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

By Jacob Bastian*

The rise of working mothers radically changed the US economy and the role of women in society. In one of the first studies of the 1975 introduction of the Earned Income Tax Credit, I find that this program increased maternal employment by 6 percent, representing 1 million mothers and an elasticity of 0.58. The EITC may help explain why the US has long had such a high fraction of working mothers despite few childcare subsidies or parental leave policies. I also find suggestive evidence that this influx of working mothers affected social attitudes and led to higher approval of working women. (*JEL* H24, J16, J22, J31, K34, Z13)

A surprising difference between the United States 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 United States, compared to 25 to 45 percent in other developed countries (OECD 2007).[1] The United States was not always an outlier in this regard: the number of working mothers in recent decades is also high by US historical standards (Goldin 1990, Costa 2000)[2] and is puzzling since few childcare subsidies or family-friendly work policies (e.g., paid parental leave) exist in the United States (Ruhm 1998). This paper finds that the 1975 introduction of

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[1] Cross-country comparisons of working mothers are not straightforward: many countries count mothers on paid parental leave as employed (OECD 2007). The 2003 employment rates of mothers with kids under 3 in Austria, Finland, and Sweden were 80.1, 52.1, and 72.9 percent, but excluding mothers on paid parental leave yields lower rates of 40.1, 33.8, and 45.1 percent (OECD 2007, 57).

[2] Only 20 percent of married women with infants worked in 1973, compared to 62 percent in 2000 (Goldin 2006).

the Earned Income Tax Credit (EITC) may help explain this puzzle. Not only do I find that the EITC played an important role in the rise of working mothers but also that this program led to more positive social attitudes toward working women.

Time-series evidence shows that the relative employment of mothers—compared to women without children—rapidly increased after 1975 (Figure 1, panel A and Figure 2, panel A). Between 1975 and 1980, the relative employment of mothers rose by about 5 percentage points, closing the employment gap between these 2 groups by 20 percent. Using March Current Population Survey data and a dynamic difference-in-difference (DD) approach, I show that much of the 1975–1980 increase in the *relative* employment of mothers can be attributed to the 1975 EITC. Interestingly, the unadjusted trend in maternal employment is quite similar to the regression-adjusted trend that controls for a rich set of individual- and state-by-year-level covariates (Figure 1, panel B and Figure 2, panel B). The EITC also increased labor force attachment and work intensity, raising average annual work hours by 5.7 percent (35 hours) and earnings by 7.3 percent ($750 in 2013 dollars). Results imply a participation elasticity of 0.58, in line with other estimates of this period (Blau and Kahn 2005, Heim 2007, Chetty et al. 2012).

Consistent with the 1975 EITC causing this rise in employment, I find larger responses from mothers more likely to be EITC eligible and null responses from placebo groups of women and mothers not eligible for EITC benefits. Responses varied by marital status, spousal earnings, and education in a manner consistent with a simple labor-supply model. I use the placebo group of EITC-ineligible mothers in a triple differences (DDD) specification to net out contemporaneous policies and trends (e.g., birth control, divorce, abortion laws) affecting all mothers: the DDD estimate corroborates the DD result (2.6 and 3.3 percentage points).[3]

My estimates suggest that the 1975 EITC encouraged about one million mothers to begin working by 1980. Yet this is unlikely to capture the full impact of the EITC on society. In Section VI, I use General Social Survey (GSS) data to examine whether this influx of working mothers affected social attitudes toward working women (“gender-equality preferences”). This hypothesis is motivated by evidence that such attitudes are malleable and increase with exposure to working women: Fernández, Fogli, and Olivetti (2004) and Olivetti, Patacchini, and Zenou (2016) find that having a working mother—and having friends with working mothers—leads to stronger gender-equality preferences in adulthood. Additionally, Finseraas et al. (2016) shows that exposure to female colleagues reduces discriminatory attitudes. With these results in mind, the attitudes of millions of Americans may have been affected when a million mothers began working after 1975.[4]

To estimate the impact of the EITC on gender-equality preferences, I use a two-sample, two-step process in which I characterize and exploit geographic heterogeneity in the EITC response and test whether states with larger EITC responses experienced larger attitude changes after 1975. Using both the *actual* state EITC

[3] Online Appendix Figures A.5 and A.6 show no apparent effect on maternal employment among states that legalized abortion and no-fault divorce before other states.

[4] Google ngrams (Michel et al. 2011) provide descriptive evidence that the rise of working mothers was salient and that references to working mothers became much more common after the mid-1970s (Figure 3).

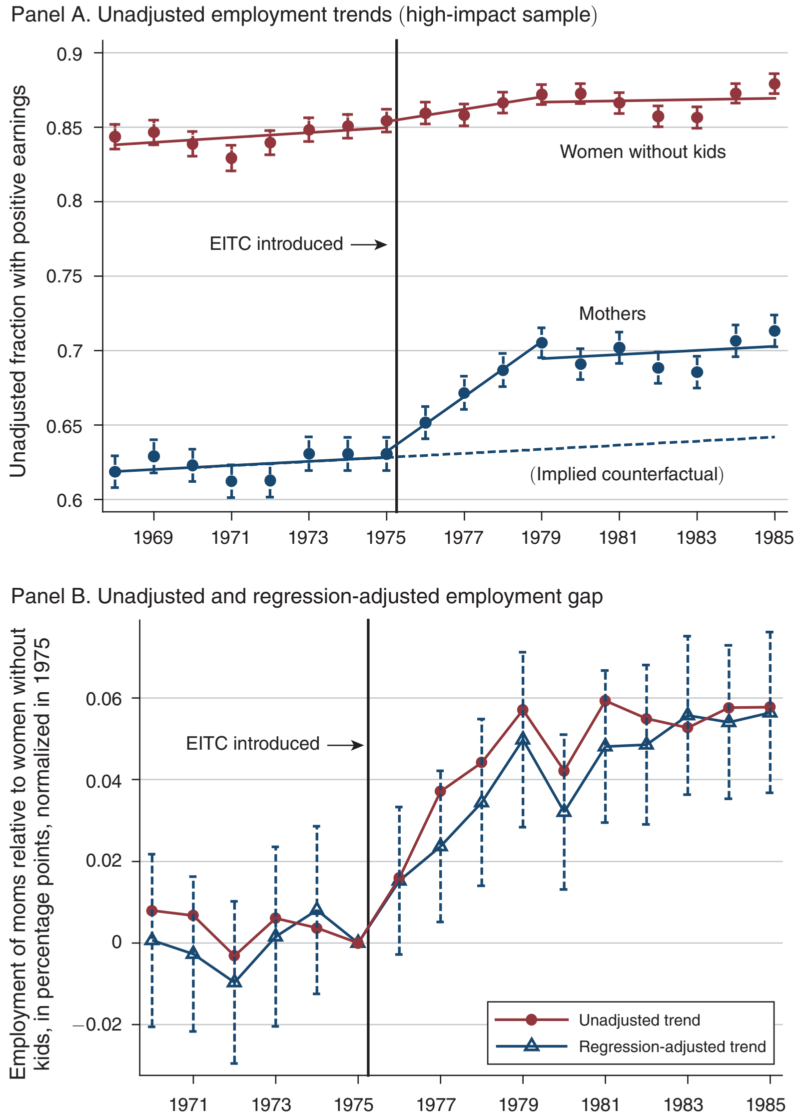

FIGURE 1

Notes: Employment is defined as positive income. Best fit lines are shown for 1969–1975, 1975–1979, 1979–1985. Regression-adjusted employment gap estimates are from a logit, and the full set of controls are from Table 2, column 4. The estimates are jointly statistically insignificant for all years before 1975 (p-value 0.42). "High-impact" sample includes all women 18–50 and excludes married women with spousal earnings above \$36,000 in 2013 dollars (corresponding to the 1975 EITC income limit), full-time students, disabled women, and retired women. Kids are 0–18 years old or 19–23 if a student. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.

Source: 1969–1986 March CPS data

response and the *predicted* response (based on preexisting state demographic traits to help alleviate concerns about the potential endogeneity of gender-equality preferences and EITC response), I find that states with larger EITC responses had larger increases in preferences for gender equality after 1975. Preference changes occurred

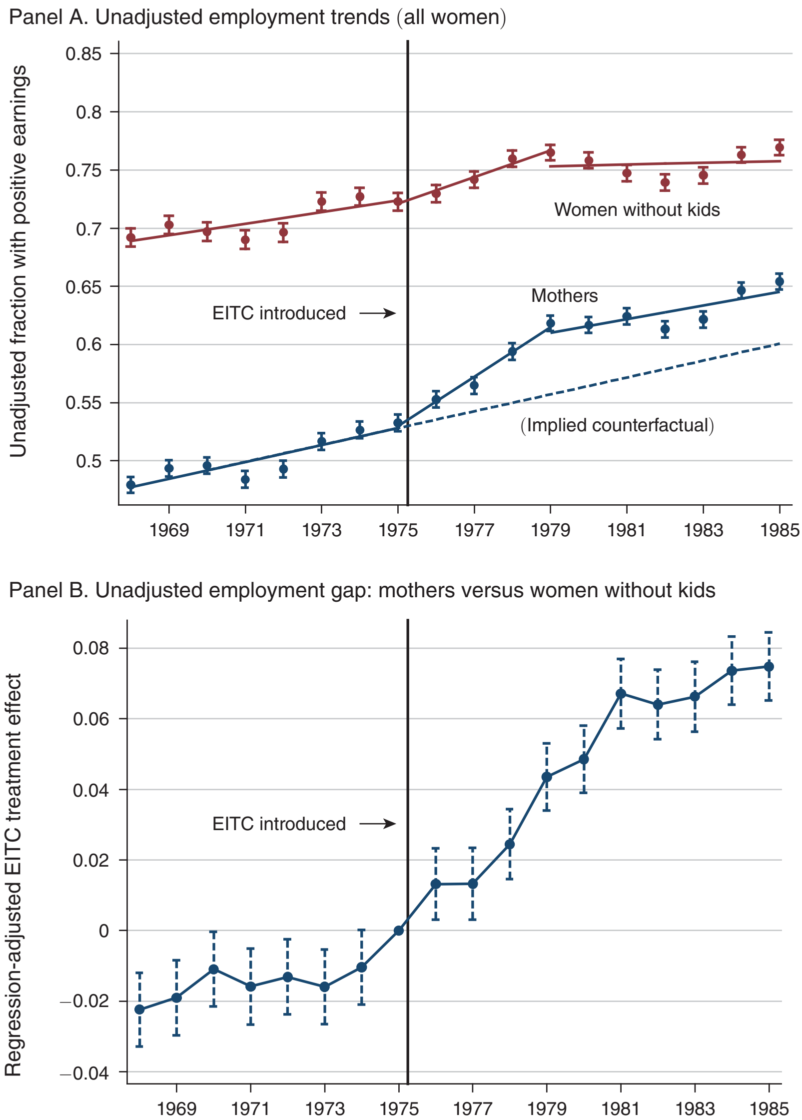

FIGURE 2

Notes: Sample includes all women 18–50. Figures are compiled in a manner similar to panels A and B of Figure 1. Best fit lines are shown for 1969–1975, 1975–1979, and 1979–1985. The estimates are jointly statistically insignificant for all years before 1975 (p-value 0.38). The relative rise in maternal employment after 1981 appears to reflect a decline in employment among women without kids.

among both men and women, within and across regions, and do not appear to be driven by preexisting attitudes, demographics, or general trends in social norms. Subgroup analysis confirms larger preference changes among people more likely to know these newly working women: younger and lower-educated adults. I also use a placebo outcome on racial-equality preferences to test and rule out the possibility that states with higher EITC responses were simply experiencing changes in various types of social attitudes.

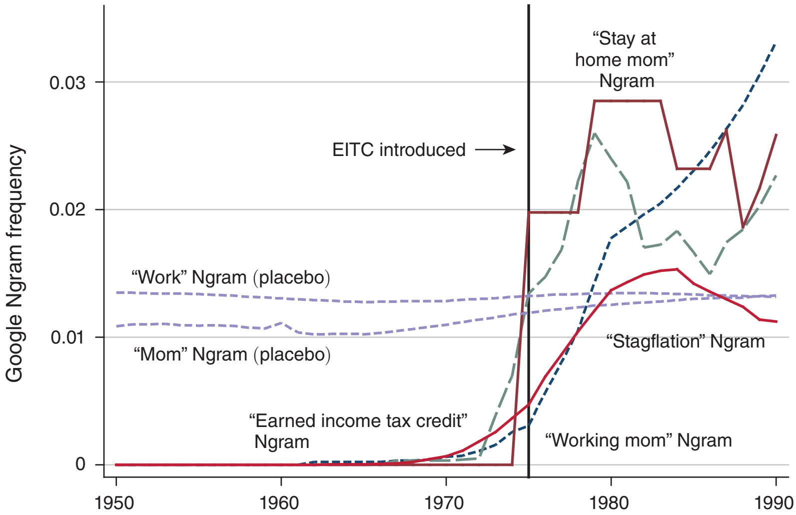

Figure 3. Rise of Working Mothers Was Salient: Evidence from Ngrams

Notes: Google Books Ngram Viewer charts annual frequencies of search strings found in over 5 million sources—and over 500 billion words—or about a 4 percent sample of all possible books and sources (Michel et al. 2011). The vertical axis measures the relative frequency with which each phrase is used in sources printed between 1950 and 1990. More details and links to data are in online Appendix F.

In one of the first studies of the 1975 introduction of the EITC, I find that the EITC encouraged a million mothers to begin working and affected the social attitudes of millions of Americans.

I. EITC History and Known Effects of the EITC

The EITC came to exist partly as a response to the 1960s War on Poverty, which succeeded in improving health (Almond, Hoynes, and Schanzenbach 2011; Goodman-Bacon 2013; Bailey and Goodman-Bacon 2015) and decreasing poverty but also had unintentional work disincentives (Moffitt 1992, Hoynes 1996, Hoynes and Schanzenbach 2012). Welfare dependency came to be seen as a growing social problem, and momentum built for a guaranteed annual income with support from well-known economists (Friedman 1962, Tobin 1969). In 1970, the US House of Representatives passed such a plan—the Family Assistance Plan—that had the backing of President Richard Nixon and would have replaced welfare. However, the US Senate never passed the plan because of disagreement about how generous the program should be and concerns about potential work disincentives. An alternative program called the Work Bonus Plan—with work requirements—was introduced by Senator Russell Long in 1972. A version of this bill was eventually passed as the EITC and signed into law by President Gerald Ford on March 29, 1975. See Liebman (1998) and Ventry (2000) for a detailed history of the EITC.

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 ($4,000 nominal dollars). The EITC was available to parents with earnings

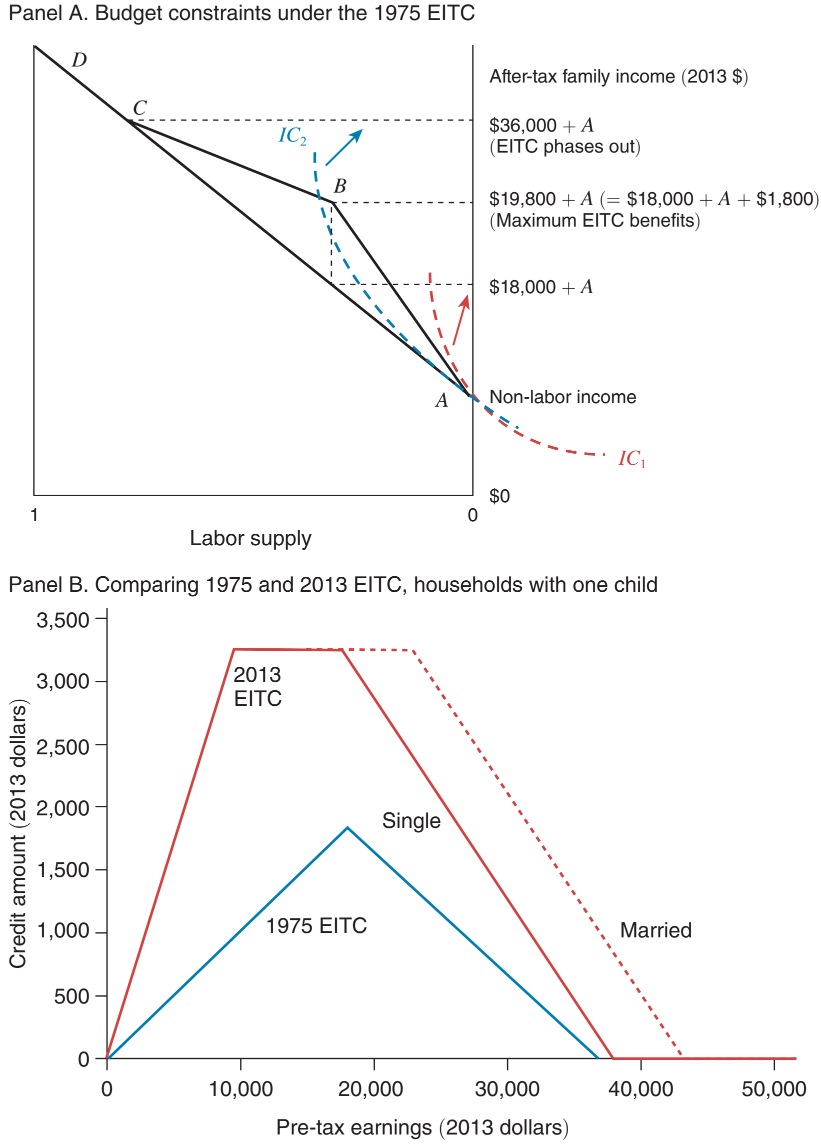

Figure 4

Notes: Author's calculation from 1975 and 2013 EITC parameters. The 1975 EITC phased in and out at 10 percent. EITC benefits phase out with adjusted gross income. The 2013 EITC for 1 child phased in and out at 34 and 15.98 percent.

above $18,000, but benefits decreased at a rate of 10 percent and reached 0 for earnings above $36,000 (Figure 4, panel A and panel B).[5] At this time, there were no

[5] Panel A of Figure 4 shows a budget constraint under the EITC, and panel B of Figure 4 illustrates the "phase in" and "phaseout" portion of the EITC schedule while contrasting the 1975 EITC schedule with the 2013 EITC. Benefits phase out with adjusted gross income.

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 (see online Appendix C for details) and has grown into one of the largest antipoverty programs in the United States, redistributing $66 billion to 28 million individuals and lifting 6.2 million people—including 3.2 million children—out of poverty in 2013 (Center on Budget and Policy Priorities 2015). The EITC has raised maternal employment (Dickert, Houser, and Scholz 1995; Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Hotz and Scholz 2006; Eissa, Kleven, and Kreiner 2008); increased earnings (Dahl, DeLeire, and Schwabish 2009); improved health (Evans and Garthwaite 2014); decreased poverty (Neumark and Wascher 2001, Meyer 2010, Hoynes and Patel 2015); and helped children of EITC recipients by improving health (Hoynes, Miller, and Simon 2015; Averett and Wang 2015), test scores (Chetty, Friedman, and Rockoff 2011; Dahl and Lochner 2012), and longer-run outcomes like educational attainment (Manoli and Turner 2014, Bastian and Michelmore 2018) and employment and earnings (Bastian and Michelmore 2018). See Nichols and Rothstein (2015) and Hoynes and Rothstein (2016) for recent EITC literature reviews.

Although much is known about the EITC, almost nothing is known about the 1975 introduction or how the EITC affects attitudes toward working women. I show that the 1975 EITC encouraged one million mothers to begin working, which subsequently increased approval of working women.

Almost all studies of the EITC ignore the program's first decade. Although there was little policy variation before 1986, the 1975 introduction was itself a large policy change that has received surprisingly little attention due to the common misconception that the original EITC was too small to have had much of an effect.[6] However, online Appendix Figure A.1 shows that the 1975 EITC was large in at least three ways: first, almost half of all households had earnings below the EITC income limit; second, benefits were quite high, up to $1,800 in 2013 dollars; and third, a 10 percent earnings subsidy represented a substantial increase in potential earnings. Other reasons to expect a large impact are that female labor supply was more elastic during this period than in later decades (Blau and Kahn 2005, Heim 2007), and the fraction of mothers on the margin of working declined with subsequent program expansions (Björklund and Moffitt 1987, Heckman and Vytlacil 1999).

[6] As seen in the following representative quotes, "Between it's beginning in 1975 and [the] Tax Reform Act of 1986, the EITC was small, and the credit amounts did not keep up with inflation" (Meyer and Rosenbaum 2001, 1073). "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, 607).

II. Empirical Strategy and CPS Data

To estimate the EITC's effect on maternal employment, I first use equation (1) and DD; I compare the employment rates of women with and without kids, before and after 1975:[7]

$$(1) \qquad P(E_{ist}) = f(\beta_1 Mom_{ist} + \beta_2 Mom \times Post1975_{ist} + \beta_3 X_{ist} + \delta_{st} + \epsilon_{ist}).$$

The variable E_{ist} is binary for whether a woman is employed.[8] The terms *Mom* and *Post*1975 denote whether a woman is a mother and if the year is after 1975; $Mom \times Post1975$ is the DD variable of interest. The EITC treatment effect β_2 should be positive since the EITC subsidized work; X_{ist} are controls that vary at the individual, state, and year level; δ_{st} contain state and year fixed effects to control for national trends and state-specific traits associated with female employment; and ϵ_{ist} is an error term. Coefficients are measured in percentage points. Average marginal effects from a logit model are reported throughout (unless otherwise stated). Standard errors are robust to heteroskedasticity and clustered at the state level.

I estimate equation (1) using 1971 to 1986 March CPS data (Ruggles et al. 2015) and the sample of all 18- to 50-year-old women. The treatment group consists of mothers,[9] and the control group consists of women without children. Table 1 shows summary statistics for all 571,170 women in column 1, while columns 2 and 3 split the sample into treatment and control groups, and columns 4 and 5 split the sample by marital status. Women in the sample average 32 years old with 12.3 years of education, 12 and 6 percent are black and Hispanic, 66 percent work, average individual annual earnings are \$14,158 (\$21,418 conditional on working), average household earnings are \$45,822 (in 2013 dollars), and 41 percent have household earnings below the EITC limit. Mothers are older, less likely to be white, less likely to work, and have less education and higher household earnings. Married women are older, have more children, are less likely to work, and have higher household earnings. See online Appendix F for data and sample details.

Testing for Parallel Pretrends.—Panel A of Figure 1 and panel A of Figure 2 show unadjusted 1970 to 1985 employment trends for women with and without kids and preview the regression-adjusted results. From 1970 to 1975, the employment gap between mothers and women without kids was stable at 24 percentage

[7] Women with and without children are different in many ways (see Table 1). Although comparing women with and without children is not as ideal as comparing mothers with, say, one versus two children, this DD approach has also been used by Eissa and Liebman (1996) and Meyer and Rosenbaum (2001). Parallel trends are shown in the Section III.

[8] I focus on employment since this is where most EITC benefits are and since this margin generally manifests greater responsiveness to wage variation than work hours (Heckman 1993). I assume working mothers did not displace nonmothers (Neumark and Wascher 2011). However, even if an increase in working mothers led to declines in earnings (Leigh 2010, Rothstein 2010), this apparently did not lead to a general equilibrium effect where the employment of nonmothers decreased (see Figure 1, panel A and Figure 2, panel A).

[9] To match the definition of EITC-eligible children, I define mothers as having at least one child 18 or under or having a child between 19 and 23 who is in school full-time.

Table 1—Summary Statistics

Variable	All women (1)	Mothers (2)	Women without kids (3)	Married (4)	Not married (5)
Age	32.1	34.7	28.2	34.3	28.2
	(9.4)	(8.1)	(10.0)	(8.7)	(9.4)
Years of education	12.3	12.1	12.7	12.3	12.3
	(2.6)	(2.5)	(2.6)	(2.5)	(2.6)
Married	0.64	0.82	0.36	1.00	0.00
	(0.48)	(0.38)	(0.48)	(0.00)	(0.00)
Black	0.12	0.12	0.12	0.07	0.20
	(0.32)	(0.32)	(0.33)	(0.26)	(0.40)
Hispanic	0.06	0.07	0.05	0.06	0.06
	(0.24)	(0.25)	(0.22)	(0.23)	(0.24)
Any kids under five	0.24	0.39	0.00	0.32	0.09
	(0.43)	(0.49)	(0.00)	(0.47)	(0.29)
Number of kids	1.35	2.22	0.02	1.76	0.63
	(1.45)	(1.24)	(0.14)	(1.43)	(1.19)
Employed	0.66	0.58	0.79	0.60	0.77
	(0.47)	(0.49)	(0.41)	(0.49)	(0.42)
Individual earnings (2013 $)	$14,158	$11,854	$17,701	$12,936	$16,341
	(17,573)	(16,473)	(18,592)	(17,061)	(18,249)
Individual earnings (2013 $) (Conditional on earnings > 0)	$21,418	$20,609	$22,322	$21,500	$21,304
	(17,653)	(17,069)	(18,241)	(17,310)	(18,124)
Household earnings (2013 $)	$45,822	$54,177	$32,969	$62,320	$16,341
	(40,612)	(41,378)	(35,779)	(40,341)	(18,249)
Household earnings (2013 $) (Conditional on earnings > 0)	$52,312	$60,963	$38,501	$66,519	$21,304
	(39,287)	(38,896)	(35,804)	(38,181)	(18,124)
Household earnings below EITC limit	0.41	0.30	0.57	0.21	0.76
	(0.49)	(0.46)	(0.50)	(0.41)	(0.43)
Household earnings below EITC limit (Conditional on earnings > 0)	0.28	0.19	0.42	0.14	0.53
	(0.45)	(0.39)	(0.49)	(0.35)	(0.50)
Annual weeks worked	27.4	24.0	32.7	25.5	30.8
	(22.5)	(22.8)	(21.1)	(22.8)	(21.7)
Annual weeks worked (Conditional on weeks worked > 0)	39.5	38.8	40.2	39.4	39.5
	(16.0)	(16.2)	(15.7)	(15.9)	(16.2)
Weekly hours worked	19.3	16.5	23.7	17.5	22.5
	(19.7)	(19.2)	(19.7)	(19.3)	(19.9)
Weekly hours worked (Conditional on hours worked > 0)	34.9	34.0	36.0	34.3	35.8
	(12.5)	(12.7)	(12.0)	(12.5)	(12.3)
Observations	571,170	350,798	220,372	370,767	200,403

Notes: Individual March CPS weights used. The sample contains all women 18 to 50 years old. Standard deviations are in parentheses. A total of 376,919 observations have positive earnings, 500,471 have positive household earnings, 397,210 have positive weeks worked last year, and 315,902 have positive hours worked last week.

Source: 1971–1986 March CPS data

points. Between 1975 and 1979, the relative employment of mothers increased, and the gap narrowed to 18 percentage points, where it remained from 1979 to 1985 (Figure 1, panel B). Although employment levels differed for these groups, employment trends were parallel before 1975 (p-values 0.42 and 0.38 for Figure 1, panel B and Figure 2, panel B).

Table 2—The 1975 EITC Increased Maternal Employment, Robust to Various Sets of Controls

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mom × Post1975	0.048	0.049	0.040	0.033	0.034	0.042	0.035
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Controls							
Mom, Post1975	X	X	X	X	X	X	X
State and year fixed effects		X	X	X	X	X	X
Demographic controls			X	X	X	X	X
Unemployment rate				X	X	X	X
"Kitchen-sink" controls							X
Observations	571,170	571,170	571,170	571,170	571,170	571,170	571,170
Model	Logit	Logit	Logit	Logit	Probit	OLS	OLS
R^2						0.146	0.164
Mean dependent variable across years and across treatment and control groups = 0.66							
Mean dependent variable for treatment group in 1975 = 0.53							

Notes: Sample includes all women 18 to 50 years old. The dependent variable is binary employment, defined as having positive earnings. CPS weights and equation (1) are used, and average marginal effects from logit, probit, or OLS regression are shown. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level. Demographic controls include married, welfare income, number of children, any children under five, age cubic, birth cohort, years of education quadratic, nonwhite-mom, nonwhite-post1975, age-mom, and married-post1975. Unemployment rate includes state-year employment-to-population ratios and interactions with kid and married. "Kitchen-sink" controls include unemployment rate-age, nonwhite-welfare, nonwhite-married, number children-married, child less than five-married, married-welfare income, education years-married, education-child less than five, education-nonwhite, a nonwhite-age cubic, unemployment rate-nonwhite; fixed effects for nonwhite-year, married-year, nonwhite-state, birth-year, state-year, state-married, state-child less than five, state-year-nonwhite, and state-year-married; and annual inflation and state-year manufacturing employment interacted with low education (<12 years), having children, and married.

Source: 1971–1986 March CPS data

III. The EITC and Extensive Margin Labor Supply

A. *Average Treatment Effects*

I estimate the average effect of the EITC on maternal employment using equation (1) and March CPS weights, and adding controls cumulatively across columns in Table 2. Column 1 controls for whether each observation is a mother (*Mom*),[10] whether the observation occurs after 1975 (*Post*1975), and the DD variable of interest (*Mom* × *Post*1975). Column 2 adds state and year fixed effects to account for idiosyncratic state traits and annual shocks affecting all women. Since the CPS did not uniquely identify all states before 1977, I merge states into the 21 smallest possible geographical units to provide a balanced panel (details in online Appendix F).[11] Column 3 adds demographic controls to account for demographic-led increases in maternal employment and to account for the fact that mothers are on average older, from a different birth cohort, have less education, and are more likely to be married or nonwhite (Table 1). Column 4 adds state-by-year unemployment rates (interacted

[10] Restricting *Mom* to those with a child born before 1975 avoids potential fertility responses to the EITC but affects the composition of mothers over time. This approach yields a similar DD: 0.030 (0.006).

[11] So few clusters may bias the standard errors (Angrist and Pischke 2009; Cameron, Gelbach, and Miller 2008, 2011). Block bootstrap yields similar standard errors. Clustering at the year-by-(mother/nonmother) level also yields statistically significant estimates with slightly larger standard errors of 0.0158.

with marital status and having kids) to control for economic conditions (US Bureau of Labor Statistics 2019, US Bureau of Economic Analysis 2016). Columns 5 and 6 show that results in column 4 are robust to using probit or OLS. Finally, column 7 adds a "kitchen-sink" set of controls that interacts each control (along with annual inflation and state-by-year manufacturing employment) with year, state, marital status, having kids, and race.[12] These interactions flexibly account for the impact of economic conditions, changing demographics, and general trends on employment.

Across each set of controls, the DD estimate is stable between 3.3 and 4.9 percentage points (or 6.2 and 9.2 percent from a baseline of 53 percent)[13] and significant at the 99 percent level. Results imply that about one million mothers began working because of the 1975 EITC.[14] The EITC is responsible for about a quarter 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. I use a logit model and the set of controls in column 4 throughout the rest of the analysis (unless otherwise specified).[15] Results are robust to alternate binary definitions of working based on earnings, weeks worked, or labor force participation (online Appendix Table A.2); using alternate age cutoffs (online Appendix Table A.3); not using CPS weights (estimate is 0.030 [0.007]); and additional robustness checks (online Appendix B).[16]

B. *Heterogeneous and Subgroup Treatment Effects*

If the EITC led to this increase in working mothers, then this effect should have varied by the likelihood of receiving EITC benefits. In Table 3, I test whether the treatment effect varied in a way consistent with the EITC causing this rise in maternal employment.

Heterogeneous Treatment Effects: Marital Status.—There are two main reasons why married mothers should respond less to the EITC than unmarried mothers. First, since EITC benefits are determined by household earnings, spousal earnings could push the household out of EITC eligibility (point C in Figure 4, panel A). Second, spousal earnings increase the likelihood that the highest feasible indifference curve

[12] In online Appendix Table A.1, I show various intermediate sets of controls between columns 6 and 7, including without welfare (which is endogenous with working).

[13] Results are intent-to-treat effects: about 20 percent of households are EITC eligible and do not claim the EITC or are EITC-ineligible families and do (Scholz 1994). Liebman (1997, 2000) find that 89 and 95 percent of women allocated to the treatment and control groups filed taxes appropriately in the 1980s. Random misallocation implies that the estimates should be scaled up by 19 percent (Eissa and Liebman 1996).

[14] Sixty percent of the 53.8 million women 18–50 in 1980 are mothers (March CPS); 3.3 percentage points of these mothers corresponds to about 1 million mothers.

[15] Although the results are quite robust, statistical inference is challenging in this context with such crude policy variation. Table A.7 in the online Appendix confirms what panel B of Figure 2 suggests: the DD estimate is positive for various placebo-year cutoffs, not just 1975; permutation tests on various placebo-year cutoffs show parallel pretrends up through 1975 but reject parallel pretrends for placebo cutoffs up to or after 1976; each of the 1970–1978 annual estimates is not statistically different than 1975, but each of the 1979–1985 estimates is.

[16] Online Appendix B shows that results are robust to model choice, sample period, and reweighting to account for group composition and CPS data imputations; that women with more than one child had larger responses because they were more likely to have completed their fertility; and that small changes in EITC recipients and increases in working mothers are compatible.

Table 3—Heterogeneous and Subgroup Treatment Effects of the 1975 EITC on Employment

Subgroup:	All	Married				Education	"High-impact"	Men
Description: Variables	Larger response among unmarried mothers (1)	All married women (2)	Spouse earning below EITC kink point (3)	Placebo: Spouse earning above EITC kink point (4)	Response negatively correlated with spousal earnings (5)	Larger response among lower-education mothers (6)	Larger response among "high-impact" sample (7)	No response among married men (8)
Mom × Post1975	0.005	0.017	0.045	0.009	0.065	−0.011	0.018	
	(0.008)	(0.009)	(0.013)	(0.010)	(0.011)	(0.009)	(0.007)	
Mom × Post1975 × Unmarried	0.079							
	(0.006)							
Mom × Post1975 × Spousal Earnings (in 10,000s of 2013 $)					−0.009			
					(0.001)			
Mom × Post1975 × (≤12 Years of Education)						0.050		
						(0.004)		
Mom × Post1975 × (13–15 Years of Education)						0.034		
						(0.006)		
Mom × Post1975 × High-Impact							0.033	
							(0.007)	
Dad × Post1975								0.003
								(0.004)
Observations	571,170	370,767	67,277	303,490	370,767	571,170	571,170	343,219
Mean dependent variable:	0.66	0.60	0.53	0.62	0.60	0.66	0.75	0.89
Mean dependent variable for 1975 treatment group:	0.53	0.51	0.46	0.52	0.51	0.53	0.58	0.89

Notes: All samples are limited to 18–50-year-olds. Binary dependent variable employment for positive earnings. CPS weights, equation (1) are 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. Each column reflects a separate regression with the full set of controls from Table 2, column 4. The variable *Mom* × *Post*1975 implicitly refers to *Mom* × *Post*1975 × *Married* in column 1 and *Mom* × *Post*1975 × (≥16 *Years of Education*) in column 6. Results in column 5 are similar, using "predicted spousal earnings" to avoid potentially endogenous spousal-earning responses to the EITC (Table A.5). "High-impact" sample excludes EITC-ineligible women with spousal earnings above the EITC limit (column 4) and women not in the labor force due to a disability, health reason, or being a 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 the "high-impact" sample of moms in 1975 is 0.63 (see Figure 1, panel A). The mean dependent variable for 1975 mothers with low, medium, and high education in column 6 is 0.45, 0.54, and 0.59. Results for men in column 8 are similar for married or unmarried men. The EITC kink point in columns 3 and 4 was $4,000 (nominal dollars) in 1975 and increased to $5,000 in 1979 (or about $18,000 in 2013 dollars). For years before 1975, I convert spousal earnings to real 1975 dollars and use a $4,000 cutoff. Combining the two regressions in columns 3 and 4 and testing the two estimates show that they are statistically different (p-value $=$ 0.0007). In column 6, testing that (i) the high-education response is zero, (ii) the high-education response is equal to the medium-education response, (iii) the high-education response is equal to the low-education response, and (iv) the low- and medium-education responses are equal yields p-values of 0.23, 0.0005, <0.0001, and <0.0001.

Source: 1971–1986 March CPS data

is achieved with zero labor supply (point A in Figure 4, panel A). As a result, the EITC may discourage labor supply for married mothers with spousal earnings in or near the EITC phaseout region (Eissa and Hoynes 2004).

I test for this heterogeneity in Table 3, column 1, where I add the variable *Mom* × *Post*1975 × *Unmarried* to equation (1) and interpret its coefficient (7.9 percentage points) as the treatment effect of the EITC on unmarried mothers relative to married mothers. I interpret the sum of the 2 coefficients in column 1

(8.4 percentage points or 12.7 percent from a base of 65.6 percent) as the overall effect of the EITC on unmarried mothers.[17]

I estimate the EITC's effect on married mothers in two ways. In column 1, I pool all women and find an insignificant 0.5 percentage point effect. In column 2, I restrict the sample to married women and estimate a marginally significant 1.7 percentage point effect. The average DD estimate in Table 2 is a weighted average of these effects on married and unmarried women.[18] These results largely align with prior EITC research that consistently finds a larger response among single mothers.[19]

Among married mothers, EITC responses should vary by spousal traits and earnings. Mothers with very low spousal earnings should have responded to the EITC much like unmarried mothers. Restricting the sample to EITC-eligible married women with spouses earning below the EITC kink point,[20] the EITC increased the employment of this group by 4.5 percentage points (Table 3, column 3). I also test for a negative correlation between spousal earnings and EITC response by adding a variable to equation (1) that interacts $Mom \times Post1975$ with spousal earnings. Column 5 shows that the treatment effect on married women with 0 spousal earnings was 6.5 percentage points and declined by 0.9 percentage points for every \$10,000 (2013 dollars) in spousal earnings. These results are nested in online Appendix Figures A.7 and A.8, which use \$10,000 bins of spousal earnings and show that the largest response came from women with the lowest-earning spouses. Results are robust to using "predicted" spousal earnings (online Appendix Table A.5).[21] Finally, in online Appendix Figure A.8, I find suggestive evidence of a negative effect for married mothers with spousal earnings in the EITC phaseout region, which makes sense since these households would lose EITC benefits with a secondary earner.

Married mothers with spouses earning above the EITC kink point were not eligible for additional EITC benefits and faced the same work incentives before and after 1975.[22] If the EITC appeared to increase the employment of this placebo group of mothers, this could indicate that an omitted factor is biasing up the results. However, Table 3, column 4 shows a null effect on this placebo group, and small effects can be ruled out. Estimates in columns 3 and 4 are statistically different (p-value 0.01).

These estimated effects on women by marital status are consistent with the EITC being the causal mechanism.

[17] For comparison, the 1986 and 1993–1996 EITC expansions increased the number of unmarried working mothers by 2.8 and 6.1 percentage points (Eissa and Liebman 1996; Hoynes, Miller, and Simon 2015).

[18] Since 64 percent of the sample is married, $0.64 \times 0.005 + 0.36 \times 0.084 = 0.033$.

[19] Eissa and Liebman (1996), Meyer and Rosenbaum (2001), Grogger (2003), and Hotz and Scholz (2006) find positive effects on unmarried mothers. Contrary to the null or small positive effects I find, Ellwood (2000), Eissa and Hoynes (2004), and Bastian and Jones (2020) find small negative effects on married mothers in years after the 1980s.

[20] This kink point was \$18,000 in 2013 dollars (the bottom fifth of spousal earnings).

[21] Treating a married woman's work decision like a second mover in a two-person sequential game where the primary earner's labor supply does not depend on his spouse's labor supply (Eissa and Hoynes 2004) is a stretch but not completely unrealistic since 1970s-male labor supply was inelastic (Blundell and MaCurdy 1999). Also, the EITC is based on household earnings, and no additional EITC benefits should arise from intrahousehold labor supply substitution. Positive responses among some married women are also found by Eissa and Hoynes (2004, table 8; 2006b).

[22] With spousal earnings in the EITC's phaseout region, a mother would maximize EITC benefits by not working.

Heterogeneous Treatment Effects: Education.—Education is often used as a proxy for EITC eligibility and generally considered to be a fixed characteristic unlikely to be endogenous with the EITC.[23] Table 3, column 6 adds two variables to equation (1), $Mom \times Post1975 \times (\leq 12 YrsEd)$ and $Mom \times Post1975 \times (13\text{–}15 YrsEd)$, so that the coefficient on $Mom \times Post1975$ denotes the treatment effect for mothers with at least 16 years of education. EITC response should be negatively correlated with education, and mothers with a 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 mothers with fewer than 12, between 13 and 15, and 16 or more years of education had employment responses to the EITC of 3.9, 2.3, and −1.1 percentage points (or 8.7, 4.3, and −1.9 percent). These estimates are all statistically different from each other (p-values < 0.001), and I cannot reject that the high-education response is 0 (p-value 0.31).

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 4) as well as women less able to respond to the employment incentives of the EITC: disabled women, retired women, and full-time students.[24] I estimate the effect on this group by adding a variable to equation (1) that interacts $Mom \times Post1975$ with a binary for being in this "high-impact" group. The 2 estimates in Table 3, column 7 show that these mothers had an EITC response of about 5.1 percentage points (or 8.1 percent).

Additional Heterogeneous Treatment Effects.—In online Appendix Table A.4, I find larger responses among younger mothers and mothers of younger children and similar responses from white and nonwhite mothers.

Regarding men, since over 90 percent of males were already working in the 1970s and their participation elasticity was near 0 (Blundell and MaCurdy 1999), it should not be surprising that the EITC had no detectable effect on males (0.3 percentage points) in Table 3, column 8.

C. *Implied Elasticities*

Following Chetty et al. (2012, online Appendix B), I calculate the participation elasticity as the post-1975 change in log employment rates divided by the post-1975 change in the log net-of-tax earnings from working. I account for various taxes (EITC, income tax, payroll tax, dependent deduction) and transfers (AFDC, food stamps, WIC). In online Appendix D and Table D.1, I show my back-of-the-envelope approach and calculate a participation elasticity—for a representative unmarried

[23] Women with less than, exactly, and more than 12 years of education had average household earnings of \$21,000, \$45,000, and \$53,000. Low-education mothers were more than twice as likely to be EITC eligible as high-education mothers (42 and 20 percent).

[24] The fraction of women in this sample smoothly increased over time due to falling marriage rates. In the 1970s, disability rates were slowly rising (Autor and Duggan 2003), and educational attainment was steadily increasing (Figure A.2).

Table 4—Triple Differences Corroborates Difference-in-Differences

Third difference: Variables	EITC-eligible mothers versus non-EITC-eligible mothers (1)	EITC-eligible mothers versus fathers (in Table 3, column 8) (2)
Mom × Post1975 × EITC Eligible	0.026	
	(0.008)	
Parent × Post1975 × Woman		0.025
		(0.009)
Observations	571,170	606,309

Notes: Samples are limited to 18–50-year-olds. Binary dependent variable employment equals 1 for positive earnings. EITC-eligible mothers are unmarried mothers and married mothers with low spousal earnings (Table 3, columns 1 and 3). Non-EITC-eligible mothers have higher spousal earnings (Table 3, column 4). Sample of men in column 2 are married men (from Table 3, column 8). Results are similar if unmarried men are used. Equation (3), CPS weights, and the full set of controls from Table 2, column 4 are used along with interactions of each control with EITC-eligible mothers, 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.

Source: 1971–1986 March CPS data

mother of one child—of 0.58 (0.15) and a total intensive plus extensive margin elasticity—from the annual earnings estimates in Table 4—of 0.47 (0.13). For the full sample of all women, I calculate a participation elasticity of 0.63. Although these elasticity estimates are larger than those for more recent years, they are consistent with female elasticity estimates for this period (see Goldin 1990 and online Appendix D).

D. *DDD Corroborates DD Estimates*

Splitting the sample of mothers into EITC eligible and EITC ineligible (Table 3, column 4) creates a third difference for DDD. An omitted factor affecting the employment of all mothers could bias DD (discussed in Section V), which is why DDD "may generate a more convincing set of results" (Angrist and Pischke 2009, 182). Now,

$$(2) \qquad P(E_{ist}) = f(\beta_1 Mom \times Post1975 \times Treat_{ist} + \beta_2 X_{ist} + \delta_{st} + \epsilon_{ist}).$$

The estimate of β_1 is 2.6 percentage points (Table 4, column 1), similar to DD, and suggests that factors affecting all mothers (e.g., abortion and divorce laws, birth control) may not pose a threat to the DD estimates.[25] When men from Table 3, column 8 are used as a comparison group, I find a similar DDD estimate in Table 4, column 2 (2.5 percentage points).

E. *Extensive Margin Results: Annual DD Estimates*

I estimate annual effects of the EITC and test if the DD results are driven by outliers or general trends by replacing $Mom \times Post1975$ in equation (1)

[25] Equation (2) also controls for *Treat*, *Mom* × *Treat*, *Post*1975 × *Treat*, *Mom* × *Post*1975 along with interactions of each control with *Treat* for a more flexible model.

with $Mom \times Year_y$ for $y \in [1970, 1985]$. I omit $y = 1975$, and estimates measure the annual effect of being a mother on the probability of working relative to 1975. Using the "high-impact" sample, panel B of Figure 1 shows that these estimates closely resemble the unadjusted time-series trend. Relative to 1975, the estimates on $Mom \times Year_y$ are jointly insignificant (p-value 0.42) for $y \in [1970, 1975]$, become increasingly positive for $y \in [1975, 1979]$, and remain positive and relatively stable for $y \in [1979, 1985]$. The 1975–1979 increase may suggest that it took mothers a few years to learn about the EITC, similar to the response to the 1986 and 1993 EITC expansions (Eissa and Liebman 1996, Meyer and Rosenbaum 2001).[26]

IV. Annual Work Hours and Earnings

A. *Average Treatment Effects*

Results above show that the EITC increased maternal employment and imply that earnings and work hours should also have been affected. Results in Table 5 use equation (1), an OLS specification, and replace the binary employment outcome with annual work hours and earnings (in 2013 dollars). For each outcome, I show results for three samples of women: the "high-impact" group (from Table 3, column 7), all women (from Table 2), and the EITC-ineligible placebo group (from Table 3, column 4). Among the "high-impact" sample, the EITC led to increases of 63.9 annual work hours and \$1,249.10 in annual earnings (Table 5, columns 1 and 4). Among the sample of all women, the EITC led to smaller increases in work hours (35.1) and earnings (\$750.30) (columns 2 and 5).[27] Results capture intensive and extensive margins but primarily reflect the latter (see Section IVB).[28]

Among the placebo group, columns 3 and 6 show that the EITC had a statistically insignificant effect on work hours (2.4) and earnings (\$438), which corroborates the placebo test in Table 3, column 4.

B. *The EITC and the Distribution of Hours and Earnings*

Where in the earnings and work-hours distribution did these newly working mothers enter? It would raise concerns if these newly working mothers earned above the EITC limit. To investigate this, I use the "high-impact" sample and estimate equation (1) but with a binary outcome variable for having annual earnings or work hours in a particular range.

For annual earnings (in 2013 dollars), the most common response to the EITC was to earn between \$10,000 and \$20,000, which encompassed the most gener-

[26] The EITC does not pay until the following tax refund; it could take a year before EITC recipients became aware of the EITC (Liebman 1998). To test whether EITC response required an understanding of the tax code (Chetty, Friedman, and Saez 2013; Bhargava and Manoli 2015), I plot the annual response of the high-impact sample by education subgroup but do not find quicker responses by higher-education mothers (Figure A.9).

[27] As a percent, these four estimates are 8.1, 10.3, 5.7, and 7.3.

[28] This is consistent with Meyer (2002), Saez (2002), and Eissa and Hoynes (2006a).

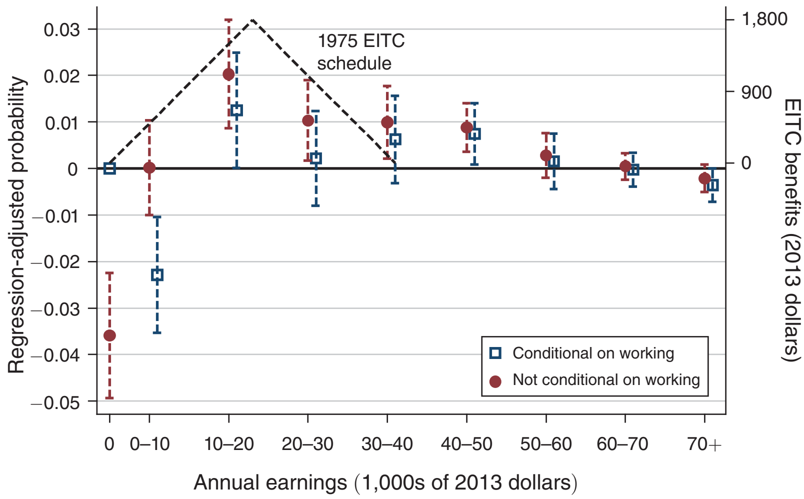

Figure 5. Effect of the EITC on the Distribution of Annual Earnings

Notes: Full set of controls and "high-impact" sample are used. Each estimate is from a logit regression of having earnings in each range. Not conditional on working, the mean dependent variables are 0.25, 0.22, 0.16, 0.16, 0.10, 0.06, 0.03, 0.02, 0.01, and conditional on working are 0.0, 0.29, 0.21, 0.19, 0.14, 0.08, 0.04, 0.02, 0.02. Standard errors are robust to heteroskedasticity and clustered at the state level.

Source: 1971–1986 March CPS data

Table 5—The EITC Effect on Annual Work Hours and Earnings (Intensive + Extensive Margins)

Dependent variable:	Annual work hours			Annual earnings (2013 $)		
Sample: Variables	"High-impact" group (1)	All (2)	EITC-ineligible placebo group (3)	"High-impact" group (4)	All (5)	EITC-ineligible placebo group (6)
Mom × Post1975	63.9	35.1	2.4	1,249.1	750.3	438.0
	(15.4)	(11.8)	(17.1)	(219.7)	(219.1)	(394.9)
Observations	236,814	571,170	303,490	236,814	571,170	303,490
R^2	0.222	0.168	0.140	0.305	0.214	0.170
Mean dependent variable:	1,045	834	755	16,422	13,992	13,295
Mean dependent variable 1975 mothers:	790	611	549	12,137	10,258	9,660

Notes: Each column represents a separate OLS regression with CPS weights and the full set of controls from Table 2, column 4. All samples are limited to women 18 to 50 years old. "High-impact" sample is from Table 3, column 7. EITC-ineligible placebo group is from Table 3, column 4. Annual work hours are constructed by multiplying weeks worked last year 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. Qualitatively similar results using imputed hourly wage (annual earnings divided by annual work hours, with 0 assigned if annual work hours equals 0, even if reported annual earnings is positive) as outcome: 0.85 (0.19), 0.38 (0.015), and 0.07 (0.27), which represent percent increases of 11, 6, and 1 for 1975 mothers. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.

Source: 1971–1986 March CPS data

ous portion of the EITC schedule and suggests that many of these newly working mothers received the EITC (Figure 5). The minimum wage during this period was

$7 to $9 per hour, and since online Appendix Figure A.11 shows that the most common EITC response was to work full-time,[29] this maps to about $14,000 to $18,000 per year, consistent with Figure 5.[30]

Recent evidence suggests that intensive margin responses may be an important and overlooked margin (Kline and Tartari 2016). Because of the EITC structure (Figure 4, panel B), mothers face a higher marginal tax rate in the EITC phaseout region than women without kids. In online Appendix Table A.6, I restrict the sample to working women and test whether mothers were less likely to have household earnings in this range after 1975. Using various sets of traits, I predict the probability that household earnings are in the EITC phaseout region and control for this variable. Columns 1–3 use DD and show that mothers were 1.8 percentage points less likely to earn in this region. Columns 2–3 show a large effect on EITC-eligible mothers and a null effect on EITC-ineligible mothers (−3.7 and −0.4 percentage points). Similarly, columns 4–6 use DDD and show that EITC-eligible mothers were 3.5 percentage points less likely to earn in the EITC phaseout region.

Finally, using IRS Statistics of Income (SOI) data (see footnote 33), I also find suggestive evidence that the EITC affected the composition of tax filers. Consistent with Table 3, column 1, the fraction of unmarried tax filers increased after 1975 in a pattern similar to panel B of Figure 1 (see online Appendix B.6 and Figure B.3).

C. *Quantile Analysis*

I now characterize the EITC's effect on the full earnings distribution. I use the high-impact sample and the regression behind Table 4, but instead of average effects, I estimate the effect at each centile of the earnings distribution. Figure 6 and online Appendix Figure A.12 show unconditional and conditional (on the full set of controls) quantile analysis. 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) are effects on quantiles, not on individual mothers, since rank preservation would require strong assumptions or panel data (Bitler, Gelbach, and Hoynes 2003).

The EITC had the largest effect on the earnings of the sixty-third centile, with a positive but steadily decreasing effect on either side of this centile, becoming small and insignificant for the top centiles (Figure 6).[31] The EITC had no effect on the lowest four deciles as these mothers did not work before or after 1975. Together, these QDD estimates drive the average effects in Table 4.

[29] Online Appendix Figure A.11 also shows that mothers were less likely to have zero work hours and may have increased part-time work. Restricting the sample to working women to isolate intensive margin responses, Figure 5 and online Appendix Figure A.11 also show (noisy) evidence of more mothers working over 1,000 hours and earning between $10,000 and $20,000.

[30] Figure 5 also suggests that mothers were slightly more likely to earn between $20,000 and $50,000. However, online Appendix Figure A.10 shows that this may reflect natural wage growth of these mothers through 1985 since there is no such effect when I end the sample in 1977.

[31] Conditional on the full set of controls, online Appendix Figures A.12 and A.13 show a positive and steadily declining effect beginning near the middle of each distribution.

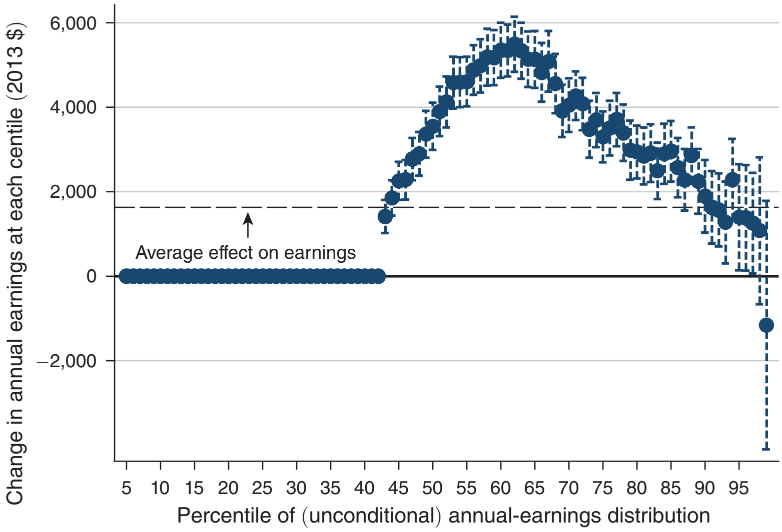

Figure 6. Effect of the EITC on Earnings (Unconditional Quantile DD)

Notes: Unconditional quantile treatment effects are estimated by regressing earnings on *Mom*, *Post*1975, and *Mom* × *Post*1975 (as in Table 2, column 1) using the "high-impact" sample. The average effect is \$1,625 compared to \$1,249 in Table 5, column 4 that uses the full set of controls. The mean dependent variables at deciles 1 to 9 for mothers in 1975 are 0; 0; 0; 0; 2,066; 8,503; 17,229; 25,895; 36,707.

Source: 1971–1986 March CPS data

V. Ruling Out Contemporaneous Shocks to Employment

In addition to parallel trends, a causal interpretation of DD requires that no contemporaneous factor affected the relative employment of mothers. Even though the 1970s was a period of inflation, oil and food price shocks, and two recessions, in the following discussion, I find little evidence of confounding policies or trends that affected maternal employment.

The first oil shock began in 1973 when the Organization of Arab Petroleum Exporting Countries proclaimed an oil embargo against the West in response to the Yom Kippur War. This led to a quadrupling of oil prices by March 1974, double-digit inflation and food-price increases, and a recession 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 1980 and 1982. Although a recession ended around the time the EITC began, it is not obvious why this would have affected the relative employment of mothers since no such increase occurred after the 1980–1982 recessions (Figure 1, panel A and Figure 2, panel A).[32] To account for these factors, I control

[32] A permanent price increase could increase labor supply through an income effect, but these shocks were temporary and should not have differentially affected mothers.

for annual inflation, state-by-year employment, and manufacturing employment and allow these variables to vary by family size, marital status, and education.

Two potential identification threats include public-program cuts, which could increase maternal employment via an income effect, or a sudden change in demographic traits associated with employment and unrelated to the EITC. However, public assistance expanded in the 1970s (a period of "welfare explosion") (Moffitt 2003): AFDC, Food Stamps, WIC, and payroll taxes all increased or were flat (online Appendix Figure A.3 also describes these programs). Also, trends in marriage, fertility, education, and male earnings were smooth (online Appendix Figure A.2).[33] I control for the impact of welfare and demographics on employment and allow them to vary by state, year, and race.

Perhaps the most serious potential confounder is the 1976 Child and Dependent Care Tax Credit (CDCTC), a nonrefundable tax credit for childcare expenses. I investigate whether this policy affects my analysis in three ways. First, I look at the fraction of EITC recipients who received CDCTC benefits (using IRS SOI data):[34] only 1 percent of EITC-eligible tax filers received any CDCTC benefits compared to 30 percent of EITC-ineligible tax filers with children (Figure A.4), corroborating previous evidence that most CDCTC benefits go to upper-middle-class families (Maag, Rennane, and Steuerle 2011). Second, restricting the sample to women *ineligible* for the EITC and *eligible* for the CDCTC, I do not detect an increase in working mothers after 1975 (Table 3, column 4). Third, I examine the subsequent 1981 CDCTC expansion (rate increased from 20 to 30 percent) and find that although CDCTC benefits doubled after 1981 (online Appendix Figure A.4), this pattern bears little resemblance to the maternal employment trends in panel A of Figure 1 and panel B of Figure 1.[35] Together, this evidence suggests that the CDCTC had a minimal effect on the population affected by the EITC.

In conclusion, I find little evidence of confounding policies or trends that affected the relative employment of mothers. Additional potential confounders are discussed in online Appendix C. If anything, the expansion of public assistance 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.

VI. Effects on Attitudes toward Working Women

If the 1975 EITC encouraged a million mothers to begin working, this likely had subsequent effects on the country. Although there is a large literature showing various effects of the EITC, how this program may have affected social attitudes toward working women has remained understudied.

[33] I cannot rule out a threshold-crossing model (Schelling 1971) where a continuously changing covariate has a discrete impact on an outcome.

[34] SOI data are de-identified samples of US federal individual income tax returns with detailed income information but little demographic information (US Treasury Department, Internal Revenue Service 1960–1990). SOI weights are used. More details in online Appendix B.

[35] Panel A of Figure 2 suggests that the 1981 CDCTC may have increased employment for mothers with relatively high spousal earnings (the group in Table 3, column 4). DDD analysis in Table 4 nets out any employment effect on this group.

Google ngrams (Michel et al. 2011) show that in the mid-1970s, the phrases *working mom* and—the previously redundant—*stay at home mom* began to be used much more often (Figure 3). These trends suggest that the rise of working mothers was a salient phenomenon and reflect changes in language and attitudes toward the role of women in society. After 1975, people were more likely to have working-female family members, friends, and coworkers while media stories about working mothers became more common.[36]

An emerging literature shows that gender-equality preferences can be altered via *exposure* to working women. Fernández, Fogli, and Olivetti (2004) and Olivetti, Patacchini, and Zenou (2016) show that having a working mother—and having friends with working mothers—as a child increases gender-equality preferences as an adult. Finseraas et al. (2016) shows that having female colleagues reduces discriminatory attitudes. These results suggest that the attitudes of millions of Americans may have been affected when the EITC led one million mothers to begin working in the late 1970s.[37]

A. *Empirical Strategy*

I characterize and exploit geographic heterogeneity in EITC responses and use a two-sample, two-step approach to test whether states with larger EITC responses had larger changes in gender-equality preferences. Gender-equality preferences are defined as approving of working women and are created from GSS data (Smith et al. 2016), an appealing source for measuring these social attitudes since the survey question is consistent over time and begins in 1972, providing a few baseline years before 1975.[38] Online Appendix Table A.8 shows GSS sample summary statistics, and Table A.9 shows that gender-equality preferences are positively correlated with education, having a working mother, and being younger, female, unmarried, and white.

I aggregate the gender-equality preferences of 8,713 adults ages 18–60 observed between 1972 and 1985 to the state-by-year level using GSS weights. I then construct a state panel on gender-equality preferences before and after 1975 and create the variable $\Delta GenderEquality_s^{post1975-pre1975}$—the change in the fraction of a state's adults who approve of working women—by subtracting the pre-1975 state average from the 1976–1985 state average.[39]

I use CPS data and the specification from Table 2, column 4 to estimate the state-level, EITC-led increase in working mothers (*EITCResponse*):

$$(3)\quad P(E_{ist}) \;=\; f\Big(\beta_1 Mom_{ist} + \sum_s \beta_{2s} Mom \times Post1975_{is} + \beta_3 X_{ist} + \delta_{st} + \epsilon_{ist}\Big).$$

[36] Media has been shown to affect a variety of outcomes (discussed in online Appendix B).

[37] Fernández, Fogli, and Olivetti (2004, 1250) help motivate Section VI by assuming that "as more women joined the labor force, attitudes towards these women changed in society at large."

[38] The GSS question asks, "Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?" Such approval rose from 20 to 80 percent between the 1930s and the 1990s (online Appendix Figure A.14).

[39] Results are robust to extending the GSS sample to any year between 1980 and 2000 (online Appendix Figure A.15). Ideally, I would construct a state-by-year panel, but since GSS samples are relatively small, I pool years to increase statistical power. A noisy measure of attitudes is not a problem if the measurement error is orthogonal to the covariates in equation (4).

Equation (3) modifies the country-level DD in equation (1) and estimates β_{2s}, state-level DDs.[40] I rename $\hat{\beta}_{2s}$, $EITCResponse_s$, and estimate:

$$(4) \qquad \Delta GenderEquality_s^{post1975-pre1975} = \gamma EITCResponse_s + \delta \Delta X_s + \epsilon_s.$$

The term γ measures the effect of a percentage point increase in state EITC response on the change in the fraction of a state's population with gender-equality preferences after 1975. Since the treatment variable is a generated regressor, standard errors are bootstrapped (Pagan 1984, Hardin 2002, Murphy and Topel 2002).[41] Controls X_s account for other state traits. Regressions are weighted by state population since observations represent grouped data. Results are robust to using individual-level GSS data (online Appendix Tables A.11 and A.12) and alternate weights or standard error specifications (see Table 7 notes).

B. *Results*

Using OLS, equation (4), and no controls, Figure 7 shows that each percentage point increase in state EITC response led to a 1.9 percentage point increase in state-level preferences for gender equality (p-value 0.02). The magnitude appears plausible: the interquartile effect is 5 percentage points, comparable to 1.5 more years of education or being a decade younger but less than having a working wife or having racial-equality preferences (online Appendix Table A.9). Online Appendix E shows similar results using less parametric approaches, and a permutation test shows that the result is unlikely to occur by chance.[42] Results are similar by gender and robust to region FE, reflecting changes within and across regions (Table 7 notes and columns 1–2).

One threat to my hypothesis would be if changes in gender-equality preferences coincided with changes in demographics or other attitudes unrelated to the EITC or working women. To test whether an omitted factor is driving the results in Figure 7, I reestimate equation (4) with controls for various demographic and social-attitude variables.[43] Across controls, the estimate is a stable 1.8–2.1 percentage points (Table 6, panel A).

It does not appear that changes in gender-equality preferences can be explained by demographics or general trends in social attitudes.

[40] Results are robust to estimating equation (3) with various sets of controls, using OLS, probit, or logit, and extending the CPS sample to various years (online Appendix Table A.11).

[41] As a first-difference estimator, equation (4) nets out the problem of omitted variables and is unbiased and consistent under the condition $E[u_{it} - u_{it-1} | x_{it} - x_{it-1}] = 0$, which 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).

[42] I run a variant of the permutation test in Buchmueller, DiNardo, and Valletta (2011) and randomly reassign state attitude changes, with replacement, from the set of state attitude changes; reestimate equation (4); record γ; and iterate 1,000 times. Online Appendix Figure A.16 shows the actual estimate is in the top 3 percent of permutations.

[43] Although it is impossible to control for every state trait that may be correlated with increases in working mothers and with attitudes toward working women, the GSS has data on a wide range of topics (e.g., racial attitudes, voting behavior, religion, attitudes toward public assistance, mother's work, and education). Furthermore, state-level response to the EITC (estimated in equation (3)) accounts for changes in demographic traits and economic conditions and isolates the increase in working mothers due to the EITC.

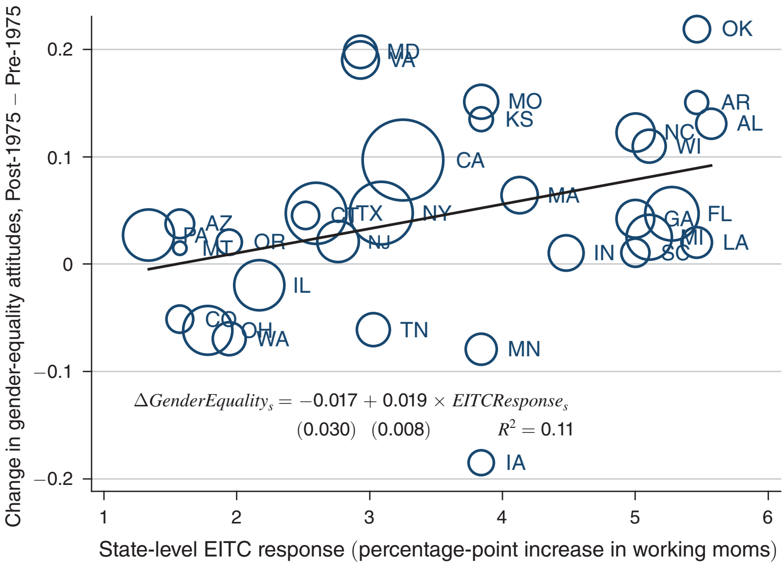

Figure 7. EITC Response and Increased Approval of Working Women

Notes: Sample contains adults 18 to 60 years old. Change in gender-equality attitudes is calculated by subtracting the pooled 1972–1975 state average from the 1976–1985 average using GSS weights. Years are pooled to increase power. State EITC response is estimated from equation (3). Unweighted regression with block-bootstrapped standard errors. Table 7 notes show similar results for other specifications.

Source: 1972–1985 GSS data

C. *Dose Response*

If the EITC did affect gender-equality preferences through exposure to working women, then people more likely to know these newly working women should have had larger preference changes. Since the EITC had a larger effect on lower-educated mothers (Table 3, column 6), lower-educated adults were more likely to know (or even be) these women. Table 7, columns 3–4 reestimate equation (4) but divide the sample into adults with more or fewer than 12 years of education. For lower-educated adults, the estimate of γ in equation (4) is 0.031 (p-value 0.003), and for higher-educated adults, it is -0.009 (p-value 0.49).[44] These estimates are statistically different at the 99 percent level and confirm that people more likely to know these newly working women did have larger preference changes.

Running individual-level regressions—instead of aggregating the GSS data to the state level—yields corroborating evidence that state EITC response affected gender-equality preferences and that the largest attitude changes occurred among people more likely to know these newly working women: unmarried, lower-educated, and younger adults (online Appendix Table A.12).

[44] Panels A and B of Table 7 show similar results without and with controls.

Table 6—EITC and Gender-Equality Preferences: OLS and 2SLS Approaches

Variables	(1)	(2)	(3)	(4)
Panel A. OLS				
EITC-led increase in working mothers (in percentage points)	0.019	0.018	0.021	0.019
	(0.008)	(0.011)	(0.009)	(0.011)
Observations	32	32	32	32
R^2	0.109	0.359	0.373	0.551
Variables	(1)	(2)	(3)	(4)
Panel B. 2SLS (state EITC response as an IV for the total state increase in working women)				
Increase in working women (in percentage points)	0.011	0.010	0.012	0.009
	(0.006)	(0.006)	(0.006)	(0.005)
Observations	32	32	32	32
First-stage estimate	1.70	1.85	1.69	2.08
	(0.54)	(0.58)	(0.50)	(0.74)
Kleibergen-Paap F-statistic	9.9	10.1	11.2	7.9
State controls (the pre-1975 to post-1975 change)				
State demographics		X		X
State attitudes			X	X

Notes: Gender-equality preferences are constructed from the GSS variable *fework,* which asks respondents whether married women should work. GSS sample reflects adults 18 to 60 years old in 32 states. State-level, EITC-led increase in working mothers is estimated in equation (3). For 2SLS, state response to the EITC is used as an instrument for the unadjusted increase in state female employment after 1975. Controls are constructed by subtracting the pooled 1972–1975 GSS state average from the 1976–1985 GSS state average using GSS sample weights. State demographics include average values of age, education, married, mothers, nonwhite, employment, earnings, had a mother who worked, and mother's education. State attitudes include political affiliation, religion, and attitudes toward race and welfare. Education is measured in years, mother is fraction of women who are mothers, employment rate denotes labor force participation, and earnings denotes real log earnings. Mom worked and mom education are constructed from GSS variables *mawk16* and *maeduc*, democrat from *partyid*, racial equality from *racpres*, religious from *reliten*, and too much welfare from *natfare*. Heteroskedasticity-robust standard errors are in parentheses. Regressions are weighted by state population. Similar results using other weights or standard error specifications (see Table 7 notes).

Source: 1972–1985 restricted GSS data with state-level identifiers

D. *Placebo Outcome: Changes in Racial-Equality Preferences*

Since attitudes toward gender and race were correlated with the same traits (Table A.9), it is conceivable that an omitted factor—other than the EITC—drove changes in both attitudes. Two ways to test for this is to use racial attitudes as a control (as in Table 6, column 5 and Table 7, panel B) or as a placebo outcome: Table 7, column 6 shows that state EITC responses had no detectable effect on racial attitudes (p-values 0.49 and 0.31 with and without controls), suggesting that changes in gender-equality preferences do not seem to be driven by general trends in attitudes.

E. *Reverse Causation*, *Noisy Data*, *and Mean Reversion*

Perhaps the most obvious threat to the results in Figure 7 is reverse causation: for example, if higher-responding states already had higher approval of working women before 1975. In Table 7, columns 6 and 7, I follow the approach in Acemoglu, Autor, and Lyle (2004) and test for a positive relationship between state EITC response and pre-1975 gender-equality preferences. I find an insignificant relationship between state EITC response and the 1972 to 1975 preference trend (p-value 0.26) and inter-

Table 7—EITC and Gender-Equality Preferences: Subgroups, Placebo Tests, and Testing for Mean Reversion

	Subgroup					Largest EITC response in states with lowest pre-1975 approval of working women		Ruling out mean reversion by controlling for pre-1975 attitudes
				Placebo tests				
Variables	Men (1)	Women (2)	Low-education (3)	High-education (4)	Racial-equality preferences (5)	1974 attitudes (6)	1972–1975 attitude trend (7)	(8)
Panel A. No controls								
EITC-led increase in working mothers (in percentage points)	0.018	0.017	0.031	−0.009	0.008	−0.028	0.022	0.016
	(0.011)	(0.009)	(0.010)	(0.013)	(0.012)	(0.013)	(0.019)	(0.007)
1974 fraction of state population that approves of working women								−0.389
								(0.099)
Observations	32	32	32	32	32	32	32	32
R^2	0.046	0.080	0.183	0.017	0.014	0.109	0.039	0.392
Panel B. Full set of controls from Table 6, column 4								
EITC-led increase in working mothers (in percentage points)	0.020	0.019	0.036	−0.018	0.016	−0.025	0.019	0.018
	(0.015)	(0.014)	(0.012)	(0.016)	(0.016)	(0.018)	(0.022)	(0.010)
1974 fraction of state population that approves of working women								−0.342
								(0.144)
Observations	32	32	32	32	32	32	32	32
R^2	0.544	0.524	0.577	0.329	0.232	0.553	0.548	0.670

Notes: Gender-equality preferences are constructed from the GSS variable *fework,* which asks respondents whether married women should work. GSS sample reflects adults 18 to 60 years old in 32 states. State-level EITC response is estimated from equation (4). The outcome variable is constructed by subtracting the pooled 1972–1975 GSS state average from the 1976–1985 GSS state average. Low and high education are defined as fewer than 12 or at least 12 years of education. Although column 7 shows that states with the lowest approval of working women had the largest EITC responses, column 8 shows that even when these pre-1975 attitudes are controlled for, state EITC response is still associated with an increase in approval of working women. Heteroskedasticity-robust standard errors are in parentheses. Regressions are weighted by state population. Panel A, column 1 (identical to Figure 7) robustness checks: estimate is 0.023 (0.008) when block bootstrapped and unweighted. Results are similar unweighted and weighted by the inverse of the standard error from equation (3) (column 1 estimates become 0.023 [0.008] and 0.022 [0.008]). Including region fixed effects yields noisier estimates: 0.017 (0.012).

Source: 1972–1985 restricted GSS data with state-level identifiers

estingly, a *negative* relationship between state EITC response and the 1974 preference level. This negative estimate suggests that the EITC may have led to an attitude "catch up" among states with lower gender-equality preferences.[45]

Since states with the *lowest* approval of working women before 1975 had the *largest* increase in approval of working women after 1975, it is possible that Figure 7 is simply due to mean reversion. In this context, mean reversion could reflect data limitations and relatively small GSS sample sizes or real convergence in social norms across states over time.

[45] If states that voted for the 1975 EITC benefited the most from it, perhaps the EITC was the outcome, not the cause, of changing attitudes. To test this, I regress state EITC response on the fraction of a state's senators and house representatives who voted for the 1975 EITC legislation. Online Appendix Figure A.17 shows that, in fact, the opposite is true: states voting against the EITC had higher EITC responses, and thus preference changes were larger in places less likely to be in favor of a social program such as the EITC.

A way to test for mean reversion is to see if state EITC response is still associated with attitude changes when controlling for pre-1975 attitudes. Table 7, column 8 shows that while pre-1975 attitudes are significantly associated with post-1975 attitude changes (corroborating column 7), EITC response still has an independent effect on attitude changes, although the estimate falls from 0.019 to 0.016 (0.019 to 0.018 with controls), perhaps suggesting that 10–20 percent of the estimate in Figure 7 is due to mean reversion.

Another way to investigate mean reversion is to see if states with higher EITC responses (and lower approval of working women) continued to have larger increases in approval of working women in the 1980s and 1990s. As shown by Charles, Guryan, and Pan (2009, figure 4), places with the lowest approval of working women in the 1970s also had the lowest approval in later decades. If mean reversion drove attitude changes after 1975, it should also have driven attitude changes in later decades. Online Appendix Figure A.18 reestimates equation (4), but instead of 1975, it measures attitude changes after placebo years in the 1980s and 1990s. I find that EITC response is not associated with attitude changes after these placebo years, suggesting that mean reversion may not explain post-1975 attitude changes either.

Finally, since state EITC responses estimated in equation (3) may be measured noisily, I carry out a few 2SLS approaches to deal with this potential measurement error. One approach uses the state EITC response as an instrument for the total post-1975 change in state female labor force participation (FLFP). Each predicted percentage point increase in FLFP increases approval of working women by 1.6–2.6 percentage points (Table 6, panel B). Another approach uses individual-level data and state-by-post-1975 fixed effects as instruments for total state-by-year FLFP (and thus not using the estimated state EITC response at all). With a very strong first stage, each predicted percentage point increase in FLFP increases approval of working women by a precise 0.2–0.3 percentage points (online Appendix Table A.11, panel B).

These 2SLS approaches suggest that effects on social attitudes are not simply driven by noise and lack of statistical power.

F. 2*SLS: Predicted Attitude Changes from Pre*-1975 *State Traits*

In this section, I exploit pre-1975 state traits ($X_s^{pre1975}$) and the heterogeneous EITC responses in Table 3 to *predict* state EITC response and test whether *predicted* EITC response is associated with changes in gender-equality preferences. This 2SLS approach helps alleviate concerns about the potential endogeneity of attitudes and EITC response. The equations for the two stages are shown in equations (5) and (6):

$$(5) \qquad EITCResponse_s = \zeta X_s^{pre1975} + \kappa \Delta X_s + \nu_s,$$

$$(6) \qquad \Delta GenderEquality_s^{post1975-pre1975} = \theta \widehat{EITCResponse_s} + \lambda \Delta X_s + \epsilon_s.$$

Here, $EITCResponse_s$ comes from equation (3), and $\widehat{EITCResponse_s}$ is the predicted state EITC response from the first stage. The 2SLS exclusion restriction is

Table 8—2SLS: Predicting State EITC Response and Attitude Changes from Pre-1975 Traits

Pre-1975 state trait used to predict state EITC response	Average female education	Fraction of unmarried mothers	Average male earnings	Education + single mothers	Education + male earnings	Education + single mothers + male earnings
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. No controls						
1 percentage point in predicted state EITC response	0.061	0.007	0.063	0.045	0.062	0.047
	(0.041)	(0.047)	(0.036)	(0.030)	(0.031)	(0.018)
Observations	32	32	32	32	32	32
First-stage F-statistic	3.4	2.3	8.5	4.4	4.3	6.1
Panel B. Full set of controls from Table 6, column 4						
1 percentage point in predicted state EITC response	0.025	0.089	0.048	0.026	0.031	0.036
	(0.015)	(0.095)	(0.026)	(0.016)	(0.011)	(0.011)
Observations	32	32	32	32	32	32
First-stage F-statistic	6.5	0.4	2.1	3.2	3.8	2.8

Notes: Gender-equality preferences are constructed from the GSS variable *fework,* which asks respondents whether women should work. GSS sample reflects adults 18 to 60 years old in 32 states. State-level EITC response is estimated from equation (3). Each estimate reflects the 2SLS approach shown in equations (5) and (6). Female education is measured in years. Unmarried mothers is the fraction of mothers who are single. Average male earnings are measured in real 1975 dollars. Columns 1–3 use single traits, and columns 4–6 use multiple traits to predict state-level EITC response. Heteroskedasticity-robust standard errors are in parentheses. Regressions are weighted by state population; unweighted results are similar.

Source: 1972–1985 restricted GSS data with state-level identifiers

that $X_s^{pre1975}$ only affected attitude changes through state EITC response. Although this restriction cannot be directly tested, I find suggestive evidence for it: the 1972–1975 change in attitudes is uncorrelated with each $X_s^{pre1975}$ (p-values range from 0.42 to 0.84).

In Table 8, columns 1–6, $X_s^{pre1975}$ contains one or more of the following traits: average years of female education, fraction of mothers who were unmarried, and average male earnings. Across specifications, the analysis is somewhat underpowered, with first-stage F-statistics generally between 3 and 9. Each *predicted* percentage point in state EITC response is associated with an increase in gender-equality preferences. Using all 3 traits, I find an estimate of 3.6 and 4.7 percentage points with and without controls.

Using various approaches, I find suggestive—but consistent—evidence that EITC-led increases in working mothers led to increased social approval of working women.[46]

VII. Summary

In one of the first systematic studies of the 1975 introduction of the EITC, I find that this program led to a 6 percent increase in maternal employment, which represents about 1 million mothers and a participation elasticity of 0.58. Regression-adjusted and unadjusted time-series trends show that the relative employment of mothers began to increase after 1975 (Figure 1, panels A and B). Consistent with the EITC being responsible for this rise in employment, I find larger responses from mothers

[46] Since these results are based on crude variation and a relatively small sample size, future research should carry out this type of analysis in other contexts.

more likely to be EITC eligible and null responses from placebo groups not eligible for EITC benefits (Table 3). Using a placebo group of EITC-ineligible mothers in a DDD specification to net out contemporaneous policies (e.g., birth control, divorce laws, abortion) yields similar estimates.

In hindsight, the employment effect of the 1975 EITC should not be that surprising: female labor-supply elasticity was larger during this period (Blau and Kahn 2005, Heim 2007), and the 10 percent wage subsidy of the EITC represented a large increase in potential earnings.[47] Although much was already known about the rise of working *women* (Killingsworth and Heckman 1986; Goldin 1990; Fernández, Fogli, and Olivetti 2004), this study helps explain why so many *mothers* began working in the 1970s.

The 1970s also provide a clean policy environment to evaluate the 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 welfare reductions and the Family Medical Leave Act, which increased maternal employment (Ruhm 1998, Moffitt 1999).

This EITC-led increase in working mothers also appears to have increased approval of working women. States with larger EITC responses—and larger *predicted* responses based on pre-1975 traits—had larger increases in attitudes approving of women working. Results do not appear to be driven by changes in demographics or general trends in social attitudes and are larger among people more likely to know these newly working mothers.[48] Since social attitudes toward working women and the number of working women are endogenous, I use a plausibly exogenous increase in female employment to show that increases in working women affect attitudes toward working women. I conclude that the 1975 EITC played an important role in the rise of US working mothers and in fostering egalitarian social attitudes.

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[47] This paper may also help resolve an anomaly observed by Smith and Ward (1985): although real wage growth explains most of the increase in the female labor supply between 1950 and 1980, after 1970 the growth rate of female labor supply rose as the real wage growth rate fell (Parkman 1992).

[48] Regarding external validity and whether working women can affect social attitudes toward women in other contexts, in an earlier draft (shown in Appendix G), I find evidence of attitude changes due to the large increase in working women during World War II (Bastian 2017).

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