The Rise of Working Mothers and the 1975 Earned Income Tax Credit

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Abstract

The rise of working mothers radically changed the U.S. economy and the role of women in society. Time-series data show a rapid increase in the employment of mothers — relative to women without children — beginning in the mid-1970s. In one of the first systematic studies of the 1975 introduction of the Earned Income Tax Credit (EITC), I show that this program led to a 4-percentage-point (or 7.5 percent) rise in maternal employment — representing about one million mothers — and conclude that the 1975 EITC can help explain why the U.S. has such a high fraction of working mothers despite few childcare subsidies or parental leave policies. I then test whether the EITC affected social attitudes towards working women. I find that states with larger EITC responses — and larger predicted responses based on pre-1975 demographic and occupational traits — had larger post-1975 increases in attitudes approving of women working. Results are largest among lower-education men — who were most exposed to these newly working women — and do not appear to be driven by pre-1975 attitudes, changes in demographics, or general trends in social norms. As a check on whether large increases of female workers can affect social attitudes, I also find attitude changes from increased female employment during World War II.

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A surprising difference between the U.S. and other developed countries is the large number of mothers in paid work, especially new mothers. By 2000, 56 percent of mothers with infants worked in the U.S. compared to 25 to 45 percent in other developed countries (OECD 2007). The U.S. was not always an outlier in this regard; the number of working mothers in recent decades is also high by U.S. historical standards (Goldin 1990, Costa 2000) and is puzzling since few childcare subsidies or family-friendly work policies (e.g. paid parental leave) exist in the U.S. (Ruhm 1998). This paper argues that the 1975 introduction of the Earned Income Tax Credit (EITC) can help explain this puzzle. I find that the EITC played an important role in the rise of working mothers, which subsequently led to more positive social attitudes towards working women.

Time-series evidence shows that the relative employment of mothers — compared to women without children — rapidly increased after 1975 (Figure 1A). In fact, between 1975 and 1980, the relative employment of mothers rose by about 5-percentage points, closing the employment gap between these two groups by 25 percent. Using March Current Population Survey (CPS) data and a dynamic difference-in-differences (DD) approach, I show that most of the 1975-to-1980 increase in the relative employment of mothers can be attributed to the 1975 EITC. Interestingly, the unadjusted trend in maternal employment is nearly identical to the regression-adjusted trend that controls for a rich set of individual- and state-by-year-level covariates (Figure 1B). This increase in employment also led to stronger labor-force attachment and work intensity: my estimates imply that the EITC increased average annual work hours by 7.3 percent (43 hours), annual earnings by 9.6 percent ($965 2013 dollars), and hourly wages by 6 percent ($0.41 2013 dollars). Results imply a participation elasticity of 0.6 to 0.8, larger than recent estimates (Chetty et al. 2012), but in line with estimates of female-employment elasticity during this period (Blau and Kahn 2005; Heim 2007).

Consistent with the 1975 EITC being the causal mechanism behind this rise in employment, I find larger responses from mothers more likely to be EITC-eligible and null responses from placebo groups of mothers not eligible for EITC benefits. I also use this placebo group of EITC-ineligible mothers in a triple differences (DDD) specification to net out contemporaneous policies (e.g. birth control, divorce laws, abortion) affecting the employment of all mothers: DDD estimates corroborate the DD results. Heterogeneous responses varied by marital status, spousal earnings, education, and age in a manner consistent with a simple

1Cross-country comparisons of working mothers are not straightforward since many countries count mothers on paid parental leave as employed (OECD 2007). For example, the 2003 employment rates of mothers with infants (under 3) in Austria, Finland, and Sweden is generally reported as 80.1, 52.1, and 72.9 percent, however, excluding mothers on paid parental leave results in significantly lower rates of 40.1, 33.8, and 45.1 percent (OECD 2007, p.57). Details on parental leave policies across countries found here: http://www.oecd.org/els/soc/PF2_1_Parental_leave_systems.pdf.
labor-supply model. As with previous EITC studies, I find a null average response among married mothers, but allowing for heterogeneous treatment effects shows that married women with lower-earning spouses responded positively to the EITC, much like unmarried mothers. This result is not particular to the 1970s: I find a similar response to the 1986 and 1993 EITC expansions as well. This result is intuitive but has been missed by previous studies that focus on average treatment effects.

My estimates suggest that the EITC encouraged about one million mothers to begin working after 1975, however, this is likely to underestimate the overall effect of the EITC. In section 7, I use data from the General Social Survey (GSS) to examine whether the EITC — and the large influx of working mothers — affected social attitudes towards working women (“gender-equality attitudes”). This hypothesis is motivated by evidence showing that such attitudes are malleable and increase with exposure to working women: Fernández et al. (2004) and Olivetti et al. (2016) find that having a working mother — and having friends with working mothers — leads to more positive gender-equality attitudes in adulthood. Additionally, Finseraas et al. (2016) shows that exposure to female colleagues reduces discriminatory attitudes.

With these results in mind, it is possible that the attitudes of millions of Americans were affected when a million mothers began working after 1975.

My empirical strategy for estimating the impact of the EITC on gender-equality attitudes is a two-sample, two-step process where I characterize and exploit geographic heterogeneity in the EITC response and test whether states with larger EITC responses experienced larger changes in gender-equality attitudes after 1975. I use both the actual state EITC response and the predicted state EITC response — based on state demographic and occupational traits in place before 1975. Using the predicted response helps alleviate concerns about the potential endogeneity of gender-equality attitudes and EITC response. I find that states with larger EITC responses experienced larger increases in gender-equality attitudes after 1975. Various tests suggest that these results are not driven by pre-1975 attitudes, demographics,

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2 Additional evidence that attitudes are influenced by exposure are provided by Beaman et al. (2012) — who exploit a randomized natural experiment in India and find that a mandated increase in female leadership positions increased female role models and the career aspirations and educational attainment of young females —, Stouffer et al. (1949) — who shows that during WWII, white soldiers that served in mixed-race companies had higher racial-equality attitudes than white soldiers that did not —, and Finseraas and Kotsadam (2015) — who find that personal contact with ethnic minorities reduces discriminatory attitudes. There is also experimental evidence showing that such attitudes are malleable (Heilman and Martel, 1986; Lowery et al., 2001; Dasgupta and Asgari, 2004; Dasgupta and Rivera, 2008).

3 Further evidence that rise of working mothers was a salient phenomenon and exposure to working women increased after 1975 is shown in Figure 2, which uses Google n-grams to show that the phrases working mom and — the previously redundant — stay at home mom became much more common after the mid-1970s, suggesting that media exposure to working mothers increased. Google ngrams (http://books.google.com/ngrams) charts annual frequencies of words and phrases using over 5 million sources — over 500 billion words — printed between 1500 and 2008 (Michel et al., 2011).
or general trends in social norms. Attitude changes are observed for both men and women, both within and across regions. Since I find that mothers with lower education were most affected by the EITC, men with lower education were more likely to be exposed to these newly working women; I verify that these men experienced the largest changes in gender-equality attitudes. I also use a placebo social attitude on racial equality to test and rule out the possibility that states with high EITC responses were simply experiencing changes in various types of egalitarian attitudes.

In sum, this study adds to our understanding of the EITC — one of the most consequential social programs in U.S. history — by evaluating the 1975 program introduction and finding that it had important effects on both maternal employment and social attitudes towards working women.

1. EITC History and Known Effects of the EITC

The 1975 EITC came to exist partly as a response to the 1960s War on Poverty. Although many War on Poverty programs succeeded in improving health and decreasing poverty (Almond et al., 2011; Hoynes et al., 2011; Bailey and Goodman-Bacon, 2015; Goodman-Bacon, 2013), some also had implicit work disincentives (Moffitt, 1992; Hoynes, 1996; Hoynes and Schanzenbach, 2012). By the 1970s, welfare — not poverty — was perceived as the dominant social problem (O’Connor, 1998). In an attempt to reduce poverty, simplify the welfare system, and encourage work, momentum built for a guaranteed annual income and had support from economists Milton Friedman (Friedman, 1962) and James Tobin (Tobin, 1969). The U.S. House of Representatives passed such a plan — the Family Assistance Plan — with the backing of President Richard Nixon in 1970 (Rhys-Williams, 1943) was among the first to outline this type of program. The FAP would have guaranteed an annual income of $3,100 (2013 dollars) for each parent and $1,800 for each child, a family of four would have received $9,800 (less than the real 1970 poverty line of about $23,000 for a family of four). Benefits would phase out at 50 percent when household earned income surpassed $4,400 (Trattner, 2007, p. 315). See New York Times April 17, 1970.

Liberals and Democrat presidential candidate McGovern wanted a much higher guarantee, roughly $35,000 (2013 dollars).
The 1975 EITC was a refundable tax credit that provided a 10 percent earnings subsidy to working parents with annual household earnings up to $18,000 in 2013 dollars (or $4,000 in nominal dollars). The EITC also provided benefits to working parents with earnings above $18,000, but these benefits decreased at a rate of 10 percent and reached zero for earnings above $36,000 (Figures 3A and 3B). At this time, there were no additional EITC benefits for having more than one child and benefits did not vary by state or marital status.

Since 1975, the EITC has been expanded many times and has grown into one of the largest anti-poverty program in the U.S., redistributing $66 billion to 28 million individuals and lifting 6.5 million people — including 3.3 million children — out of poverty in 2013 (CBPP, 2014). The EITC has been shown to raise maternal employment (Dickert et al., 1995; Eissa and Lieberman, 1996; Meyer and Rosenbaum, 2001; Hotz and Scholz, 2006; Eissa et al., 2008), increase earnings (Dahl et al., 2009), and improve health (Evans and Garthwaite, 2014). The EITC has also decreased poverty (Scholz, 1994; Neumark and Wascher, 2001; Meyer, 2010; Hoynes and Patel, 2015) and helped children of EITC recipients by improving short-run outcomes like infant health (Hoynes et al., 2015), childhood health (Averett and Wang, 2015), and test scores (Chetty et al., 2011; Dahl and Lochner, 2012), and long-run outcomes like educational attainment (Manoli and Turner, 2014; Bastian and Michelmore, 2015) and employment and earnings (Bastian and Michelmore, 2015). The EITC’s unintended consequences include lower pre-tax wages of low-skill workers (Leigh, 2010; Rothstein, 2010) and possible effects on fertility and marriage. See Nichols and Rothstein (2015) for a recent review of the EITC literature.

Although much is known about the EITC, almost nothing is known about the 1975 introduction or about how this policy may have affected attitudes towards working women. This paper shows that the 1975 EITC encouraged about one million mothers to begin working — more than the 1986 and 1993 EITC expansions combined — and challenged gender-role

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7 To be EITC eligible, married couples had to file taxes jointly and families had to have at least one child living in their home for more than half the year (“residency test”) that was under 19, under 24 if a full-time student, or any age if disabled. Before 1987 taxfilers did not have to provide Social Security numbers for dependents. Until 1990 taxfilers had to demonstrate they provided at least half the costs of maintaining the household (“support test”), implying that cash and in-kind public assistance had to be less than half of the household budget (Holtzblatt, 1991; Holtzblatt et al., 1994). Since I do not observe tax filing, I assume all single women file taxes as household head, all married couples file joint taxes, and all family members under 19 (or 24 if a full-time student) are dependent children. I also treat all subfamilies within a household as separate tax-filing units and potentially EITC-eligible.

8 Figure 3A shows a budget constraint under the EITC and Figure 3B illustrates the “phase-in” and “phase-out” portion of the EITC schedule while contrasting the 1975 EITC schedule with the 2013 EITC. See Table B2 notes for details.

9 Baughman and Dickert-Conlin (2009) and Bastian (2016) find positive effects on fertility. For marriage, Dickert-Conlin and Houser (2002), Herbst (2011), and Michelmore (2015) find negative effects, while Bastian (2016) finds positive effects.
attitudes in a context where men were often the breadwinners and women the homemakers.

Almost all studies of the EITC ignore the first decade of the program’s existence.\textsuperscript{11} Although there was little policy variation in the first decade of the EITC, the 1975 introduction was itself a large policy change that has received surprisingly little attention. One reason that the 1975 EITC has been overlooked is the common misconception that the original EITC was too small to have had much of an employment effect.\textsuperscript{12} However, Figure A1 shows that the 1975 EITC was large in at least three ways. First, about half of all households had earning below the EITC income limit; second, cash benefits were quite high, up to $1,800 in 2013 dollars (about 10 percent of the poverty line for a single mother with two kids); third, a 10 percent wage subsidy represented a substantial year-over-year increase in potential earnings. Two additional reasons to expect the 1975 EITC to have had a large impact is that female labor supply elasticity was larger during this period than in later decades (Blau and Kahn, 2005; Heim, 2007) and — ceteris paribus — the fraction of mothers on the margin of working generally declines with each subsequent program expansion, reflecting decreasing marginal treatment effects (Björklund and Moffitt, 1987; Heckman and Vytlacil, 1999).\textsuperscript{13}

2. Conceptual Framework

Since the 1975 EITC was a wage subsidy for low-income parents it should have had a positive effect on the employment of mothers and no effect on women without children and higher-income mothers.\textsuperscript{14} Intuition for this can be formalized in the following framework where a woman divides her one unit of time between labor, leisure, and home production. Her utility is a function of consumption $c(.)$, leisure $L$, and cost of working $g_{st}(.)$. Individuals are denoted by $i$, states by $s$, and years by $t$.

$$V(c(.), L, g_{st}(.)) = \max_{l, h, l \in [0, 1]} \left[ c(l_i, w_i, n_i, h_i, k_i) + L^\alpha_i - g_{st}(l_i, k_i) \right]$$

\textsuperscript{11}Bastian and Michelmore (2015) is the only other empirical paper that I know of to use the first decade of the EITC.

\textsuperscript{12}As the following representative quotes indicate: “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” (Meyer and Rosenbaum, 2001). “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” (Eissa and Liebman, 1996).

\textsuperscript{13}See Figure 1 in Moffitt (1999) for a simple graphical illustration of decreasing marginal treatment effects.

\textsuperscript{14}This implicitly assumes that employers do not substitute women in the control group and hire their treatment group counterparts. My estimation strategy assumes this, but see Neumark and Wascher (2011) for a discussion.
Consumption $c(.)$ is a function of her labor supply $l_i$, wage $w_i$, non-labor income $n_i$ (e.g. spousal wage, welfare benefits), home production good $h_i$, and kids $k_i$. Accounting for the EITC requires an interaction between $w_i$ and $k_i$ since only working parents were eligible for the EITC. The cost of working $g_{it}(l_i, k_i)$ is a function of her labor supply $l_i$ and kids $k_i$ (e.g. childcare costs). Working can be thought of as either a binary or continuous decision and a woman chooses to work (more) if the additional consumption benefits of working outweigh the costs. The EITC was an exogenous increase in $w_i$ for EITC-eligible mothers, which made working a more attractive use of time compared to leisure $L_i$ and home production $h_i$, especially for mothers with lower non-labor income (Figure 3B).

To estimate the effect of the EITC on maternal employment, I use a difference-in-differences (DD) approach that compares the employment rates of mothers and women without children (first difference) in the years before and after 1975 (second difference). I approximate equation (1) with the following non-linear model that estimates the probability that each woman works.

$$P(E_{ist}) = f(\beta_1 Mom_{ist} + \beta_2 Mom \times Post1975_{ist} + \beta_3 X_{ist} + \delta_{st} + \epsilon_{ist})$$

(2)

Individuals, states, and years are indexed by $i$, $s$, and $t$. $E_{ist}$ is binary for whether a woman is employed, $Mom_{ist}$ is binary for whether a woman has any children, $Post1975_{ist}$ is binary for whether the year is after 1975 and the EITC exists, and $Mom \times Post1975_{ist}$ is the DD variable of interest. The EITC treatment effect is $\beta_2$ and should be positive since the EITC encouraged mothers to work. $X_{ist}$ contains a set of controls — described below — that vary at the individual, state, and year level. Although the 1975 EITC did not vary by state, I include state-by-year fixed effects $\delta_{st}$ to control for time-varying state policies and characteristics; $\epsilon_{ist}$ is an idiosyncratic error term. Each coefficient is measured in percentage points. Unless otherwise stated, estimates come from a logit model and average-marginal effects are reported throughout. Standard errors are calculated using the delta method, robust to heteroskedasticity, and clustered at the state-level to account for any correlation of state-level unobserved characteristics. March CPS weights are used throughout, although unweighted results are similar (see footnote 39).

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15 At this time the EITC did not offer additional benefits to having more than one child.
16 Restricting $Mom$ to women with a child born before 1975 avoids concerns about potential fertility responses to the EITC. However, this results in mothers with older youngest children over time, affecting the composition of mothers. This has little effect on results (see footnote 30) and I do not make this restriction.
17 Results are nearly identical using probit and are slightly larger using OLS. See Appendix B.
18 I only observe 21 states-groups; such few clusters may bias the standard errors (Angrist et al., 2009; Cameron et al., 2008, 2012). Block bootstrap results in very similar standard errors.
3. Data and Empirical Strategy

In this section I describe the data, sample of women, and necessary conditions for a causal interpretation of difference in differences.

3.1. Descriptive Statistics

To estimate the effect of the EITC on maternal employment, I use 1971 to 1986 March Current Population Survey data (Ruggles et al., 2015) — corresponding to employment in 1970 to 1985 — and the sample of all 16- to 45-year-old women. The treatment group consists of mothers and the control group consists of women without children. Table 1 shows summary statistics for all 550,904 women in column 1, while columns 2 and 3 split the sample into treatment and control groups. Women in the sample average 29 years old with 12.1 years of education, 11 percent are Black, 9 percent are Hispanic, 65 percent are employed and have average annual earnings (in 2013 dollars) of $12,826 (or $19,685 conditional on working), and 82 percent have income below the 1975 EITC limit (or 47 percent conditional on working). Mothers tend to be older, have slightly less education, are more likely to be non-white, and are less likely to work. Figure A2 shows the income distribution of all women in the sample.

Figures 1A and 1B show unadjusted CPS employment trends for mothers and women without children between 1970 and 1985, and preview the regression-adjusted DD results. From 1970 to 1975 the employment gap between mothers and women without kids was stable at 24-percentage points. Between 1975 and 1979 the relative employment of mothers steadily increased and the gap narrowed to 18-percentage points, where it remained from 1979 through 1985. Although the level of employment differed for these two groups — and Table 1 shows that some characteristics differed too — Figure 1A shows that employment trends before 1975 are parallel (p-value = 0.56).

The May Outgoing Rotation Group is in some ways more attractive than the March CPS (e.g. measuring wages, see Lemieux, 2006), but these data begin in 1973 and do not sufficiently account for pre-1975 trends.

To be consistent across regressions, I exclude the 5239 women missing spousal earnings information. This decision has a negligible impact on estimates (omitted).

Figures 1A and 1B use the “high-impact” sample described in section 4.2.5, a sub-sample of all 16 to 45 year old women.

Figure 1A also shows parallel trends after 1979, which may suggest that it took mothers a few years to fully respond to the EITC, consistent with what Eissa and Liebman (1996) and Meyer and Rosenbaum (2001) find for the 1986 and 1993 EITC expansions.
3.2. Ruling Out Contemporaneous Shocks to Employment

In addition to parallel trends, a causal interpretation of DD also requires that no other contemporaneous event or policy affected the relative employment of mothers. An unaccounted for policy or macroeconomic event that increased the relative employment of mothers would bias up the estimated employment effect of the EITC. Even though the 1970s was a period of inflation, oil and food price shocks, and two recessions, in the following discussion I fail to find evidence of any such confounder.

The first oil shock of the 1970s began in October 1973 when the Organization of Arab Petroleum Exporting Countries 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 food-price increases, and a recession marked by stagflation lasting from November 1973 to March 1975. A few years later the second oil shock began when global oil production decreased due to the Iranian Revolution. This preceded the double-dip recession that occurred between January and July 1980, and between July 1981 and November 1982. Although a recession ended around the time the EITC began, it is not obvious why this should have affected the relative employment of mothers and Figure 1A shows no such increase after the 1980-1982 recessions.

One potential threat to identification could involve cuts to public programs that differentially affected mothers. Such changes could make mothers worse off and increase the need to work more via an income effect. However, during the 1970s public assistance programs were being expanded; (Moffitt (2003) refers to this period as a time of “welfare explosion”), which likely dulled the response to the EITC, decreased maternal employment, and biases results in this paper towards zero. Figure A3 shows that trends in welfare, Food Stamps, Women, Infants, and Children (WIC), and payroll taxes were flat or increasing. Another potential identification threat could operate through a change in contemporaneous family characteristics associated with employment and unrelated to the EITC. However, Figure A4 shows that changes in marriage, fertility, education, and male earnings were smooth in the

23 Theoretically a permanent price or tax increase could increase labor supply through an income effect, but the 1970s price shocks were temporary and should not differentially have affected mothers.

24 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 and likely had a small negative effect on employment (Hoyes and Schanzenbach 2012). WIC provides in-kind benefits to pregnant women and mothers with children up to age 5, began rolling out at the county-level in 1972, and had small negative labor-supply effects (Fraker and Moffitt 1988; Hagstrom 1996; Keane and Moffitt 1998; Currie 2003; Hoyes et al. 2011). Figure 1 shows that the weighted percentage of counties with WIC in place rose from 0 percent in 1973, to 60 percent in 1975, to 100 percent in 1979. Fang and Keane (2004) Figure 1 also shows that the number of families receiving AFDC continued rising through the 1970s.
mid-1970s and therefore should not have affected the relative employment of mothers — although I cannot rule out a threshold crossing model (Schelling [1972]) where a continuously changing covariate has a discrete impact on an outcome.

Perhaps the most serious potential confounder to my study is the (non-refundable) 1976 Child Care Tax Credit (CCTC). The 1976 CCTC was a 20 percent tax credit for child care expenditures — up to $2,000 for the first child and $4,000 (nominal) for two or more children. One way to investigate whether this policy confounds my analysis is to look at the fraction of EITC recipients that also received CCTC benefits. Using the Internal Revenue Service 1976-1985 Statistics of Income Public Use Data (SOI), I find that among EITC-eligible taxfilers, only 1 percent received any CCTC benefits, compared to 30 percent of EITC-ineligible taxfilers (Figure A5). This 30-fold difference corroborates other studies showing that the CCTC mainly benefited upper-middle class families (TPC [2011]). Another way to investigate whether this policy confounds my analysis is to see if subsequent CCTC expansions in the early 1980s affected maternal employment. Although trends in SOI data confirm that CCTC benefits doubled after 1982 (Figure A5), this pattern bears no apparent resemblance to the employment patterns in Figures 1A and 1B. Together, this evidence suggests that the CCTC had a minimal effect on the population affected by the 1975 EITC.

Other policies that could potentially pose identification threats include the advent of birth control, Head Start preschool, the 1972 Equal Employment Opportunity Act (EEOA) that mandated equal pay for equal work for women, legalized abortion in 1973, the 1974 Equal Credit Opportunity Act (ECOA) that allowed women to take out loans without a male co-signer, the 1978 Pregnancy Discrimination Act (PDA) that made it illegal to fire women for being pregnant and required employers to treat pregnancy as a temporary disability, and changes in divorce laws during the 1960 and 1970s. Regarding why these policies are unlikely to pose a serious threat to my empirical strategy: the birth control pill first became available in 1960 and was available in most states before the mid-1970s (Bailey).

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25Interacting each control with state and year also accounts for this (Table 2 column 6). The EITC may have had small effects on marriage and fertility. See footnote 10.

26SOI data are de-identified samples of U.S. Federal Individual Income Tax returns that begin in 1960 and have rich income information, but little demographic information. SOI sampling weights used to reflect population average. More details in Appendix B and here: http://users.nber.org/~taxsim/gdb/.

27The CCTC was expanded in 1981 to 30 percent of child care expenses up to $2,400 for one child and $4,800 for two or more. Furthermore, in the Omnibus Budget Reconciliation Act of 1981 a number of new block grants to the states were created, leading some states to expand child care facilities.

28Using DD and DDD approaches (Table 3 column 5 and Table B1 column 2), I am unable to detect a positive impact on employment among mothers ineligible for the EITC and eligible for the CCTC. See Appendix B for details. Averett et al. (1997) uses a structural model and find evidence that the CCTC increased the labor supply of mothers in their twenties with young children in 1987, after additional CCTC expansions where the rate was increased to 30 percent and was re-structured as a sliding scale to provide more benefits to lower-earning households.
Head Start began in the mid-1960s; the EEOA already applied to most states outside the South before 1972 (Altonji and Blank (1999), footnote 54); before abortion was legalized nationwide in 1973, four states legalized abortion in 1970 (Alaska, Hawaii, New York, and Washington) and I find that these states had maternal employment trends similar to other states (results omitted); the ECOA likely did not have a direct impact on maternal employment (see Chandler and Ewert (1976); Smith (1977); Elliehausen and Durkin (1989) for more details); the PDA had little effect on maternal labor supply since mothers bore the whole cost of the mandated PDA benefits and the overall return to work remained the same (Gruber 1994), although Mukhopadhyay (2012) uses a structural model and finds a positive effect of the PDA on pregnant women and mothers of young children, however, the PDA did not become law until October 1978 and Figures 1A and 1B show that most of the rise in maternal employment had already occurred by then; finally, divorce has been rising in the US since 1960 and was propelled higher with the introduction of unilateral-divorce laws (Johnson and Skinner 1986; Peters 1986; Parkman 1992; Wolfers 2006). Isolating California, the first state to pass no-fault divorce in 1970, I find female and maternal employment trends similar to other states (omitted).  

I conclude that there does not appear to be any policies or events that affected the employment of mothers. If anything, the expansion of public assistance programs during the 1970s would have led to slight decreases in maternal employment, implying that results in this paper may underestimate the employment effects of the 1975 EITC.

Finally, another advantage of studying the 1975 EITC is that the 1970s provides a cleaner policy environment than later decades in which to evaluate the employment effects of the EITC. By the 1980s, policymakers were cutting public benefits and nudging low-income women into the labor force and the 1990s EITC expansion coincided with major reductions in welfare and the Family Medical Leave Act, both of which increased maternal employment (Hoyes 1996; Ruhm 1998; Moffitt 1999).

4. The EITC and Extensive Margin Labor Supply

In this section I first describe the average effect of the EITC on maternal employment and then analyze how this effect varied by subgroup. Heterogeneous analysis shows larger responses from mothers more likely to be EITC eligible and mothers eligible for more EITC benefits. Notably, I also find positive responses among married mothers with low-earning spouses. Additional robustness checks found in Appendix B are also briefly described.

29Choo (2015) finds that no-fault divorce is not correlated with divorce rates, but rather a fall in the growth rate of divorce.
4.1. Average Treatment Effects

Having found no evidence of confounding policies in the previous section, I now use equation (2) to estimate the average treatment effect of the EITC on the employment of mothers. In Table 2, I show the average effect of this program on the employment of all 16- to 45-year-old women, using 1970 to 1985 CPS data and various sets of controls. Each column in Table 2 controls for whether each observation is a mother (Mom) whether the observation occurs after 1975 (Post1975) and the DD variable of interest (Mom x Post1975) since mothers were eligible for the EITC after it began to exist in 1975.

Table 2 cumulatively add covariates in each column left to right: Column 1 uses no additional controls and column 2 adds state- and year-fixed effects to account for idiosyncratic state characteristics and annual shocks affecting all women. Column 3 adds demographic controls that include an age cubic, an education quadratic, number of children, welfare income, binary variables for nonwhite, married, and having a child under 5 years old, and interactions between nonwhite and mom, nonwhite and post1975, age and mom, and married and post1975. Controlling for these demographic controls is important since mothers are on average older, have less education, and more likely to be married and nonwhite. Many of these variables are also associated with rises in female employment, controlling for them will help ensure that results do not pick up effects unrelated to the EITC. Column 4 adds annual state and federal unemployment rates — and interactions between these measures of unemployment with mom and married — to control for the effects of business cycles and local economic conditions on employment. Table 2 also shows that estimates are robust to a “kitchen-sink” set of controls that includes double and triple interactions between year, state, and race with each control variable (see Table 2 notes for a full covariate list). Interaction controls in columns 3 to 6 allow for a flexible model and controls for economic conditions and policies that may have differentially impacted women that are married, nonwhite, or mothers.

30Restricting moms to women with at least one child born before 1975 avoids concerns about potential fertility responses to the EITC, however this affects the composition of the treatment group over time. See footnote 16 Making this restriction yields a similar estimate of 0.037 (0.009) instead of 0.040 (0.009).
31Post years begin in 1976 and is dropped when year fixed effects are used.
32Prior to 1977, CPS did not uniquely identify all states and groups of smaller states were bunched together. To provide a balanced panel of consistently defined geographical units, I merge states into the smallest possible consistent unit. This yields the following 21 units I 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, and MI-WI.
33Welfare eligibility and welfare amount are endogenous with labor supply and number of children. However, since results are not sensitive to this control I keep it as a control.
34To show the relative magnitudes of different models, column 5 uses OLS and the set of controls from column 4 — to crosswalk the logit and OLS results — and column 6 adds the additional “kitchen-sink” set of controls.
Across each set of controls the DD estimate is positive, significant at the 99 percent level, and stable between 4.0- and 5.0-percentage points (or 7.5 and 9.5 percent from a baseline of 53 percent). Results imply that about one million mothers began working because of the 1975 EITC, more than from the 1986 and 1993 EITC expansions combined. This estimate also implies that the EITC is responsible for about a third of the 12-percentage point rise in absolute maternal employment and a fifth of the 10-percentage point rise in overall female employment between 1975 and 1985. My preferred specification is the more conservative logit model and the set of controls in column 4 that I use throughout the rest of the analysis unless otherwise specified. Table A1 shows that the DD estimate is similar for alternate binary definitions of working based on earnings, weeks worked, or labor-force participation. Results are also robust to not using CPS weights and alternate age cutoffs.

4.2. Heterogeneous and Subgroup Treatment Effects

Although Table 2 shows that the average effect of the EITC on maternal employment was positive, the treatment effect should have varied by the likelihood of benefitting from the EITC and by the amount of EITC benefits. Groups of mothers with higher household earnings — such as those that were married, higher-educated, and of prime earning age — should have responded less since they were less likely to have household earnings below the EITC eligibility limit. With this in mind, I use the full set of controls from Table 2 column 4 and show in Table 3 that treatment effect heterogeneity varied in a predictable manner.

35Baseline employment rate of 53 percent is for the full sample, which differs from the 63 percent shown in Figure 1A for the high-impact sample (section 4.2.5). Results are intent-to-treat effects. Since the number of EITC-eligible families do not claim the EITC and the number of EITC-ineligible families do claim the credit is about 20 percent (Scholz, 1994). Liebman (1997, 2000) finds that 89 and 95 percent of women allocated to the treatment and control groups filed taxes appropriately in the 1980s. Assuming that this misallocation occurs at random, Eissa and Liebman (1996) argue that this contamination should result in scaling up the employment effects of the EITC by 19 percent. This would imply that my estimate of 4-percentage points should actually be closer to 5-percentage points.

3652.833 million 15 to 44 year old women are in the 1980 Census, 47 percent of which are mothers (CPS), so 4 to 5 percentage points implies that 1 to 1.25 million mothers entered into paid employment because of the EITC. Eissa and Liebman (1996) find that the 1986 EITC expansion led to a 2.8-percentage-point increase in the employment of single mothers, representing 164,000 mothers. Meyer and Rosenbaum (2001) find an 11.3-percentage-point increase in the employment of single mothers between 1984 and 1996. Attributing the full (11.3-2.8=) 8.5-percentage-point increase to the 1993 EITC expansion implies 500,000 mothers. However, this should be considered an upper bound since during this time welfare was being scaled back and the Family Medical Leave Act was passed, both of which increased maternal employment.

37OLS assigns a predicted probability of working larger than 1 to 2.1 percent of observations but a predicted probability of less than 0 to only 0.4 percent of observations.

38Table A1 may also suggest that the increase in female labor supply may have outpaced labor demand since the unemployment rate also appears to have increased because of the EITC.

39Unweighted result is 0.034 (0.009) instead of 0.040 (0.009). Age upper bounds of 35, 45, 55, and 65 yield estimates of 0.046 (0.009), 0.040 (0.009), 0.037 (0.007), and 0.043 (0.006).
way, supporting the hypothesis that the 1975 EITC was indeed behind this increase in maternal employment. Traits associated with these heterogeneous responses are also used in section 7.8 to predict state-level EITC response and show that states with larger EITC responses had larger changes in social attitudes towards working women after 1975.

4.2.1. **Heterogeneous Treatment Effects: Marital Status**

There are at least two reasons why married mothers should respond less to the EITC than unmarried mothers. One is that, since EITC eligibility is determined by household earnings, spousal earnings will make it more likely that a mother’s earnings will push the household out of EITC eligibility (left of point C in Figure 3A). The second reason can be illustrated in the simple framework of a utility-maximizing agent that divides her time between labor and leisure. Higher non-labor income (point A in Figure 3A) increases the likelihood that a woman’s highest feasible indifference curve intersects point A where her labor supply is zero. Since married women tend to have higher non-labor income (i.e., spousal earnings) than single women, they should have had a relatively smaller response to the EITC. I test and verify this heterogeneity in column 1, where I add \( \text{Mom} \times \text{Post1975} \times \text{Unmarried} \) to equation (2) and interpret the coefficient (11.5 percentage points) as the treatment effect of the EITC on unmarried mothers relative to married mothers.

To estimate the average effect of the EITC on married mothers, I carry out two approaches in Table 3 columns 1 and 2. In column 1, I find a statistically significant effect on married women of 2.7 percentage points (\( \text{Mom} \times \text{Post1975} \)). In column 2 I restrict the sample to married women and show that the EITC had a statistically insignificant effect of 1.6 percentage points. These estimates largely align with prior EITC research that has consistently found a positive labor supply response among single mothers, but a null response among married women.\(^{41}\)

Even though married mothers had a smaller EITC response than unmarried mothers, there should have been substantial heterogeneity among married mothers that varied by spousal earnings. Mothers with very low spousal earnings should have been affected by the EITC much like unmarried mothers, while mothers with high spousal earnings should not have been affected by the EITC and faced the same work incentives before and after 1975. To test this, I model a woman’s decision to work as if she knew her spouse’s earnings in advance. Although this is a strong assumption, it may not be completely unrealistic on average since

\(^{40}\)Home production could also be included.

male labor supply during this period was fairly inelastic (Blundell and MaCurdy 1999). This approach treats a married woman’s decision to work like a second mover in a two-person sequential game, where the primary earner does not adjust his labor supply in response to his spouse’s labor supply.\(^4^2\)

Although I find a statistically insignificant average response among married mothers to the 1975 EITC (Table 3 column 2), among married mothers with lower-earning spouses I find a large positive effect (columns 3 and 4). Restricting the sample to married women with spouses earning below the 1975 EITC income limit, column 3 shows that the EITC increased the employment of this group by 4.9-percentage points.\(^4^3\) Another way to test for a negative correlation between spousal earnings and EITC response is by adding a variable to equation (2) that interacts Mom x Post1975 with spousal earnings. Estimates in column 4 show that the EITC’s treatment effect on married women with zero spousal earnings was 6.2-percentage points and this effect declined by 0.9-percentage points for each additional $10,000 (2013 dollars) in spousal earnings. This is one of the first papers to show that the EITC had a positive effect on the employment of married mothers with low-earning spouses.\(^4^4\)

Finally, married mothers with spouses earning above the 1975 EITC kink point can be used as a placebo group since they were not eligible for EITC benefits. If it appears that the EITC increased the employment of these mothers after 1975, this could indicate that an omitted policy or trend is biasing my results upward. Table 3 column 5 shows a null treatment effect on this placebo group and small employment effects can be statistically ruled out. I also use this placebo group as a third difference for triple differences analysis (Table B1 column 2), which yields similar results as the DD estimates.

\(^4^2\)This follows the approach of Eissa and Hoynes (2004) and assumes the household distribution of labor supply is not endogenous. Of course household labor supply is a joint decision between spouses, and if it becomes more attractive for a mother to begin working, it is possible that the other spouse would decrease their labor supply. However, since the EITC is calculated from household earnings, no additional EITC benefits should arise from substituting labor supply between spouses.

\(^4^3\)The 1975 EITC kink point was $18,000 (2013 dollars), which corresponds to roughly the lowest quartile of male earners. This estimate is nested in Figure 3 which uses the entire spousal-earnings distribution and shows a negative correlation between the treatment effect and spousal earnings among married women. In Figure 4 the DD estimate is positive and largest for married women with spouses earning below $10,000; this estimate declines as women with spouses earning below $20,000, $30,000, etc., are incrementally added to the sample. These percentage-point DD estimates are larger when displayed as percent effects since women with lower-earning spouses had lower employment rates to begin with.

\(^4^4\)I find a similar response to the 1986 and 1993 EITC expansions (Table A2). Eissa and Hoynes (2004) anticipate this heterogeneity and acknowledge the EITC will incentivize some married mothers to work: “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.”
4.2.2. Heterogeneous Treatment Effects: Education

Education is often used as a proxy for EITC eligibility since it is correlated with having income below the EITC-eligibility limit\(^{45}\) and generally considered to be a fixed characteristic unlikely to be endogenous with the EITC\(^{46}\). Table 3 column 6 shows estimates from a regression identical to equation (2) except that it adds two variables, \( \text{Mom} \times \text{Post} \times (<12 \text{ YrsEd}) \) and \( \text{Mom} \times \text{Post} \times (12 - 15 \text{ YrsEd}) \). As a result, the coefficient on \( \text{Mom} \times \text{Post} \) denotes the treatment effect for women with at least 16 years of education and the two new variables denote the treatment effect for these two categories of mothers relative to higher-education mothers. I expect the estimate on \( \text{Mom} \times \text{Post} \) to be near zero, since women with at least a four-year college degree are a quasi-placebo group unlikely to have household earnings below the EITC income limit. In line with this prediction, I find that the EITC response is negatively correlated with education: mothers with less than 12, between 12 and 15, and 16 or more years of education had employment responses to the EITC of 5.1, 4.3, and -0.2 percentage points (or 11.3, 8.0, and -0.3 percent). This result supports the hypothesis that the 1975 EITC was behind this increase in maternal employment.

4.2.3. Heterogeneous Treatment Effects: Age

There are at least two reasons to expect younger mothers to be more responsive to the EITC. One is that younger women are more flexible, with smaller adjustment costs of choosing to work (Allen, 1981; Lombard, 2001). Another reason is that since earnings generally increase over the life cycle (Mincer, 1958, 1962; Becker, 1964; Ben-Porath, 1967), younger workers are more likely to earn below the EITC eligibility limit and be eligible for EITC benefits\(^{47}\). Table 3 column 7 estimates equation (2) with the additional variable \( \text{Mom} \times \text{Post}1975 \times \text{Age} \). The two estimates in column 7 suggest that the treatment effect is 9.2 percentage points for the youngest women in my sample and declines by 3.3 percentage points for every 10 years older that a mother is. As with previous regressions, the full set of controls accounts for any rise in female labor supply due to education or cohort effects.

\(^{45}\)In my sample, women with less than, exactly, and more than 12 years of education have average annual earnings of $5,148, $12,953, and $19,443 (2013 dollars).

\(^{46}\)Although there is evidence that the EITC increases the education of children of EITC recipients (Manoli and Turner (2014); Bastian and Michelmore (2015)), there is little evidence that the educational attainment of EITC recipients is affected.

\(^{47}\)In my sample, a bivariate regression of real annual earnings on age yields an estimate of 387.2(2.5). Since the age upper bound in my sample is 45, I ignore the parabolic nature of life-cycle earnings.
4.2.4. **Heterogeneous Treatment Effects: Race**

Whether white or nonwhite women were more affected by the EITC is not theoretically straightforward. Two reasons to suspect that nonwhite mothers were *less* affected by the EITC are that nonwhite mothers before 1975 were more likely to already be working (55 percent compared to 49 percent) and more likely to have non-labor income in the form of welfare (16 percent compared to 4 percent). However, reasons to suspect that nonwhite mothers were *more* affected by the EITC are that before 1975 nonwhite mothers had lower earnings and lower spousal earnings (both unconditional and conditional on working or being married), were less likely to be married, and were more likely to be mothers — making it more likely that they met both the income and children requirements for the EITC. Ultimately, this is an empirical question and is particular to the context of the 1975 EITC. Table 3 column 8 estimates equation (2) with the additional variable $Mom \times Post_{1975} \times White$ and finds that white and nonwhite mothers had statistically identical responses to the EITC of about 3.8 percentage points.

4.2.5. **Heterogeneous Treatment Effects: High-Impact Group**

Another way to verify larger effects from mothers most affected by the EITC is to construct a “high-impact” sample that omits EITC-ineligible married mothers with higher-earning spouses (Table 3 column 5) as well as women less able to respond to the employment incentives of the EITC: disabled, retired, and full-time students. I estimate the effect on this group by adding a variable to equation (2) that interacts $Mom \times Post_{1975}$ with a binary for being in this “high-impact” group. Table 3 column 9 shows that the high-impact sample had a response to the EITC 3.8-percentage-points larger than the non-high-impact sample, for a total effect of about 5.9 percentage points.

4.2.6. **Heterogeneous Treatment Effects: Men**

For completeness, I investigate whether the EITC had an effect on men. Two reasons that the EITC may not have had much of an effect is that male employment was quite high in the 1970s (over 90 percent) and male labor supply elasticity was small (Blundell and MaCurdy, 1999). Indeed, Table 3 column 10 shows that the EITC had an insignificant effect on men (0.2 percentage points) and this result holds for the sample of single men or married men.

To summarize, the 1975 EITC only had an effect on the employment of unmarried mothers and married mothers with lower-earning spouses.

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48Results in Autor and Duggan (2003) and trends in Figure A1 suggest that concerns about the endogeneity of disability and education are not a major concern during my sample period.
4.3. Which Occupations Did These Working Mothers Enter Into?

To investigate which occupations these newly working mothers entered into, I create a number of binary occupation variables and use these variables as dependent variables in equation (2), along with the full set of controls from Table 2 column 4 and the high-impact group. Figure 5 shows that the EITC led to increases in clerical desk jobs, teachers and librarians, professional white-collar jobs, and in service jobs — including housekeepers and cleaners. Results show that mothers were no more likely to work as managers, salesmen, craftsmen, or in blue-collar jobs such as construction or laborers. Echoing the previous results, mothers were much less likely to have no occupation. These occupations are used in section 7.8 to predict state-level EITC response and show that states with larger EITC responses had larger changes in social attitudes towards working women after 1975.

4.4. Extensive Margin Results: Robustness Checks

Since a DD estimator averages effects from years before and after 1975, estimating annual effects of the EITC on maternal employment tests whether the DD results in Tables 2 and 3 are being driven by outliers or a general trend. To estimate this dynamic DD regression, I replace \( \text{Mom} \times \text{Post} \) in equation (2) with \( \text{Mom} \times \text{Year}_y \) where \( y \in [1970, 1985] \). I omit \( \text{Mom} \times \text{Year}_{1975} \) and each estimate can be interpreted as the effect of being a mother on the probability of working relative to 1975. Figure 1B shows that these annual estimates closely resemble the unadjusted employment trend over this period. Relative to maternal employment just before the EITC comes to exist, the estimates on \( \text{Mom} \times \text{Year}_y \) are jointly statistically insignificant for \( y \in [1970, 1975] \) (p-value 0.56), become increasingly positive for \( y \in [1976, 1979] \), and then remain positive and relatively flat for \( y \in [1979, 1985] \) (statistically identical, p-value 0.16). The gradual increase between 1975 and 1979 suggests that it may have taken mothers a few years to learn about the EITC, mirroring the pattern after previous EITC expansions (Eissa and Liebman 1996; Meyer and Rosenbaum 2001).

In Appendix B I show that the results stand up to a number of additional robustness checks including model choice, choice of sample period, triple differences analysis (using EITC-ineligible married women with higher-earning spouses as a control group), and various

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49 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, and 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 on the margin of working might try working for one year, discover that they ended up better off than they expected, and decide to remain employed. To test whether EITC response required an in-depth understanding of public policy and the tax code (Chetty et al., 2013), I plot the annual employment response (similar to Figure 1B) by education subgroup. Results are noisy but I do not find evidence that higher-educated mothers responded any faster than lower-education mothers (omitted).
reweighting techniques to account for potentially changing group composition as well as alternate ways to treat data imputations. I also verify larger responses from mothers eligible for more EITC benefits. In Appendix B, I also discuss how flat EITC beneficia why I observe larger responses from women with more than one child.

5. The EITC and Intensive Margin Labor Supply

In this section I show that the EITC had a significant effect on the earnings and work hours of mothers, although this appears to be driven by extensive margin responses. I also explore where in the earnings and work-hours distribution these newly working mothers fall, what quantiles of the earnings and work-hour distribution are most affected most by the EITC, and whether self-employed workers bunch in IRS tax data (following Saez (2010)).

5.1. Annual Work Hours, Annual Earnings, and Hourly Wages

Results in Tables 2 and 3 show that the EITC had a substantial impact on the extensive margin labor supply of mothers, implying that earnings and work hours should also have been affected. Results in Table 4 use the full set of controls from Table 2 column 4 and equation (2), but with an OLS specification that replace binary employment with annual work hours, annual earnings, and imputed hourly wages as the dependent variable. For each outcome I show results for three samples of women: the high-impact group (from Table 3 column 9), all women (from Table 2), and the EITC-ineligible placebo group of married women with spouses earning above the EITC limit (from Table 3 column 5). Among the high-impact sample the EITC led to increases of 73.7 annual work hours, $1556.6 annual earnings (2013 dollars), and $0.94 hourly wages (Table 4 columns 1, 4, and 7). Among the sample of all women, the EITC led to smaller increases in annual work hours (43.1), earnings ($964.8), and hourly wages ($0.41) (columns 2, 5, and 8).

Among the placebo group, columns 3, 6, and 9 show that the EITC had a statistically insignificant effect on work hours (-5.6), annual earnings (450.9), and hourly wages (0.02); this corroborates the placebo test in Table 3 column 5 and suggests that the causal mechanism behind this rise of working mothers is

Theoretically, some individuals will have an incentive to decrease labor supply in order to benefit from the EITC, however empirical evidence for this has remained largely elusive (Meyer 2002; Saez 2002; Eissa and Hoynes 2006). I also fail to detect a statistically significant average intensive margin response among previously working mothers; results in Table 4 are largely driven by extensive-margin responses. Although there is evidence of bunching among self-employed workers at the first EITC kink point (LaLumia 2009; Saez 2010), this may reflect reporting since there is no third-party reporting of self-employed income. On the other hand, see Kline and Tartari (2016) that use a revealed preferences approach to show relatively large intensive margin responses to welfare reform.
the EITC and not some other policy or general trend affecting all mothers.

5.2. The EITC and the Distribution of Hours and Earnings

So far, I have argued that the EITC encouraged many mothers to work, but where in the annual hours and earnings distribution did they enter when they begin working? To investigate this, I estimate regressions resembling equation (2) but with a binary outcome variable for having annual work hours or annual earnings within a particular range. Figures 4 and 7 show the estimate on *Mom x Post* using the full set of controls from Table 2 column 4 and the “high-impact” sample to focus on mothers most affected by the EITC. These figures also serve as robustness checks since it would raise concerns if mothers entering the labor force immediately earned above the EITC limit. I also run each regression a second time on the sample of working women, which addresses the composition of working mothers and look for evidence of an intensive-margin response among previously working mothers.

For annual work hours, Figure 6 shows that the most common response to the EITC was to work full-time, full-year (about 2000 annual hours). Consistent with extensive-margin results above, Figure 6 shows that mothers were about 4 percentage points less likely to work zero hours. Although estimates on positive annual hours below 2000 are not statistically significant, point estimates suggest that the EITC may have led to small increases in part-time work as well. When the sample is restricted to working women, estimates are not statistically significant but suggest that the EITC led to less mothers working under 1000 annual hours and more mothers working over 1000 annual hours.

For annual earnings, the most common response to the EITC was to earn between $10,000 and $20,000 (in 2013 dollars), which encompasses the most generous portion of the EITC schedule (Figure 7). This makes sense, the minimum wage during this period was between $7 and $9 per hour (in 2013 dollars) and a full-time worker would earn about $14,000 to $18,000 per year. Figure 7 also shows that mothers were about 4 percentage points less likely to have zero earnings. Although not statistically significant, Figure 7 suggests that mothers were also slightly more likely to earn between $20,000 and $50,000. When the sample is restricted to working women, estimates are similar, suggesting that mothers were less likely to earn below $10,000 and more likely to earn between $10,000 and $20,000.

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51 Work hours suffers from measurement error for two reasons: one, I use the midpoint of the categorical weeks worked variable (non-categorical variable not available until 1976 CPS; two, weeks refers to last year and hours refers to last week. Combining variables reduces noise, though results are qualitatively similar for either variable alone. See Bound et al. (2001) for a discussion of measurement error in survey data.

52 Without panel data it is not possible to determine whether these women were new labor market entrants.

53 For hourly wage (omitted), the most common response to the EITC (unconditional and conditional on work) was to earn less than $6 per hour (2013 dollars). This is below the $7 to $9 minimum wage of this period and could indicate that these women were actually earning below the minimum wage — between
5.3. Quantile Analysis: Earnings and Hours Worked

I use quantile regression to characterize the effect of the EITC on the full distribution of annual work hours and annual earnings. To do this I use the regression behind Table 4 columns 1 and 4, however, instead of estimating average effects I estimate the effect of the EITC at each centile of the distribution of each dependent variable. Instead of minimizing the sum of squared residuals like OLS, quantile regression uses heteroskedasticity as a feature of the data and minimizes a weighted sum of the absolute value of the residuals (Koenker, 2005). These quantile difference-in-differences (QDD) estimates should be interpreted as effects at quantiles and not on quantiles, since it is unclear whether rank preservation holds without making strong assumptions or using panel data (see Bitler et al. (2003) for a detailed discussion). Each regression uses the full set of controls from Table 2 column 4 and the high-impact sample to focus on mothers most affected by the EITC.

Figures 8 and 9 show that the EITC had the largest effect on the annual work hours of the 54th centile and the annual earnings of the 44th centile, with a steadily decreasing positive effect higher up the distribution, and no effect on lower centiles since mothers in these centiles did not work before or after 1975. These QDD results show that the EITC had the largest effect on the middle of the earnings and hours distribution, and these large gains drive the positive average effects in Table 4.

5.4. Bunching Among Self-Employed

I use 1975-1985 IRS Statistics of Income Public Use Data (SOI) to look for bunching among self-employed taxfilers (following Saez (2010)). Figure 10A shows bunching at the EITC kink point among EITC-eligible taxfilers with positive self-employed (business schedule C) income, both for the 1975-1978 EITC schedule and the expanded 1979-1984 EITC schedule. Figures 10A and 10B show no bunching among EITC-ineligible taxfilers (claiming zero children) with positive self-employed income, as well as both EITC-eligible and EITC-ineligible taxfilers with wage earnings and zero self-employed income. There is only evidence of bunching among EITC-eligible taxfilers with positive self-employed income. This likely reflects misreporting 1979 and 1981, 5.7 to 6.8 percent of all workers earned below the federal minimum wage (BLS, 2015) — or could reflect measurement error since I impute hourly wage with a categorical measure of weeks worked.

54 The top few centiles are noisy and omitted. Since some mothers provide information about hours or earnings (but not both), these figures show effects starting at different places in the distribution of each dependent variable. Puzzlingly, these figures may suggest that the EITC had a small effect on the top few deciles of the earnings and hours distribution. Although these estimates are fairly small — increases of about 1.5 percent since the 90th percentile of annual hours and earnings of mothers in 1975 was 2,040 and $36,000 — one possible explanation is that Mom x Post is picking up something else not being controlled for, perhaps work experience, which is not available in the CPS and is known as a key reason that the gender wage gap began to close in the 1980 (O’Neill and Polachek, 1993; Blau and Kahn, 2000).
since there is no third-party reporting for self-employed income \cite{LaLumia2009, Saez2010}, but this may also reflect real increases in self-employed mothers \cite{Lim2015}. 

Following the approach in Saez (2010), I use a quasi-linear and iso-elastic utility function, bandwidths of $1000, $1500, $2000, and calculate elasticities from bunching of 0.23, 0.52, 0.77 in 1975-1978 and 0.58, 1.28, 2, 22 in 1979-1985. Results align closely with Saez (2010).\footnote{Saez (2010) finds bunching at the first EITC kink point in the late 1980s through the 2000s, but does not investigate the first decade of the EITC; my results can be seen as corroborating Saez (2010).}

See Appendix C for calculations.

6. Comparing Magnitudes and Elasticities

Estimates suggest that the 1975 EITC led to a 7.5 percent increase in employment (Table 2), a 6.3 percent increase in labor force participation (Table A1), and a 9.6 and 7.3 percent increase in annual earnings and work hours (Table 4) among 16 to 45 year old women. Using annual information on taxes and public assistance, I calculate the extensive margin labor supply elasticity as the change in log employment rates divided by the change in log after-tax earnings minus public assistance available to non-workers.\footnote{To calculate elasticities I use a representative household: an unmarried mother with one child and annual earnings of $19,160 (2013 dollars), which is the median income in my sample.}

Elasticities are compensated (Hicksian) elasticities unless otherwise specified. Table A1 shows that across different binary definitions of working, the estimated extensive margin elasticity ranges between 0.6 and 0.8.\footnote{Many married women were not EITC eligible due to high spousal earnings, so these estimates may need to be scaled up. The employment effect on the “high-impact” sample depends on the specification and ranges from 4- to 5.9-percentage points (Figures 6 and 7, Table 3 column 9), which maps to an elasticity of 0.7 to 0.95 since baseline employment of “high-impact” mothers was 57.3 percent in 1975.}

See Appendix C for complete details.

Although these elasticities are larger than more recent estimates — including those found in Chetty et al. (2012) that range from 0.13 to 0.43 — my estimates are consistent with other estimates of female labor supply elasticity during this time. Goldin (1990) finds that wage elasticities increased over the first half of the twentieth century, peaked around 1950, and has been declining ever since. Bowen and Finegan (1969) finds compensated elasticities in 1940 and 1950 of 1.35 and 1.55; LaLumia (2008) finds a large elasticity of about 2 in 1948; Mincer (1962) finds an elasticity of 2.03 in 1950; Cain (1966) finds 1.6 in 1950; Bowen and Finegan (1969) finds 0.67 in 1960; Fields (1976) finds 0.52 in 1970; Blundell and MaCurdy (1999) shows that empirical studies using data from the 1970s and 1980s produce a wide range of uncompensated elasticity estimates, with an average of about 0.8; Blau and Kahn (2005) and Heim (2007) find that the uncompensated elasticity was about 0.6 in 1980 and
has steadily decreased over time to about 0.3 in 2000.\footnote{Mroz (1987) discusses many of these early studies. The 1968-1982 negative income tax experiments yield elasticities of 0.2 to 0.3 (e.g. Burtless and Hausman (1978); Robins (1985); Munnell (1987)).}

Since labor supply elasticity is a function of the tax code (Saez et al., 2012) and varies across groups of people and time, my elasticity estimates should be interpreted as reflecting a particular population at a particular time in the U.S.

### 7. The EITC and Attitudes Towards Working Women

Since results above suggest that the 1975 EITC encouraged about a million mothers to begin working, this program likely had subsequent effects on the country. There is a large literature showing that the EITC affected children of EITC recipients (see section 1 for a summary), mostly in positive ways.\footnote{Although Bastian and Michelmore (2015) find less time spent between kids and mothers.} Another effect of the EITC — that has remained understudied — is how this program affected social attitudes towards working women. After 1975, people were more likely to have female family members and friends that worked, workers were more likely to have female coworkers, and media stories about working moms became more common.\footnote{Google ngrams (Michel et al., 2011) show that beginning in the mid-1970s, print media started using the phrases working mom and — the previously redundant — stay at home mom much more often (Figure 2), reflecting changes in attitudes towards women and expectations about women in society. This increased exposure to working women could have increased or decreased approval of working women (“gender-equality attitudes”). I characterize and exploit geographic heterogeneity in the EITC response and use a two-sample two-stage approach to test whether states with larger EITC responses experienced larger changes in gender-equality attitudes.\footnote{This analysis fits into a relatively recent literature that emphasizes the role of attitudes and social norms in economic analysis. Becker (1957) and Arrow (1971) began incorporating tastes, discrimination, and social influences into economic theory. Direct inclusion of attitudes and beliefs into the utility function started with Akerlof and Dickens (1982) and continued with Akerlof and Kranton (2000, 2002, 2010) and Bénabou and Tirole (2006). There is a long-standing sociology literature on gender-role attitudes (Thornton and Freedman, 1979; Thornton et al., 1983; Plutzer, 1988; Lottes and Kuriloff, 1992; Bolzendahl and Myers, 2004); but these studies largely focus on describing how attitudes have evolved over time and identifying traits correlated with these attitudes. In the economics literature, there is evidence that gender-role attitudes are passed on intergenerationally (Fernandez and Fogli, 2009; Alesina et al., 2011; Farré and Vella, 2013) and that these attitudes affect female labor market outcomes (Fortin, 2005; Charles et al., 2009; Bertrand et al., 2015; Fortin, 2015; Pan, 2013; Janssen et al., 2016). However, unlike these studies, my goal is to characterize a determinant — not the consequences — of these attitudes.}}
equality attitudes. First, I test whether the magnitude of the state EITC response is associated with changes in gender-equality attitudes; second, I test for reverse causation and whether state EITC response can be explained by pre-existing gender-equality attitudes or changes in demographics or other social attitudes; third, I test whether attitude changes are concentrated among lower education men — who were more likely to know these newly working women; fourth, I use a placebo outcome on racial attitudes to test whether states with higher EITC responses were simply experiencing changes in various types of social norms; fifth, I test whether predicted state EITC response — predicted from pre-1975 state demographic and occupational traits to help alleviate concerns about the endogeneity of attitudes and EITC response — is also correlated with changes in gender-equality attitudes; and finally, as a check on whether increases in female employment can affect gender-equality attitudes, I look at another large increase in female employment — due to World War II mobilization — and find similar attitude changes.

7.1. Determinants of Social Attitudes and Related Literature

There is a literature showing that attitudes towards gender and race can be altered via exposure. Beaman et al. (2012) show that a mandated increase in female leadership positions in India raised career aspirations and educational attainment of girls, partly due to an increase in female role models. Stouffer et al. (1949) shows that after serving in a mixed-race company, white soldiers had higher racial-equality attitudes than white soldiers that did not. Stouffer et al. (1949) also shows descriptive evidence that Southern white men serving in Northern states decreased racist attitudes, while Northern white men serving in Southern states increased racist attitudes (Chart XVII and Table 22). This implies that egalitarian attitudes can be affected positively or negatively.

There is also a large experimental literature showing that attitudes are malleable. E.g. Heilman and Martell (1986); Lowery et al. (2001); Dasgupta and Asgari (2004).

Two of the three papers — I am aware of — to examine determinants of gender-role attitudes are Fernández et al. (2004) and Olivetti et al. (2016). These two papers find evidence that having a working mother — and having friends with working mothers — during childhood leads to more positive gender-equality attitudes in adulthood due to early socialization. A third paper, Finseraas et al. (2016), shows that exposure to female colleagues reduces discriminatory attitudes. Changes in social attitudes can be decomposed into population turnover and individual change (Firebaugh, 1992). Fernández et al. (2004) and Olivetti et al. (2016) focus on population turnover (intergenerational change) and Finseraas et al. (2016) focuses on individual change. Fernández et al. (2004) also acknowledge the importance of individual change: “Of course, as more women joined the labor force, attitudes towards these women changed in society at large.” My approach measures the combined effect from both individual change and population turnover. Panel data would enable me to disentangle these two. Restricting GSS by birth cohort instead of age yields samples too small to make such inferences.
gender-role attitudes of millions of Americans were affected when the EITC led one million mothers to begin working in the late-1970s.

### 7.2. Attitudes and the Two-Sample, Two-Stage Approach

I measure gender-equality attitudes with the General Social Survey (GSS) question, “Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?” I define “gender-equality attitudes” as approving of women working. The GSS is an attractive way of measuring these attitudes because the question is consistent over time, begins in 1972, providing a few baseline years before 1975, and includes a rich set of individual traits and attitudes. Traits positively correlated with gender-equality attitudes include education, income, having a working mother or spouse, and racial-equality attitudes, while traits negatively correlated with gender-equality attitudes include age, married, and nonwhite (Table 5).

I measure the change in state-level gender-equality attitudes after 1975 with the variable \( \Delta GenderEquality_s^{(1976-85)-(1972-75)} \). Using restricted GSS data geo-coded at the state level, I construct this variable by first averaging individual male attitudes in each state (using sampling weights) in years before 1975 (1972-1975) and after 1975 (1975-1985), then subtracting the 1972-1975 average from the 1976-1985 average. Positive values reflect more positive gender-equality attitudes over time.

I use March CPS data to estimate the state-level response to the EITC:

\[
P(E_{its}) = f(\beta_1 Mom_{its} + \sum_s \beta_2 s Mom \times Post1975_{its} + \beta_3 X_{its} + \gamma_s + \delta_t + \epsilon_{its})
\]  

which modifies equation (2), uses OLS, and estimates a separate DD (in percentage points) for each state. I rename \( \beta_2 s \), \( EITC Response_s \), the estimated state response to the EITC.

Using these two variables, I estimate the following regression.

\[
\Delta GenderEquality_s^{(1976-85)-(1972-75)} = \gamma EITC Response_s + \delta \Delta X_s^{(1976-85)-(1970-75)} + \epsilon_s
\]

\( \gamma \) measures the average effect of a percentage point increase in state EITC response on the change in gender-equality attitudes after 1975. Since the treatment variable is a generated regressor, standard errors are bootstrapped.\(^{66}\)

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\(^{66}\) This question was occasionally asked by Gallup beginning in 1936; Figure 11 shows the fraction of adults approving was under 20 percent in the mid-1930s and had grown to more than 80 percent by the mid-1990s.

\(^{67}\) Results robust to OLS, logit, or probit models, choice of controls, and March CPS sample years (see footnote 69).
2002. $X_t^{(1976−85)}−(1970−75)$ are state-level controls to account for demographics and other attitudes unrelated to gender roles; controls can be measured in the pre-1975 level or in the pre-1975 to post-1975 difference.

Using equation (4) and no controls, Figure 12 shows that state EITC response is positively correlated with increases in gender-equality attitudes after 1975. On average, each percentage point increase in state EITC response led to a 1.7-percentage-point increase in gender-equality attitudes (p-value=0.01).

See Appendix D for less parametric approaches.

7.3. Reverse Causation: Did Attitudes Drive EITC Response?

Perhaps the most obvious threat to my hypothesis — that the EITC-led increase in working mothers affected social attitudes — would be if higher-responding states already had more positive gender-equality attitudes before 1975, either in the 1974 level or in the 1972-1975 trend. In Figures 13A and 13B I following the approach in Acemoglu et al. (2004) and find a statistically insignificant relationship between state EITC response and the 1974 level of male gender-role attitudes (Figure 13A, p-value=0.27) and the 1972 to 1975 trend in male gender-role attitudes (Figure 13B, p-value=0.60). If anything, these two figures suggest that higher-responding states had lower gender-equality attitudes before 1975, perhaps suggesting that the EITC led to a “catch up” among states with lower gender-equality attitudes.

Another way to test for reverse causality is to see if passage of the EITC was driven by lawmakers in states experiencing changes in attitudes before 1975. If states that voted for the 1975 EITC were also the states that benefited the most from it, then perhaps the EITC was an outcome of — not the cause of — changing gender-equality attitudes. To test this, I compare the fraction of each state’s congressmen (Senators and House Representatives) that voted for the 1975 EITC legislation with each state’s EITC response. Figure A7 shows that, in fact, the opposite is true: states voting against the 1975 EITC had higher EITC

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Equation (4) is a first-difference estimator, which nets out the problem of omitted variables and is unbiased and consistent under the condition $E[u_{it}−u_{i,t−1}|x_{it}−x_{i,t−1}] = 0$. This assumption is less restrictive than the assumption of weak exogeneity for unbiasedness when pre-1975 and post-1975 components are separated in a fixed effects estimator (Wooldridge, 2015). Results are similar using either approach.

Results are robust to using OLS, probit, or logit to estimate equation (3), are stable between 0.017 and 0.019, and significant at about the 99 percent level. Results are unweighted and robust to weighting by state population or by the inverse of the standard error in equation (3) to weight up states with a more precisely estimated EITC response. Results are similar with region fixed effects. Even though I measure these effects through 1985, these are short run, partial equilibrium effects. The state EITC response varied from about 0 to 10 percentage points, with the 25th and 75th percentiles equal to 2.8 and 6.2 percentage points. This suggests that the interquartile (25th vs 75th percentile) effect on attitudes is about 5.8 percentage points, comparable to having two more years of education, being born a decade later, or being a decade younger, and smaller than having a working wife or being willing to vote for a black president (Table 5).

As with most legislation, the Tax Reduction Act of 1975 contained a number of other spending and tax provisions ([https://www.gpo.gov/fdsys/pkg/STATUTE-89/pdf/STATUTE-89-Pg26.pdf](https://www.gpo.gov/fdsys/pkg/STATUTE-89/pdf/STATUTE-89-Pg26.pdf)).
responses and experienced larger subsequent attitude changes. This result also suggests that attitude changes were the largest in places less likely to be in favor of a social program like the EITC.

7.4. Subgroup Analysis

Since the EITC had a larger employment effect on lower-education mothers (Table 3 column 6), lower-education males would have been more likely to know these women. With this in mind, if the EITC did affect gender-equality attitudes, it should have had a relatively larger effect on lower-education males. Figures 14A and 14B re-estimate equation (4), but divide the sample into men with 12 or less years of education and men with more than 12 years of education. These two figures show that the change in gender-equality attitudes among the lower-education group was indeed larger. Among the lower-education men, each percentage point increase in the state-level EITC response led to a 2.8-percentage-point increase in gender-equality attitudes (p-value=0.01), whereas for the higher-education males the estimate is a statistically insignificant 1.0-percentage points (p-value=0.63). This corroborates the hypothesis that women who began working in response to the EITC affected the attitudes of people around them.

7.5. Permutation Test

How likely is it that the relationship between attitude changes and state EITC response in Figure 14A occurred by chance? Using a variant of the permutation test in Buchmueller et al. (2011), I randomly reassign a new attitude change \( \Delta GenderEquality_{(1976-85)-(1972-75)} \) to each state from the set of all state attitude changes, with replacement. I then re-estimate equation (4), record the estimate of \( \gamma \), and iterate 1000 times. Figure 15 shows the distribution of these point estimates and indicates that the actual estimate in Figure 14A is in the top 0.01 percent of permutations and is therefore unlikely to have occurred by chance.

7.6. Controlling for Demographics and Other Social Attitudes

Another possible threat to my hypothesis would be if changes in gender-equality attitudes could be explained by demographic changes or changes in other social attitudes. This could imply that an omitted variable or trend unrelated to the EITC is driving the correlation in Figure 12. To test this, I re-estimate equation (4) and add one control variable at a time.

\[71\] One other subgroup of interest is defined by marital status: the attitudes of married men were more affected by the influx of working women — 0.026 (0.009) — more than unmarried men — 0.013 (0.017).
to account for state-level education, marriage, age, race, employment and earnings, parental employment and education, as well as political and religious affiliation, views on public assistance, and racial-equality attitudes. Each control variable $X_{s}^{(1976−85)}−(1970−75)$ is created by subtracting the 1970 to 1975 average from the 1976 to 1985 state-level average.

Table 6 shows that across various controls, state EITC response is still positively correlated with changes in gender-equality attitudes. Table 6 column 1 uses no controls and replicates the result in Figure 12. Columns 2 to 9 show that estimates are stable across each control. Panel A shows results among men are between 0.015 and 0.020. Results in Table 6 are statistically significant and indicate that the relationship between changes in gender-role attitudes and state EITC responses cannot be explained by changes in state demographic traits or general changes in social attitudes.

Results so far focus on the attitudes of males, but the attitudes of females were also likely affected by the large influx of working mothers into paid employment. Table 6 Panel B shows that the effects are smaller for women, but still positive and statistically significant. Across each control, each percentage point increase in state EITC response led to a 0.012 to 0.016 increase in gender-equality attitudes among women.

7.7. Placebo Outcome: Changes in Racial-Equality Attitudes

Since racial-equality attitudes are generally correlated with the same individual-level traits as gender-equality attitudes (Table 5 panels A and B), it is conceivable that an omitted state policy or trend — other than the EITC — could be driving changes in both gender- and racial-equality attitudes. One way to test for this is to use racial-equality attitudes (GSS question: would you vote for a black president?) as a control variable (Table 6 column 11). Another approach is to use changes in racial attitudes as a dependent variable and re-estimate equation (4). Figure 16 shows that the relationship between changes in racial-equality attitudes and state EITC response is statistically insignificant (p-value 0.56). This result suggests that higher-responding states were not simply experiencing larger changes in various attitudes for reasons unrelated to the EITC.

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72 Fernandez et al. (2004) shows that mother’s employment affects gender-role attitudes.

73 Table A3 shows summary statistics for these state-level variables, pooling pre-1975 years (1972-1975) and post-1975 years (1976-1986). See Appendix F for data sources and variable descriptions.

74 Results are robust to using pre-1975 levels $X_{s}^{(1970−75)}$. Depending on data availability, controls are constructed from Census, CPS, GSS data. See Appendix F for details.

75 Another placebo test is to regress changes in attitudes — using placebo years between 1985 and 1995 as cutoffs instead of 1975 — on state response to the 1975 EITC. Figure A6 shows that for each of these cutoffs, state EITC response is not significantly correlated with changes in attitudes. If high-responding states continued to experience changes in gender-equality attitudes after the early 1980s — Figure 1B shows that maternal employment had fully responded to the 1975 EITC by about 1980 — this could suggest an alternate causal mechanism. (Although I do not attempt to model the exact mechanism or timing by which
7.8. Using Predicted State EITC Responses from Pre-1975 Traits

Results above show the relationship between state response to the 1975 EITC and changes in gender-equality attitudes. However, if state differences in gender-equality attitudes before 1975 drove the state EITC response, then my argument that the EITC affected attitudes would be somewhat circular. Although Figures 13A and 13B provide evidence that gender-role attitudes before 1975 did not determine state EITC response, I now use predicted EITC response — from pre-1975 state demographics and occupational structure — in an attempt to purge the endogenous part of the EITC response and test whether predicted EITC responses are associated with attitude changes. To calculate the predicted state EITC response, I regress EITC Response on pre-1975 state-level demographic and occupational traits $X_{pre1975}^s$ and generate the predicted EITC response $\text{EITC Response}_{s}$. To see if this predicted EITC response is correlated with changes in gender-role attitudes, I regress $\Delta \text{GenderEquality}_{s}^{(1976-85)-(1972-75)}$ on $\text{EITC Response}_s$.

To show that predicted state response to the EITC affected attitudes, four conditions should be met: First, $X_{pre1975}^s$ should be correlated with $\Delta \text{GenderEquality}_{s}^{(1976-85)-(1972-75)}$. This is a reduced-form version of the two-step regression. Second, $X_{pre1975}^s$ should be correlated with EITC Response. This is the first stage in the two-stage least squares (2SLS) approach where I generate EITC Response. Third, EITC Response should be correlated with $\Delta \text{GenderEquality}_{s}^{(1976-85)-(1972-75)}$, which is the second stage of the 2SLS regression. For each variable $X_{pre1975}^s$, this third estimate should be similar and can be interpreted as the effect of an additional exogenous percentage point increase in maternal employment on gender-equality attitudes. Fourth, $X_{pre1975}^s$ should not be correlated with changes in gender-equality attitudes before 1975. Conditions one and four together suggest that $X_{pre1975}^s$ did not have a direct effect on gender-equality attitudes but only affected attitudes indirectly through state EITC response.

Although power is low, scatterplot and regression evidence that predicted EITC response the employment of mothers affects attitudes, as there clearly could be a lag between these two phenomena.) If anything, higher-responding states had lower gender-equality attitudes before 1975, which could suggest that “catch up” is occurring.

The exclusion restriction requires that $X_{pre1975}^s$ be correlated with state EITC response and not correlated with changes in gender-equality attitudes except through the indirect effect of the state EITC response. This is unlikely to completely hold for all of these variables. Although testing the exclusion restriction is not directly possible, one way to address it is to add a control for the pre-1975 variable level in Figure 17 panel C and Table 7 (omitted). If the estimate on this pre-1975 variable is zero, this would suggest that the pre-1975 variable only affects attitude changes via state EITC response.

A complementary approach would be to save the residuals from the regression of EITC Response on $X_{pre1975}^s$ and regress attitude changes on these residuals. This bivariate regression (omitted) yields an estimate statistically indistinguishable from zero, suggesting that the explained portion of EITC response is responsible for much of the attitude changes.
— predicted from pre-1975 female education — is associated with attitude changes is shown in Figure 17. Female education was shown in Table 3 column 6 to have a strong, negative correlation with individual response to the EITC. Figure 17 Panel A shows that state-level female education pre-1975 is negatively correlated with the change in gender-role attitudes after 1975 (p-value=0.08). Panels B and C show the first and second stages of the 2SLS regression. Panel B shows that average female education is highly correlated with the state EITC response (p-value=0.001); Panel C uses the predicted EITC response from Panel B and shows that this predicted response is positively correlated with changes in gender-equality attitudes after 1975 (p-value=0.06). Panel D shows that female education is not statistically correlated with the 1972 to 1975 change in attitudes (p-value=0.54)

In Table 7, I repeat the exercise in Figure 17 for a number of additional pre-1975 state-level demographic (columns 1 to 6) and occupational traits (columns 7 to 11). To conserve space, I just show the 2SLS estimate of the predicted EITC response on gender-role attitudes after 1975 (corresponding to panel C in Figure 17). For demographic traits, column 1 replicates Figure 17 and uses female education, columns 2 and 3 use white and nonwhite single mothers, column 4 uses fraction females, column 5 uses male earnings, and column 6 uses males not in the labor force. For occupational traits, column 7 uses the fraction of jobs that are teachers and librarians, column 8 uses housekeepers and cleaners, column 9 uses the number of bakers and food makers, column 10 uses the number of male metal and wood workers, column 11 uses the number of male construction workers. Columns 7-9 are largely female-dominated jobs and columns 10-11 are male dominated. These occupations provide information about the occupational structure and type of jobs available in each state and how easy (or appealing) it would be for women to work if they chose to do so. To show that these results do not merely reflect regional differences, panel B adds region fixed effects.

Across each trait, the predicted EITC response was positive and between 0.02 and 0.07, however not all estimates are significant at the 95 percent level. Interestingly, when regional fixed effects are added, the estimates become more precise, consistently between 0.02 and 0.03. In general, both the actual and predicted state EITC responses indicate that the EITC positively affected gender-equality attitudes.

7.9. Did WWII Increase in Female Employment Affect Attitudes?

If it is true that the increase in working women due to the 1975 EITC affected attitudes about women, then the same pattern should exist during other periods of large, sudden increases in female employment. During World War II, many women began working to make up for

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Unfortunately 1972-1975 is a short period and estimates are imprecise.
the millions of men that joined the military, and the number of women that joined the labor force was higher in places with higher mobilization rates (Acemoglu et al., 2004; Goldin and Olivetti, 2013a). Testing whether states with higher mobilization rates experienced larger increases in gender-equality attitudes is possible since Gallup began asking questions related to gender equality in the 1930s and identifies individuals by state. I follow the approach behind equation (4), except that instead of constructing a state panel on attitudes before and after 1975, I construct a state panel on attitudes before and after WWII. I also use WWII mobilization rates as the treatment variable since Goldin and Olivetti (2013b) shows that mobilization rates are positively correlated with increases in female employment and that about two-thirds of this mobilization rate can be explained by largely exogenous state-level factors (e.g. farmers, race, age, fatherhood, and marriage).

Figure 18 resembles the scatterplot in Figure 12 and shows a positive correlation between mobilization rates and changes in gender-equality attitudes after WWII (p-value = 0.003). This provides corroborating evidence that large increases in female employment can lead to changes in attitudes about the role of women in society.

8. Summary

In one of the first systematic studies of the 1975 introduction of the EITC, I show that this program led to a 7.5 percent rise in maternal employment — representing about one million mothers, more women than were affected by the previously studied 1986 and 1993 EITC expansions combined. This finding aligns with time-series evidence that the relative employment of mothers began to increase after 1975 (Figures 1A and 1B). In hindsight, this should not be surprising since female labor-supply elasticity was quite large during this period (Blau and Kahn, 2005; Heim, 2007) and the 10-percent wage subsidy of the 1975 EITC represented a large year-over-year increase in potential earnings. Interestingly, results in this paper may help resolve an anomaly observed decades ago by Smith and Ward (1985): although real wage growth explains most of the increase in the female labor supply between 1950 and 1980, in the period after 1970, the growth rate of female labor supply rose as the

80 Contemporaneous source LIFE magazine from August 9, 1941 confirms the ramp up in female employment, “In 1941 only 1% of aviation employees were women, while this year they will comprise an estimated 65% of the total. Of the 16 million women now employed in the U.S., over a quarter are in war industries.”

81 Mobilization rates from Goldin and Olivetti (2013) Table A1. I focus on white adults here because WWII mobilization likely had a larger effect on white women. As Goldin and Olivetti (2013) state, “black womens [labor force] participation was high before the war and many were in agricultural occupations.” Attitudes restricted to white adults to match the mobilization rate of white men. Attitudes of men and women pooled, though results by gender are still significant (see Figure 18 notes).

82 WWII mobilization rates are not correlated with state responses to the 1975 EITC (p-value=0.38).
real-wage growth rate fell (Parkman, 1992).

Consistent with the 1975 EITC being the causal mechanism behind this rise in employment, I find larger responses from mothers more likely to be EITC eligible and null responses from placebo groups of mothers not eligible for EITC benefits. In Appendix B, I use this placebo group of EITC-ineligible mothers in a triple differences (DDD) specification to net out contemporaneous policies (e.g. birth control, divorce laws, abortion) affecting the employment of all mothers. DDD estimates corroborate the DD results and imply that estimates in this paper are not driven by general trends in the employment of mothers.

I also find that the EITC-led influx of mothers entering the labor force had a subsequent effect on social norms and beliefs about the role of women in society (section 7). I find that states with larger EITC responses — and states with larger predicted responses due to pre-1975 demographic and occupational traits — had larger post-1975 increases in attitudes approving of women working. Results do not appear to be driven by pre-1975 attitudes, changes in demographics, or general trends in social attitudes. As a robustness check, I also find attitude changes due to another large and sudden increase in female employment during World War II. Although social attitudes about women working and women working are clearly intertwined, I use two historical episodes of largely exogenous increases in female employment to show that increases in working women had an effect on attitudes about the role of women in society. The 1975 EITC played an important role in the rise of working mothers and in fostering more positive gender-equality attitudes.
References


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9. Figures and Tables
Fig. 1A. Unadjusted Employment Trends for Mothers and Women without Kids

Fig. 1B. Employment Gap between Mothers and Women without Kids

Notes: 1970-1986 March CPS data. Employment defined as positive income. Estimates and standard errors in Figure 1A calculated by regressing employment on a constant for each group-year. Best fit lines are shown for 1969-75, 1975-79, 1979-85. In Figure 1B unadjusted relative employment – relative to women without kids – calculated by regressing employment on whether a woman has a kid and a constant for each year. Regression adjusted annual gap are estimates of \( \text{Mom} \times \text{Year} \) with the set of controls from Table 2 see section 4.1 for full details. The estimates on \( \text{Mom} \times \text{Year} \) are jointly statistically insignificant for all years before 1975 (p-value 0.56) and the 1979 to 1985 estimates are statistically identical (p-value 0.16). Sample is the “high-impact” sample discussed in section 4.2.5, which includes all women 16-45 and excludes married women with spousal earning above the 1975 EITC limit of $36,000 (in 2013 $), as well as full-time students, disabled, and retired. Kids defined as 0-18 years old.
Fig. 2. Rise of Working Moms After 1975 Was Salient, Evidence from Google Ngrams

Notes: Google Books Ngram Viewer is an online search engine (http://books.google.com/ngrams) that charts frequencies of any set of comma-delimited search strings using a yearly count of n-grams found in over 5 million sources — and over 500 billion words — printed between 1500 and 2008 (Michel et al. 2011). This represents about a 4 percent sample of all possible books and sources. The vertical axis measures the relative frequency that “earned income tax credit”, “working mom”, and “stay at home mom” are used in sources printed between 1950 and 1990. For scaling purposes, earned income tax credit is multiplied by 10,000, working mom is multiplied by 100,000, and stay at home mom is multiplied by 3,800,000. Because of this, the levels within ngrams are comparable over time but levels across ngrams are not. Each ngram includes plural and capitalized variants of these phrases; stay at home mom also uses variants of the word mother. Sources:

https://books.google.com/ngrams/graph?content=working+moms&year_start=1950&year_end=1990&corpus=15&smoothing=10&share=&direct_url=t1%3B%2Cworking%20moms%3B%2C%20mothers%3B%2C%20moms%3B%2C%20stay%20at%20home%20mom%3B%2Cstay%20at%20home%20moms%3B%2C%20stay%20at%20home%20mother%3B%2C%20stay%20at%20home%20mothers%3B%2C0
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Accessed 9/5/16.
Fig. 3A. Budget Constraint Under the 1975 EITC

Fig. 3B. Comparing 1975 and 2013 EITC Schedules for Households with One Eligible Child

Notes: Author’s calculation from 1975 and 2013 EITC parameters. 1975 EITC phased in and out at 10 percent. 2013 EITC for one child phased in and out at 34 and 15.98 percent. The following is an abbreviated history of 1975-2013 changes to the EITC schedule: 1979, a plateau region was added to the EITC schedule; 1986, the phase-in rate was increased to 14 percent and the EITC parameters were indexed to inflation; 1990, additional EITC benefits became available to parents with two or more children; 1993, benefits were extended to adults without children (though at a low rate of 7.65 percent); 1993 to 1996, the phase-in rate increased to 34 percent and 40 percent for households with one and two or more children; 2003, the plateau region of the EITC schedule was extended to married couples to decrease the marriage penalty; 2009, additional EITC benefits became available to parents with three or more children.
Fig. 4. EITC Response Negatively Correlated with Spousal Earnings Among Married Mothers

Notes: 1971-1986 March CPS data. Spousal earnings in 1000s of 2013 dollars. Married women with missing spousal earnings dropped. Each estimate is from a separate logit regression that uses the full set of controls from Table 2 column 4 and the sample of all married women with spouses earning below each specified amount. Treatment effects are estimates of $Mom \times Post$ in equation (2). Sample sizes for these nine regressions are 48264, 66603, 97981, 136185, 178837, 219573, 252676, 275127, and 321147. For reference, each spousal earning cutoffs maps to the following percentiles of the married male income distribution: $10,000 (15th), $20,000 (21st), $30,000 (31st), $40,000 (42nd), $50,000 (56th), $60,000 (68th), $70,000 (79th), $80,000 (86th), and the last point has no upper bound. The mean dependent variable (binary employment) for these nine regressions are 0.49, 0.54, 0.58, 0.61, 0.62, 0.62, 0.62, 0.61, and 0.59, which explains why the treatment effect as a percent is steeper than it is in percentage points. CPS weights used and average-marginal effects from logit regression are shown. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Fig. 5. Which Occupations Did These Newly Working Mothers Enter Into?

Notes: 1971-1986 March CPS data. The following professions are defined by the following occ1950 codes: Professional 0-99, Manager 200-290, Clerical 300-390, Teacher/Librarian use occ1990 codes 155-165, Sales 400-490, Craftsmen 500-595, Services 700-790, Construction/Laborers use occ1990 codes 558-599, none 999. Set of controls from Table 2 column 4 and “high-impact” sample used. Each estimate is from a different logit regression of having the specified occupation. The fraction of the sample in each of these occupations are 0.12, 0.04, 0.27, 0.04, 0.05, 0.01, 0.18, 0.01, 0.21. Clerical jobs and service jobs employed the most mothers and showed the largest increase in jobs for these newly working mothers. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Fig. 6. Effect of the EITC on Annual Work Hours Distribution

Notes: 1971-1986 March CPS data. Full set of controls from Table 2 column 4 and “high-impact” sample used. Each estimate is from a different logit regression of having annual work hours in the specified range. The mean dependent variable for the seven unconditional regressions are 0.35, 0.11, 0.09, 0.08, 0.11, 0.18, and 0.08; conditional on working these are 0.18, 0.13, 0.11, 0.10, 0.14, 0.23, and 0.10. Sample sizes are 230,399 and 173,752. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Fig. 7. Effect of the EITC on Annual Earnings Distribution

Notes: 1971-1986 March CPS data. Full set of controls from Table 2 column 4 and “high-impact” sample used. Each estimate is from a different logit regression of having annual earnings in the specified range. The mean dependent variable for the nine unconditional regressions are 0.25, 0.27, 0.15, 0.13, 0.10, 0.06, 0.03, 0.01, 0.01; conditional on working these are 0.0, 0.35, 0.20, 0.18, 0.13, 0.07, 0.04, 0.02, 0.02. Sample sizes are 230,399 and 173,752. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Fig. 8. Effect of the EITC on Annual Work Hours (Quantile Dif in Dif)

Notes: 1971-1986 March CPS data. Each estimate uses the full set of controls from Table 2 column 4 and the “high-impact” sample and mimics the regression behind Table 4 column 1 except instead of average effects results shown are the effect of $Mom \times Post$ at each decile. High-impact sample size 230,399. The mean dependent variable at deciles 1 to 9 are 0, 0, 0, 224, 765, 1320, 1836, 2040, 2040; for mothers in 1975 the mean dependent variables are 0, 0, 0, 0, 168, 990, 1653, 2040, and 2040. The number of quantiles with zero response differs between Figures 8 and 9 since some observations report earnings or hours but not both. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Fig. 9. Effect of the EITC on Annual Earnings (Quantile Dif in Dif)

Notes: 1971-1986 March CPS data. Each estimate uses the set of controls from Table 2 column 4 and the “high-impact” sample and mimics the regression behind Table 1 column 1 except instead of average effects results shown are the effect of $Mom \times Post$ at each decile. High-impact sample size 230,399. The mean dependent variable at deciles 1 to 9 are 0, 0, 1305, 4534, 9283, 15686, 22647, 30411, and 41136; for mothers in 1975 the mean dependent variables are 0, 0, 0, 168, 4814, 12159, 20030, 28132, 38552. The number of quantiles with zero response differs between Figures 8 and 9 since some observations report earnings or hours but not both. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.
Notes: 1975-1985 IRS Statistics of Income public use files. Sample consists of taxfilers with positive self-employed (business) income. Data on children not available in 1976 so I proxy for EITC-eligible as having at least one dependent at home. Following Saez (2010), I use a quasi-linear and iso-elastic utility function and calculate elasticities from bunching using bandwidths of $1000, $1500, $2000 to be 0.23, 0.52, 0.77 in 1975-1978 and 0.58, 1.28, 2, 22 in 1979-1985.
Fig. 11. Gender-Equality Attitudes Increasing Over Time

Notes: Gender-equality attitudes constructed from the binary survey question, “Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?” Data sources include the restricted state-identified 1972-1998 General Social Survey (GSS) data and datasets obtained from the Roper Center (http://ropercenter.cornell.edu/CFIDE/cf/action/ipoll/index.cfm) including Gallup data (Gallup 1936, 1938, 1945, 1970, 1975) and Connecticut Mutual Life Insurance Company data (CMLIC 1980). GSS weights used to construct annual averages. Other datasets are unweighted. Respondents include male and female adults of all ages.
Fig. 12. State EITC Response and Change in Gender-Equality Attitudes (Post1975-Pre1975)

Notes: 1972-1985 restricted GSS data with state-level identifiers. Gender-equality attitudes constructed from the GSS variable *fework*. GSS sample reflects males 18 to 60 years old. GSS sample contains individuals from 32 states during the 1970s. Changes in gender-equality attitudes is calculated by subtracting the pooled 1972-1975 state-average attitude from the 1976-1985 state-average attitude using GSS sample weights. I pool years before and after 1975 to increase power. State-level EITC response is estimated from equation (3). The best fit line reflects the bivariate regression of the state-level change in attitudes on the state EITC response. Bootstrapped standard errors computed from 1000 replications with replacement.
Fig. 13A. Ruling Out Reverse Causation: EITC Response and 1974 Attitude Level

Fig. 13B. Ruling Out Reverse Causation: EITC Response and 1972-1975 Attitude Trend

Notes: 1972-1985 restricted GSS data with state-level identifiers. Gender-equality attitudes constructed from the GSS variable *fework*. GSS sample reflects males 18 to 60 years old. GSS sample contains individuals from 32 states during the 1970s. In Figure 13A, state-level EITC response is estimated from equation (3) and 1974 state-level attitudes are constructed by averaging individual attitudes using GSS sample weights. In Figure 13B, state-level EITC response is estimated from equation (3) and 1972-1975 change in state-level attitudes are constructed by subtracting the 1972 state average of individual attitudes from the 1975 state average, using GSS sample weights. The best fit line reflects the bivariate regression of the state-level change in attitudes on the state EITC response. Bootstrapped standard errors computed from 1000 replications with replacement.
Fig. 14A. Larger Effect of EITC on Attitudes of Lower-Education Males

\[ \Delta \text{GenderEquality}_{\text{Lower}} = -0.050 + 0.028 \times \text{EITC Response} \]

\[(0.063) \quad (0.010) \quad R^2 = 0.19\]

Fig. 14B. No Effect of EITC on Attitudes of Higher-Education Males

\[ \Delta \text{GenderEquality}_{\text{Higher}} = -0.008 + 0.010 \times \text{EITC Response} \]

\[(0.066) \quad (0.020) \quad R^2 = 0.02\]

Notes: Lower education males in Panel A have 12 or less years of education, higher education males in Panel B have at least 13 years of education. See Figure 12 for sample and details.
Notes: Permutation test consists of randomly reassigning (with replacement) state attitude changes to each state and re-regressing attitude changes on EITC response. 1000 iterations. This is a modified version of the modified Fisher permutation in Buchmueller, DiNardo, and Valletta (2011). The actual point estimate is .0284 and is in the top 0.01 percent of these permutations. This suggests that the bivariate regression in Figure 14A is likely not due to chance.
Fig. 16. Placebo Attitude on Racial Equality Uncorrelated with State EITC Response

\[ \Delta \text{RacialEquality} = 0.041 - 0.004 \times \text{EITC Response}, \]

\( (0.033) \quad (0.007) \quad R^2 = 0.01 \)

Notes: 1972-1985 restricted GSS data with state-level identifiers. Racial-equality attitudes constructed from the GSS variable racpres. GSS sample reflects males 18 to 60 years old. GSS sample contains individuals from 32 states during the 1970s. Change in racial equality attitudes is calculated by subtracting the pooled 1972-1975 state-average attitude from the 1976-1985 racial-average attitude using GSS sample weights. I pool years before and after 1975 to increase power due to relatively few GSS observations in some state-year cells. State-level EITC response is estimated from equation (3). The best fit line reflects the bivariate regression of the state-level change in attitudes on the state EITC response. Bootstrapped standard errors computed from 1000 replications with replacement.
Fig. 17. Predicted EITC Response (from 1974 Female Educ) and Attitude Change

Notes: 1972-1985 restricted GSS data with state-level identifiers. State-level traits created by averaging GSS observations ages 18-60. Female education is correlated with Post1975-Pre1975 change in gender-equality attitudes and the state level EITC response, but is not correlated with the 1972-1975 change in gender-equality attitudes. Panel C regresses the predicted EITC response from the best fit line in Panel B on the state EITC response. This shows that not only is the actual state EITC response correlated with changes in attitudes (Figure 12), but so is the predicted response, which is an effort to purge the endogenous part of the state EITC response. Panel D shows that, if anything, the pre-1975 attitude trend was positively correlated with education, suggesting that the post-1975 change in attitudes is not a continuation of a pre-existing trend.
Fig. 18. WWII Increase in Female Employment Also Led to Changes in Gender Attitudes

Notes: Data source Roper Center (http://ropercenter.cornell.edu/CFIDE/cf/action/ipoll/index.cfm) and Berinsky and Schickler (2011). Gallup datasets and survey questions used. Gallup (1937a,b,c): “Are you in favor of permitting women to serve as jurors in this state?” Gallup (1937c): “Would you vote for a woman for President if she qualified in every other respect?” Gallup (1938): “Do you approve of a married woman earning money in business or industry if she has a husband capable of supporting her?” Gallup (1939): “A bill was introduced in the Illinois State Legislature prohibiting married women from working in business or industry if their husbands earn more than $1,600 a year ($133 a month). Would you favor such a law in this state?” Gallup (1945): “If the party you most often support nominated a woman for Governor of this state, would you vote for her if she seemed qualified for the job?” “If the party whose candidate you most often support nominated a woman for President of the United States, would you vote for her if she seemed best qualified for the job?” “Would you approve or disapprove of having a capable woman in the President’s cabinet?” “A woman leader says not enough of the capable women are holding important jobs in the United States government. Do you agree or disagree with this?” “Would you approve or disapprove of having a capable woman on the Supreme Court?” Change in attitudes (After WWII - Before WWII) created by, first, coding each binary response so that 1 represents gender-equality attitudes; second, averaging each survey question at the state-year level, third averaging the five (November) 1945 questions at the state level to create “After WWII” and average the six 1937-1939 questions at the state level to create “Before WWII.” Unfortunately, it is not possible to compare exact questions immediately before and after WWII but estimates are very similar if any one or two of the survey questions are omitted: point estimates span 0.017 and 0.007, p-values span 0.001 and 0.065 for these 20+ regressions. Estimates are also positive and statistically significant when the attitudes of men and women are analyzed separately: for men 0.0120 (0.0057) and for women 0.0106 (0.0041).
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<th>Mothers</th>
<th>Women without Kids</th>
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<td>(6.9)</td>
<td>(7.6)</td>
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<td>12.1</td>
<td>12.2</td>
</tr>
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<td>(2.6)</td>
<td>(2.5)</td>
<td>(2.6)</td>
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<td>(17,359)</td>
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<td>(16.7)</td>
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<td>33.8</td>
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<td>(13.1)</td>
<td>(12.8)</td>
<td>(13.3)</td>
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Observations                                | 550,904   | 310,875 | 240,029

Notes: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). Sample contains all women 16 to 45 years old. Standard deviations are in parentheses.
Table 2: The 1975 EITC and the Employment of All Women

Mean Dependent Variable Across Years and Across Treatment and Control Groups = 0.65
Mean Dependent Variable for Treatment Group in 1975 = 0.53

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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<td>0.046***</td>
<td>0.040***</td>
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Controls

- State and Year FE  
- Demographic Controls  
- Unemployment Rate  
- Additional Interactions

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<th>Observations</th>
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<th>550,904</th>
<th>550,904</th>
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<td>Logit</td>
<td>Logit</td>
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<td>R-squared</td>
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<td>0.174</td>
<td>0.174</td>
<td>0.174</td>
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</table>

Notes: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). Sample includes all women 16 to 45 years old. Dependent variable binary employment for having positive earnings. CPS weights, equation (2) used and average marginal effects from logit or OLS regression are shown. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level. FE denotes fixed effects. Demographic controls include married, welfare income, number of children, any children under 5, age cubic, years of education quadratic, nonwhite-kid, nonwhite-post1975, age-kid, and married-post1975. Unemployment rate includes both annual federal unemployment rates and state-year employment-to-population ratios and interactions between these measures of unemployment and kid and married. Additional interactions include unemployment rate-age, nonwhite-welfare, nonwhite-married, number children-married, child less than 5-married, married-welfare income, education years-married, education-child less than 5, education-nonwhite, a nonwhite-age cubic, unemployment rate-nonwhite, and fixed effects for nonwhite-year, married-year, nonwhite-state, birth-year, state-year, state-married, state-child less than 5, state-year-nonwhite, and state-year-married. *** p<0.01, ** p<0.05, * p<0.1.
Table 3: Heterogeneous and Subgroup Treatment Effects of the 1975 EITC on Employment

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<th>Description:</th>
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<th>Education</th>
<th>Age</th>
<th>Race</th>
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<th>Single Men</th>
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<td>0.61</td>
<td>0.54</td>
<td>0.61</td>
<td>0.62</td>
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<td>0.51</td>
<td>0.46</td>
<td>0.51</td>
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<td>0.016</td>
<td>0.049***</td>
<td>0.062***</td>
<td>0.007</td>
<td>0.092***</td>
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<td>Mom x Post1975 x White</td>
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<tr>
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<td>Dad x Post1975</td>
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<td>55,313</td>
<td>321,147</td>
<td>265,834</td>
<td>550,904</td>
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Notes: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). All samples limited to 16 to 45 year olds. Dependent variable binary employment for having positive earnings. CPS weights, equation (2) used and average-marginal effects from logit regression are shown. Standard errors are computed by the delta method, are robust to heteroskedasticity, and clustered at the state level. Each column reflects a separate regression with the set of controls from Table 2 column 4. The variable *Mom x Post1975* refers to *Mom x Post1975 x Married* in column 1, *Mom x Post1975 x (<=12 Yrs Ed)* in column 6, and *Mom x Post1975 x Non-High-Impact Sample* in column 9. "High-impact" sample excludes EITC-ineligible women with spousal earnings above the EITC limit (column 5 sample) and women not in the labor force due to a disability, health reason, or full-time student in order to capture women most in a position to respond to the employment incentives of the EITC. The mean dependent variable for 1975 mothers with less than, equal to, or more than 12 years of education in column 1 is 0.45, 0.54, and 0.59. The EITC income limit in columns 3 and 5 was $4,000 nominal dollars in 1975 and increased to $5,000 in 1979 (or about $18,000 in 2013 dollars). *** p<0.01, ** p<0.05, * p<0.1.
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<td>&quot;High-Impact&quot;</td>
<td>All</td>
<td>&quot;High-Impact&quot;</td>
</tr>
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<td>Mean Dependent Variable:</td>
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<td>765 (2)</td>
<td>15,149 (4)</td>
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<td>Mean Dep. Var. 1975 Mothers:</td>
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Notes: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). Each column represents a separate regression with the set of controls from Table 2 column 4. All samples limited to women 16 to 45 years old. EITC-ineligible placebo group have spousal earnings above the 1975 EITC income limit of $18000 2013 dollars. Annual work hours are constructed by multiplying weeks worked and hours worked last week. Weeks worked is given as an interval until 1975. I use this variable for all years to be consistent and assign the midpoint of the interval. Weekly work hours refers to the week prior to the March CPS interview. Imputed hourly wage calculated by dividing annual earnings by annual work hours, zero assigned if annual work hours equals zero (even if reported annual earnings is positive). CPS weights used and average effects from OLS regressions shown. Standard errors are robust to heteroskedasticity and clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.
Table 5: Individual Traits Correlated with Egalitarian Attitudes Related to Gender and Race

Panel A: Egalitarian Gender Role Attitudes

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Controls

| State FE              | X         | X         | X         | X         | X         | X         | X         | X         | X         | X         |
|                      |           |           |           |           |           |           |           |           |           |           |
| Year FE              | X         | X         | X         | X         | X         | X         | X         | X         | X         | X         |
|                      |           |           |           |           |           |           |           |           |           |           |
| Observations         | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 1,773     | 8,512     |
|                      | 0.074     | 0.030     | 0.089     | 0.036     | 0.037     | 0.053     | 0.046     | 0.043     | 0.044     | 0.113     |

Panel B: Placebo Outcome: Egalitarian Racial Attitudes

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</table>

Controls

| State FE              | X         | X         | X         | X         | X         | X         | X         | X         | X         | X         |
|                      |           |           |           |           |           |           |           |           |           |           |
| Year FE              | X         | X         | X         | X         | X         | X         | X         | X         | X         | X         |
|                      |           |           |           |           |           |           |           |           |           |           |
| Observations         | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 8,512     | 1,773     | 8,512     |
|                      | 0.046     | 0.031     | 0.062     | 0.036     | 0.052     | 0.042     | 0.038     | 0.042     | 0.044     | 0.086     |

Notes: 1972-1985 restricted GSS data with state-level identifiers. The samples in Panels A and B consists of all men ages 18 to 60. Egalitarian gender-role attitudes constructed from the GSS variable *fework*, which asks respondents whether married women should work; positive values represent egalitarian attitudes. Race-equality preferences comes from the GSS variable *rangep*, which asks respondents whether they would vote for a black president. Data about whether one's mother work was often missing. Robust standard errors clustered at the state level in parentheses.

***p<0.01, **p<0.05, *p<0.1
Table 6: Maternal Employment Led to an Increase in Gender-Equality Preferences

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<th>Changes in Other Social Attitudes</th>
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<td>(2a)</td>
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<td>Variables</td>
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<td>0.020***</td>
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<td>(0.006)</td>
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<td>R-squared</td>
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<td>0.481</td>
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</table>

|          | Panel B: Female Gender-Role Attitudes |                                  |
|          | (1b)   | (2b)   | (3b)   | (4b)   | (5b) | (6b) | (7b) | (8b) | (9b) | (10b) | (11b) | (12b) | (13b) |
| Variables | State-Level Increase in Maternal Employment due to EITC (in Percentage) |                      |
|          | 0.013** | 0.013** | 0.013** | 0.012* | 0.013** | 0.016*** | 0.012** | 0.012* | 0.013** | 0.014*** | 0.013* | 0.013** | 0.013** |
|          | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.005) | (0.006) | (0.006) | (0.006) | (0.006) | (0.005) | (0.006) | (0.006) |
| Observations | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 | 32 |
| R-squared   | 0.177 | 0.182 | 0.182 | 0.186 | 0.200 | 0.292 | 0.182 | 0.186 | 0.196 | 0.204 | 0.322 | 0.180 | 0.182 |

Notes: 1972-1985 restricted GSS data with state-level identifiers. Gender-equality preferences constructed from the GSS variable fework which asks respondents whether women should work. GSS sample reflects males 18 to 60 years old in 32 states. This binary response is coded so that 1 represents gender-equality preferences. State-level EITC response estimated from equation (3). Each variable constructed by subtracting the pooled 1972-1975 GSS state-average from the 1976-1985 GSS state-average using GSS sample weights. Education measured in years, married =1 if marital status is married, race measures fraction nonwhite, working denotes labor-force participation (working full-time, working part-time, temporarily laid off, or unemployed), earnings denotes log earnings. Mom worked and mom education constructed from GSS variables mawk16 and maedue, democrat from partyid, racial equality from racpres, religious from religion, and too much welfare from welfare. Results robust to using 1970 Census measures where applicable. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
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<tr>
<th>Variable Used to Predict State EITC Response</th>
<th>White Single Moms</th>
<th>Nonwhite Single Moms</th>
<th>Fraction Female</th>
<th>Male Earnings</th>
<th>Fraction Men Not Working</th>
<th>Teachers, Librarians</th>
<th>Housekeepers, Cleaners</th>
<th>Bakers, Food Makers</th>
<th>Metal, Wood Workers (Men)</th>
<th>Construction (Men)</th>
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<tbody>
<tr>
<td>Predicted State-Level Increase in $\text{EITC}$ (in Percentage Points)</td>
<td><strong>0.026</strong>*</td>
<td>0.031**</td>
<td>0.060</td>
<td>0.039</td>
<td>0.059</td>
<td>0.032**</td>
<td><strong>0.036</strong></td>
<td>0.037***</td>
<td><strong>0.076</strong></td>
<td><strong>0.157</strong>*</td>
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<td>Maternal Employment due to EITC (in Percentage Points)</td>
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<td>(0.015)</td>
<td>(0.035)</td>
<td>(0.024)</td>
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<td>0.126</td>
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Panel B: Predicting EITC Response and Change in Attitudes (with Region FE)

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<th>White Single Moms</th>
<th>Nonwhite Single Moms</th>
<th>Fraction Female</th>
<th>Male Earnings</th>
<th>Fraction Men Not Working</th>
<th>Teachers, Librarians</th>
<th>Housekeepers, Cleaners</th>
<th>Bakers, Food Makers</th>
<th>Metal, Wood Workers (Men)</th>
<th>Construction (Men)</th>
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<tbody>
<tr>
<td>Predicted State-Level Increase in $\text{EITC}$ (in Percentage Points)</td>
<td><strong>0.023</strong>*</td>
<td>0.023**</td>
<td>0.023**</td>
<td>0.022**</td>
<td>0.025***</td>
<td><strong>0.021</strong></td>
<td><strong>0.023</strong></td>
<td><strong>0.022</strong></td>
<td>0.017</td>
<td>0.022**</td>
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<tr>
<td>Maternal Employment due to EITC (in Percentage Points)</td>
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<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.009)</td>
<td>(0.009)</td>
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<tr>
<td>R-squared</td>
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<td>0.118</td>
<td>0.172</td>
<td>0.104</td>
<td>0.121</td>
<td>0.117</td>
<td>0.073</td>
<td>0.113</td>
</tr>
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</table>

Notes: 1972-1985 restricted GSS with state-level identifiers. Gender-equality preferences constructed from the GSS variable fwork which asks respondents whether women should work. GSS sample reflects males 18 to 60 years old in 32 states. This binary response is coded so that 1 represents gender-equality preferences. State-level EITC response estimated from equation (3). Implicit behind each estimate is the four-panel scatterplot in Figure 18 and each estimate corresponds to Figure 18 Panel D. Results robust to using 1970 Census measures where applicable. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
10. **Appendix A: Additional Figures and Tables**

Fig. A1. Trends in EITC Income Eligibility Limit and Max Potential Benefits

![Graph showing trends in EITC income eligibility limit and max potential benefits.](image)

**Notes:** Author’s calculation from IRS EITC data and 1975-2013 March CPS data on household earned income (adding each spouse’s earnings).

Fig. A2. Female Income Distribution, 1970-1985, Ages 16-45

![Graph showing female income distribution.](image)

**Notes:** Author’s calculation from 1971-1986 March CPS data. The EITC kink point and EITC limit refer to the real value in 2013 dollars of the 1975 EITC schedule ($18,000 and $36,000).
Fig. A3. Ruling Out Confounding Policies (Tax Rate, WIC, AFDC, Food Stamps)

Notes: Author’s calculation from AFDC/TANF data, Food Stamps/SNAP data, WIC data, and payroll tax data.

Fig. A4. Ruling Out Confounding Trends (Fertility, Marriage, Education, Male Earnings)

Notes: Author’s calculation from 1968 to 2015 March CPS. Sample includes individuals 16 to 45 years old and refer to women unless men is specified.
Fig. A5. 1976 Child Care Tax Credit Affects a Small Number of EITC-Eligible Taxfilers

Notes: Author’s calculations from 1976-1985 IRS Statistics of Income Public Use data files. Sample restricted to taxfilers with earned income or business income; this eliminates taxfilers with only dividend, interest, capital gains, pensions, farm, and alimony income. EITC eligibility imputed to taxfilers with dependents and earnings plus business earnings below the annual EITC income limit. This is imperfect since dependents do not necessarily denote children and I am not able to observe whether taxfilers actually claimed the EITC. Refundable portion of the EITC is given in the data, but this does not include households who benefit from the EITC through decreased tax liabilities and undercounts EITC recipients.

Fig. A6. Placebo Test: EITC Response Uncorrelated with Placebo Year Attitude Changes

Notes: 1980-1997 restricted GSS data with state-level identifiers. Each estimate is from the bivariate regression of the change in attitudes (difference between average state attitudes in the five years after each cutoff minus the average state attitude in the five years before each cutoff, using GSS sample weights) on state response to the 1975 EITC. See Figure 12 for additional details.
Fig. A7. State EITC Response Negatively Correlated with Voting for 1975 EITC

Notes: Data on House of Representatives voting from https://www.govtrack.us/congress/votes/94-1975/h67 and data on Senate voting from https://www.govtrack.us/congress/votes/94-1975/s112. Congressmen include House Representatives and Senators pooled. State EITC response comes from equation (3). Red, hollow circles denote states that are not in GSS data (since GSS did not interview all 50 states during the 1970s and 1980s). Blue, solid circles denote states in GSS and appear in previous scatterplots. Estimates are similar (but noisier) for each group of states separately.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean Dependent Variable:</td>
<td>0.651</td>
<td>0.611</td>
<td>0.501</td>
<td>0.682</td>
<td>0.491</td>
<td>0.604</td>
<td>0.054</td>
</tr>
<tr>
<td>Mean Dependent Variable for Mothers in 1975:</td>
<td>0.530</td>
<td>0.502</td>
<td>0.417</td>
<td>0.563</td>
<td>0.395</td>
<td>0.495</td>
<td>0.045</td>
</tr>
<tr>
<td>VARIABLES</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td>Mom x Post1975</td>
<td>0.040***</td>
<td>0.038***</td>
<td>0.035***</td>
<td>0.035***</td>
<td>0.032***</td>
<td>0.031***</td>
<td>0.009***</td>
</tr>
<tr>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.008)</td>
<td>(0.003)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>556,143</td>
<td>556,143</td>
<td>556,143</td>
<td>556,143</td>
<td>556,143</td>
<td>556,143</td>
<td>556,143</td>
</tr>
<tr>
<td>Implied Extensive Margin Labor Supply Elasticity:</td>
<td>0.734</td>
<td>0.736</td>
<td>0.813</td>
<td>0.608</td>
<td>0.786</td>
<td>0.613</td>
<td>--</td>
</tr>
</tbody>
</table>

Note: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). Sample includes all women 16 to 45 years old. Dependent variable binary employment for having positive earnings. CPS weights, equation (2), and the set of controls from Table 2 column 4 are used. Each estimate is from a separate logit regression, average marginal effects from logit or OLS regression are shown. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level. Each elasticity is calculated as the change in log employment rates divided by the change in log after-tax earnings. For column 1 this is \([\log(0.530+0.040) - \log(0.530) ]/(\log(18270-5256)-\log(16184-4401))\). Detailed explanation in Appendix C. *** p<0.01, ** p<0.05, * p<0.1.
<table>
<thead>
<tr>
<th>Sample:</th>
<th>1986 EITC Expansion</th>
<th>1993 EITC Expansion</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Employed</td>
<td>Annual Work Hours</td>
</tr>
<tr>
<td>Variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mom x Post EITC Expansion</td>
<td>0.017</td>
<td>0.056***</td>
</tr>
<tr>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(17.199)</td>
</tr>
<tr>
<td>Kid x Post EITC Expansion x</td>
<td>-0.009***</td>
<td></td>
</tr>
<tr>
<td>Spousal Income (1000s of 2013 S)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.135</td>
<td>0.137</td>
</tr>
</tbody>
</table>

Note: Columns 1-4 follow Eissa and Liebman (1996) and examine the effect of the 1986 EITC expansion on the employment of 16-44 year old females using 1985-1987 (pre EITC expansion) and 1989-1991 (post EITC expansion) March CPS data. Columns 5-8 follow Eissa and Hoyes (2004) and examine the effect of the 1993 EITC expansion on the employment of 25-54 year old females using 1989-1992 (pre EITC expansion) and 1993-1996 (post EITC expansion) March CPS data. Binary employment is defined as having positive hours of work (to match the definition in these two papers). Annual work hours equals weekly work hours -- which refers to the week prior to the March CPS interview -- times weeks worked last year. Regressions for binary employment reflect average marginal effects from a logit regression and weekly work hours use OLS. In each regression the set of controls is used from Table 2 column 4 and CPS weights are used. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level. Married women with missing spousal earnings were dropped. Estimates are quite similar to the 1975 EITC (Table 3 columns 2 and 4). *** p<0.01, ** p<0.05, * p<0.1.
Table A3: Summary Statistics (State-Level GSS Data)

<table>
<thead>
<tr>
<th>Variable</th>
<th>1972-1975 Years Pooled</th>
<th>1976-1986 Years Pooled</th>
</tr>
</thead>
<tbody>
<tr>
<td>Average Age</td>
<td>Mean 36.83 Std. Dev. 1.88 Min 32.08 Max 39.74</td>
<td>Mean 37.15 Std. Dev. 1.32 Min 34.00 Max 39.49</td>
</tr>
<tr>
<td>Average Education</td>
<td>Mean 12.33 Std. Dev. 0.62 Min 11.04 Max 13.60</td>
<td>Mean 12.62 Std. Dev. 0.62 Min 11.22 Max 13.69</td>
</tr>
<tr>
<td>Fraction Married</td>
<td>Mean 0.71 Std. Dev. 0.09 Min 0.58 Max 0.88</td>
<td>Mean 0.62 Std. Dev. 0.06 Min 0.51 Max 0.74</td>
</tr>
<tr>
<td>Fraction Nonwhite</td>
<td>Mean 0.12 Std. Dev. 0.11 Min 0.00 Max 0.39</td>
<td>Mean 0.17 Std. Dev. 0.12 Min 0.01 Max 0.40</td>
</tr>
<tr>
<td>Employment Rate</td>
<td>Mean 0.67 Std. Dev. 0.08 Min 0.43 Max 0.88</td>
<td>Mean 0.75 Std. Dev. 0.06 Min 0.52 Max 0.85</td>
</tr>
<tr>
<td>Average Earned Income (2013 $)</td>
<td>Mean 27,726 Std. Dev. 4,471 Min 19,593 Max 39,731</td>
<td>Mean 27,073 Std. Dev. 4,030 Min 19,685 Max 34,295</td>
</tr>
<tr>
<td>Fraction Female</td>
<td>Mean 0.55 Std. Dev. 0.05 Min 0.42 Max 0.67</td>
<td>Mean 0.57 Std. Dev. 0.05 Min 0.48 Max 0.70</td>
</tr>
<tr>
<td>Average Gender-Equality Attitudes</td>
<td>Mean 0.26 Std. Dev. 0.18 Min -0.04 Max 0.75</td>
<td>Mean 0.36 Std. Dev. 0.10 Min 0.17 Max 0.57</td>
</tr>
<tr>
<td>Fraction of Women Single Moms</td>
<td>Mean 0.29 Std. Dev. 0.12 Min 0.09 Max 0.62</td>
<td>Mean 0.40 Std. Dev. 0.06 Min 0.27 Max 0.55</td>
</tr>
<tr>
<td>Fraction Democrat</td>
<td>Mean 0.57 Std. Dev. 0.11 Min 0.38 Max 0.81</td>
<td>Mean 0.55 Std. Dev. 0.08 Min 0.34 Max 0.75</td>
</tr>
<tr>
<td>Average Racial-Equality Attitudes</td>
<td>Mean 0.26 Std. Dev. 0.23 Min -0.52 Max 0.55</td>
<td>Mean 0.33 Std. Dev. 0.13 Min 0.04 Max 0.61</td>
</tr>
<tr>
<td>Fraction Religion Important</td>
<td>Mean 0.46 Std. Dev. 0.11 Min 0.21 Max 0.63</td>
<td>Mean 0.45 Std. Dev. 0.07 Min 0.33 Max 0.63</td>
</tr>
<tr>
<td>Preference for Less Welfare (Standardized)</td>
<td>Mean 0.00 Std. Dev. 0.23 Min -0.56 Max 0.44</td>
<td>Mean 0.11 Std. Dev. 0.17 Min -0.19 Max 0.41</td>
</tr>
<tr>
<td>Fraction with a Working Mother at 16</td>
<td>Mean 0.68 Std. Dev. 0.11 Min 0.44 Max 0.94</td>
<td>Mean 0.69 Std. Dev. 0.08 Min 0.52 Max 0.84</td>
</tr>
<tr>
<td>Average Education of Mother</td>
<td>Mean 11.19 Std. Dev. 0.38 Min 10.42 Max 12.00</td>
<td>Mean 11.45 Std. Dev. 0.28 Min 10.88 Max 12.00</td>
</tr>
<tr>
<td>Individuals Observed</td>
<td>Mean 69.41 Std. Dev. 52.02 Min 19 Max 246</td>
<td>Mean 211.47 Std. Dev. 157.46 Min 38 Max 763</td>
</tr>
</tbody>
</table>

Number of States: 32

Notes: 1972-1985 restricted GSS data with state-level identifiers. State-level averages created by averaging individuals observed in 1972, 1974, and 1975 and individuals observed in 1977, 1978, 1982, 1983, 1985, and 1986. These are the years the GSS provides information on gender role attitudes before 1986. Age, education, married, nonwhite, employment, earned income, gender equality attitudes, democrat, racial equality attitudes, religion, want less welfare, had working mother, education of mother are averaged over men and women age 18 to 60. Fraction of women single moms is the number of working moms divided by the number of women in each state. Democrat defined as 1 if having a political party identification as strong democrat, not strong democrat, independent near democrat, and a 0 if strong republican, not strong republican, independent near republican, independent, or other party. Racial equality attitudes defined as would vote for a black president. Religion important is a 1 if strength of religious affiliation is strong or somewhat strong and is a 0 if not very strong or no religion. Want less welfare is constructed from a variable asking if there is too much, too little, or just about right amount of welfare; answers are standardized at 1974 levels and higher values indicate a belief that welfare is too high. GSS only surveyed 33 states until 1977, 34 states from 1979-1982, 36 in 1983, and 40 from 1985-1986. To be consistent I only keep states observed in each year. One state (West Virginia) is dropped because there are few observations in the GSS and CPS and the state EITC response is an outlier (-10 percentage points). Results are similar if this state is included.
11. Appendix B: Additional Robustness Checks

11.1. Model Choice and Sample Period

In Figure B1, I show that the estimated DD treatment effect is robust to a probit, logit, or OLS model as well as when to end the sample after 1975. As would be expected from Figure 1B, the treatment effect is small if the sample ends soon after 1975, but grows and flattens out as more years after 1975 are included. OLS results are consistently larger than from a logit or probit (see footnote 31).

11.2. Larger Response from Mothers Eligible for More EITC Benefits

Conditional on year and spousal earnings (if any), I calculate maximum potential EITC benefits in 2013 dollars (\(\text{MaxEITC}\)) and run a regression identical to equation (2) except with the additional variable \(\text{Mom} \times \text{Post1975} \times \text{MaxEITC}\). For mothers with non-earning spouses and unmarried mothers, the value of \(\text{MaxEITC}\) varied by year and ranged between $1,100 and $1,700 since the EITC schedule was not pegged to inflation until 1986; for married mothers with a working spouse earning above the EITC kink point (placebo group from Table 3 column 5), \(\text{MaxEITC}\) was zero; for married women with a spouse earning below the EITC kink point, \(\text{MaxEITC}\) was equal to 10 percent of the difference between the EITC kink point and her spouses earnings. For example, a mother with spousal earnings of $10,000 and an EITC kink point of $16,000 would have a \(\text{MaxEITC}\) value of $600. See Figure 3B for the 1975 EITC schedule and see Figure B2 for a histogram of \(\text{MaxEITC}\). Table B1 column 1 shows that a $1,000 (2013 dollars) increase in \(\text{MaxEITC}\) is associated with a 4.5-percentage point increase in maternal employment. This regression also carries out the placebo test from Table 3 column 5 in a slightly different way: the estimate of \(\text{Mom} \times \text{Post}\) is now statistically insignificant (that is, a mother after 1975 is no more likely to work than before 1975 if she is eligible for zero EITC benefits) and the effect of the EITC is loaded onto \(\text{Mom} \times \text{Post} \times \text{MaxEITC}\).

11.3. Triple Differences Corroborates DD Estimates

Splitting the sample into the placebo group from Table 3 column 5 and all other women — all unmarried women and married women with spouses earning below the EITC income limit — creates a third difference (\(\text{Treat}\)) with which to run triple differences (DDD) analysis. DD results would be biased if an unaccounted for policy or trend affected the relative employment of all mothers. As discussed in section 3.2 the 1976 Child Care Tax Credit is one such
potential policy. The following DDD logit model extends equation (2), uses the controls from Table 2 column 4 including interactions with these variables with \( \text{Treat} \) for a more flexible model, and adds controls for \( \text{Treat, Mom x Treat, Post1975 x Treat} \), and the DDD variable of interest \( \text{Mom x Post1975 x Treat} \).

\[
P(E_{ist}) = f(\beta_1 \text{Mom x Post1975 x Treat}_{ist} + \beta_2 X_{ist} + \delta_{ist} + \epsilon_{ist}) \quad (B1)
\]

Table B1 column 2 shows that the estimate of \( \beta_1 \) is 4.5 percentage points, statistically identical to the DD estimate of 4.0 percentage points, and suggests that policies affecting the employment of all mothers (e.g. abortion laws, birth controls, the 1976 Child Care Tax Credit) does not pose a threat to my DD estimates. Table B2 column 3 shows a similar estimate when single men are used as a comparison group against all women. Although males were also eligible for the EITC, Table 3 column 10 shows a null response among single men, which aligns with the observation that male labor supply during this period was fairly inelastic (Blundell and MaCurdy 1999). This DDD estimate is 4.4 percentage points.

**11.4. Potentially Endogenous Fertility and Group Composition**

In addition to using controls, another way to account for endogenous fertility, marital status, and group composition, is by reweighting women observed after 1975 to look like mothers before 1975. Although regression controls should largely account for any changing composition of mothers over time, reweighting acts as an additional robustness check (DiNardo, 2002). I construct two sets of weights — DiNardo et al. (1996), DFL, and inverse propensity IP — in the following two step process: First I use a logit and a parsimonious set of observable characteristics — six age bins, three education bins, state, and dummies for married, nonwhite, and mother — to estimate the probability than each observation in the sample is

---

1. This is one reason that (Angrist et al. 2009, p. 182) state that a DDD model “may generate a more convincing set of results” than a DD estimator. DDD is akin to estimating equation (2) twice, once for a treatment group eligible for the EITC (e.g. single mothers and married mothers with lower-earning spouses) and again for a control group ineligible for the EITC (e.g. married women with higher-earning spouses). The difference between these two DD estimates is the intuition behind the DDD estimate. (One difference is that covariate estimates in two separate DD regressions can differ, unlike in a single regression.) If the DD estimate for the control group is near zero, then the DDD estimate of the maternal employment response to the EITC should resemble the DD estimate.

2. 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). Probit and logit produce very similar results.
from a year before 1975.

\[ P(Pre75_{ist}) = f(\beta_1 Age_{ist} + \beta_2 Edu_{ist} + \beta_3 State_{ist} + \beta_4 Married_{ist} + \beta_5 Race_{ist} + \beta_6 Mom_{ist} + \epsilon_{ist}) \]  

(B2)

Each observation is assigned a probability \( p \) of being from a year before 1975, which is used to create DFL and IP weights by assigning each observation a weight of \( p/(1 - p) \) and \( 1/p \). Women are weighted less if they are less likely to be observed before 1975 (e.g. a woman with a college education) and weighed more if they are more likely to be observed before 1975 (e.g. a woman with low education or high fertility). Figure B3 verifies that the characteristics of women before and after 1975 overlap sufficiently and have common support (Busso et al., 2014). Re-estimating equation (2) with these new weights yields estimates of 4.2 and 3.7 percentage points (Table B1 columns 4 and 5), similar to the baseline estimate of 4.0.

11.5. March CPS Imputations

In 1975 the Census changed its hot deck procedure for imputing missing earnings (Bound and Freeman, 1992) and could affect the results in Tables 2, 3, and B1 since I define employment as having positive earnings (although Table A1 shows that estimates are similar for other binary definitions of working based on hours and employment status). The percentage of observations with imputed earnings in the sample is zero before 1975, but between 1975 and 1985 is 5.0, 4.5, 5.2, 4.3, 1.2, 1.1, 1.0, 1.0, 1.0, 1.0, 1.0, and 1.0. In Table B2 I show that the DD estimate is robust to various ways of treating observations with imputed earnings data. Column 1 shows the baseline DD estimate using the default CPS imputation and column 2 simply drops all imputed observations. In columns 3 and 4 I use equation (B2) and a logit to predict the probability than an observation has missing earnings data (to account for selection and data missing not at random), create DFL and IP weights, and re-estimate equation (2) with these weights. Columns 5 and 6 reflect estimates from a bounding exercise.

---

3DiNardo et al. (1996) utilize a parsimonious set of controls that contains only 32 education-experience-gender cells. Butcher and DiNardo (2002) utilize several covariates which yields many more cells. My choice results in 1512 cells, although results do not change much with alternate decisions.

4These weights are then multiplied with the CPS sample weights and normalized to add up to 1 (DiNardo, 2002), although this has almost no effect on the estimates.

5In 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). Source IPUMS: [https://usa.ipums.org/usa/voliii/80editall.shtml#note1](https://usa.ipums.org/usa/voliii/80editall.shtml#note1).

6Imputations fall into three categories: whether the individual had any positive earnings, the amount of earnings, or both. Only in the third case is it unclear whether the individual had positive earnings and I refer to this category only as an imputation.
— somewhat similar to Manski bounds \cite{Manski:1990} — where I manually assign all observations with missing earnings data to be working or not working. These estimates range from 3.9 to 4.7 percentage points, similar to the baseline estimate of 4.0.

11.6. Additional Response from Women with Multiple Children

Since the EITC did not provide additional benefits for having more than one child until 1991, mothers with multiple children should not have responded to the EITC any more than women with only one child. I test this with the following logit model that expands equation \eqref{eq:2} and accounts for any differential impact on employment from having at least $J$ kids.

\begin{equation}
    P(E_{ist}) = f(\beta_1 \text{Post}_{1975ist} + \sum_{k=1}^{J} [\beta_{2k} \text{Mom}_{ist}^k + \beta_{3k} \text{Mom}_{ist}^k \times \text{Post}_{1975ist}] + \beta_4 X_{ist} + \epsilon_{ist}) \quad (B3)
\end{equation}

Table B3 column 1-3 show results of this regression for $J = 1, 2, 3$. Column 1 replicates estimates from Table 2 column 4 where $J = 1$. Surprisingly, in columns 2 and 3 where $J = 2$ and $J = 3$, results show that the estimate of $\beta_{3,k=2}$ is positive and significant. This means that women with at least two kids were more likely to respond to the EITC than women with exactly one child. (Column 3 shows that mothers with at least three children do respond less than women with exactly two children.) Interestingly, Eissa and Liebman \cite{Eissa:1996} also find an additional response from women with at least two children. They suggest that this may be due to the concurrent increase in the tax exemption for each dependent, which benefited families with multiple children more. During my sample period, the tax exemption for each child also increased from $750$ to $1,000$ in 1979. However, even when I restrict the post-1975 period to end in 1978 I still find a positive estimate on $\beta_{3,k=2}$ (Table B3 column 4) and conclude that increased exemptions is not driving my results.

Another potential explanation is that mothers with multiple children are more likely to have completed their fertility. If mothers that have completed their fertility are more receptive to working — especially when their children reached school age — then with cross-sectional CPS data there could be a mechanical relationship between multiple children and EITC response. I test this hypothesis in Table B3 columns 5 to 9 by restricting the sample of mothers in the treatment group to those with a youngest child at least 2, 3, 4, 5, and 6 years old. As this youngest-child age restriction increases, the additional EITC response from mothers with at least two children converges towards zero, while the estimated response of mothers with exactly one child ($\hat{\beta}_{3,k=2}$) remains positive and grows from 2.9- to 3.7-percentage points. Mothers with multiple children and a youngest child at least 5 years old are statistically no more likely to respond to the EITC than women with just one child. I
verify that this pattern holds for the 1986 and 1993 EITC expansion as well (not shown),
and conclude that the apparent additional employment increase for women with at least
two children may be explained by completed fertility. This explanation helps explain why
\[\text{Eissa and Liebman (1996)}\] also find relatively larger responses from mothers with multiple children.

11.7. Using IRS Tax Data

Since the CPS shows that the 1975 EITC had a large effect on the employment of mothers,
this should be evident in the IRS Statistics of Income (SOI) data as well, however, a few
features of the IRS SOI data make it unattractive for detecting the effects of the 1975
EITC. For one, many non-working individuals do not file taxes, so detecting an extensive
margin response is not easy. Another is that household income is reported as one aggregate
number, so it is not possible to determine whether a spouse began working. Furthermore,
IRS SOI data includes few demographic variables so it is not possible to determine the
gender, age, race, or education of the taxfiler, whether they have children — dependents are
not necessarily children —, or how old their children might be\footnote{Marital status is available. Number of children is available beginning in 1977, but before then is only available in 1970 and 1975. See \url{http://users.nber.org/~taxsim/taxsim-ndx.txt} for annual available IRS SOI variables.}

Constrained by the IRS SOI data, I still find suggestive evidence that the EITC affected
the composition of taxfilers. Using 1968 to 1985 IRS SOI data, I show that the fraction of
unmarried EITC-eligible taxfilers — section \[\ref{sec:4.2.1}\] shows that single mothers were relatively
more affected by the EITC — increased in the years after 1975 (Figure \[\ref{fig:B4}\]). The pattern
closely resembles Figure \[\ref{fig:1B}\] flat before 1975, a quick rise between 1975 and 1980, and
relatively flat again after 1980. Without knowing taxfiler gender or whether dependents
denote children, this is only suggestive evidence that the EITC affected the employment of mothers\footnote{As to whether the aggregate number of taxfilers increased, this is difficult to determine since an increase of one million working mothers over a four year period (Figures \[\ref{fig:1A} \text{ and } \ref{fig:1B}\]) corresponds to about 250,000 mothers per year, which is relatively small in comparison to the 80 million households, 100 million adults in the labor force, and 95 million taxfilers in the U.S. by 1980 (source: CPS, BLS, IRS SOI). As a result, I am not able to detect an aggregate rise in taxfilers or in the number of working households using IRS SOI or CPS data. Time-series analysis of these data would not detect a newly-working mother that was already a part of a taxfiling household.}

To corroborate the effect of the EITC with administrative tax records, I first compare
the annual number of EITC-eligible households and the amount of EITC benefits implied
by CPS data with aggregate IRS EITC statistics. Figure \[\ref{fig:B5}\] shows that the number of
EITC-eligible households and aggregate EITC benefits — that I calculate from reported

75
household children and earnings — is nearly identical to the published EITC statistics in 1975. However, in the years after 1975, the CPS undercounts EITC recipients and benefits. The ratio of the CPS numbers to the official IRS numbers drops to about 90 percent by 1978, and continues to fall to 70 percent by the mid-1980s. One reason to expect EITC benefits calculated from the CPS to be lower than the actual benefits is that 20 to 25 percent of EITC claims are paid in error due to unintentional taxfiler error, divorced parents each claiming the same child, married couples splitting their qualifying children and filing separately as household heads, or lying about having children. Liebman (2000) finds that 11 to 13 percent of EITC recipients had no children. The growing gap between CPS and IRS data in Figure B5 suggests that taxfiler error may have increased between 1975 and 1985.

Lower EITC benefits in the CPS compared to aggregate IRS numbers also indicates that despite evidence in Figure 10A of misreporting self-employed income to take advantage of EITC benefits, my employment estimates from the CPS do not appear to reflect simply reporting working after 1975 rather than actually working.

Aggregate IRS data also reveal a puzzle in light of estimates in Table 2: the number of EITC recipients and the aggregate EITC benefits remained roughly constant between 1975 and 1985 (Figure B6). One way to reconcile the positive maternal employment response to the EITC and flat EITC benefits is by considering that the EITC schedule was not pegged to inflation until 1986 and inflation was high in the years after 1975. About 6.3 million households received EITC benefits in 1975, however, it appears that most EITC recipients in 1975 were already working before the 1975 EITC came to exist since Figures 1A and 1B suggest that the maternal employment was not affected until 1976. Due to rising prices and nominal wages, within a few years some of these households would earn above the nominal EITC earnings limit and no longer receive EITC benefits, akin to “bracket creep” in the tax literature (Saez, 2003). It is possible that the increase in EITC-eligible working mothers (Table 2) and these no-longer-EITC-eligible households cancelled out and resulted in a roughly constant number of EITC recipients. The following back of the envelope calculation examines whether this is plausible. Using the earnings distribution from the 1974 IRS SOI (before any labor supply response to the EITC), I use the CPI to inflate the 1974 earnings distribution into 1975, 1976, 1977, and 1978 dollars, and calculate the number of taxfilers that were EITC-eligible in 1975 but EITC-ineligible in 1976, 1977, or 1978 due to rising nominal income. Figure B7 shows a zoomed-in view of this income distribution in...
1975 dollars and illustrates that by 1976, 1977, and 1978, 0.6, 1.0, and 1.6 percent of taxfilers eligible for the EITC in 1975 would bracket-creep out of EITC eligibility, corresponding to 700,000, 1,200,000, and 1,800,000 taxfilers.\footnote{Population growth can account for at most about half a million of these 1.8 million additional EITC recipients: IRS SOI data shows that about a quarter of taxfilers with dependents had positive earnings below the EITC limit and CPS data shows that the number of households with children steadily grew from about 34.5 million in 1975 to 35.1 million in 1978. Depending on where in the income distribution these new households fall, population growth should lead to between 200,000 and 600,000 additional EITC recipients.} Even though the stock of EITC recipients remained roughly constant in the decade after 1975, there was substantial flow in and out of EITC eligibility. This may explain why the number of EITC recipients was flat even as a million mothers entered into employment due to the EITC.

12. Figures and Tables for Appendix B

Fig. B1. DD Estimate Robust to Model Choice and Year that Sample Period Ends

Notes: Data source: 1971-1986 March CPS data. Dependent variable is binary employment defined as having positive earnings. CPS weights, equation (2) used, and average-marginal effects shown. Standard errors are computed by the delta method, are robust to heteroskedasticity, and clustered at the state level. Set of controls from Table 2 column 4 and “high-impact” sample used. Treatment effects are estimates of $Mom \times Post_{1975}$ in equation (2) where Post_{1975} starts in 1976 and extends through the year specified on the x-axis.
Notes: Data source: 1971-1986 March CPS data. Maximum potential EITC benefits (in 2013 dollars) determined by spousal income (zero if unmarried) and year. Mothers eligible for zero EITC benefits make up about 88 percent of the sample and are excluded from the histogram for scale purposes.

Notes: Data source: 1971-1986 March CPS data. Equation (B2) used to predict the probability of each observation being observed in a year before 1975. Following DiNardo et al. (1996), I choose a parsimonious set of observable characteristics X, which includes six age bins, three education bins, married and nonwhite dummy variables, and 21 state bins for a total of 1512 cells.
Fig. B4. The 1975 EITC May Have Affected the Composition of Taxfilers

Notes: Author’s calculations from 1968-1985 IRS Statistics of Income Public Use data files. Sample restricted to taxfilers with earned income or business income; this eliminates taxfilers with only dividend, interest, capital gains, pensions, farm, and alimony income. Refundable portion of the EITC is also provided in the data, but this does not include households who benefit from the EITC through decreased tax liabilities and thus undercounts EITC recipients. Years are grouped into three-year bins to reduce noise.

Fig. B5. Comparing EITC Recipients and Benefits (CPS / IRS Ratio)

Notes: Author’s calculation from 1976-1986 March CPS data and published aggregate EITC recipients and benefits [http://www.taxpolicycenter.org/statistics/eitc-recipients]. EITC recipients and benefits based on household earnings, the annual EITC schedule, and whether the household had any children.
Fig. B6. Trends in EITC Benefits and Recipients

Notes: Author’s calculations from IRS data.

Fig. B7. Bracket Creep, Nominal EITC Schedule Reconciles Table 2 and Figure B6

Notes: Author’s calculations from 1974 IRS SOI data and CPI. Sample includes taxfilers with wage earnings or business income.
Table B1: Robustness Checks: MaxEITC, Triple Differences, Reweighting

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<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
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<tr>
<td>Mom x Post1975</td>
<td>0.003</td>
<td></td>
<td></td>
<td>0.042***</td>
<td>0.037***</td>
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<tr>
<td></td>
<td>(0.008)</td>
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<td></td>
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<td>(0.008)</td>
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<tr>
<td>Mom x Post1975 x MaxEITC</td>
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<td></td>
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<tr>
<td></td>
<td>(0.005)</td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Mom x Post1975 x EITC Eligible</td>
<td></td>
<td>0.045**</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.011)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parent x Post1975 x Woman</td>
<td></td>
<td></td>
<td></td>
<td>0.044**</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
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</tbody>
</table>


Note: Data source: 1972-1981 March CPS. Binary dependent variable employment with mean 0.637 for the whole sample and mean 0.530 for the treatment group in 1975. CPS weights used and average marginal effects from probit regression are shown. Standard errors are computed by the delta method and are robust to heteroskedasticity and clustered at the state-year level. Each column represents a separate regression with the full set of controls from Table 2 column 5. Columns 1 to 4 also include the following sub-components of the DDD as controls: Parent x Post1975, Parent x EITC Eligible, and Post1975 x EITC Eligible. Likewise columns 5 and 6 include Parent x Post1975, Parent x Woman, and Post1975 x Woman. These additional controls ensure that the DDD model is saturated and the DDD estimate can be appropriately interpreted. The sample in columns 1 to 4 is the "high-impact" sample and married women with a spouse earning above the EITC kink point (these are the two groups of 16- to 45-year-old women in Table 3 columns 1 and 2). The samples in columns 2 to 4 are also restricted to spouses with earnings in the specified range. The sample in columns 5 and 6 includes all 16- to 45-year-old men and women, except column 6 only keep single men. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Alternate Ways to Treat Imputed CPS Observations

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<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<tr>
<td>Mom x Post1975</td>
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<td>0.039***</td>
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<td>0.047***</td>
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<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.008)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.009)</td>
</tr>
</tbody>
</table>

Observations: 550,904 541,748 550,904 550,904 550,904 550,904

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 computed by the delta method and are robust to heteroskedasticity and clustered at the state-year level. Full set of controls used from Table 2 column 5. 'High-impact' sample used, though results are similar for the sample of all 16 to 45 year old women.
Table B3: Completed Fertility May Explain Larger Response from Mothers with Multiple Kids

| Specification: | Baseline DD for 1+ Children | Add DD for 2+ Children | Add DD for 3+ Children | End Sample in 1978 to Rule Out DD for 2+ Children Approaches Zero for Mothers More Likely to Have Completed Fertility
<table>
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<tr>
<td>Variables</td>
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<td>Mom x Post1975</td>
<td>0.040***</td>
<td>0.029***</td>
<td>0.029***</td>
<td>0.013</td>
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<tr>
<td></td>
<td>(0.009)</td>
<td>(0.009)</td>
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<td>(0.010)</td>
</tr>
<tr>
<td>Mom of 2+ Kids x Post1975</td>
<td>0.018***</td>
<td>0.023***</td>
<td>0.020***</td>
<td>0.016***</td>
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<tr>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Mom of 3+ Kids x Post1975</td>
<td>-0.011**</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>556,143</td>
<td>488,063</td>
<td>281,746</td>
<td>556,143</td>
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</table>

Note: Data source: 1972-1981 March CPS. Binary dependent variable employment. CPS weights used and average marginal effects from probit regression are shown. Standard errors are computed by the delta method and are robust to heteroskedasticity and clustered at the state-year level. Each column represents a separate regression with the full set of controls from Table 2 column 5. Each sample includes all women 16 to 45 years old. Column 2 is identical to column 1 except it adds an indicator for having 2+ children and an interaction between having 2+ children and the year being after 1975. Column 3 includes these two variables as well as two more for having 3+ children. Columns 4 to 8 drop mothers in the treatment group with a youngest child below the specified age. *** p<0.01, ** p<0.05, * p<0.1.
13. Appendix C: Calculating Elasticities

13.1. Extensive-Margin Elasticities

In order to calculate the implied labor supply elasticity, I calculate the overall change in the returns to working compared to not working. The EITC is one aspect of this, but payroll and income taxes, as well as public assistance for those not working, must be accounted for. To calculate the labor supply elasticity I calculate the monetary value of working and not working for a single mother with one child with the median annual earnings in my sample, $19,160 (2013 dollars), which is close to the maximum possible EITC benefits during my sample period (see Figure 3B). I report compensated (Hicksian) elasticities.

In Table C1 I show the numbers underlying the elasticity calculation, which include the 1970 to 1985 annual values of the payroll and income taxes, dependent exemption, as well as the value of AFDC, Food Stamps, and WIC benefits. Between 1970 and 1974, the average after-tax value of working with pre-tax earnings of $19,160 was $15,779. Between 1975 and 1985 this number increased to $17,904 (see Table C1 for details). While the pecuniary return to working increase substantially over these years, various types of public assistance — and the return to not working — was fairly flat, increasing from an average of $2,639 between 1970 and 1974 to $3,144 between 1975 and 1985. The difference in the value of working with pre-tax earnings of $19,160 and not working increased from about $13,141 before 1975 to $14,760 after 1975. This represents a 12.3 percent increase in the returns to working in the years after 1975 and therefore maps to an extensive-margin elasticity of about 0.61 (=7.5/12.3) and an total elasticity of 0.78 (=9.6/12.3) based on annual earnings or 0.59 based on annual work hours.

13.2. Elasticities from Bunching of Self-Employed Workers (Figure 10A)

Following Saez (2010) and using quasi-linear and iso-elastic utility function, individuals maximize
\[ u(c, z) = c - \frac{n}{1 + \frac{1}{e}} \left( \frac{z}{n} \right)^{1+\frac{1}{e}}, \]
subject to \( c = (1 - t)z + R \). Where \( c \) is consumption, \( z \) is the level of earnings, \( t \) is the tax rate, \( n \) is an ability parameter distributed with density \( f(n) \), \( e \) is the compensated elasticity, and \( R \) is non-labor income. This leads to the first order condition:
\[ z = n(1 - t)^e. \]

Skipping the details in Saez (2010), the bunching elasticity can be estimated by solving the following for \( e \):

\[
B = \frac{z^*}{2} \left[ \left( \frac{1 - t_0}{1 - t_1} \right)^e - 1 \right] \left[ h(z^*_-) + \frac{h(z^*_{+})}{\left( \frac{1 - t_0}{1 - t_1} \right)^e} \right] \quad (C1)
\]
Where $z^*$ is the kink threshold, $\left(\frac{1-t_0}{1-t_1}\right)$ is the net of tax ratio at the kink, $h(z^*)_-$ and $h(z^*)_+$ is the density of the distribution just below and above the kink, and $B$ is the amount of bunching at $z^*$. For a given empirical distribution $h(z)$ and a choice of bandwidth $\delta$, $B$ is equal to the density of taxfilers with income is the range $(z^*-\delta, z^*+\delta) - (z^*-2\delta, z^*-\delta) - (z^*+\delta, z^*+2\delta)$. See Saez (2010) Figure 2 for more details and intuition.

I use this formula, the empirical earnings distribution in the SOI tax files for 1975-1978 and 1979-1984 (see Figure 10A for nominal EITC schedules), and bandwidths of $1000$, $1500$, and $2000$ to calculate the implied bunching elasticity.

For 1975-1978 and $\delta=$$1000$: $z^*=$$4000$, $\frac{1-t_0}{1-t_1} = 1.2$, $B = .0114$, $h(z^*)_-$ = 0.0000582, and $h(z^*)_+ = 0.0000788$. Yielding $e=0.23$.

For 1975-1978 and $\delta=$$1500$: $z^*=$$4000$, $\frac{1-t_0}{1-t_1} = 1.2$, $B = .0173$, $h(z^*)_-$ = 0.0000388, and $h(z^*)_+ = 0.0000525$. Yielding $e=0.52$.

For 1975-1978 and $\delta=$$2000$: $z^*=$$4000$, $\frac{1-t_0}{1-t_1} = 1.2$, $B = .0191$, $h(z^*)_-$ = 0.0000291, and $h(z^*)_+ = 0.0000394$. Yielding $e=0.77$.

For 1979-1984 and $\delta=$$1000$: $z^*=$$5000$, $\frac{1-t_0}{1-t_1} = 1.1$, $B = .0204$, $h(z^*)_-$ = 0.0000642, and $h(z^*)_+ = 0.0000838$. Yielding $e=0.58$.

For 1979-1984 and $\delta=$$1500$: $z^*=$$5000$, $\frac{1-t_0}{1-t_1} = 1.1$, $B = .0299$, $h(z^*)_-$ = 0.0000428, and $h(z^*)_+ = 0.0000559$. Yielding $e=1.28$.

For 1979-1984 and $\delta=$$2000$: $z^*=$$5000$, $\frac{1-t_0}{1-t_1} = 1.1$, $B = .0388$, $h(z^*)_-$ = 0.0000321, and $h(z^*)_+ = 0.0000419$. Yielding $e=2.22$.

Saez (2010) finds elasticities among self-employed workers in the range of 0.7 to 1.6, depending on bandwidth choice.
### Table C1: Calculating Labor Supply Elasticity for a Representative Unmarried Mother Earning $19,160 (2013 Dollars)

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<td>2570</td>
<td>4.8</td>
<td>123</td>
<td>0</td>
<td>360</td>
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<td>2712</td>
<td>127</td>
<td>0</td>
<td>552</td>
<td>679</td>
<td>2303</td>
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<td>1972</td>
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<td>5.2</td>
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<td>516</td>
<td>2922</td>
<td>162</td>
<td>0</td>
<td>612</td>
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<td>2149</td>
<td>11972</td>
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<td>1973</td>
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<td>0</td>
<td>407</td>
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<td>205</td>
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<td>7475</td>
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Return to Work (1970-1974 Average) = 11,784
Absolute Increase in Return to Work (Post1975-Pre1975) = 1,240
Percent Increase in Return to Work (Post1975-Pre1975) = 10.5

Implied extensive margin elasticity (% change in employment / % change in return to working = 7.5/10.5) = 0.71
Implied extensive margin elasticity (% change in labor force participation / % change in return to working = 6.3/10.5) = 0.60
Implied intensive + extensive margin elasticity from annual earnings (% change in labor supply / % change in return to working = 9.6/10.5) = 0.91
Implied extensive + extensive margin elasticity from annual work hours (% change in labor supply / % change in return to working = 7.3/10.5) = 0.69

Notes: The increase in the return to working largely reflects the 1975 EITC as well as the Revenue Act of 1978 which lowered taxes on low-income earners. Although this tax cut likely increased labor supply, the difference-in-differences empirical strategy in this paper nets out the effect of the Revenue Act of 1978 since this tax cut applied to both women with and without children. The increase in public assistance (AFDC, WIC, Food Stamps) in the mid-1970s likely dulled the employment effect of the 1975 EITC, pushing the estimates in this paper towards zero.
14. **Appendix D: Did the EITC Affect Attitudes? Less Parametric Approaches**

Results in Figure 12 show that each percentage point increase in EITC response led to a 1.7 percentage point increase in positive state gender-equality attitudes. However, if this relationship is not linear — such as with decreasing marginal treatment effects — an OLS specification could be a poor approximation of the true relationship between EITC response and changes in attitudes.

One way to test this is to divide up EITC response into a number of categories and regress changes in attitudes on each of these binary categories simultaneously. Results in Figure D1 show estimates from a regression resembling equation (4), but with four binary variables instead of the continuous variable $EITCResponse_s$. The excluded group represents states with an EITC response less than 0.9 percentage points and the other four categories span values between 0.9 and 10.0 percentage points. Figure D1 shows that state EITC response has an increasingly positive effect on gender-equality attitudes and roughly approximates the predicted effect from a linear OLS specification. This semi-parametric approach shows that OLS closely approximates the effect of the EITC on gender-equality attitudes.

A second approach is to use locally weighted regression (Cleveland, 1979). Figure D2 shows that when the regression behind Figure 12 is locally weighted, the slope is steepest between about 0 to 2, and 6 to 10 percentage points, and fairly flat between 2 and 6 percentage points. Since the scatterplot only consists of 32 states — and many of these states are based on relatively few GSS observations — it is unclear what conclusions can be drawn from this pattern. Perhaps the relationship is approximately linear. Perhaps the influx of working mothers actually had an increasing, then stable, then increasing again, effect on attitudes.

15. **Figures for Appendix D**
Fig. D1. Comparing Categorical EITC Response with Constant, Linear Effect

Notes: Results come from a single regression resembling equation (4) except that $\text{EITCResponse}_s$ is replaced with four binary variables for having an EITC response between 0.9 and 2.5, 2.5 and 7.4, 7.4 and 8.2, or 8.2 and 10. These four variables are assigned values on the x-axis of 2.5, 5, 7.5, 10. Intervals chosen to roughly balance the statistical power of each category. Sample sizes of each group (including the omitted group with $\text{EITCResponse}_s$ between 0 and 0.9 percentage points) are 5, 7, 15, 2, 3.

Fig. D2. Locally Weighted Regression

Notes: Locally weighted regression (Cleveland, 1979) showing the effect of state EITC response on changes in attitudes. Stata command `lowess` used and default setting: running-line least squares, tricube weighting function, bandwidth of 0.8.
16. Appendix E: Data Appendix

The following information is intended to be detailed enough to replicate my sample.


I use 1971 to 1986 March CPS (Ruggles et al., 2015) downloaded in December 2014 (2,461,704 observations). I replace year with year-1 to match the survey year with the work year. I define EITC-eligible households as having at least one child 18 or under, or an adult child between 19 and 23 and in school full time. Households are defined as unique combinations of variables year and serial. I then drop individuals under 16 (668,453 observations) and the 442 observations with a CPS weight (wtsupp) of 0, leaving me with 1,792,809 observations. Husbands defined as married males. 1,093,714 households have 1 married male, 7,579 have two, 121 have three, and 2 households have four husbands. Each sub-family within a household is assumed to be a separate tax-filing family unit. After dropping women with missing spousal earnings (15,126 observations), dropping males (837,755), and dropping women over 45 (388,951 observations), I am left with 550,904 women aged 16 to 45, which is the sample used in the main analysis and matches the number of observations in tables using the full sample of women.

The following is a discussion of variables used in employment analysis. Missing incwage values of 99999 assigned to be 0 for 574 observations. 8 observations with missing education dropped as are 65 observations with missing state. (Numbers based on sample of 1,792,809 observations.) Weeks worked assigned as the midpoint of the categorical variable wkswork. Post1975 begins in 1976. Welfare comes from incwelfr, married defined as marstat equals 1 or 2, and nonwhite created from race and hispan. Age is rounded to bins of two so that birth year, year, and age can all be controlled for; age squared and cubed based on actual age though. Spousal earnings created from incwage and matching a male husband to a female wife; single women assigned zero spousal earnings. States are not identified individually until (working year) 1976. For consistent “states” over time I define 21 “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, and MI-WI. National unemployment rates comes from BLS: http://www.bls.gov/cps/cpsaat01.htm. State-year employment to population ratios created from state-year measures of total employment (found here: http://www.bea.gov/regional/downloadzip.cfm) under “Local area personal income accounts” file CA25, row 2 in each state file) and state-year measures of population (found at same link under “Local area personal income accounts” file CA25, row 3 in each state file).
This data source begins in 1969. Dollars adjusted to real dollars (when specified) using the Consumer Price Index. Occupations detailed in Figure 5 notes.

16.2. **IRS Statistics of Income Public Use Files**

Analysis behind Figures 10A and 10B and bunching elasticities calculated in Appendix C use 1975 to 1984 SOI data. Sample restricted to taxfilers with positive wages and salaries (data11) or positive schedule C business net income (data17). EITC-eligible children determined by data106, children at home. In 1976 this variable was not available and I instead use data8 for number of total dependents.

Analysis behind Figure A5 use 1976 to 1985 SOI data. EITC-eligible taxfilers defined those with wage earnings or business schedule C income, with a child dependent, and with income below the EITC income limit. Child Care Tax Credits given by SOI variable data64.

Section 11.7 uses 1968 to 1985 SOI data. Marital status given by SOI variable data2. Number of taxfilers in Figure B4 determined from SOI weight data1.

16.3. **General Social Survey Data**

I use restricted GSS data with state-level identifiers. Gender-equality attitudes defined from GSS variable fework and racial-equality attitudes from racpres. Log income from conrinc and is in 1000s. Democrat defined as partyid values between 0 and 2, religious defined as reliten values of 1 or 3, too much welfare defined from natfare, mom worked and mom education defined from mawk16 and maedyrs.

In each regression, \(N=32\) since I drop one outlier (West Virginia) that has an EITC response of -10 percentage points and GSS only surveyed 33 states before 1975. Not dropping the outlier has almost no effect on the results. To be consistent over time, I only keep the states that have observations in each year.

Figure 11 includes adults of all ages (18+) and pools men and women. All other GSS analysis is restricted to adults ages 18-60. This cutoff does not have much of an effect on the results, however when the age cutoff is lowered sufficiently, the sample size and power shrinks, and results become less statistically significant (e.g. age 30 cutoff). Unless otherwise specified (e.g. Table 3 Panel B), all analysis is also restricted to only the attitudes of men.

Results define the post-1975 period through 1985 and include years 1977, 1978, 1982, 1983, and 1985. The other questions do not have the outcome variable of interest. Results are similar if 1985 (or if 1983 and 1985) is excluded. As would be expected from the employment trends in Figures 1A and 1B, the effect on attitudes is larger if 1977 is excluded from the post-1975 period.
16.4. Gallup Data

Data obtained from Roper Center (http://ropercenter.cornell.edu/CFIDE/cf/action/ipoll/index.cfm) and Berinsky and Schickler (2011). The following Gallup datasets and survey questions were used for analysis in Figure 18. Gallup (1937a,b,c): “Are you in favor of permitting women to serve as jurors in this state?” Gallup (1937c): “Would you vote for a woman for President if she qualified in every other respect?” Gallup (1938): “Do you approve of a married woman earning money in business or industry if she has a husband capable of supporting her?” Gallup (1939): “A bill was introduced in the Illinois State Legislature prohibiting married women from working in business or industry if their husbands earn more than $1,600 a year ($133 a month). Would you favor such a law in this state?” Gallup (1945): “If the party you most often support nominated a woman for Governor of this state, would you vote for her if she seemed qualified for the job?” , “If the party whose candidate you most often support nominated a woman for President of the United States, would you vote for her if she seemed best qualified for the job?”, “Would you approve or disapprove of having a capable woman in the President’s cabinet?”, “A woman leader says not enough of the capable women are holding important jobs in the United States government. Do you agree or disagree with this?”, “Would you approve or disapprove of having a capable woman on the Supreme Court?”

Please contact the author (jacobbas@umich.edu) with any data questions.