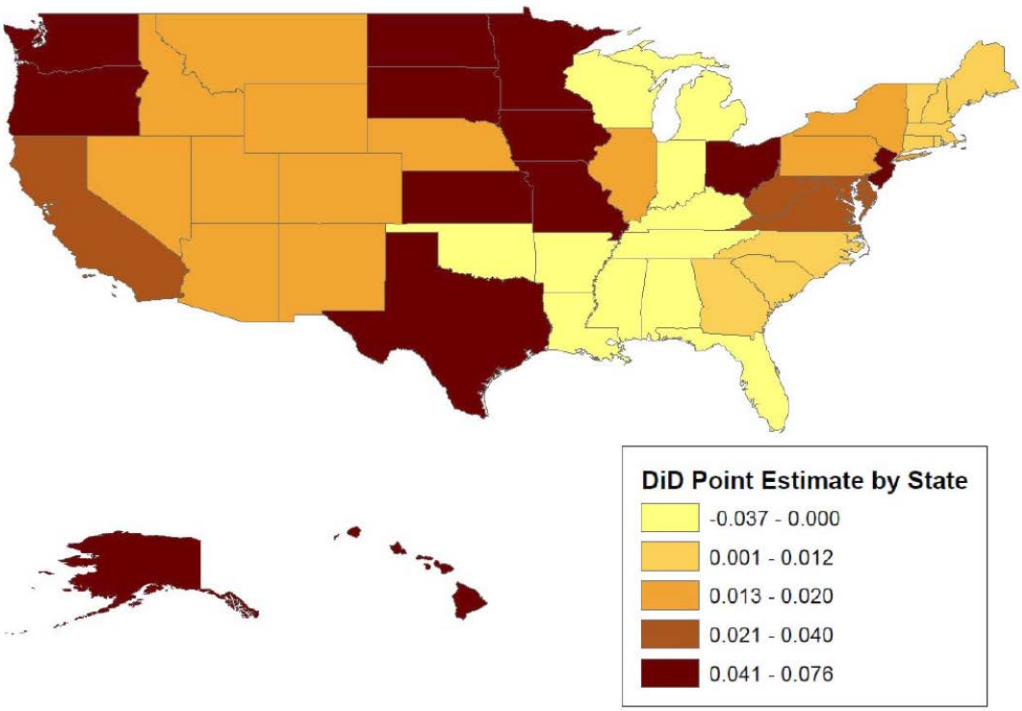


*Figure 9: Treatment Effect by State*

See text for details

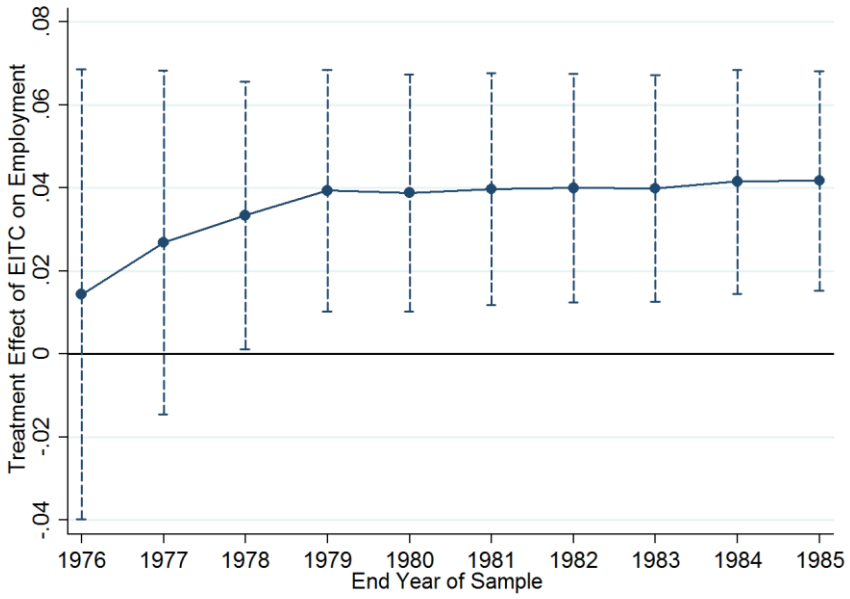


*Figure 10: Robustness of DD Estimate to when Sample Period Ends*

See text for details

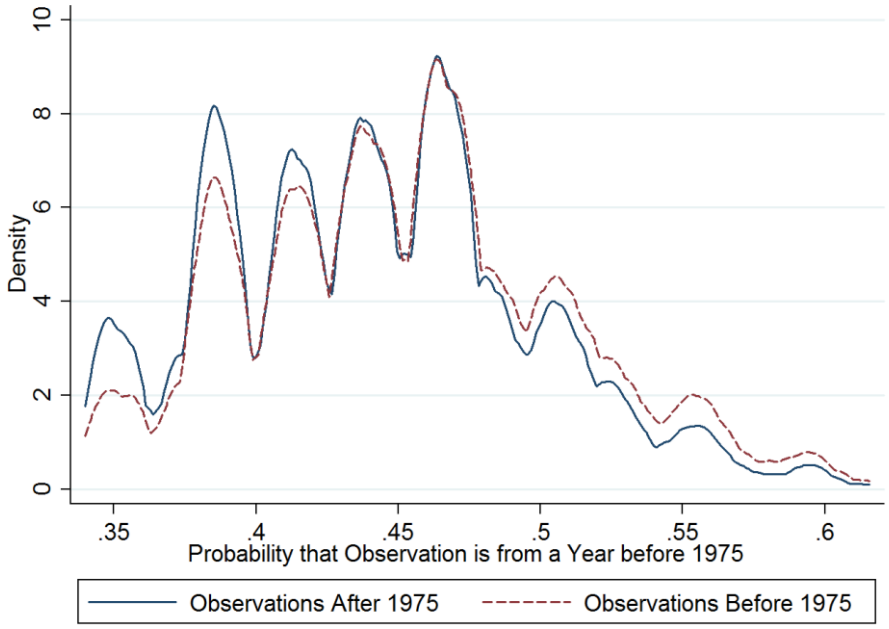


*Figure 11: Density Overlap Plot of X for Years Before and After 1975*

See text for details

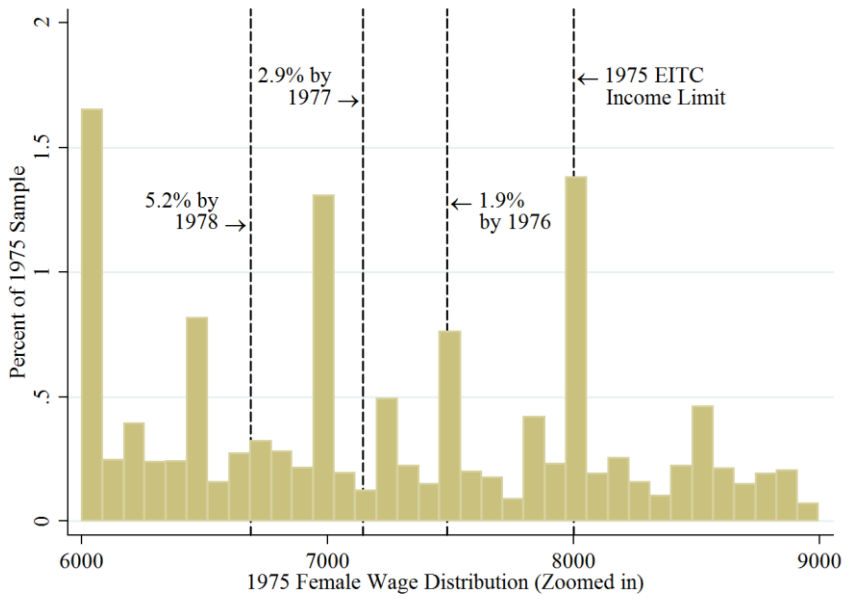


*Figure 12: Percent of Working Women that "Bracket-Creep" out of EITC Eligibility*

See text for details

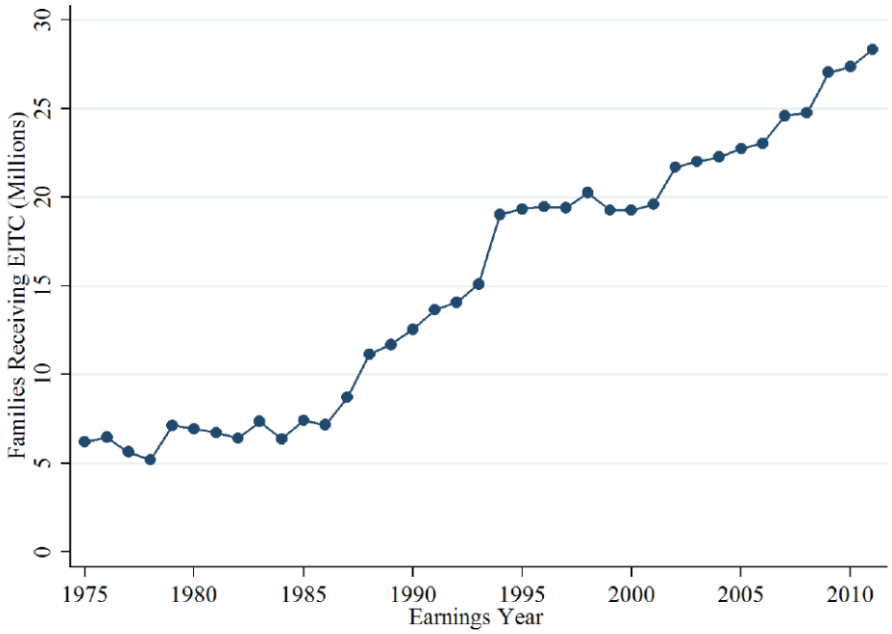


*Figure A1: Annual EITC Recipients (in Millions)*

See text for details

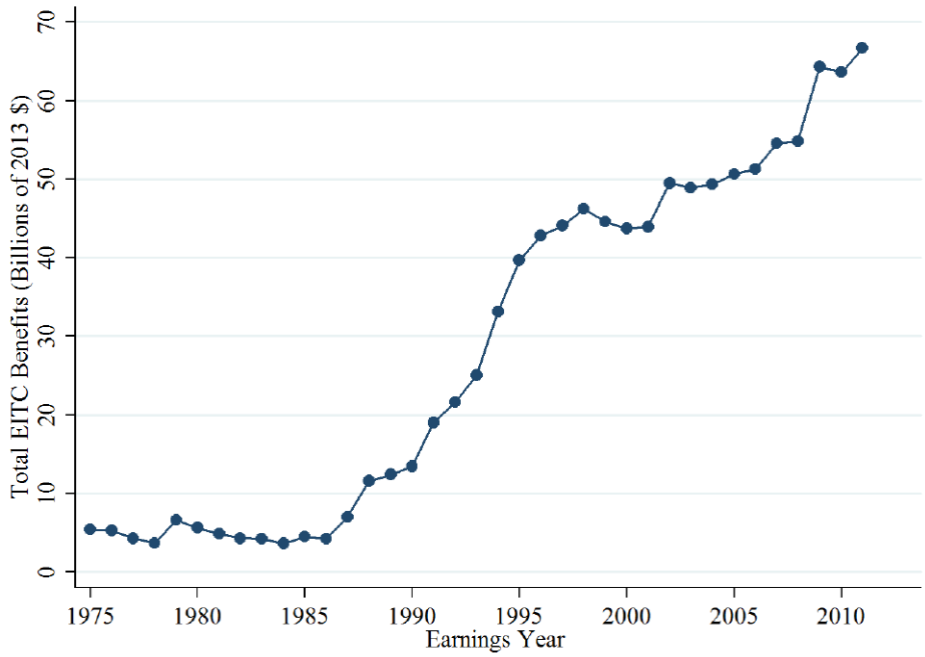


*Figure A2: Total EITC Benefits (in Billions of 2013 Dollars)*

See text for details

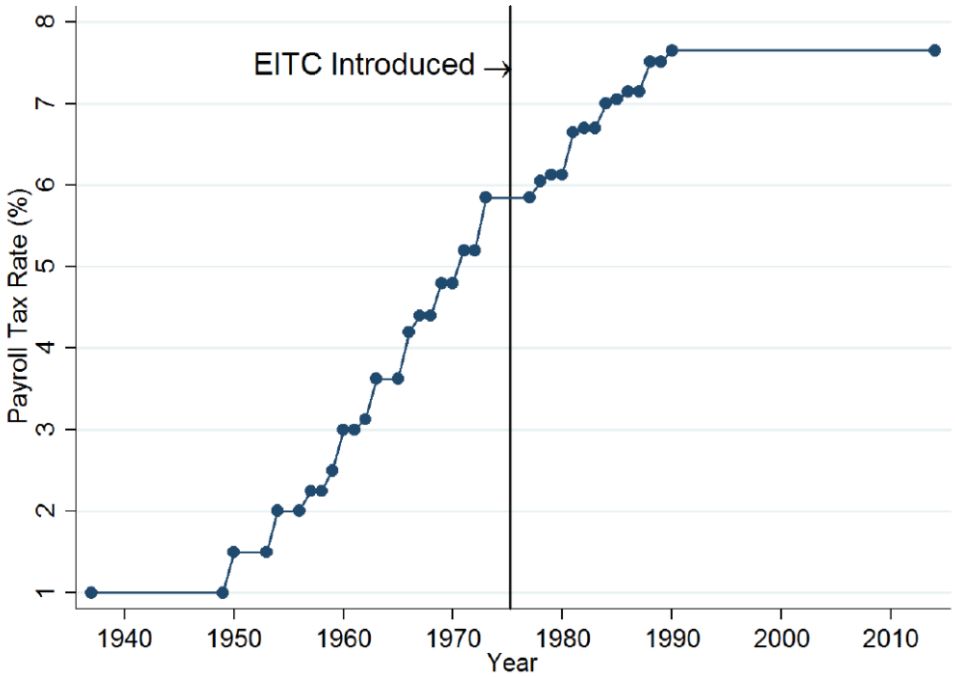


*Figure A3: Annual Payroll Tax Rate*

See text for details

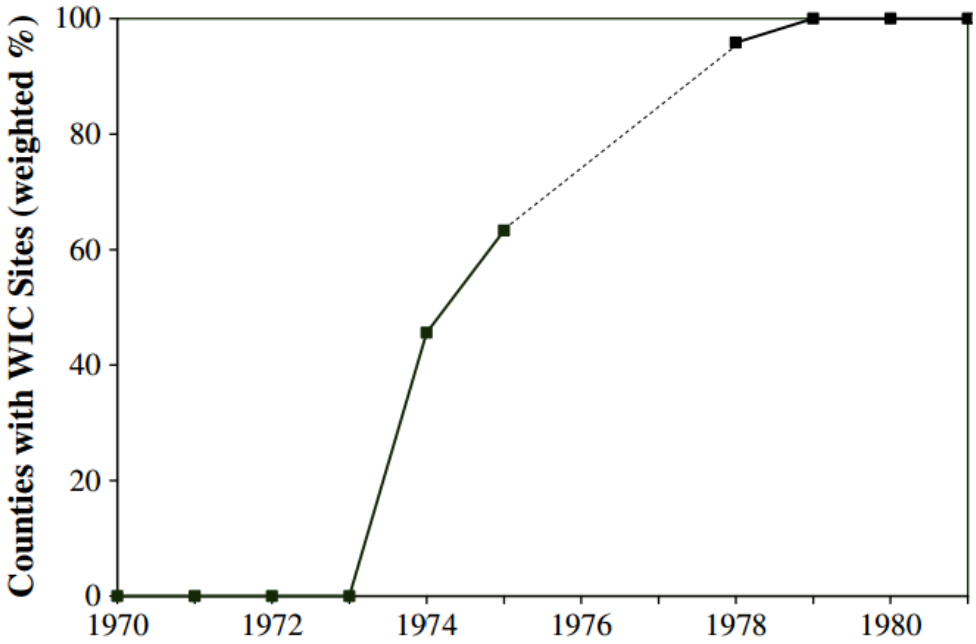


*Figure A4: Counties with WIC (from Hoynes, Page, Stevens 2011)*

See text for details

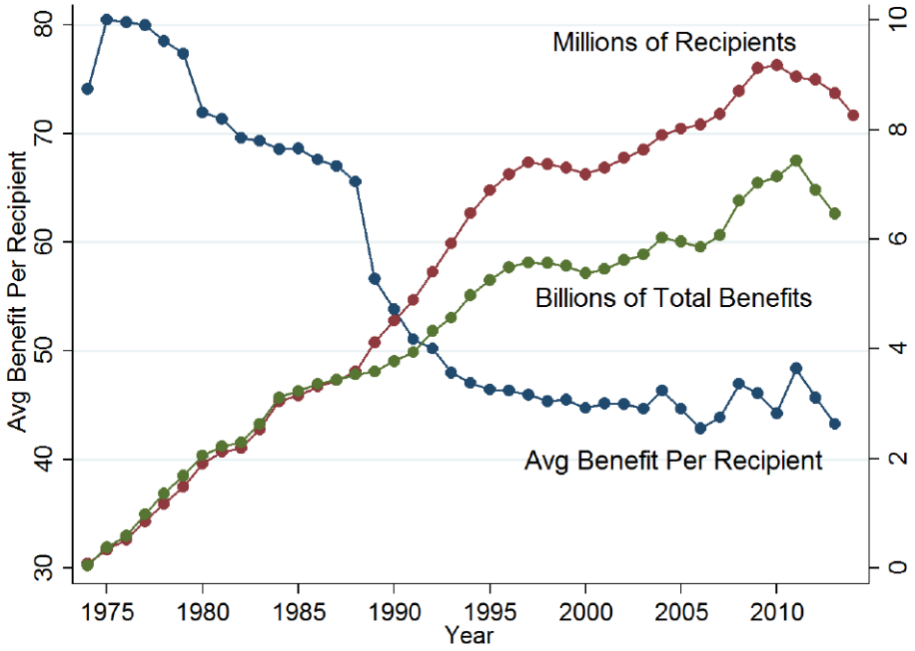


*Figure A5: WIC Trends*

See text for details

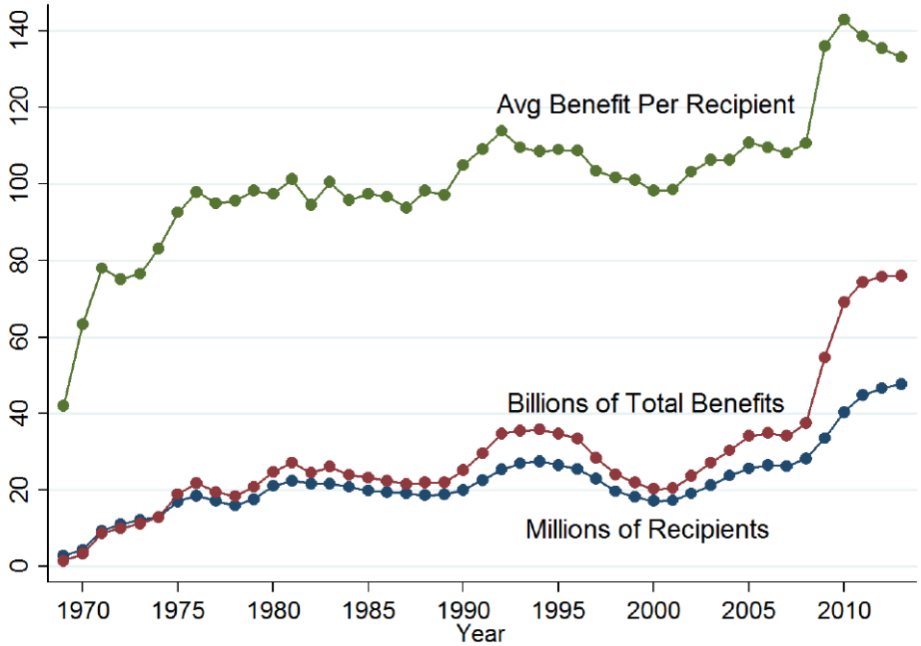


*Figure A6: Food Stamps Trends*

See text for details

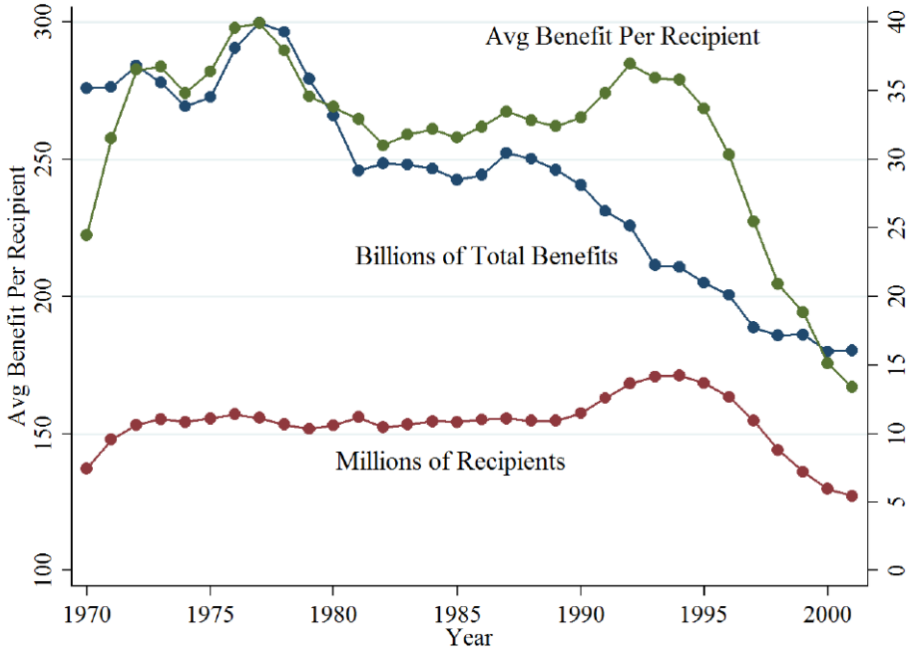


*Figure A7: AFDC Trends*

See text for details

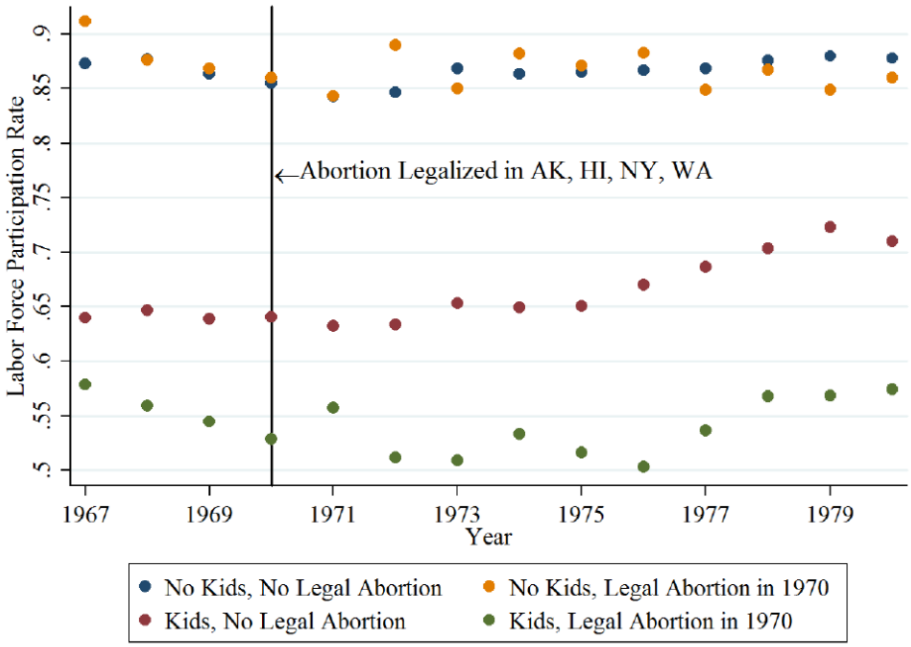


*Figure A8: Comparing Female Employment in States with and without 1970 Legalized Abortion*

See text for details. Only point estimates shown.

# Table 1: Summary Statistics

| Variable | All | Kids | No Kids | Pre 1975 | Post 1975 | Married | Single |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Age | 27.23 | 32.68 | 23.67 | 27.22 | 27.23 | 31.97 | 25.8 |
| | (8.43) | (7.35) | (7.09) | (8.63) | (8.28) | (8.03) | (8.02) |
| Years of Education | 12.02 | 11.51 | 12.36 | 11.83 | 12.17 | 11.83 | 12.08 |
| | (2.55) | (2.6) | (2.46) | (2.57) | (2.53) | (2.77) | (2.48) |
| Nonwhite | 0.28 | 0.35 | 0.23 | 0.3 | 0.26 | 0.24 | 0.29 |
| | (0.45) | (0.48) | (0.42) | (0.46) | (0.44) | (0.43) | (0.46) |
| Kids Under 5 | 0.16 | 0.4 | 0 | 0.18 | 0.14 | 0.34 | 0.1 |
| | (0.37) | (0.49) | (0) | (0.38) | (0.35) | (0.47) | (0.31) |
| Family Size | 3.6 | 4.04 | 3.32 | 3.81 | 3.44 | 4.04 | 3.47 |
| | (2.1) | (1.7) | (2.27) | (2.16) | (2.03) | (1.74) | (2.18) |
| Earned Income (Nominal) | 4005.72 | 3400.98 | 4400.17 | 2937.74 | 4861.78 | 2669.51 | 4408.41 |
| | (4819.35) | (4666.01) | (4876.58) | (3603.36) | (5457.93) | (4134.07) | (4937.12) |
| Earned Income (2013 $) | 15749.82 | 13359.45 | 17308.98 | 14742.69 | 16557.11 | 10792.19 | 17243.88 |
| | (18161.74) | (17651.61) | (18319.71) | (17952.04) | (18288.22) | (16094.15) | (18481.27) |
| Income in EITC Range | 0.83 | 0.86 | 0.82 | 0.9 | 0.78 | 0.9 | 0.81 |
| | (0.37) | (0.35) | (0.39) | (0.3) | (0.42) | (0.3) | (0.39) |
| Earned Income Conditional on Working (Nominal) | 5325.67 | 5682.05 | 5162.43 | 4041.73 | 6294.11 | 4960.94 | 5398.1 |
| | (4883.64) | (4838.7) | (4895.47) | (3660.82) | (5435.98) | (4515.87) | (4950.25) |
| Earned Income Conditional on Working (2013 $) | 20939.65 | 22319.75 | 20307.5 | 20282.91 | 21435 | 20055.88 | 21115.14 |
| | (18162.23) | (17904.38) | (18244.42) | (18193.79) | (18122.82) | (17192.12) | (18343.79) |
| Income in EITC Range Conditional on Working | 0.58 | 0.46 | 0.67 | 0.63 | 0.55 | 0.44 | 0.63 |
| | (0.49) | (0.5) | (0.47) | (0.48) | (0.5) | (0.5) | (0.48) |
| Labor Force Participation (Income>0) | 0.75 | 0.6 | 0.85 | 0.73 | 0.77 | 0.54 | 0.82 |
| | (0.43) | (0.49) | (0.35) | (0.45) | (0.42) | (0.5) | (0.39) |
| Labor Force Participation (Hours>0) | 0.79 | 0.66 | 0.87 | 0.76 | 0.8 | 0.64 | 0.83 |
| | (0.41) | (0.47) | (0.34) | (0.42) | (0.4) | (0.48) | (0.38) |
| Annual Weeks Worked | 30.63 | 26.01 | 33.64 | 29.36 | 31.65 | 24.8 | 32.39 |
| | (21.41) | (22.76) | (19.9) | (21.64) | (21.16) | (22.64) | (20.7) |
| Annual Weeks Worked Conditional on Working | 38.98 | 39.34 | 38.81 | 38.38 | 39.45 | 38.48 | 39.1 |
| | (16.05) | (16.1) | (16.01) | (16.31) | (15.82) | (16.4) | (15.96) |
| Weekly Hours Worked | 22.69 | 18.85 | 25.2 | 21.56 | 23.61 | 17.13 | 24.37 |
| | (19.22) | (20.11) | (18.19) | (19.23) | (19.16) | (19.94) | (18.68) |
| Weekly Hours Worked Conditional on Working | 33.55 | 35.96 | 32.49 | 33.1 | 33.89 | 34.76 | 33.3 |
| | (13.49) | (12.49) | (13.77) | (13.63) | (13.37) | (13.92) | (13.39) |
| Observations | 132272 | 52638 | 79634 | 53415 | 78857 | 31464 | 100808 |

Note: Data source: 1972-1981 March CPS. Sample contains women 16-45 who are single or have husbands that earn under the EITC limit. I exclude women who were ill, disabled, retired or in school, and women with negative earnings or negative household earnings, and women with nonzero earnings and zero weeks of work. CPS weights used. Standard deviations are in parentheses.

## Table 2: Estimating Employment Treatment Effects

| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Kid | -0.247*** | -0.246*** | -0.029*** | -0.028*** | 0.002 | 0.001 |
| | (0.008) | (0.008) | (0.006) | (0.006) | (0.041) | (0.045) |
| Post1975 | 0.022*** | 0.052*** | 0.021*** | -0.026** | -0.021** | -0.027** |
| | (0.007) | (0.008) | (0.007) | (0.011) | (0.010) | (0.011) |
| Kid*Post1975 | 0.028** | 0.029** | 0.023*** | 0.023*** | 0.039*** | |
| | (0.011) | (0.011) | (0.008) | (0.008) | (0.007) | |
| Kid*1976 | | | | | | 0.017 |
| | | | | | | (0.014) |
| Kid*1977 | | | | | | 0.029** |
| | | | | | | (0.011) |
| Kid*1978 | | | | | | 0.038*** |
| | | | | | | (0.008) |
| Kid*1979 | | | | | | 0.059*** |
| | | | | | | (0.010) |
| Kid*1980 | | | | | | 0.046*** |
| | | | | | | (0.012) |
| Married | | | -0.218*** | -0.218*** | -0.224*** | -0.224*** |
| | | | (0.003) | (0.003) | (0.025) | (0.025) |
| Welfare income ($1000s) | | | -0.098*** | -0.098*** | -0.101*** | -0.101*** |
| | | | (0.003) | (0.003) | (0.003) | (0.003) |
| Any Children Under 5 | | | -0.096*** | -0.096*** | -0.102*** | -0.103*** |
| | | | (0.004) | (0.004) | (0.005) | (0.005) |
| Number of Children | | | -0.013*** | -0.013*** | -0.016*** | -0.016*** |
| | | | (0.002) | (0.002) | (0.002) | (0.002) |
| Nonwhite | | | -0.041*** | -0.041*** | -0.116*** | -0.116*** |
| | | | (0.003) | (0.003) | (0.005) | (0.005) |
| State Employment-Population Ratio | | | | 0.008*** | 0.008*** | 0.008*** |
| | | | | (0.001) | (0.001) | (0.001) |
| Controls | | | | | | |
| State and Year | | X | X | X | X | X |
| Age Cubic and Education Quadratic | | | X | X | X | X |
| National Unemployment Rate | | | | X | X | X |
| Interaction Controls | | | | | X | X |
| Observations | 132,272 | 132,272 | 132,272 | 132,272 | 132,272 | 132,272 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. National unemployment rate is collinear with year fixed effects, but enters in through the interactions in columns 5 and 6. Interaction controls include age*post75, nonwhite*kid, nonwhite*post75, age*kid, married*post, urate*married, empratio*kid, and urate*kid. Specification (5) is my preferred regression and will be used for the rest of the analysis. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit.

## Table 3: Estimating Employment Treatment Effects by Subgroup

| | | Subgroup | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All | Marital Status | | Education | | | Race | | Age | | | Region | | | |
| | | Married | Single | <HS | =HS | >HS | Black | White | 16-25 | 26-35 | 36-46 | NE | MW | S | W |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) | (15) |
| Kid | 0.002 | -0.242* | -0.068** | 0.211** | -0.071 | -0.030 | 0.345*** | 0.077 | 0.079 | -0.088 | -0.050 | -0.119 | 0.201** | -0.010 | -0.226 |
| | (0.041) | (0.126) | (0.034) | (0.089) | (0.043) | (0.048) | (0.068) | (0.065) | (0.062) | (0.067) | (0.116) | (0.116) | (0.085) | (0.061) | (0.140) |
| Post1975 | -0.021** | 0.012 | -0.020** | -0.089*** | -0.003 | 0.019 | -0.090*** | -0.002 | -0.028** | 0.002 | 0.022 | -0.051*** | -0.023 | -0.011 | 0.086* |
| | (0.010) | (0.028) | (0.009) | (0.020) | (0.013) | (0.012) | (0.019) | (0.011) | (0.012) | (0.016) | (0.024) | (0.018) | (0.020) | (0.018) | (0.044) |
| Kid*Post75 | 0.039*** | 0.029 | 0.024*** | 0.063*** | 0.021** | 0.038*** | 0.041*** | 0.038*** | 0.040*** | 0.033*** | -0.007 | 0.036*** | 0.052*** | 0.029** | 0.023 |
| | (0.007) | (0.019) | (0.007) | (0.014) | (0.008) | (0.008) | (0.015) | (0.009) | (0.009) | (0.010) | (0.014) | (0.013) | (0.012) | (0.014) | (0.015) |
| Obs | 132,272 | 31,464 | 100,808 | 40,802 | 53,088 | 38,382 | 18,768 | 95,483 | 67,767 | 36,773 | 27,732 | 28,890 | 33,386 | 38,645 | 31,351 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Controls are from Table 2 column 5 and include: state and year fixed effects, demographics, employment information, and interaction terms. The sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit. Each column further restricts this sample to the indicated subgroup.

## Table 4: Interacting DD with Spousal Income

| | All Married Women | |
|---|---|---|
| Variables | (1) | (2) |
| Kid | -0.4366*** | -0.4575*** |
| | (0.0643) | (0.0646) |
| Post1975 | 0.0625*** | 0.0693*** |
| | (0.0136) | (0.0138) |
| Kid*Post1975 | 0.0053 | 0.0526*** |
| | (0.0103) | (0.0105) |
| Kid*Post1975*Spousal Income (in $1000s) | | -0.0032*** |
| | | (0.0002) |
| Observations | 192,631 | 192,631 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Each column uses the full set of controls from Table 2 column 5: state and year fixed effects, demographic and employment information, and interaction terms. The sample in column 1 includes my sample defined in Table 2 as well as women with husbands earning over the EITC eligibility limit. The sample in columns 2 and 3 include only the married women from my sample defined in Table 2 as well as women with husbands earning over the EITC eligibility limit.

## Table 5: DD Estimates for Education Subgroups by Marital Status

| | Single Women | | | | Married Women (with Low Earning Spouses) | | | | All Married Women | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All | <HS Educ | =HS Educ | >HS Educ | All | <HS Educ | =HS Educ | >HS Educ | All | <HS Educ | =HS Educ | >HS Educ |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Kid | -0.068** | 0.117 | -0.140*** | 0.010 | -0.242* | 0.037 | -0.307** | -0.502** | -0.421*** | -0.130 | -0.519*** | -0.463*** |
| | (0.034) | (0.083) | (0.045) | (0.044) | (0.126) | (0.267) | (0.154) | (0.201) | (0.066) | (0.136) | (0.089) | (0.085) |
| Post1975 | -0.020** | -0.059** | -0.023 | 0.001 | 0.012 | -0.083 | 0.122*** | 0.026 | 0.058*** | -0.013 | 0.085*** | 0.068*** |
| | (0.009) | (0.026) | (0.018) | (0.017) | (0.028) | (0.052) | (0.039) | (0.044) | (0.013) | (0.025) | (0.020) | (0.017) |
| Kid*Post1975 | 0.024*** | 0.073*** | 0.007 | 0.006 | 0.029 | -0.012 | 0.012 | 0.060* | 0.006 | -0.012 | 0.012 | 0.008 |
| | (0.007) | (0.016) | (0.009) | (0.010) | (0.019) | (0.035) | (0.027) | (0.032) | (0.010) | (0.019) | (0.013) | (0.013) |
| Observations | 100,808 | 31,513 | 39,389 | 29,906 | 31,464 | 9,289 | 13,699 | 8,476 | 192,631 | 42,294 | 93,979 | 56,358 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Each column uses the full set of controls from Table 2 column 5: state and year fixed effects, demographic and employment information, and interaction terms. The sample in columns 1-4 include the married women from my sample defined in Table 2. The sample in columns 5-8 include the single women from my sample defined in Table 2. The sample in columns 9-12 include all married women

## Table 6: Interacting DD Estimate with Metropolitan Status

| | Baseline DD | Interact DD with Metropolitan Status |
|---|---|---|
| Variables | (1) | (2) |
| Kid | 0.002 | -0.004 |
| | (0.041) | -0.041 |
| Post1975 | -0.021** | -0.020* |
| | (0.010) | (0.011) |
| Mid-Sized City | | 0.004 |
| | | (0.003) |
| Non-MSA | | -0.018*** |
| | | (0.005) |
| Kid*Post1975 | 0.039*** | 0.033*** |
| | (0.007) | (0.008) |
| Kid*Post1975*Mid-Sized City | | 0.003 |
| | | (0.007) |
| Kid*Post1975*Non-MSA | | 0.017** |
| | | (0.008) |
| Observations | 132,272 | 132,272 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Controls and sample are as defined in other table notes. I group metropolitan status into three categories to capture those living in one of the 33 largest metro areas that are defined in every year of my data, those living in other (not as large) metro areas, and those living in non-metro, rural areas. Those living in big cities are the omitted category in column 3. Non-MSA are areas with less than 50,000 population and can be interpreted as more rural areas (Nelson 1986). In order to carry out more detailed cost of living analysis like in Fitzpatrick and Thompson (2010) I would need more variation and better data. It is difficult to use the 1970s March CPS data to get more variation since there are only 21 consistent state-groups and these can only be broken down further into metropolitan area, non-metropolitan area. Only 33 individual cities are consistently identified throughout my sample period.

## Table 7: Interacting DD Estimate With Maximum Potential EITC

| | Baseline Probit Regression | Interact DD with EITC Eligibility (in $100s) |
|---|---|---|
| Variables | (1) | (2) |
| Kid | -0.101** | -0.100** |
| | (0.040) | (0.040) |
| Post1975 | 0.003 | -0.005 |
| | (0.011) | (0.011) |
| Kid*Post75 | 0.028*** | 0.008 |
| | (0.007) | (0.008) |
| Kid*Post75*Max EITC $ (in 100s) | | 0.010*** |
| | | (0.001) |
| Observations | 293,439 | 293,439 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Sample includes all women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, and women with nonzero earnings and zero weeks of work.

## Table 8: Triple Differences Using Various Comparison Groups

| | | Comparison Group | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | EITC-Ineligible Women with Spouses Earning: | | Men | | | Older Women | | |
| | Baseline DD | Over 100% of EITC Limit | Between 100% and 200% of EITC Limit | All | All Single | All Single Under 46 | 55-65 | 55-75 | Over 55 |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Kid*Post1975 | 0.039*** | | | | | | | | |
| | (0.007) | | | | | | | | |
| Kid*Post1975*EITC Eligible | | 0.022** | 0.025** | | | | | | |
| | | (0.009) | (0.011) | | | | | | |
| Kid*Post1975*Woman | | | | 0.026*** | 0.029* | 0.023 | | | |
| | | | | (0.007) | (0.016) | (0.019) | | | |
| Kid*Post1975*Age 16-45 | | | | | | | 0.027** | 0.032*** | 0.032*** |
| | | | | | | | (0.013) | (0.013) | (0.012) |
| Observations | 132,272 | 293,439 | 206,100 | 533,162 | 249,474 | 231,919 | 361,921 | 395,553 | 410,506 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Regressions in columns 2-9 also include each individual and interaction component within the DDD variable. Though Kid*Post75 is in each of the triple difference regressions, it is omitted in columns 2-9. The sample in column 1 is as defined previously. The sample in columns 2 includes the sample from column 1 plus all women married to spouses earning above the EITC limit; the sample in column 3 also includes the sample from column 1 plus women married to spouses earning between 100% and 200% of the EITC limit. The sample in column 4-6 include the sample from column 1 plus all men, all single men, and all single men under 46 respectively. The sample in columns 7-9 include the main sample plus all women 55-65, 55-75, and 55-99 respectively.

## Table 9: Women With More Than One Child

| Variables | Baseline DD Regression (1) | Add: 2nd kid (2) | Add: 3rd and 4th kid (3) |
|---|---|---|---|
| Kid | 0.002 | 0.006 | -0.006 |
| | (0.041) | (0.041) | (0.041) |
| Post1975 | -0.021** | -0.021** | -0.021** |
| | (0.010) | (0.010) | (0.010) |
| Kid*Post75 | 0.039*** | 0.026*** | 0.026*** |
| | (0.007) | (0.009) | (0.009) |
| ≥ 2 Kids | | -0.066*** | -0.069*** |
| | | (0.007) | (0.007) |
| ≥ 2 Kids*Post75 | | 0.020** | 0.017** |
| | | (0.008) | (0.008) |
| ≥ 3 Kids | | | -0.031*** |
| | | | (0.008) |
| ≥ 3 Kids*Post75 | | | 0.015 |
| | | | (0.010) |
| ≥ 4 Kids | | | -0.002 |
| | | | (0.010) |
| ≥ 4 Kids*Post75 | | | -0.021** |
| | | | (0.010) |
| Observations | 132,272 | 132,272 | 132,272 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit.

## Table 10: Accounting for Potentially Endogenous Kids After 1975

| | Baseline Regression | Restricting Sample to Kids Born Pre 1975 |
|---|---|---|
| Variables | (1) | (2) |
| Kid | 0.002 | -0.018 |
| | (0.041) | (0.043) |
| Post1975 | -0.021** | -0.022** |
| | (0.010) | (0.010) |
| Kid*Post75 | 0.039*** | 0.040*** |
| | (0.007) | (0.008) |
| Observations | 132,272 | 124,459 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit. Column 2 also excludes families whose only EITC-eligible children were born after 1975.

## Table 11: Reweighting

| | | | Reweighting Sample After 1975 to Look like Sample Before 1975 | |
|---|---|---|---|---|
| | Baseline Probit | Baseline Logit | Using DFL | Using IPW |
| Variables | (1) | (2) | (3) | (4) |
| Kid | 0.002 | 0.022 | 0.027 | 0.015 |
| | (0.041) | (0.043) | (0.077) | (0.040) |
| Post1975 | -0.021** | -0.023** | -0.052*** | -0.019* |
| | (0.010) | (0.011) | (0.020) | (0.010) |
| Kid*Post1975 | 0.039*** | 0.039*** | 0.040*** | 0.036*** |
| | (0.007) | (0.007) | (0.009) | (0.007) |
| Observations | 132,272 | 132,272 | 132,272 | 132,272 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit.

## Table 12: Lower-Education Sample

| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Kid | -0.218*** | -0.216*** | -0.018* | -0.018** | 0.157* | 0.151* |
| | (0.010) | (0.010) | (0.009) | (0.009) | (0.083) | (0.086) |
| Post1975 | 0.009 | 0.023* | 0.016 | -0.069*** | -0.067*** | -0.069*** |
| | (0.011) | (0.013) | (0.012) | (0.017) | (0.018) | (0.021) |
| Kid*Post1975 | 0.029** | 0.031** | 0.039*** | 0.040*** | 0.043*** | |
| | (0.015) | (0.014) | (0.011) | (0.011) | (0.013) | |
| Kid*1976 | | | | | | 0.017 |
| | | | | | | (0.019) |
| Kid*1977 | | | | | | 0.035** |
| | | | | | | (0.018) |
| Kid*1978 | | | | | | 0.050** |
| | | | | | | (0.023) |
| Kid*1979 | | | | | | 0.066*** |
| | | | | | | (0.016) |
| Kid*1980 | | | | | | 0.040* |
| | | | | | | (0.021) |
| Married | | | -0.196*** | -0.196*** | -0.214*** | -0.215*** |
| | | | (0.006) | (0.006) | (0.034) | (0.034) |
| Welfare income ($1000s) | | | -0.126*** | -0.126*** | -0.129*** | -0.129*** |
| | | | (0.004) | (0.004) | (0.004) | (0.004) |
| Any Children Under 5 | | | -0.123*** | -0.124*** | -0.141*** | -0.141*** |
| | | | (0.005) | (0.005) | (0.006) | (0.006) |
| Number of Children | | | -0.014*** | -0.014*** | -0.015*** | -0.015*** |
| | | | (0.002) | (0.002) | (0.002) | (0.002) |
| Nonwhite | | | -0.003 | -0.004 | -0.114*** | -0.114*** |
| | | | (0.005) | (0.005) | (0.008) | (0.008) |
| State Employment-Population Ratio | | | | 0.014*** | 0.016*** | 0.016*** |
| | | | | (0.002) | (0.002) | (0.002) |
| Controls | | | | | | |
| State and Year | | X | X | X | X | X |
| Age Cubic | | | X | X | X | X |
| National Unemployment Rate | | | | X | X | X |
| Interaction Controls | | | | | X | X |
| Observations | 73,807 | 73,807 | 73,807 | 73,807 | 73,807 | 73,807 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. National unemployment rate is collinear with year fixed effects, but enters in through the interactions in columns 5 and 6. Interaction controls as defined in Table 2. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit.

## Table 13: Alternate Ways to Treat Imputed CPS Observations

| | Baseline (using March CPS imputations) | Dropping Imputed Observations | Using DFL Weights | Using IPW | Assigning 0 to all Imputed Obs | Assigning 1 to all Imputed Obs |
|---|---|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
| Kid | 0.002 | 0.008 | -0.054 | 0.086 | -0.049 | -0.001 |
| | (0.041) | (0.042) | (0.033) | (0.056) | (0.045) | (0.041) |
| Post1975 | -0.021** | -0.022** | -0.011 | -0.036*** | -0.039*** | -0.021** |
| | (0.010) | (0.011) | (0.010) | (0.013) | (0.011) | (0.010) |
| Kid*Post1975 | 0.039*** | 0.040*** | 0.034*** | 0.045*** | 0.045*** | 0.039*** |
| | (0.007) | (0.007) | (0.006) | (0.009) | (0.007) | (0.007) |
| Observations | 132,272 | 128,577 | 128,577 | 128,577 | 132,272 | 132,272 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5: state and year fixed effects, demographics, employment information, and interaction terms. Sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit.

# Table A1: Robust to Excluded Groups of Women

| | | Sample Also Includes the Following Women: | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Baseline Sample | Not in Labor Force Due to: | | | | | Not in Sample Due to Irregular Earnings: | | | | Missing Spouse Info | All Women From (2)-(11) |
| | | In School Full-Time | Personal Reasons | Health | Disabled | Retired | Income<0 | Household Income<0 | Family Income<0 | Income>0 & Weeks Worked=0 | | |
| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Kid | 0.0015 | 0.1035** | -0.0012 | 0.0015 | -0.0217 | -0.0018 | 0.0032 | 0.0060 | 0.0013 | -0.0021 | -0.0009 | 0.0666 |
| | (0.0414) | (0.0479) | (0.0415) | (0.0414) | (0.0428) | (0.0415) | (0.0413) | (0.0417) | (0.0413) | (0.0412) | (0.0413) | (0.0487) |
| Post1975 | -0.0212** | -0.0202* | -0.0214** | -0.0212** | -0.0279*** | -0.0218** | -0.0213** | -0.0204* | -0.0214** | -0.0225** | -0.0188* | -0.0251** |
| | (0.0105) | (0.0120) | (0.0104) | (0.0105) | (0.0107) | (0.0105) | (0.0105) | (0.0104) | (0.0105) | (0.0105) | (0.0106) | (0.0122) |
| Kid*Post1975 | 0.0389*** | 0.0446*** | 0.0388*** | 0.0389*** | 0.0394*** | 0.0390*** | 0.0390*** | 0.0382*** | 0.0389*** | 0.0392*** | 0.0357*** | 0.0411*** |
| | (0.0073) | (0.0076) | (0.0072) | (0.0073) | (0.0076) | (0.0073) | (0.0072) | (0.0073) | (0.0072) | (0.0073) | (0.0072) | (0.0075) |
| Observations | 132,272 | 162,523 | 132,457 | 132,274 | 135,606 | 132,312 | 132,465 | 132,577 | 133,113 | 132,539 | 135,553 | 170,971 |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. Controls are from Table 2 column 5 and include state and year fixed effects, demographic and employment information, and interaction terms.

## Table A2: Showing the Impact of Each Individual Control Variable on the DiD Estimate

| Variables | None | Married | Welfare income | Any kids under 5 | Number of kids | Nonwhite | Age | Age^2 | Age^3 | Years of education | (Years of educ)^2 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Kid | -0.247*** | -0.184*** | -0.199*** | -0.189*** | -0.129*** | -0.237*** | -0.270*** | -0.261*** | -0.254*** | -0.217*** | -0.218*** |
| | (0.008) | (0.009) | (0.007) | (0.008) | (0.008) | (0.008) | (0.009) | (0.009) | (0.008) | (0.008) | (0.008) |
| Post1975 | 0.022*** | 0.016** | 0.026*** | 0.022*** | 0.022*** | 0.020*** | 0.022*** | 0.022*** | 0.022*** | 0.015** | 0.015** |
| | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.006) | (0.007) | (0.007) | (0.007) | (0.006) | (0.007) |
| Kid*Post1975 | 0.028** | 0.023** | 0.037*** | 0.022** | 0.018 | 0.028** | 0.028** | 0.028** | 0.028** | 0.022** | 0.023** |
| | (0.011) | (0.011) | (0.009) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |

| Variables | Year dummies | State dummies | Emp-Pop Ratio | Unemp. rate | Nonwhite* kid | Nonwhite* post1975 | Age*kid | Married* post75 | Unemp rate *Married | Emp-Pop ratio * kid | Unemp. rate * kid |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (12) | (13) | (14) | (15) | (16) | (17) | (18) | (19) | (20) | (21) | (22) |
| Kid | -0.247*** | -0.246*** | -0.246*** | -0.247*** | -0.243*** | -0.245*** | -0.308*** | -0.244*** | -0.190*** | -0.404*** | -0.243*** |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) | (0.014) | (0.008) | (0.008) | (0.032) | (0.026) |
| Post1975 | 0.054*** | 0.022*** | 0.010 | 0.022*** | 0.022*** | 0.047*** | 0.022*** | 0.036*** | 0.018*** | 0.022*** | 0.022*** |
| | (0.011) | (0.006) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) | (0.007) |
| Kid*Post1975 | 0.028** | 0.029** | 0.028** | 0.028** | 0.028** | 0.036*** | 0.028** | 0.081*** | 0.028** | 0.017 | 0.029** |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.012) | (0.011) | (0.012) | (0.012) |

Note: *** p<0.01, ** p<0.05, * p<0.1. Data source: 1972-1981 March CPS. Binary dependent variable labor force participation. CPS weights used. Standard errors are robust to heteroskedasticity and clustered at the state-year level. The sample includes women 16-45 excluding women who were ill, disabled, retired or in school, as well as women with negative earnings or negative household earnings, women with nonzero earnings and zero weeks of work, and women with husbands earning over the EITC eligibility limit. This table is primarily an exercise in transparency.