New Evidence on the Effect of Public Policy on Employment, Intergenerational Mobility, Family Structure, and Social Attitudes Towards Working Women

by

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This achievement is dedicated to my loving wife. Thank you for your kindness and support.

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ABSTRACT

My dissertation finds new evidence that public policy can be used to reduce poverty, increase economic opportunity, and encourage egalitarian social attitudes in the United States. I focus on the Earned Income Tax Credit (EITC), a wage subsidy that has become one of the most important parts of the U.S. social safety net. By 2013, the EITC redistributed $66 billion to over 28 million low-income households and lifted 9.4 million individuals out of poverty. I show that the EITC affects mothers' labor-market outcomes, the education and earnings of children of EITC recipients, marriage and fertility decisions, and social attitudes towards working women.

Chapter 1: The rise of working mothers radically changed the U.S. economy and the role of women in society. In one of the first studies of the 1975 EITC, I find that this program increased maternal employment by 7 percent, representing one million mothers. The EITC can help explain why the U.S. has long had such a high fraction of working mothers despite few childcare subsidies or parental-leave policies. This influx of working mothers likely had subsequent effects on the country: I find evidence that the EITC affected social attitudes and led to higher approval of working women.

Chapter 2: Using four decades of variation in the federal and state EITC, we estimate the impact of the EITC on education and employment outcomes on children exposed to EITC expansions in childhood. Reduced-form results suggest that an additional $1,000 in EITC exposure when a child is 13 to 18 years old increases the likelihood of completing high school (1.3 percent), completing college (4.2 percent), being employed as a young adult (1.0 percent), and earnings by 2.2 percent. Instrumental variables analysis reveals that the primary channel through which the EITC improves these outcomes is increases in pre-tax family earnings.

Chapter 3: There has long been a concern that public assistance programs in the U.S. discourage marriage among lower-income couples. The EITC provides a marriage bonus to some couples but a marriage penalty to others, and encourages some households to have more children but others to have less. The overall average effect of the EITC is therefore theoretically ambiguous and existing empirical evidence has been mixed. Using over 30 years of household panel data – and a novel approach that controls for current fertility and marital status – I find that state EITC expansions have positive average effects on both fertility and marriage. Marriage effects are largest for currently unmarried adults and give pause to concerns about the negative effects of the EITC on marriage. These results also imply that some estimates in the EITC literature may be biased, since endogenous switching from the control to the treatment group (defined by marital status or number of children) would violate the stable-group-composition condition required by difference in differences.

CHAPTER 1

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

1.1 Introduction

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).[1] 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 finds that the 1975 introduction of the Earned Income Tax Credit (EITC) can 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 towards working women.

Time-series evidence shows that the relative employment of mothers – compared to women without children – rapidly increased after 1975 (Figure 1.1A). 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 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 1.1B). This

[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 was 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, p.57). Details on parental leave policies across countries: http://www.oecd.org/els/soc/PF2_1_Parental_leave_systems.pdf.

increase in employment increased labor-force attachment and work intensity: estimates imply that the EITC increased average annual work hours by 7.3 percent (43 hours) and earnings by 9.6 percent ($965 2013 dollars). Results imply a participation elasticity of 0.6 to 0.8, larger than most 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 causing this rise in employment, I find larger responses from mothers eligible for more EITC benefits and null responses from placebo groups of mothers not eligible for any 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 (e.g. birth control, divorce laws, abortion) affecting all mothers: the DDD estimate corroborates the DD result (4.5 and 4.0 percentage points).

My estimates suggest that the EITC encouraged about one million mothers to begin working. However, this finding is unlikely to capture the full impact of the EITC on American society. In section 1.7, I use data from the General Social Survey (GSS) to examine whether the EITC-led influx of working mothers affected social attitudes towards working women ("preferences for gender equality"). This hypothesis is motivated by evidence showing that such attitudes are malleable and increase with exposure to working women: Fernandez, 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.[2]

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 changes in gender-equality preferences after 1975. I use both the *actual* state EITC response and the *predicted* state EITC response, based on preexisting state demographic and occupational traits. Using the predicted response helps alleviate concerns about the potential endogeneity of gender-

[2] Google ngrams (Michel et al. 2011) provides descriptive evidence that the rise of working mothers was salient (Figure 1.2) and that references to working mothers became much more common after the mid-1970s.

equality preferences and EITC response. States with larger EITC responses experienced larger increases in gender-equality preferences after 1975. Preference changes are observed among both men and women, within and across regions, and do not appear to be driven by preexisting attitudes, changes in demographics, or general trends in social norms. Since 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, and I verify larger preference changes among these men. 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 attitudes.

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

1.2 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 and decreasing poverty (Almond, Hoynes, and Schanzenbach 2011; Hoynes, Page and Stevens 2011; Bailey and Goodman-Bacon 2015; Goodman-Bacon 2016), but also had unintentional work disincentives (Moffitt 1992, Hoynes 1996, Hoynes and Schanzenbach 2012).[3] Welfare dependency was seen as a major social problem (O'Connor 1998) and momentum built for a guaranteed annual income with 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 – in 1970 with the backing of President Nixon and would have replaced AFDC.[4] However, the U.S. 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 Louisiana Senator Russell Long in 1972. A version of this bill was eventually passed as the Earned Income Tax Credit (EITC) and signed into law by President Ford on March 29, 1975. See Liebman (1998) and Ventry (2000) for a detailed history.

[3] See Bailey and Danziger (2013) for a detailed review of War on Poverty programs and their effects.
[4] FAP would have guaranteed $3,100 (2013 dollars) for each parent and $1,800 for each child – $9,800 for a family of four (the 1970 poverty line was 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. Rhys-Williams (1943) was among the first to outline this type of program.

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).[5] The EITC also provided benefits to working parents with earnings above $18,000, but benefits decreased at a rate of 10 percent and reached zero for earnings above $36,000 (Figures 3A and 3B).[6] 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[7] 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 (Center on Budget and Policy Priorities 2014). The EITC has been shown to raise maternal employment (Dickert, Houser and Scholz 1995; Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Hotz and Scholz 2006; Eissa, Kleven and Kreiner 2008), increase earnings (Dahl, DeLeire and Schwabish 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 health (Hoynes, Miller and Simon 2015; Averett and Wang 2015) and test scores (Chetty, Friedman and Rockoff 2011; Dahl and Lochner 2012), and long-run outcomes like educational attainment (Manoli and Turner 2014; Bastian and Michelmore 2017) and employment and earnings (Bastian and Michelmore 2017). The EITC's unintended consequences include lower pre-tax wages of low-skill workers (Leigh 2010; Rothstein 2010) and effects on fertility and marriage.[8] 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 how the EITC may affect attitudes towards working women. I show that the 1975

[5] 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"). This child must be under 19, under 24 if a full-time student, or any age if disabled. Before 1987, tax filers did not have to provide Social Security numbers for dependents. Until 1990, tax filers had to demonstrate they provided at least half the costs of maintaining the household ("support test"): cash and in-kind public assistance had to be less than half of the household budget (Holtzblatt 1991; Holtzblatt, McCubbin, and Gillette 1994). I do not observe tax filing and 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 student) are dependent children. I treat subfamilies within a household as separate tax-filers.
[6] Figure 1.3A shows a budget constraint under the EITC and Figure 1.3B illustrates the "phase-in" and "phase-out" portion of the EITC schedule while contrasting the 1975 EITC schedule with the 2013 EITC.
[7] See Table 1.3B notes for details.
[8] For fertility, Baughman and Dickert-Conlin (2009) and Bastian (2017) find positive effects. For marriage, Dickert-Conlin and Houser (2002), Herbst (2011), and Michelmore (2015) find negative effects, while Bastian (2017) finds positive effects.

EITC encouraged one million mothers to begin working, which subsequently affected traditional attitudes towards working women.

Almost all studies of the EITC ignore the program's first decade.[9] Although there was little policy variation before 1986, the 1975 introduction was itself a large policy change that has received surprisingly little attention, in part due to the common misconception that the original EITC was too small to have had much of an effect.[10] However, the 1975 EITC was large in at least three ways (Figure A1.1): First, about half of all households had earning below the EITC income limit; second, benefits were quite high, up to $1,800 (2013 dollars), about 10 percent of the poverty line for a mother with two kids; third, a 10 percent earnings subsidy represented a substantial year-over-year increase in potential earnings. Other reasons to expect the 1975 EITC to have had a large impact is 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 generally declines with subsequent program expansions (Bjorklund and Moffitt 1987; Heckman and Vytlacil 1999).[11]

1.3 Conceptual Framework

The EITC was a wage subsidy for low-income parents and should have increased the employment of mothers.[12] Intuition for this can be formalized in the following framework where individuals are denoted by i, states by s, and years by t.

$$U\big(c(.), L, g_{st}(.)\big) = [c(l_i, w_i, n_i, h_i, k_i) + L_i^{\alpha} - g_{st}(l_i, k_i)] \qquad (1)$$

Each woman divides her one unit of time between labor l_i, leisure L_i, and home production h_i. Consumption $c(.)$ is a function of her labor supply l_i, wage w_i, non-labor income n_i (e.g.

[9] Bastian and Michelmore (2017) is the one exception I am aware of.
[10] As can be seen in the following representative quotes: “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).
[11] See Moffitt (1999, Figure 1) for a simple graphical illustration of decreasing marginal treatment effects.
[12] I implicitly assume that employers do not substitute women in the control group for their treatment-group counterparts (see Neumark and Wascher (2011) for a discussion).

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 $g_{st}(l_i, k_i)$ is a function of labor supply l_i and kids k_i (e.g. childcare). Working can be thought of as either a binary or continuous decision. The EITC was an exogenous increase in w_i for EITC-eligible mothers, making work a relatively more attractive use of time, especially for mothers with lower non-labor income.

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

$$P(E_{ist}) = f(\beta_1 Mom_{ist} + \beta_2 Mom\ x\ Post1975_{ist} + \beta_3 X_{ist} + \delta_{st} + \epsilon_{ist}) \qquad (2)$$

E_{ist} is binary for whether a woman is employed, Mom and $Post1975$ are binary for whether a woman is a mom and whether the year is after 1975, and $Mom\ x\ Post1975$ is the DD variable of interest. The EITC treatment effect is β_2 and should be positive since the EITC encouraged mothers to work. X_{ist} is a set of controls that vary at the individual, state, and year level (described below). Although the 1975 EITC did not vary by state, I include state-by-year fixed effects δ_{st} to control for time-varying state policies and characteristics; ϵ_{ist} is an idiosyncratic error term. Coefficients are measured in percentage points. Estimates come from a logit model and average-marginal effects are reported throughout. Standard errors are 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.

1.4 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.

1.4.1 Descriptive Statistics

To estimate the EITC's effect 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.[13] Table 1.1 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 and 9 percent are Black and Hispanic, 65 percent work, average individual annual earnings of $12,826 ($19,685 conditional on working), average household earnings of $40,857 (2013 dollars), and 46 percent have household earnings below the EITC limit. On average, mothers are older, less likely to be white, less likely to work, and have less education and higher household earnings.

Figures 1A and 1B show 1970-to-1985 time-series employment trends for women with and without kids and preview the regression-adjusted results.[14] 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 increased and the gap narrowed to 18 percentage points, where it remained from 1979 to 1985. Although employment levels differed for these groups, employment trends before 1975 were parallel (p-value = 0.56).

1.4.2 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, I find no apparent evidence of confounding effects on maternal employment.

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

[13] To be consistent throughout I exclude women missing spousal earnings (this does not affect results).
[14] Figures 1.1A and 1.1B use the "high-impact" sample described in section 1.5.2.3.

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 July 1981 and November 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 1.1A).[15] To account for these factors, I control for annual inflation and state-by-year employment rates and manufacturing employment. I allow these variables to have a differential effect by family size, marital status, and education.

An example of a potential identification threat would be cuts to public programs, which would encourage low-income mothers to work more via an income effect. However, during the 1970s, public assistance programs were expanding (Moffit (2003) refers to this period as a time of “welfare explosion”), which likely dulled the response to the EITC and may bias results in this paper towards zero.[16] Figure A1.3 shows that trends in welfare, Food Stamps, Women, Infants, and Children (WIC), and payroll taxes were flat or increasing.

Another potential identification threat would be a sudden change in demographic traits associated with employment and unrelated to the EITC. However, Figure A1.4 shows that changes in marriage, fertility, education, and male earnings were smooth in the 1970s and should not have affected the relative employment of mothers.[17] To account for the effect of changing demographics on employment, I control for these variables and interact them with state, year, and race.

Perhaps the most serious potential confounder to my study is the 1976 Child and Dependent Care Tax Credit (CDCTC), a non-refundable, 20 percent tax credit for child care expenditures. I investigate whether this policy affects my analysis in three ways: First, I look at the fraction of EITC recipients that also received CDCTC benefits (using the IRS Statistics of

[15] 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 have differentially affected mothers.

[16] Food Stamps began rolling out in 1961 and were in all counties by 1975. During the 1970s, families on Food Stamps increased from about 13 to 20 million, which had small negative effects on employment (Hoynes and Schanzenbach 2012). WIC began rolling out in 1972. The percent of counties with WIC rose from 0 in 1973, to 60 in 1975, to 100 in 1979 (Hoynes, Page, and Stevens 2011) and had small negative labor-supply effects (Fraker and Moffitt 1988, Hagstrom 1996, Keane and Moffitt 1998, Currie 2003).

[17] Although I cannot rule out a threshold-crossing model (Schelling 1972) where a continuously changing covariate has a discrete impact on an outcome.

Income Public Use (SOI) data)[18]: only 1 percent of EITC-eligible tax filers received any CDCTC benefits, compared to 30 percent of EITC-ineligible tax filers with children (Figure A1.5), which corroborates other studies showing that the CDCTC mainly benefited upper-middle class families (Tax Policy Center 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 1.3 column 5). Third, I examine whether the subsequent 1981 CDCTC expansion (rate increased to 30 percent) affected maternal employment. Using SOI data, I find that CDCTC benefits doubled after 1982 (Figure A1.5), but this pattern bears no resemblance to the employment patterns in Figures 1A and 1B.[19] Together, this evidence suggests that the CDCTC had a minimal effect on the population affected by the 1975 EITC.

Other potential identification threats include 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 birth-control and divorce laws during the 1960s and 1970s. Regarding why these policies are unlikely to pose a serious threat to my empirical strategy: 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); four states legalized abortion in 1970 (AK, HI, NY, CA) and I find maternal employment trends similar to other states (results omitted); the ECOA likely did not affect maternal employment (Smith 1977; Elliehausen and Durkin 1989); the PDA had little effect on maternal labor supply since mothers bore the whole cost of the mandated PDA benefits and the return to work remained the same (Gruber 1994);[20] the birth-control pill first became available in 1960 and was available in most states before the mid-1970s (Bailey 2006); finally, divorce began rising in the 1960s (Johnson and Skinner 1986; Peters 1986;

[18] SOI 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 are used to reflect population average. More details in Appendix A2 and here: http://users.nber.org/~taxsim/gdb/.
[19] Averett, Peters, and Waldman (1997) finds that the CDCTC increased the labor supply of moms in their twenties with young children in 1987.
[20] Mukhopadhyay (2012) finds a positive labor-supply effect of the PDA on pregnant women and mothers of young children, however, the PDA did not become law until October 1978 and Figures 1.1A and 1.1B show that most of the rise in maternal employment had already occurred by then.

Parkman 1992; Wolfers 2006) and I find that California – the first state to pass no-fault divorce in 1970 – had similar maternal employment trends as the other states (results omitted).[21]

In conclusion, I do not find evidence of any factors 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.

1.5 The EITC and Extensive Margin Labor Supply

In this section I describe the average effect of the EITC on maternal employment and analyze how this effect varied by subgroup. Heterogeneous analysis shows larger responses from mothers more likely to be EITC-eligible. Notably, I also find positive responses among married mothers with low-earning spouses.

1.5.1 Average Treatment Effects

Having found no evidence of confounding policies in the previous section, I now use equation (2) and various sets of controls to estimate the average employment effect of the EITC on mothers. Each column in Table 1.2 controls for whether each observation is a mother (Mom),[22] whether the observation occurs after 1975 ($Post1975$), and the DD variable of interest ($Mom\ x\ Post1975$).

Table 1.2 cumulatively adds covariates in columns 1 to 7. Column 1 uses no additional controls and column 2 adds state and year fixed effects[23] to account for idiosyncratic state traits and annual shocks affecting all women. Column 3 adds demographic controls[24] to help

[21] Choo (2015) finds that no-fault divorce laws decreased the growth rate of divorce, not divorce rates.

[22] Restricting the treatment group to mothers with at least one child born before 1975 avoids concerns about potential fertility responses to the EITC, but affects the composition of the treatment group over time. Making this restriction yields a similar estimate of 0.037 (0.009) instead of 0.040 (0.009).

[23] Before 1977, CPS did not uniquely identify all states. I merge states into the 21 smallest possible geographical units to provide a balanced panel (details in Appendix A5). So few clusters may bias the standard errors (Angrist and Pischke 2009; Cameron, Gelbach and Miller 2008; Cameron, Gelbach and Miller 2012); block bootstrap yields similar standard errors.

[24] Demographic controls include an age cubic, an education quadratic, number of children, welfare income, and 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.

distinguish between EITC-led and demographic-led increases in maternal employment and help account for the fact that mothers are on average older, have less education, and more likely to be married and nonwhite (Table 1.1). Column 4 adds annual state and federal unemployment rates to control for the effects of economic conditions on employment; I allow these variables to vary by marital status and whether women have kids. Columns 5 and 6 show that results in column 4 are robust to probit and OLS specifications. Finally, column 7 shows that estimates are robust to 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, and race. These interactions flexibly account for the impact of economic conditions, changing demographics, and general trends on the employment of various types of women.

Across each set of controls the DD estimate is positive, significant at the 99 percent level, and stable between 3.9 and 4.9 percentage points (or 7.3 and 9.5 percent from a baseline of 53 percent).[25] Results imply that about one million mothers began working because of the 1975 EITC, about as many as from the 1986 and 1993 EITC expansions combined.[26] 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. The set of controls in column 4 is used throughout the rest of the analysis unless otherwise specified. Results are similar for alternate binary definitions of working based on earnings, weeks worked, or labor-force participation (Table A1.1).[27] Results are also robust to alternate age cutoffs (Table A1.2) and not using CPS weights.[28] Appendix A2 shows additional robustness checks.[29]

[25] 53 percent among all mothers in the sample, compared to 63 percent in Figure 1.1A for the “high-impact” sample (section 1.5.2.3). 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) and Liebman (2000) find 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, estimated employment effects of the EITC should be scaled up by 19 percent (Eissa and Liebman 1996)

[26] 52.8 million women aged 15 to 44 are in the 1980 Census, 47 percent of which are moms (CPS). 4 percentage points of these moms corresponds to about 1 million moms. For 1986, Eissa and Liebman (1996) finds 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 (likely an upper bound since welfare reform and the Family Medical Leave Act also increased maternal employment during this time (Ruhm 1998; Grogger 2003).

[27] Table A1.1 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.

[28] Unweighted result is 0.034 (0.009), statistically identical to the weighted result of 0.040 (0.009).

1.5.2 Heterogeneous and Subgroup Treatment Effects

Although the average employment effect of the EITC was positive, the treatment effect should have varied by the likelihood of receiving EITC benefits. Mothers with higher household earnings (e.g. married, higher-educated) 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 and investigate in Table 1.3 whether the treatment effect varied in a predictable way, which would support the hypothesis that the EITC was behind this increase in employment. Traits associated with these heterogeneous responses are also used in section 1.8 to predict state-level EITC response and show that states with larger EITC responses had larger increases in approval of working women after 1975. Responses by age, age of children, and race are shown in Table A1.4.

1.5.2.1 Heterogeneous Treatment Effects: Marital Status

There are at least two reasons why married mothers should have responded less to the EITC than unmarried mothers. First, since EITC eligibility is determined by household earnings, spousal earnings often pushed the household out of EITC eligibility (left of point C in Figure 1.3A). Second, married women tend to have higher non-labor income (i.e. spousal earnings) than single women, and since non-labor income increases the likelihood that a woman's highest feasible indifference curve is achieved when her labor supply is zero (point A in Figure 1.3A), married women should have had relatively smaller responses to the EITC. I verify this heterogeneity in Table 1.3 column 1, where I add $Mom\ x\ Post1975\ x\ Unmarried$ 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 1.3 columns 1 and 2. In column 1, I find a statistically significant effect on married women of 2.7 percentage points ($Mom\ x\ Post1975$). In column 2, I restrict the sample

[29] Appendix A2 shows results are robust to model choice, sample period, various reweighting techniques to account for potentially changing group composition and CPS data imputations, and explains how flat EITC beneficiaries and increases in working mothers are compatible as well as why I observe larger responses from women with more than one child.

to married women and show that the EITC had a statistically insignificant effect of 1.6 percentage points. These estimates are statistically indistinguishable and align with prior EITC research that has consistently found a larger response among single mothers.[30]

Although I find a statistically insignificant average response among married mothers to the 1975 EITC (Table 1.3 column 2), there should have been heterogeneity among married mothers that varied by spousal 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 income limit,[31] the EITC increased the employment of this group by 4.9 percentage points (column 3).[32]

Married mothers with spouses earning above the 1975 EITC kink point were not eligible for the EITC and faced the same work incentives before and after 1975. If it appears that the EITC increased the employment of this placebo group of mothers, this could indicate that an omitted policy or trend is biasing my results upward. However, Table 1.3 column 5 shows a null effect on this placebo group and small effects can be statistically ruled out. I also use this placebo group as a third difference for triple differences analysis (Table 1.4 column 1).

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. This models a woman's decision to work as if she knew her spouse's *ex post* earnings in advance. This is a strong assumption but may not be unrealistic on average since male labor supply during this period was inelastic (Blundell and MaCurdy 1999).[33] Column 4 shows that the EITC's treatment effect on married women with zero spousal earnings was 6.2 percentage points

[30] Positive responses for single moms (Eissa and Liebman 1996; Keane and Moffitt 1998; Meyer and Rosenbaum 2001; Grogger 2003; Hotz and Scholz 2006; Eissa, Kleven and Kreiner 2008). Null or negative responses for married moms (Dickert, Houser and Scholz 1995; Ellwood 2000; Eissa and Hoynes 2004).
[31] A quarter of males earned below the 1975 EITC kink point of $18,000 (2013 dollars).
[32] This result is nested in Figure 1.4 which uses the entire spousal-earnings distribution and shows the largest EITC responses came from women with the lowest earning spouses; this estimate steadily declines as women with spouses earning below $20,000, $30,000, etc., are incrementally added to the sample.
[33] This approach follows Eissa and Hoynes (2004) and 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. Of course, if it becomes more attractive for a mother to begin working, the other spouse may decrease their labor supply, but the EITC is based on household earnings and no additional EITC benefits should arise from substituting labor supply between spouses.

and this effect declined by 0.9 percentage points for each additional $10,000 (2013 dollars) in spousal earnings.[34]

1.5.2.2 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[35] and generally considered to be a fixed characteristic unlikely to be endogenous with the EITC.[36] Table 1.3 column 6 shows estimates from a regression identical to equation (2) except that it adds two variables, $Mom\ x\ Post1975\ x(<12YrsEd)$ and $Mom\ x\ Post1975\ x(12-15\ YrsEd)$, so that the coefficient on $Mom\ x\ Post1975$ now denotes the treatment effect for mothers with at least 16 years of education and the other two coefficients denote the treatment effect relative to higher-education mothers. 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 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).

1.5.2.3 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 the placebo group of EITC-ineligible married mothers with higher-earning spouses (Table 1.3 column 5) as well as women less able to respond to the employment incentives of the EITC: disabled, retired, and full-time students.[37] I estimate the effect on this group by adding a variable to equation (2) that interacts $Mom\ x\ Post1975$ with

[34] Heterogeneous responses by married women are often missed by studies focusing on *average* effects. Eissa and Hoynes (2004, Table 8) and Eissa and Hoynes (2006b) are exceptions. I find similar responses to the 1986, 1993 EITC expansions (Table A1.4). Predicting spousal earnings (based on age, education, race, number of kids, kids under 5) yields a similar estimate and standard error: 0.042 and -0.005.

[35] 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).

[36] The EITC increases the education of children of EITC recipients; there is little evidence that the education of EITC recipients is affected.

[37] Results in Autor and Duggan (2003) and trends in Figure A1.4 suggest that concerns about the endogeneity of disability and education are not a major concern during my sample period.

a binary for being in this "high-impact" group. The two estimates in Table 1.3 column 7 shows that the "high-impact" sample had an EITC response of about 5.9 percentage points (or 9.4 percent).

1.5.2.4 Heterogeneous Treatment Effects: Men

For completeness, I investigate whether the EITC affected 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 1.3 column 8 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.

1.5.3 Triple Differences Corroborate DD Estimates

Splitting the sample into the placebo and "high-impact" groups from Table 1.3 columns 5 and 7 creates a third difference for triple differences (DDD) analysis. DD could be biased if an unaccounted-for event affected the employment of all mothers (discussed in section 1.4.2).[38] The following DDD model uses the full set of controls and extends equation (2).[39]

$$P(E_{ist}) = f(\beta_1 Mom\ x\ Post1975\ x\ Treat_{ist} + \beta_2 X_{ist} + \delta_{st} + \epsilon_{ist}) \tag{3}$$

Table 1.4 shows that the estimate of β_1 is 4.5 percentage points, similar to the DD estimate of 4.0 percentage points, and suggests that policies affecting all mothers (e.g. abortion and divorce laws, birth control) may not pose a threat to my DD estimates. Table 1.4 also shows a similar DDD estimate (4.4 percentage points) when single men from Table 1.3 column 8 are used as a comparison group.

[38] This is a reason Angrist and Pischke (2009, p.182) states that DDD "may generate a more convincing set of results" than DD.

[39] I also control for $Treat$, $Mom\ x\ Treat$, $Post1975\ x\ Treat$, $Mom\ x\ Post1975$, along with interactions of each control with $Treat$ for a more flexible model.

1.5.4 Extensive Margin Results: Annual DD Estimates

Since a DD estimator compares average employment before and after 1975, estimating annual effects of the EITC on maternal employment tests whether the DD results are driven by outliers or a general trend. To estimate a dynamic DD, I replace $Mom\ x\ Post1975$ in equation (2) with $Mom\ x\ Year_y$ where $y \in [1970{,}1985]$. I omit $Mom\ x\ Year_{1975}$ and each estimate measures the effect of being a mother on the probability of working relative to 1975. Using the "high-impact" sample, Figure 1.1B shows that these estimates closely resemble the unadjusted time-series trend. Relative to 1975, the estimates on $Mom\ x\ Year_y$ are jointly insignificant for $y \in [1970{,}1975]$ (p-value 0.56), become increasingly positive for $y \in [1975{,}1979]$, and then remain positive and flat for $y \in [1979{,}1985]$ (statistically identical, p-value 0.16). The 1975-to-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).[40]

1.6 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, largely due to extensive-margin responses. I also explore where in the earnings and work-hours distribution these newly working mothers fall and what quantiles of these distributions are most affected most by the EITC. Many of these mothers had earnings that rendered them EITC-eligible, providing additional evidence that the EITC was behind this increase in working mothers.

[40] This gradual response might be because 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 (Liebman 1998). Even if taxpayers do not fully understand the EITC, those on the margin might try working for one year, discover that they ended up better off, and decide to remain employed. 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 by education subgroup; it does not appear that higher-education mothers responded faster than lower-education mothers (results omitted).

1.6.1 Annual Work Hours and Annual Earnings (Intensive + Extensive Margin Results)

Results above show that the EITC increased the extensive margin labor supply of mothers and imply that earnings and work hours should also have been affected. Results in Table 1.5 use equation (2) and an OLS specification that replaces binary employment with annual work hours and annual earnings (in 2013 dollars) as the dependent variable. For each outcome, I show results for three samples of women: the "high-impact" group (from Table 1.3 column 7), all women (from Table 1.2), and the EITC-ineligible placebo group of married women with spouses earning above the EITC limit (from Table 1.3 column 5). Among the "high-impact" sample the EITC led to increases of 73.7 annual work hours and $1556.6 in annual earnings (Table 1.5 columns 1 and 4). Among the sample of all women, the EITC led to smaller increases in annual work hours (43.1) and earnings ($964.8) (columns 2 and 5).[41] Among the placebo group, columns 3 and 6 show that the EITC had no statistically significant effect on work hours (-5.6) or annual earnings (450.9), which corroborate the placebo test in Table 1.3 column 5.

1.6.2 The EITC and the Distribution of Hours and Earnings

Where in the annual hours and earnings distribution did these newly working mothers enter? To investigate this, I estimate regressions resembling equation (2) but with a binary outcome variable for having annual work hours or earnings within a particular range. Figures 6 and 7 show the DD estimate using 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 to look for evidence of an average intensive-margin response among previously working mothers.

[41] These four estimates represent increases of 9.6, 13.1, 7.3, 9.6 percent. Some individuals had an incentive to decrease labor supply to receive the EITC, but evidence for this has remained largely elusive (Meyer 2002; Saez 2002; Eissa and Hoynes 2006a). Although, Kline and Tartari (2016) use a revealed-preferences approach and find a relatively large intensive-margin response.

For annual work hours, Figure 1.6 shows that the most common response to the EITC was to work full-time, full-year (about 2000 annual hours).[42] Consistent with results above, mothers were about 4 percentage points less likely to work zero hours. Although estimates on positive annual hours below 2000 are not statistically significant, the EITC may also have led to small increases in part-time work. When the sample is restricted to women working, estimates are not significant but suggest that the EITC led to fewer mothers working less than 1000 annual hours and more mothers working more than 1000 annual hours.

For annual earnings, the most common response to the EITC was to earn between $10,000 and $20,000 (2013 dollars), encompassing the most generous portion of the EITC schedule (Figure 1.7). During this period, the minimum wage was between $7 and $9 per hour (in 2013 dollars) and since Figure 1.6 shows that many mothers began working full time, this maps to $14,000 to $18,000 per year. Many newly working mothers received EITC benefits, suggesting the EITC is the reason they began working. Figure 1.7 also shows mothers were 4 percentage points less likely to have zero earnings and may suggest that mothers were slightly more likely to earn between $20,000 and $50,000.[43] 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.

1.6.3 Quantile Analysis

I now characterize the effect of the EITC on the full distribution of annual earnings. I use the regression behind Table 1.5, but instead of average effects I estimate the effect at each centile of the earnings distribution. 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 are effects on quantiles and not on individual women, since it is unclear whether rank preservation holds without imposing strong assumptions or using panel data (see Bitler, Gelbach, and Hoynes (2003) for a discussion). Using the full set of controls and the

[42] Hours suffers from measurement error since I use the midpoint of the categorical *weeks worked* variable (continuous variable not available until 1976 CPS) and since *weeks* refers to last year and *hours* refers to last week. Combining variables reduces noise (Bound, Brown, and Mathiowetz 2001).
[43] Without panel data it is impossible to know whether these women were new labor market entrants.

"high-impact" sample, Figure 1.9 shows that the EITC had the largest effect on the annual earnings of the 44th centile, with a positive but decreasing effect higher up the distribution. The EITC had no effect on lower centiles as these mothers did not work before or after 1975.[44] The EITC's effects varied quite a bit by centile and together drive the average effects in Table 1.5.

1.7 Implied Elasticities

The 1975 EITC led to a 7 percent increase in maternal employment (Table 1.2). Using annual information on taxes and public assistance, I calculate the 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.[45] Across different binary definitions of working, the estimated extensive-margin elasticity ranges from 0.6 and 0.8 (Table A1.1).[46] See Appendix A3 for complete details.

Female labor-supply elasticity has steadily declined since World War II (Goldin 1990). 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; in 1950 Mincer (1962) finds 2.03 and Cain (1966) finds 1.6; 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.[47]

Although my elasticity estimates 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

[44] Qualitatively similar QDD results for annual work hours. The top few centiles are noisy and omitted. Puzzlingly, these figures suggest the EITC also affected the top deciles of each distribution. Although these effects are small (about 1.5 percent since the 90th percentile of annual hours and earnings of mothers in 1975 was 2,040 and $36,000), they may be picking up something else not being controlled for, such as work experience, not available in the CPS, and a reason that the gender wage gap narrowed in the 1980s (O'Neill and Polachek 1993; Blau and Kahn 2000).

[45] 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.

[46] 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 1.6, 1.7, and Table 1.3 column 7), which maps to an elasticity of 0.7 to 0.95 since baseline employment of "high-impact" mothers was 63 percent in 1975.

[47] Mroz (1987) discusses many of these early studies. The 1968-1982 negative income tax experiments yielded elasticities of 0.2 to 0.3 (Burtless and Hausman 1978; Robins 1985).

other estimates of female labor supply elasticity during this time. Furthermore, since elasticities are a function of the tax code (Saez, Slemrod, and Giertz 2012) and vary across groups of people and time, my elasticity estimates reflect a particular population at a particular point in time.

1.8 The EITC and Attitudes Towards Working Women

If the 1975 EITC encouraged a million mothers to begin working, this likely had subsequent effects on the country. There is a large literature showing that the EITC benefited children of EITC recipients (see section 1.2 for a summary). Another effect of the EITC, that has remained understudied, is how this program may have affected social attitudes towards working women.[48]

Google ngrams (Michel et al. 2011) show in the mid-1970s, the phrases *working mom* and the previously redundant *stay at home mom* began to be used much more often (Figure 1.2). This suggests that the rise of working mothers was a salient phenomenon and reflects changes in language and attitudes towards the role of women in society. 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.[49] This increased *exposure* to working women could theoretically have increased or decreased approval of working women ("preferences for gender equality").

An emerging literature shows that gender-equality preferences can be altered via *exposure* to working women. Fernandez, Fogli, and Olivetti (2004) and Olivetti, Patacchini, and Zenou (2016) show that having a working mother – and having friends with working mothers – during childhood leads to stronger gender-equality preferences in

[48] This fits into an economics literature analyzing the role of attitudes and social norms (Becker 1957; Arrow 1971; Akerlof and Dickens 1982; Akerlof and Kranton 2000; Benabou and Tirole 2006). Gender-role preferences are passed on intergenerationally (Fernandez and Alessandra 2009; Alesina, Giuliano, and Nunn 2011; Farre and Vella 2013) and affect female labor market outcomes (Fortin 2005; Charles, Guryan, and Pan 2009; Bertrand, Kamenica, and Pan 2015; Fortin 2015; Pan 2015; Janssen, Sartore, and Backes-Gellner 2016). Unlike these studies, my goal is to characterize a determinant – not a consequence – of these attitudes. There is also a long-standing sociology literature on gender-role attitudes that largely focuses on describing the time trends and correlates of these attitudes (Thornton and Freedman 1979; Thornton, Alwin, and Camburn 1983; Plutzer 1988; Lottes and Kurilo 1992).
[49] Media and television programs have been shown to affect teen pregnancy (Kearney and Levine 2015), divorce (Chong and Ferrara 2009), and fertility (La Ferrara, Chong, and Duryea 2012). See DellaVigna and Ferrara (2015) for a recent survey of how media affects individual decision making.

adulthood.[50] Finseraas (2016) shows that exposure to female colleagues reduces discriminatory attitudes.[51] With these results in mind, the attitudes of millions of Americans may have been affected when the EITC led one million mothers to begin working in the late 1970s.

To determine whether the 1975 EITC affected gender-equality preferences, I carry out the following analysis. First, I test whether states with larger EITC responses experienced larger changes in gender-equality preferences; second, I test for reverse causation and whether state EITC responses can be explained by preexisting preferences for gender equality; third, I test whether preference 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-equality preferences 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 pre1975 state demographic and occupational traits to help alleviate concerns about the endogeneity of preferences and EITC response – is also correlated with changes in gender-equality preferences; and finally, as a check on the mechanism of whether increases in female employment can affect gender-equality preferences, I look at another large increase in female employment – due to World War II mobilization – and test for similar preference changes. Each of these pieces provide suggestive evidence that the 1975 EITC did affect social attitudes towards working women.

1.8.1 Empirical Strategy

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 had larger changes in gender-equality preferences. I define "gender-equality preferences" as approving of the General Social Survey (GSS) question, "Do you approve or disapprove of a married woman

[50] Additional evidence that attitudes can be altered via exposure has also been shown by Finseraas and Kotsadam (2015) (ethnic minorities), Beaman et al. (2012) (female aspirations), Stouffer et al. (1949) (race), and experimental evidence (Heilman and Martell 1986; Lowery, Hardin, and Sinclair 2001; Dasgupta and Asgari 2004). Related to psychology concept of intergroup contact theory (Allport 1954).

[51] Attitude changes consist of individual and intergenerational changes (Firebaugh 1992). Fernandez, Fogli, and Olivetti (2004) and Olivetti, Patacchini, and Zenou (2016) focus on intergenerational change, Finseraas et al. (2016) on individual change. Fernandez, Fogli, and Olivetti (2004, footnote 1) acknowledges the role of individual change: "as more women joined the labor force, attitudes towards these women changed in society at large." My approach captures both channels.

earning money in business or industry if she has a husband capable of supporting her?"[52] The GSS question is consistent over time, begins in 1972 and provides a few baseline years before 1975, and includes a rich set of individual traits. Approval of working women is positively correlated with education, earnings, having a working mother or wife, being younger, unmarried, and white (Table A1.5).

Using restricted GSS data, I aggregate variables to the state level and construct a state panel by pooling years before and after 1975 to increase statistical power. I measure the change in state-level gender-equality preferences after 1975 with the variable $\Delta GenderEquality_s^{(1976-86)-(1972-75)}$. I construct this variable by averaging individual preferences in each state (using sampling weights) in years before 1975 (1972-1975) and after 1975 (1976-1985), and then subtracting the 1972-1975 average from the 1976-1985 average.

I use March CPS data to estimate the following state response to the EITC.

$$P(E_{ist}) = f(\beta_1 Mom_{ist} + \sum_s \beta_{2s}\, Mom\ x\ Post1975_{ist} + \beta_3 X_{ist} + \delta_{st} + \epsilon_{ist}) \qquad (4)$$

This modifies equation (2), uses OLS, and estimates a separate DD (in percentage points) for each state. Since equation (4) uses the full set of controls, this measure isolates the EITC-led increase of working mothers in each state. I rename β_{2s}, $EITC\ Response_s$, and estimate the following regression.

$$\Delta GenderEquality_s^{(1976-85)-(1972-75)} = \gamma EITC\ Response_s + \delta X_s + \epsilon_s \qquad (5)$$

γ measures the effect of a percentage-point increase in state EITC response on the change in 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).[53] Results are similar using either approach. X_s are controls to account for state-level traits and can be measured in pre1975 levels or in the pre1975 to post1975 change.

[52] This question occasionally asked by Gallup beginning in 1936; Figure 1.11 shows approval was under 20 percent in the mid-1930s and grew to more than 80 percent by the mid-1990s.

[53] Equation (5) is a first-difference estimator, which nets out the problem of omitted variables and is unbiased and consistent under the condition $E\left[u_{i,t} - u_{i,t-1} \middle| x_{i,t} - x_{i,t-1}\right] = 0$, which is less restrictive than the assumption of weak exogeneity for unbiasedness when pre1975 and post1975 components are separated in a fixed effects estimator (Wooldridge 2015).

Using equation (5) and no controls, Figure 1.12 shows that each percentage-point increase in state EITC response led to a 1.7-percentage-point increase in male preferences for gender equality (p-value=0.01).[54] In other words, 1.7 percent more of state males approved of working women after 1975 compared to before 1975. Results are similar with region fixed effects and therefore reflect attitude changes within and across regions. Results are also positive and statistically significant, although smaller, for female gender-equality preferences (1.3 percentage points). See Appendix A4 for less parametric approaches.

1.8.2 Reverse Causation: Did Preferences Drive EITC Response?

Perhaps the most obvious threat to my hypothesis that the EITC-led increase in working mothers affected preferences, would be if higher-responding states already had stronger gender-equality preferences before 1975. In Figures 13A and 13B, I follow the approach in Acemoglu, Autor and Lyle (2004) and find an insignificant relationship between state EITC response and both the 1974 *level* of gender-role preferences (p-value=0.27) and the 1972 to 1975 *trend* (p-value=0.60). If anything, the negative point estimate in each figure suggests that higher-responding states had *lower* gender-equality preferences, perhaps suggesting the EITC led to an attitude "catch up" among states with lower gender-equality preferences.[55]

1.8.3 Subgroup Analysis

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-education mothers (Table 1.3 column 6),

[54] Robust to using OLS, probit, or logit to estimate equation (4). Results unweighted, but robust to weighting by state population or the inverse of the standard error in equation (4). The interquartile effect is 5.8 percentage points, comparable to two more years of education or being born a decade later, but less than having a working wife or having racial-equality preferences (Table A1.5).

[55] If states that voted for the 1975 EITC benefited the most from it, then perhaps the EITC was the outcome, not the cause, of changing attitudes. To test this, I regress state EITC response on the fraction of state congressmen (Senators and House Representatives) that voted for the 1975 EITC legislation. (Of course, the Tax Reduction Act of 1975 contained other spending and tax provisions: https://www.gpo.gov/fdsys/pkg/STATUTE-89/pdf/STATUTE-89-Pg26.pdf.) Figure A1.7 shows that, in fact, the opposite is true: states voting against the EITC had higher EITC responses. This suggests that preference changes were larger in places less likely to be in favor of a social program like the EITC.

lower-education males would have been more likely to know these women. Figures 1.14A and 1.14B re-estimate equation (5), but divide the sample into men with more or less than 12 years of education. For lower-education men, each percentage-point increase in the state EITC response led to a 2.8-percentage-point increase in gender-equality preferences (p-value=0.01), whereas for higher-education males the estimate is a statistically insignificant 1.0 percentage points (p-value=0.63).[56] This suggests that people most likely to know these newly working women did have larger preference changes.

1.8.4 Controlling for Demographics and Other Social Attitudes

Results above reflect bivariate regressions with no controls. A threat to my hypothesis would be if changes in gender-equality preferences coincided with demographic changes or changes in other social attitudes unrelated to working women, implying that an omitted trend unrelated to the EITC is driving the correlation in Figure 1.12. To test this, I re-estimate equation (5) with controls for state-level education, marriage, age, race, male employment and earnings, parental employment and education,[57] political and religious affiliation, views on public assistance, and racial-equality attitudes. Panel A uses pre1975-post1975 changes and Panel B uses pre1975 levels.[58] Across controls, the effect is stable between 1.5 and 2.0 percentage points. Changes in gender-equality preferences do not seem to be driven by state demographic traits or general trends in social attitudes.

1.8.5 Placebo Outcome: Changes in Racial-Equality Preferences

Since gender- and racial-equality preferences were correlated with the same traits (Table A1.5 Panels A and B), it is conceivable that an omitted policy or trend – other than the EITC – could have been driving changes in various types of attitudes. One way to test for this is to use racial attitudes (GSS question: *Would you vote for a black president?*) as a control variable, as

[56] I test how likely this correlation is due to chance with a variant of the permutation test in Buchmueller, DiNardo, and Valletta (2011): I randomly reassign a new attitude change to each state (with replacement) from the set of all state attitude changes, re-estimate equation (5), record the estimate of γ, and iterate 1000 times. Figure 1.15 shows the distribution of these point estimates; the actual estimate in Figure 1.14A is in the top 0.01 percent of permutations and unlikely to have occurred by chance.

[57] Fernandez, Fogli, and Olivetti (2004) shows that mother's employment affects gender-role attitudes.

[58] Table A1.4 shows summary statistics for these variables. Appendix A5 details data and variables.

done in Table 1.6 column 8. Another approach is to use racial-equality preferences as a placebo outcome. Figure 1.16 shows that the relationship between state EITC response and changes in racial-equality preferences after 1975 is not statistically significant (p-value 0.56) and small effects can be ruled out. States with high EITC responses were not simply experiencing changes in various attitudes unrelated to working women.[59]

1.8.6 Using 2SLS and Predicted State EITC Responses from Pre1975 Traits

Results above show the relationship between state response to the 1975 EITC and changes in gender-equality preferences. However, if these *ex post* EITC responses were in part driven by preexisting state differences in gender-equality preferences, then my argument that the EITC affected attitudes would be somewhat circular. Although Figures 1.13A and 1.13B provide evidence that gender-equality preferences before 1975 did not determine state EITC response, another approach is to *predict* state EITC response by exploiting pre1975 state traits ($X_s^{pre1975}$) and the heterogeneous responses detailed in Table 1.3, and use two-stage least squares (2SLS) to test whether *predicted* EITC responses are associated with preference changes.

To show that *predicted* state EITC response affected preferences, four conditions should be met. First, the reduced-form version of the two-step regression: $X_s^{pre1975}$ should be correlated with $\Delta GenderEquality_s^{(1976-85)-(1972-75)}$. Second, $X_s^{pre1975}$ should be correlated with $EITC\ Response_s$. This is the first stage in the 2SLS approach where I generate $\overline{EITC\ Response}_s$. Third, the second stage of the 2SLS regression, $\overline{EITC\ Response}_s$ should be correlated with $\Delta GenderEquality_s^{(1976-85)-(1972-75)}$ and can be interpreted as the effect of an additional exogenous percentage-point increase in maternal employment on gender-equality preferences.[60] Fourth, $X_s^{pre1975}$ should not be correlated with changes in gender-equality preferences before 1975. Conditions one and four together suggest that $X_s^{pre1975}$ only affected preferences indirectly through state EITC response.

[59] Another placebo test is to regress changes in preferences – using placebo years between 1985 and 1995 as cutoffs instead of 1975 – on state response to the 1975 EITC (Figure A1.8).
[60] A complementary approach saves the residuals from the regression of $EITC\ Response_s$ on $X_s^{pre1975}$, regress attitude changes on these residuals, and show that the correlation is zero (results omitted).

Figure 1.17 shows that these four conditions are met using state-level measures of female education, which has a strong, negative correlation with individual EITC response (Table 1.3). Figure 1.17 Panel A shows that state-level female education pre1975 is negatively correlated with preference changes after 1975 (p-value=0.08). Panel B shows that average female education is highly correlated with the state EITC response (p-value=0.001) and illustrates the best-fit line used to generated predicted state EITC response; Panel C shows that predicted EITC response is positively correlated with changes in gender-equality preferences after 1975 (p-value=0.06). Finally, estimates are imprecise, but Panel D shows an insignificant correlation between female education and preference changes before 1975 (p-value=0.54). The main 2SLS result in Panel C shows that predicted EITC response is associated with increases in approval of working women.

In Table A1.7, I repeat the exercise in Figure 1.17 for various other pre1975 state-level demographic and occupational traits.[61] Across traits, the effect of a percentage point of predicted EITC response on gender-equality preferences ranges from 0.02 to 0.07, but not all estimates are statistically significant. Interestingly, when region fixed effects are added (Table A1.7 Panel B) the estimates become more precise, between 0.02 and 0.03. In general, both *actual* and *predicted* state EITC responses indicate that the EITC positively affected gender-equality preferences.

1.8.7 External Validity: Female Labor Supply During WWII

If the EITC-led increase in working women affected attitudes towards working women, then the same pattern should exist during other periods of large, sudden increases in female employment. During World War II, more than 7 million women began working – compared to a total of about 14 million women working in 1940 – to make up for the 14 million men that joined the military.[62] The number of women that began working was higher in places with higher mobilization rates (Acemoglu, Autor and Lyle 2004; Goldin and Olivetti 2013b).[63]

[61] Demographic traits (columns 1-6) include female education, white and nonwhite single mothers, fraction females, male earnings, and males not in the labor force. Occupational traits (column 7-11) include the fraction of state jobs that are teachers and librarians; housekeepers and cleaners; bakers and food makers; male metal and wood workers; and male construction workers. Occupations suggest how easy (or appealing) it would be for women to work in these states. (Figure A1.8 motivates why I chose these specific occupations by showing which professions these newly working mothers entered into.) To conserve space, I just show the second stage of the 2SLS estimates (corresponding to Figure 1.17 Panel C).

[62] LIFE magazine (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

Testing whether states with higher mobilization rates experienced larger increases in gender-equality preferences is possible since Gallup began asking questions related to gender equality in the 1930s and identifies individuals by state. I follow the approach in equation (5), but I construct a state panel on preferences before and after WWII (instead of before and after 1975) and use WWII mobilization rates as the treatment variable (instead of state EITC responses).[64]

Figure 1.18 resembles the scatterplot in Figure 1.12 and shows a positive correlation between mobilization rates and changes in gender-equality preferences after WWII (p-value = 0.003).[65] This provides corroborating evidence that increases in working women can affect attitudes about the role of women in society.

1.9 Future Work

There are two main factors that merit further investigation. One, determine why the higher earnings-quantiles in Figure 1.6 appear to show an increase in earnings due to the EITC. If this cannot be accounted for, then perhaps this is driven by unobservables and could imply that these intensive-margin results should be scaled down. Two, calculate more precisely the elasticity calculated in Table A3.1 by determining the increase in employment for the representative mother and the public benefits available for this mother if she works and if she does not work.[66]

1.10 Summary

In one of the first systematic studies of the 1975 introduction of the EITC, I show that this program led to a 7 percent rise in maternal employment. Regression-adjusted and unadjusted

now employed in the U.S., over a quarter are in war industries." Mobilization began on 09/16/1940 with the Selective Training and Service Act. See Goldin and Olivetti (2013a) for details.

[63] Mobilization rates from Goldin and Olivetti (2013) Table A1. Goldin and Olivetti (2013a) shows that about two-thirds of mobilization rates can be explained by exogenous state-level factors.

[64] I focus on attitudes and mobilization of white adults because WWII mobilization had a larger effect on white women. As Goldin and Olivetti (2013) state, "black women's [labor force] participation was high before the war and many were in agricultural occupations." See Figure 1.18 notes for more details.

[65] WWII mobilization rates are not correlated with state responses to the 1975 EITC (p-value=0.38).

[66] I am grateful to my dissertation committee for pointing out these issues during my dissertation defense.

time-series trends show that the relative employment of mothers began to increase after 1975 (Figures 1A and 1B). Consistent with the EITC being responsible for 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. Using 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) yields similar estimates. In hindsight, the employment effect of the 1975 EITC should not be surprising: female labor-supply elasticity was large during this period (Blau and Kahn 2005; Heim 2007) and the 10-percent wage subsidy of the EITC represented a large year-over-year increase in potential earnings.[67]

The 1970s also provides a remarkably 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; the 1990s EITC expansion coincided with reductions in welfare and the Family Medical Leave Act, which increased maternal employment (Hoynes 1996; Ruhm 1998; Moffitt 1999).

This large increase in working mothers likely had subsequent effects on the country: I also find that the EITC-led influx of working mothers affected attitudes about the role of women in society. States with larger EITC responses – and larger *predicted* responses due to pre1975 demographic and occupational traits – had larger increases in attitudes approving of women working. Results do not appear to be driven by pre1975 attitudes, changes in demographics, or general trends in social attitudes. As for external validity, I find similar attitude changes due to the large increase in female employment during World War II. Although social attitudes about women working and women working are intertwined, I use two episodes of largely exogenous increases in female employment to show that increases in working women affected attitudes towards working women. The 1975 EITC played an important role in the rise of working mothers and in fostering egalitarian social attitudes.

[67] 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, after 1970, the growth rate of female labor supply rose as the real-wage growth rate fell (Parkman 1992).

1.11 Works Cited

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1.12 Figures and Tables

Figure 1.1A: Unadjusted Employment Trends for Mothers and Women without Kids

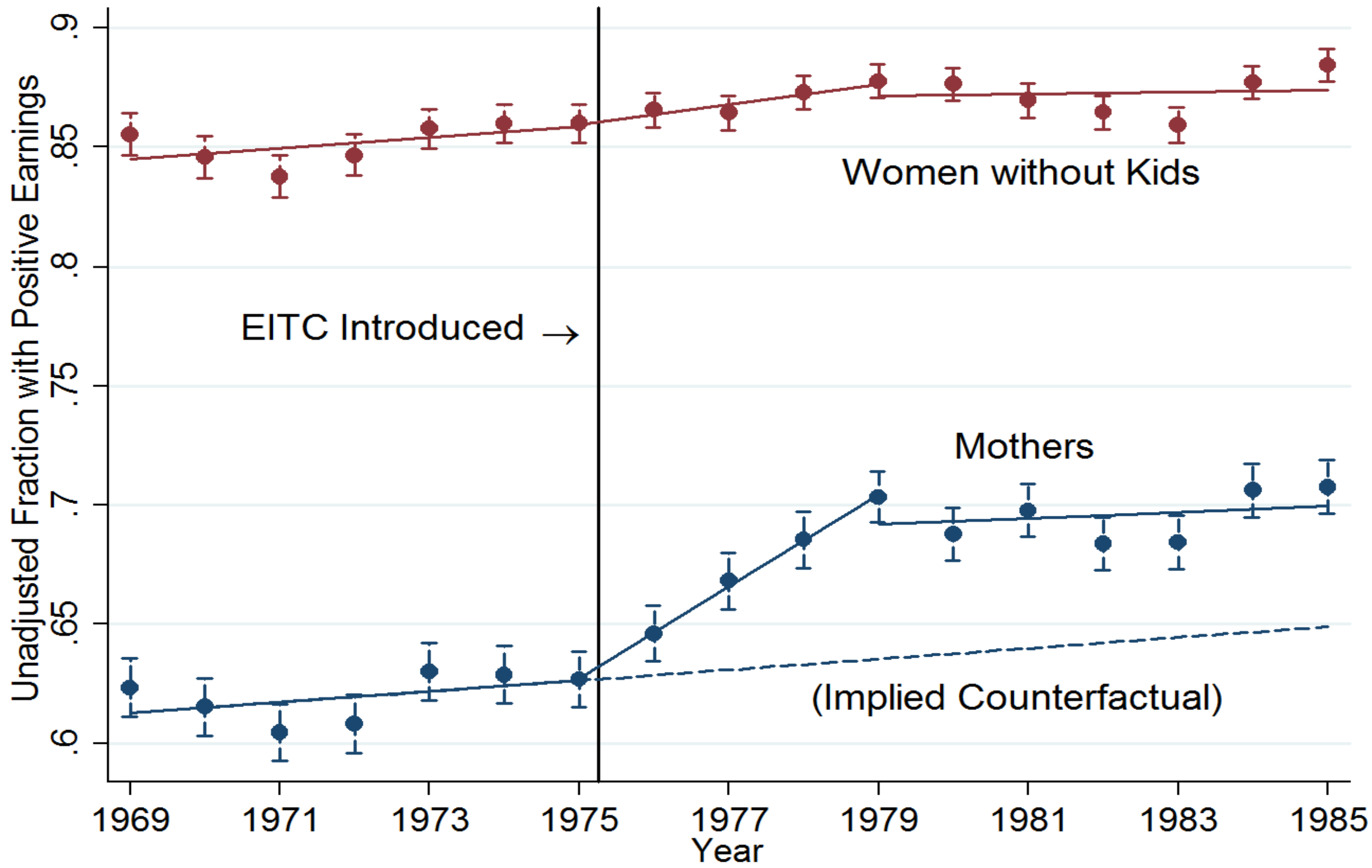
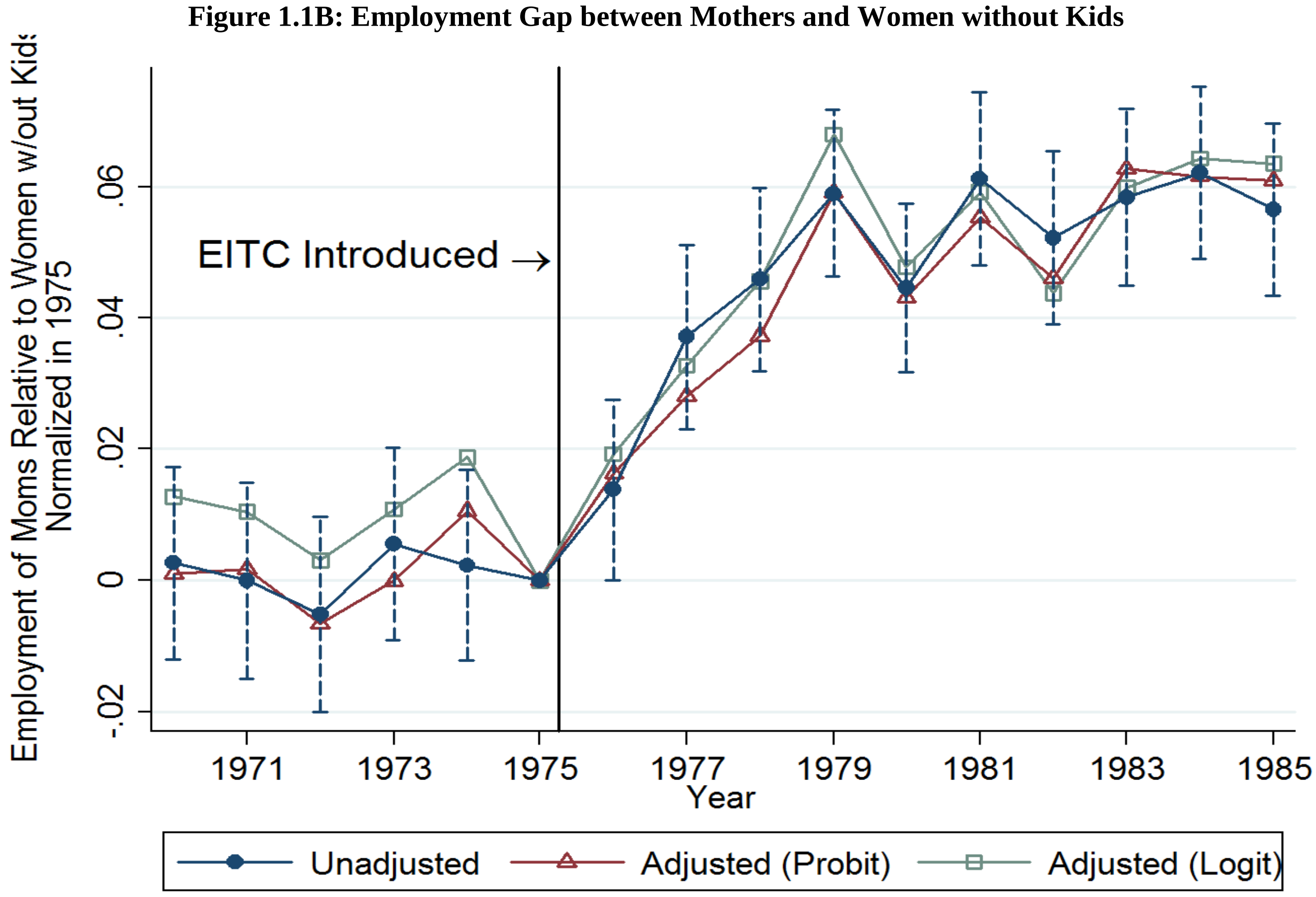
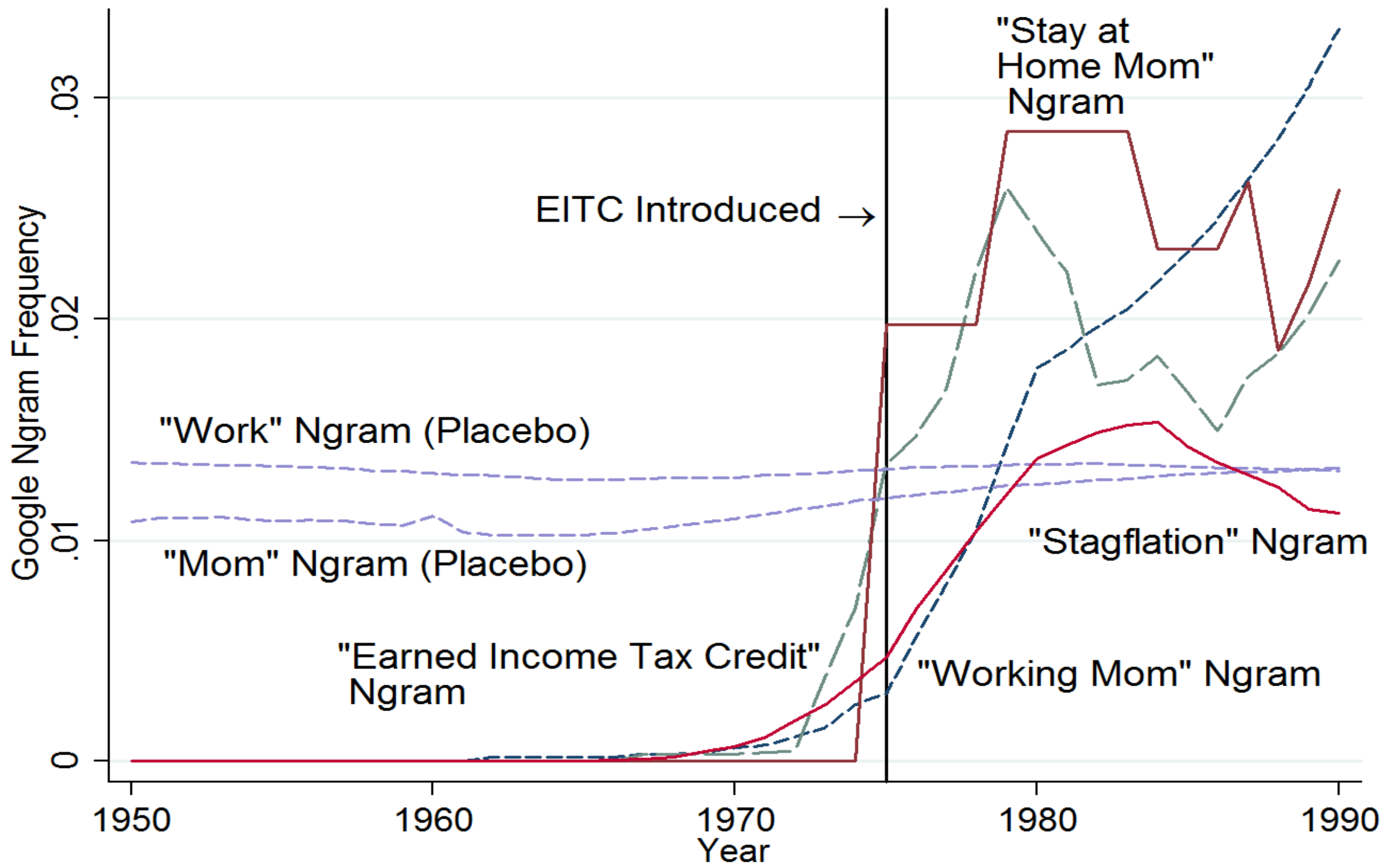


Figure 1.1B: Employment Gap between Mothers and Women without Kids



Notes: 1970-1986 March CPS data. Employment defined as having positive income. Estimates and standard errors in Panel A calculated by regressing employment on a constant for each group-year. Best fit lines are shown for 1969-75, 1975-79, 1979-85. In Panel B, 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 *Mom x Year* with the set of controls from Table 2 column 4. The estimates on *Mom x 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). The sample includes all women 16-45 and excludes married women with spousal earning above the 1975 EITC limit of $36,000 (in 2013 $) and also excludes full-time students, disabled, and retired. Sample is the "high-impact" sample in Table 3. Kids are defined as 0-18 years old.

Figure 1.2: 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. Source:
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%2Cc0,
https://books.google.com/ngrams/graph?content=earned+income+tax+credit&year_start=1950&year_end=1990&corpus=15&smoothing=3&share=&direct_url=t1%3B%2Cearned%20income%20tax%20credit%3B%2Cc0, and
https://books.google.com/ngrams/graph?content=stay+at+home+mom%2Bstay+at+home+moms%2Bstay+at+home+mother&year_start=1950&year_end=1990&corpus=15&smoothing=4&share=&direct_url=t1%3B%2C%28stay%20at%20home%20mom%20%2B%20stay%20at%20home%20moms%20%2B%20stay%20at%20home%20mother%29%3B%2Cc0,
https://books.google.com/ngrams/graph?content=working%2Bwork&year_start=1950&year_end=1990&corpus=15&smoothing=4&share=&direct_url=t1%3B%2C%28working%20%2B%20work%29%3B%2Cc0,
https://books.google.com/ngrams/graph?content=mom%2Bmother%2Bmoms%2Bmothers&year_start=1950&year_end=1990&corpus=15&smoothing=4&share=&direct_url=t1%3B%2C%28mom%20%2B%20mother%20%2B%20moms%20%2B%20mothers%29%3B%2Cc0. Accessed 9/5/16.

Figure 1.3A: Budget Constraint Under the 1975 EITC

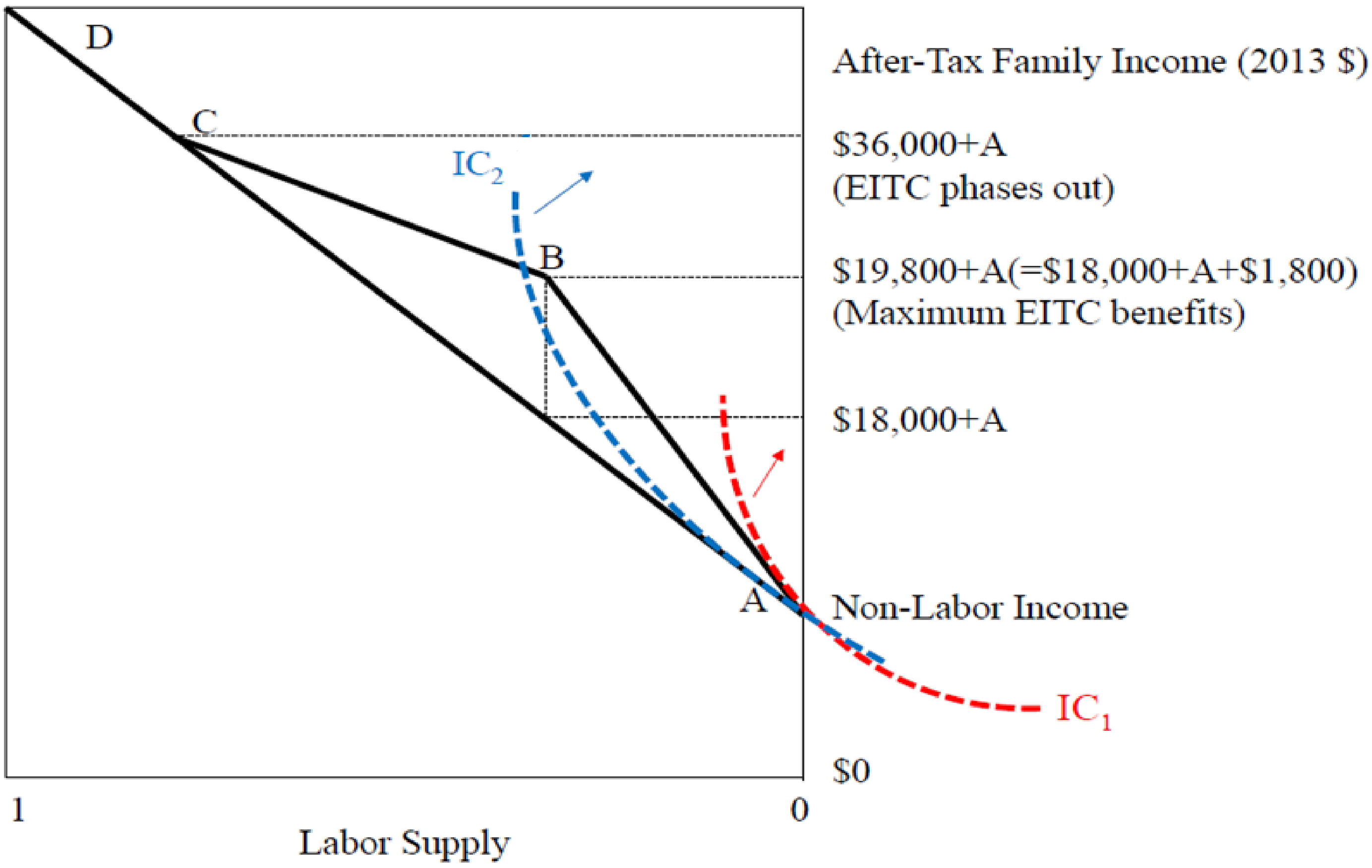
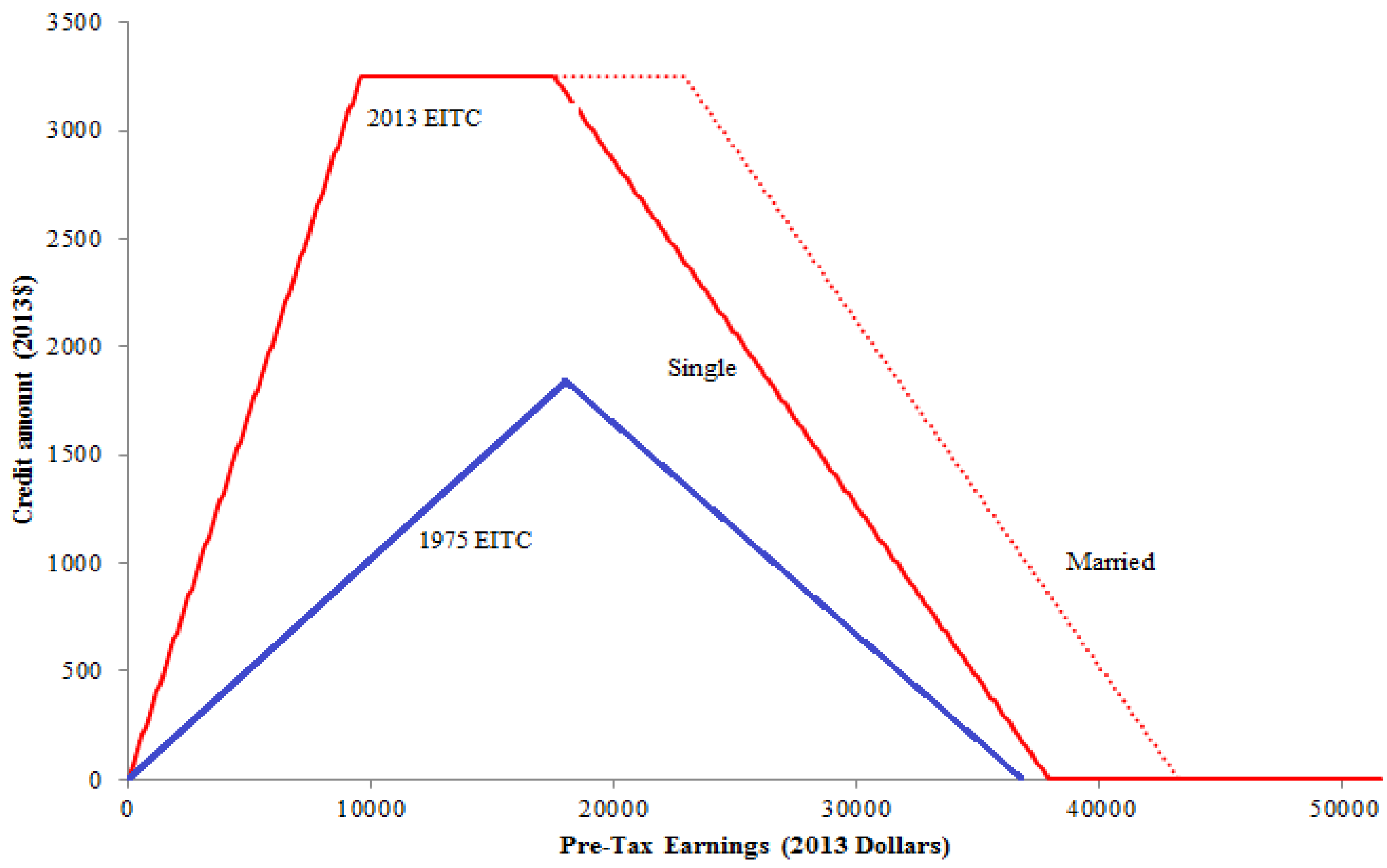


Figure 1.3B: Comparing 1975 and 2013 EITC Schedules for Households with One Eligible Child

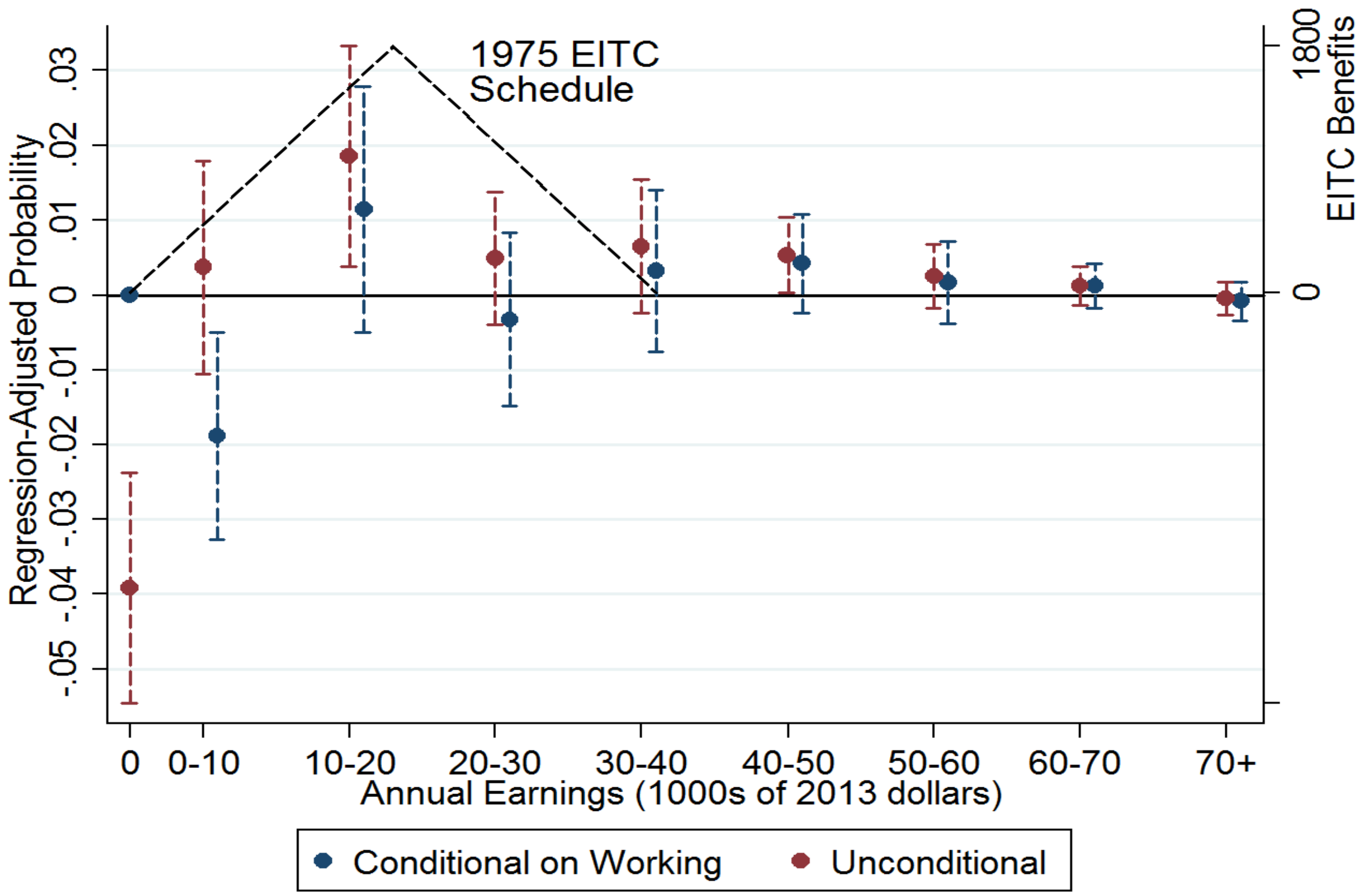


Notes: Author's calculation from 1975 and 2013 EITC parameters. 1975 EITC phased in and out at 10 percent. 2013 EITC for one child phased in and out at 34 and 15.98 percent. The following is an abbreviated history of 1975-2013 changes to the EITC schedule: 1979, a plateau region was added to the EITC 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.

Figure 1.4: EITC Effect on Annual Work Hours Distribution

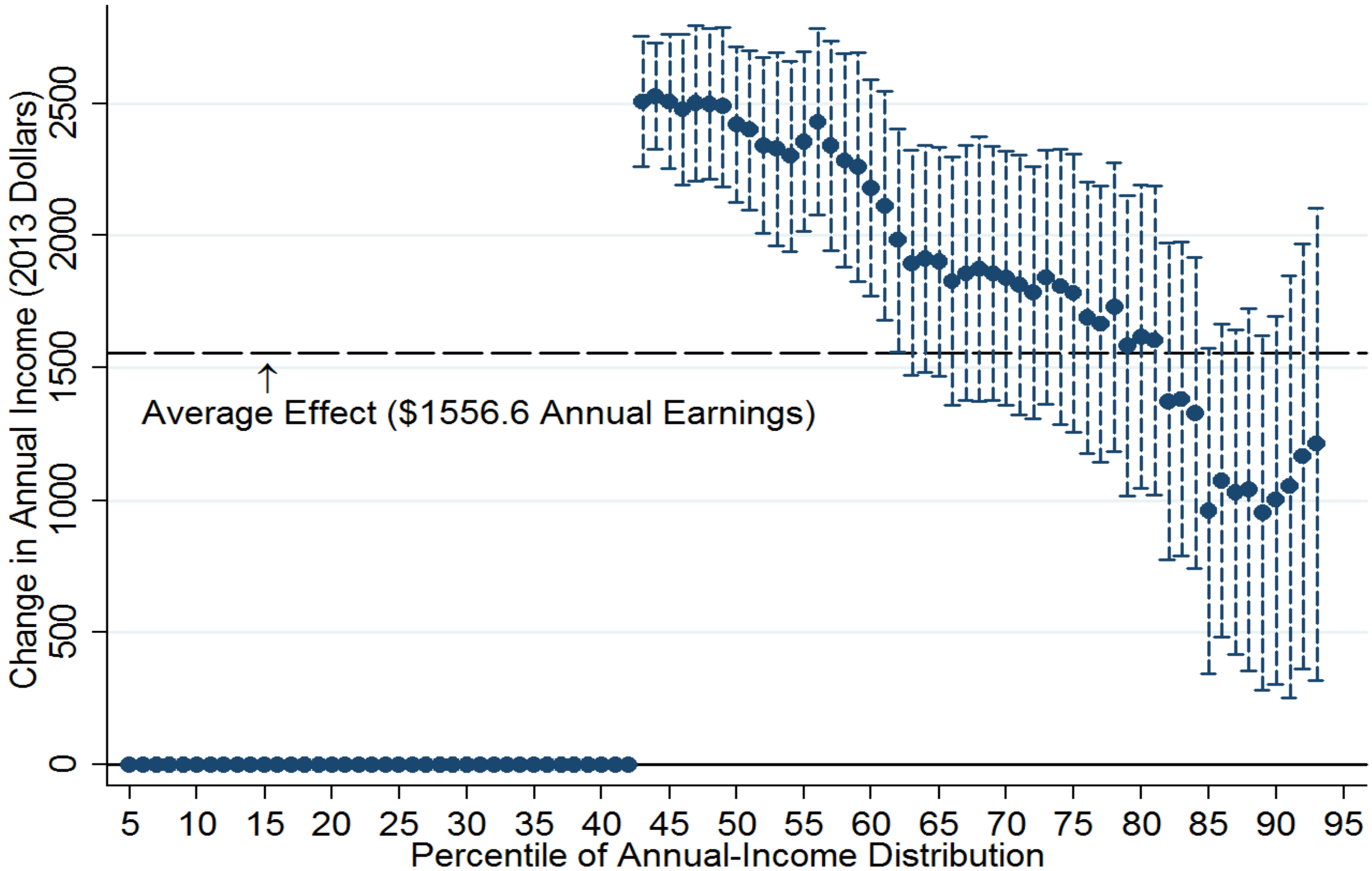
Notes: 1971-1986 March CPS data. Set of controls from Table 2 column 4 and "high-impact" sample used. Each estimate is from a different logit regression of having annual work hours in the specified range. The mean dependent variable for the seven unconditional regressions are 0.35, 0.11, 0.09, 0.08, 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.

Figure 1.5: EITC Effect on Annual Earnings Distribution



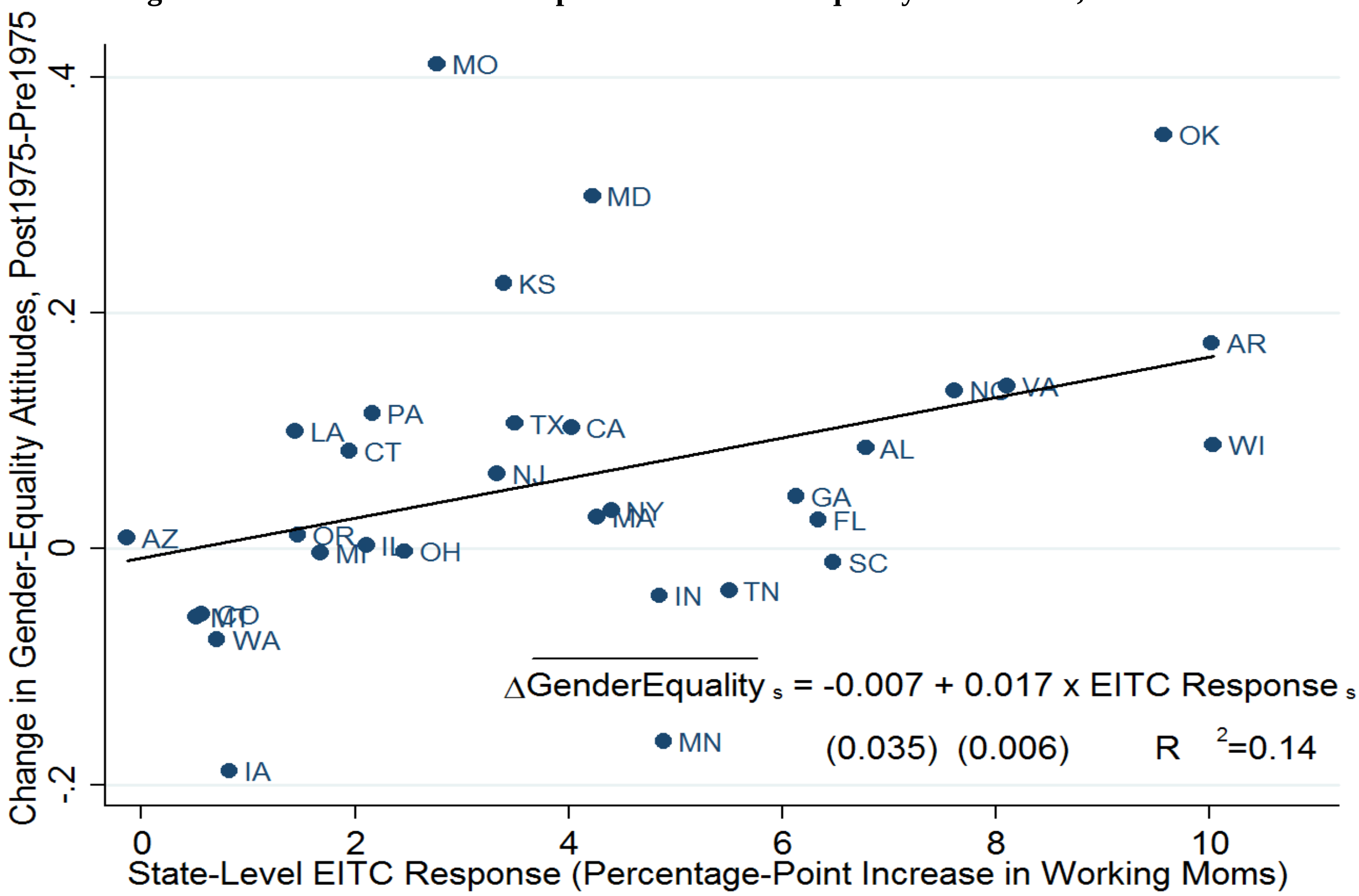
Notes: 1971-1986 March CPS data. Set of controls from Table 2 column 4 and "high-impact" sample used. Each estimate is from a different logit regression of having annual earnings in the specified range. The mean dependent variable for the nine unconditional regressions are 0.25, 0.27, 0.15, 0.13, 0.10, 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.

Figure 1.6: The 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 4 column 1 except instead of average effects results shown are the effect of *Mom x 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, 9, and 10 since some observations report earnings or hours but not both and imputed hourly wage requires both. Standard errors are computed by the delta method, robust to heteroskedasticity, and clustered at the state level.

Figure 1.7: State-Level EITC Response and Gender-Equality Preferences, 1974-1986



Notes: 1972-1985 restricted GSS data with state-level identifiers. Egalitarian gender-role 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. Change in egalitarian gender-role 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 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.

Figure 1.8A: Placebo Test: EITC Response Uncorrelated with 1974 Gender-Equality Attitudes

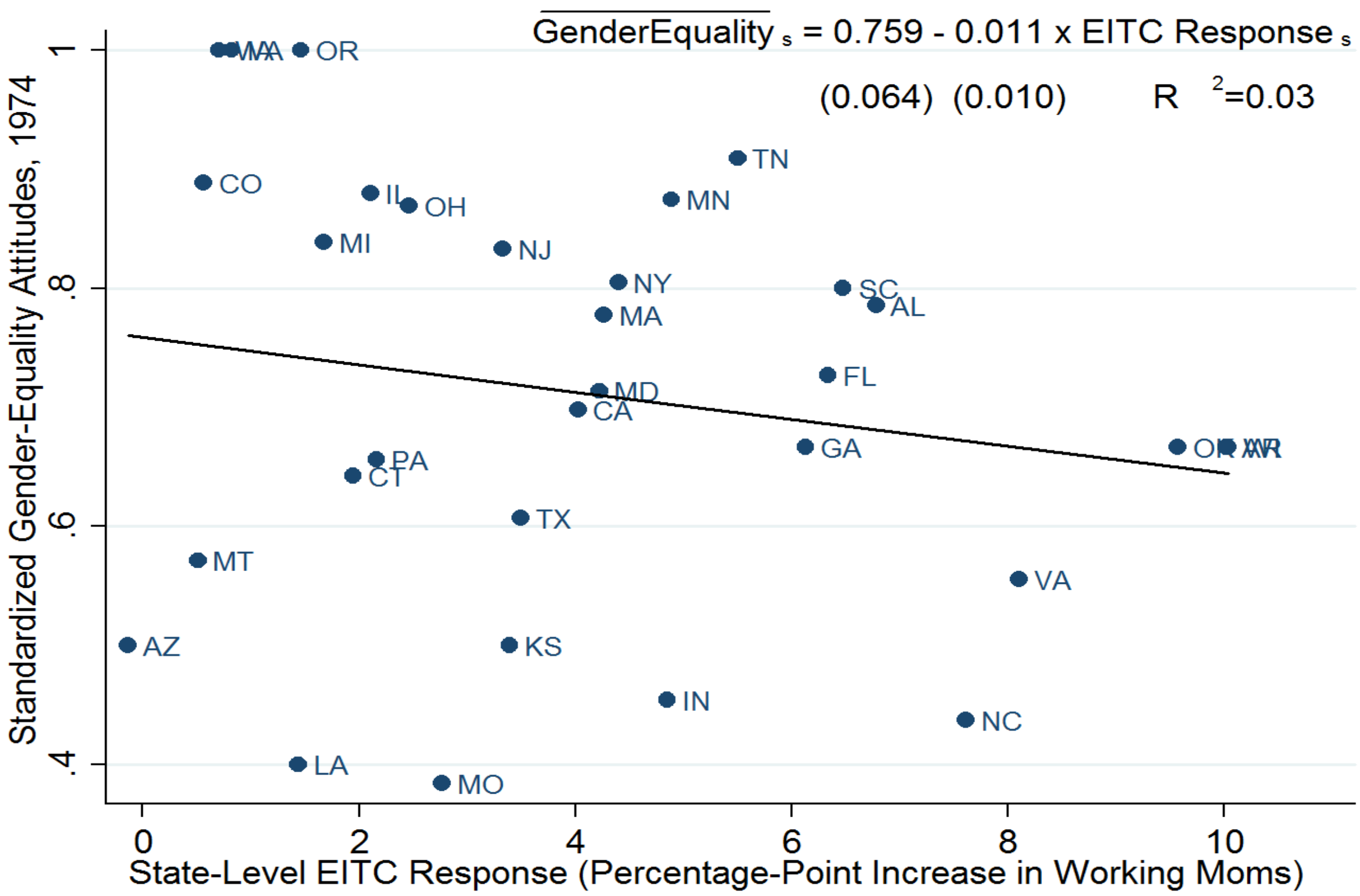
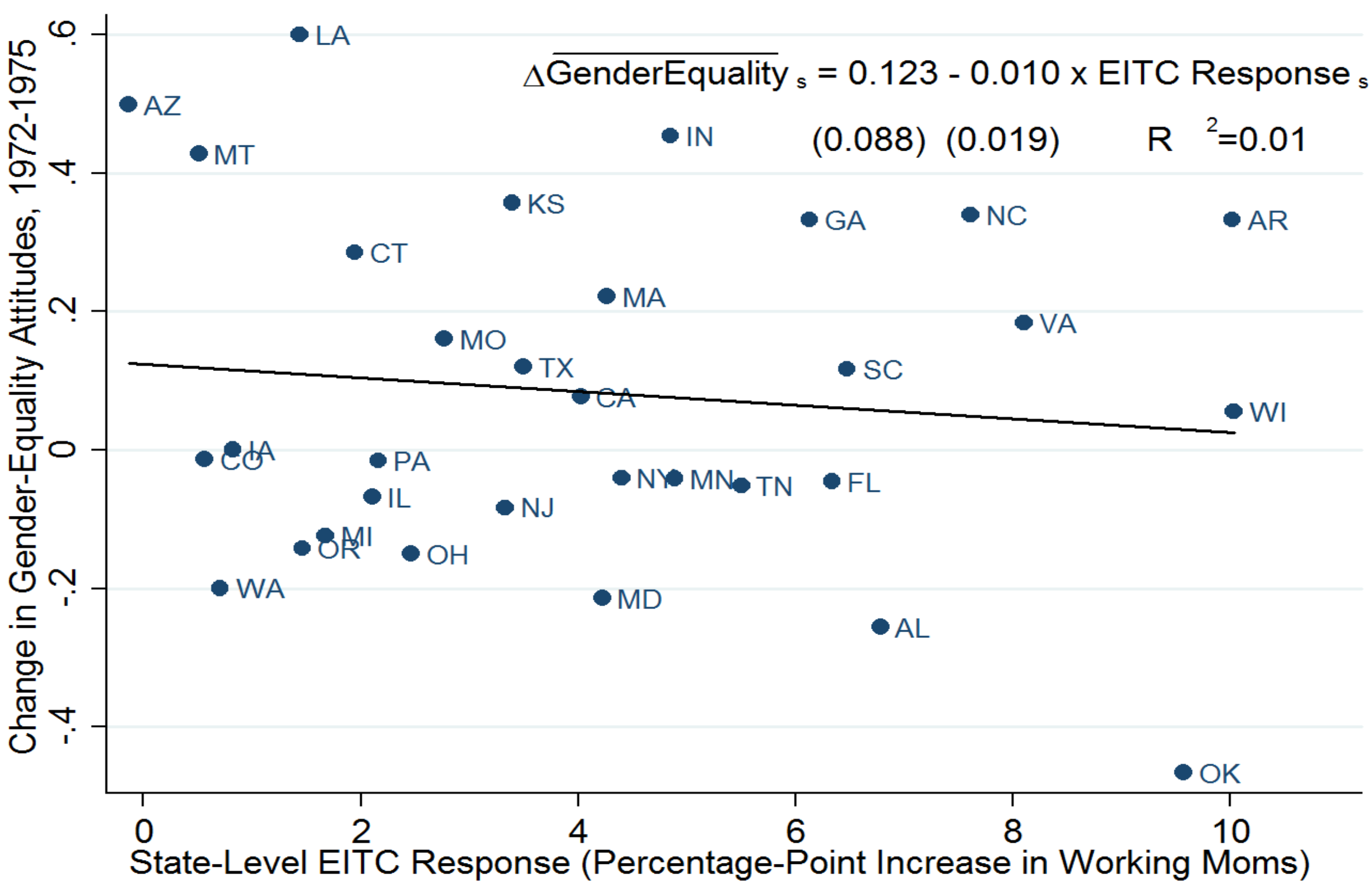


Figure 1.8B: Placebo Test: EITC Response Uncorr. with 1972-75 Gender-Equality-Attitude Trend



Notes: 1972-1985 restricted GSS data with state-level identifiers. Egalitarian gender-role 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.

Figure 1.9A: Larger Effect of EITC on Attitudes of Lower-Education Males

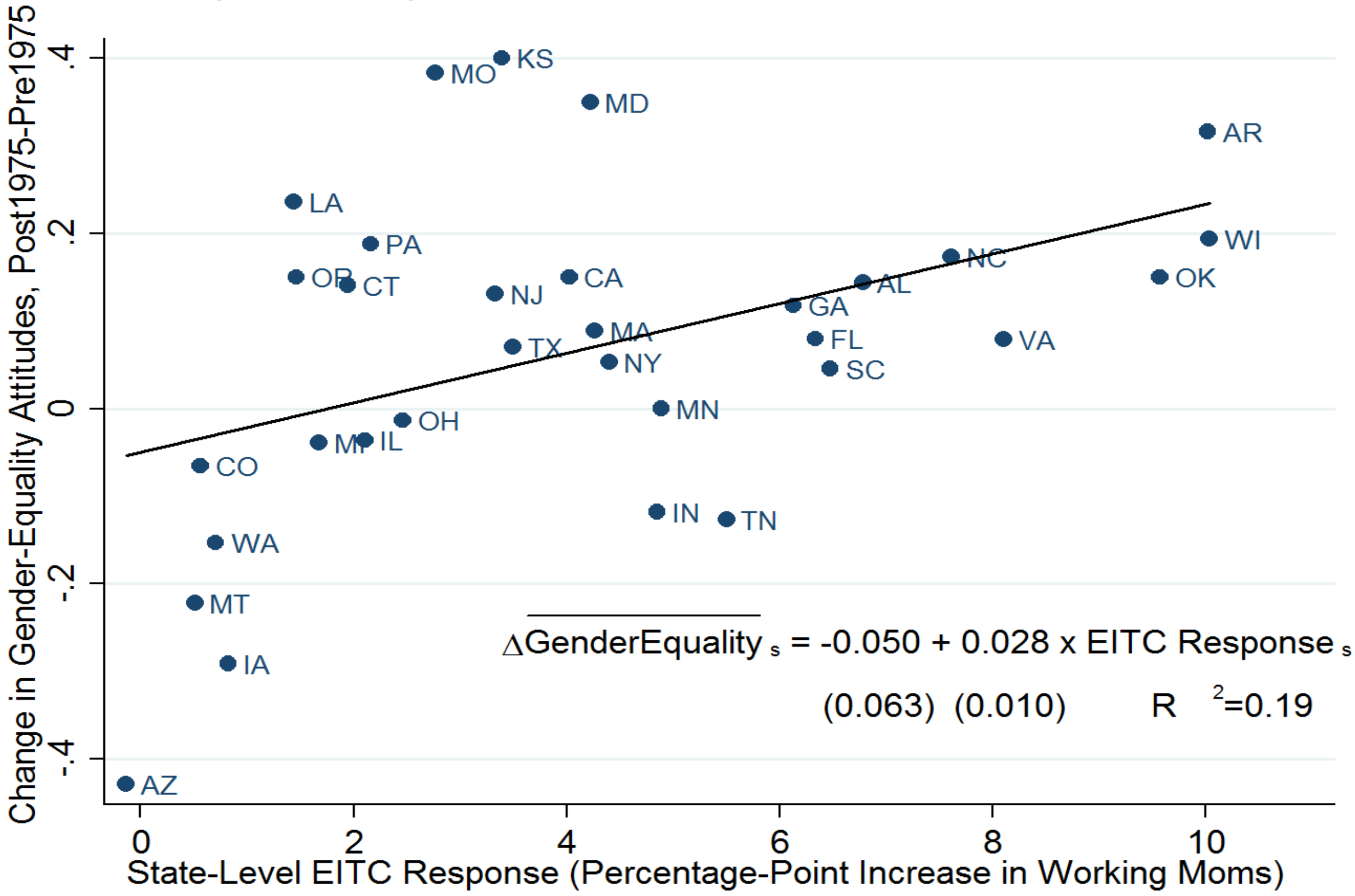
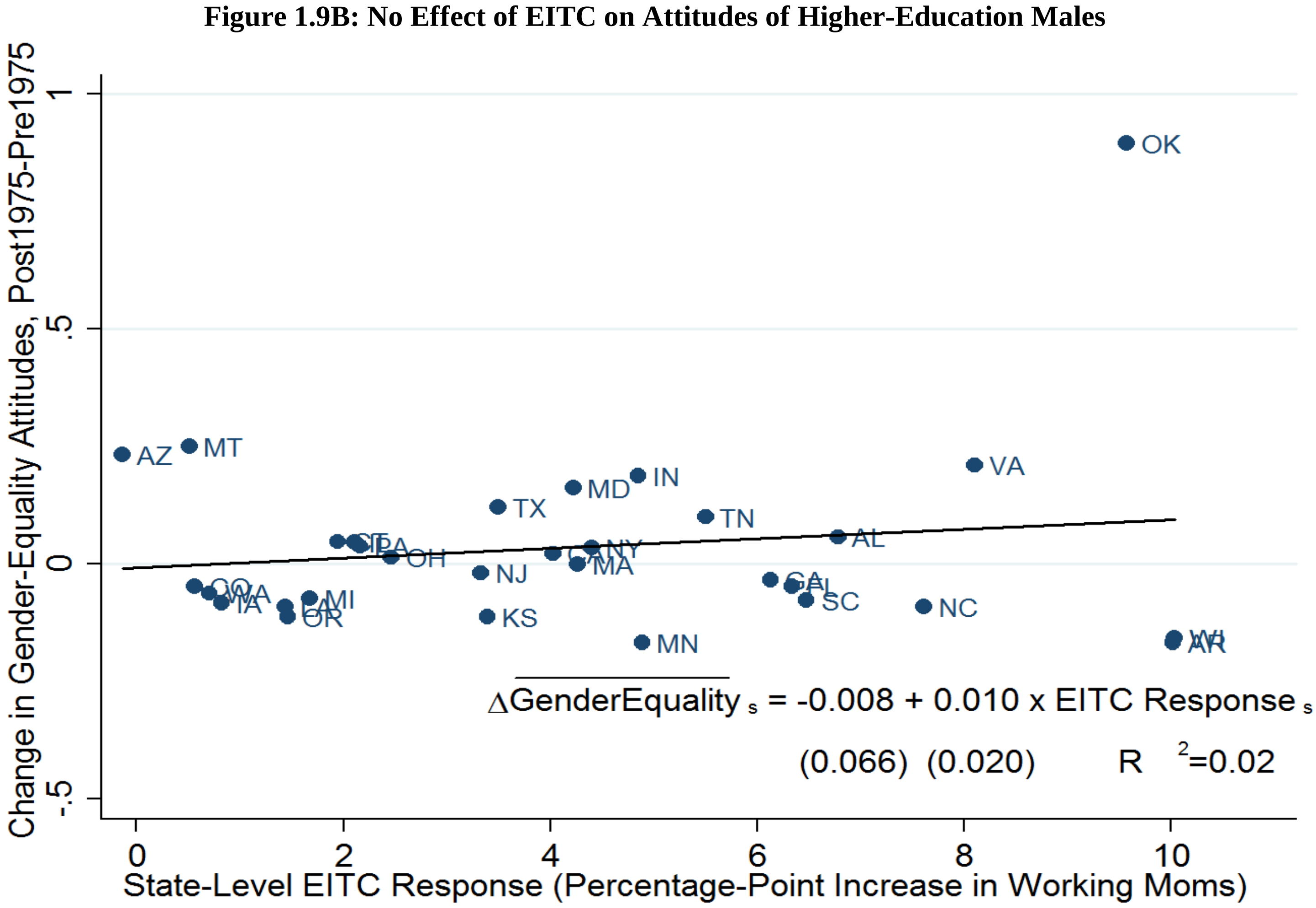
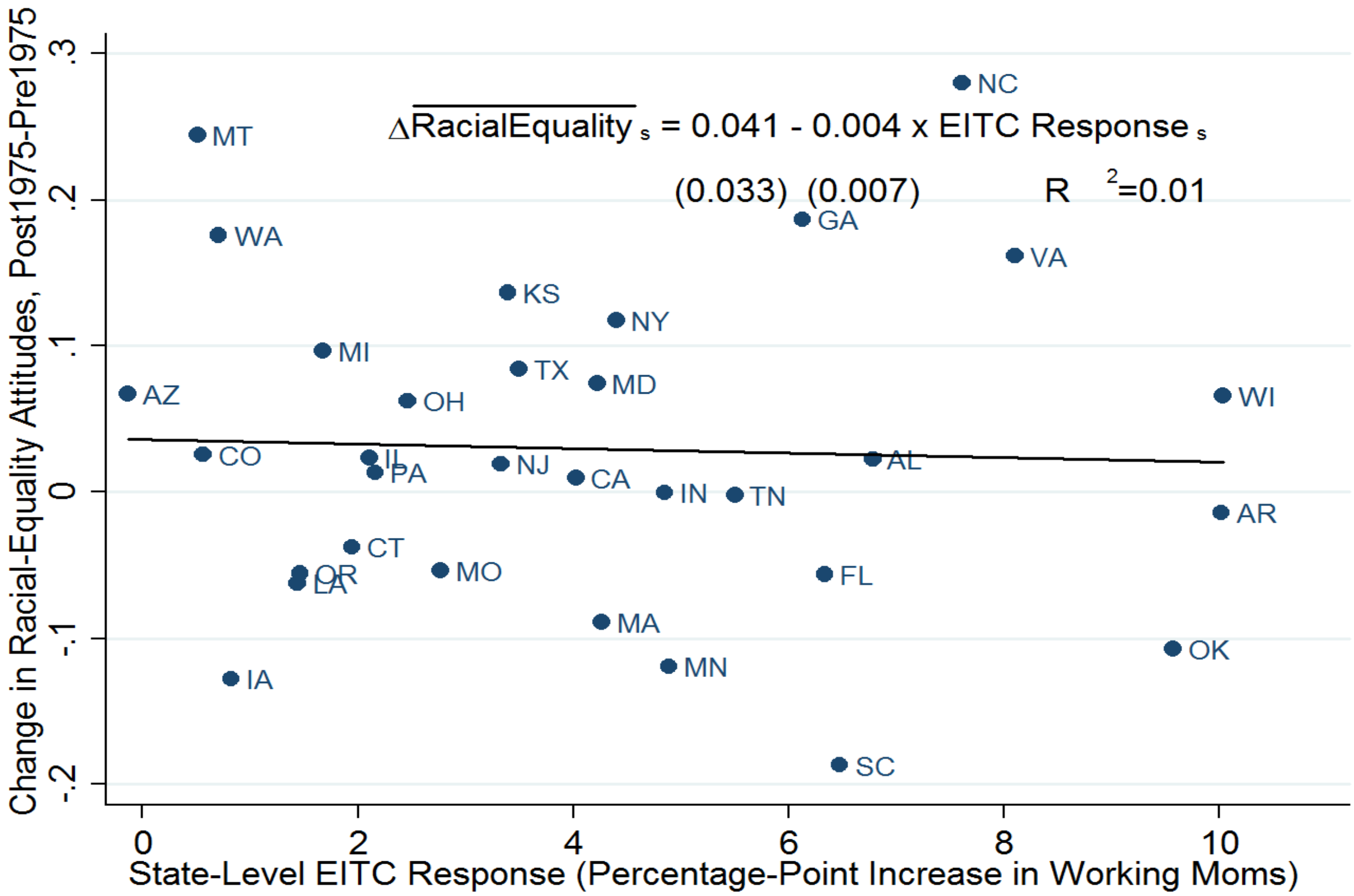


Figure 1.9B: No Effect of EITC on Attitudes of Higher-Education Males



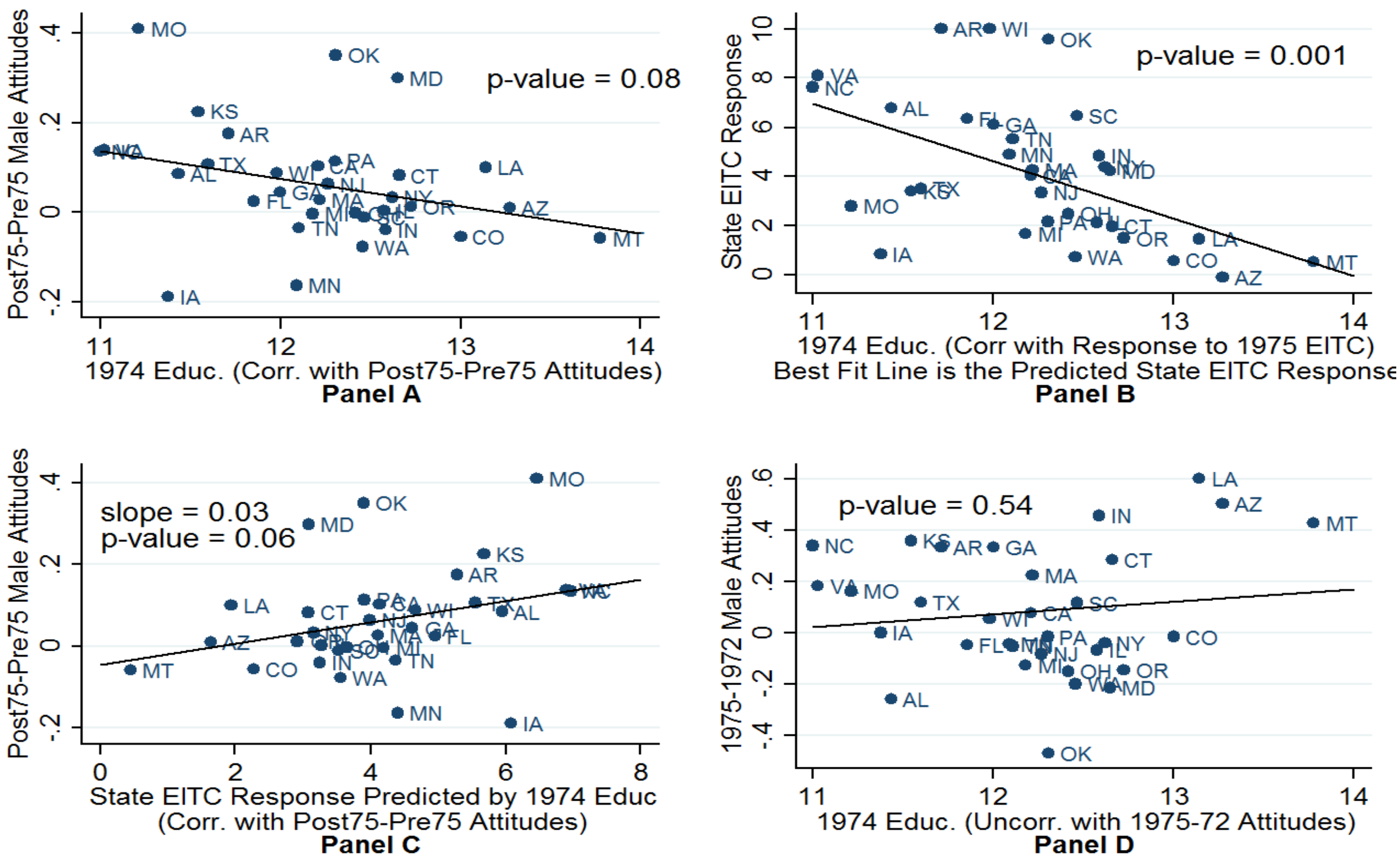
Notes: 1972-1985 restricted GSS data with state-level identifiers. Lower education males in Panel A have 12 or less years of education, higher education males in Panel B have at least 13 years of education. Egalitarian gender-role attitudes constructed from the GSS variable fework. GSS sample reflects males 18 to 60 years old. GSS sample contains individuals from 32 states during the 1970s. Change in egalitarian gender-role 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 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.

Figure 1.10: Placebo Attitude on Racial Equality Uncorrelated with State EITC Response



Notes: 1972-1985 restricted GSS data with state-level identifiers. Egalitarian racial 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.

Figure 1.11: Predicted EITC Response (from 1974 Female Educ) and Post1975-Pre1975 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 13), 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 continuiation of a pre-existing trend.

Table 1.1: Summary Statistics

	All Women	Mothers	Women without Kids
Variable	(1)	(2)	(3)
Age	29.0	32.9	24.0
	(8.4)	(6.9)	(7.5)
Years of Education	12.1	12.1	12.3
	(2.5)	(2.5)	(2.6)
Black	0.12	0.12	0.12
	(0.33)	(0.32)	(0.33)
Hispanic	0.06	0.07	0.05
	(0.24)	(0.25)	(0.22)
Kids Under 5	0.25	0.45	0.00
	(0.43)	(0.50)	(0.00)
Number of Kids	1.25	2.24	0.00
	(1.44)	(1.24)	(0.00)
Employed	0.65	0.58	0.75
	(0.48)	(0.49)	(0.43)
Individual Earnings (2013 $)	$13,028	$11,620	$14,808
	(17,007)	(16,225)	(17,789)
Individual Earnings (2013 $)	$19,960	$20,177	$19,750
(Conditional on Earnings > 0)	(17,458)	(16,866)	(18,012)
Household Earnings (2013 $)	$41,268	$53,474	$25,825
	(39,822)	(40,422)	(33,137)
Household Earnings (2013 $)	$48,755	$59,926	$32,758
(Conditional on Earnings > 0)	(38,840)	(38,005)	(34,143)
Household Earnings Below EITC Limit	0.46	0.30	0.66
	(0.50)	(0.46)	(0.47)
Household Earnings Below EITC Limit	0.31	0.20	0.45
(Conditional on Earnings > 0)	(0.46)	(0.40)	(0.50)
Annual Weeks Worked	25.9	23.6	28.9
	(22.4)	(22.6)	(21.8)
Annual Weeks Worked	38.1	38.2	37.9
(Conditional on Weeks Worked > 0)	(16.6)	(16.4)	(16.9)
Weekly Hours Worked	18.1	16.2	20.5
	(19.4)	(19.1)	(19.5)
Weekly Hours Worked	34.0	33.9	34.1
(Conditional on Hours Worked > 0)	(12.9)	(12.7)	(13.1)
Observations	550,904	310,875	240,029

Notes: Data source: 1971-1986 March CPS data. Individual March CPS weights used. Sample contains all women 16 to 45 years old. Standard deviations are in parentheses. Kids under 5 is binary. 358,955 observations have positive earnings, 466,247 have positive household earnings, 375,906 have positive weeks worked last year, and 292,911 have positive hours worked last week.

Table 1.2: The 1975 EITC and the Employment of All Women

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean Dependent Variable Across Years and Across Treatment and Control Groups=0.65							
Mean Dependent Variable for Treatment Group in 1975=0.53							
Mom x Post1975	0.049***	0.049***	0.046***	0.040***	0.041***	0.050***	0.039***
	(0.007)	(0.006)	(0.007)	(0.009)	(0.008)	(0.008)	(0.008)
Controls							
State and Year FE		X	X	X	X	X	X
Demographic Controls			X	X	X	X	X
Unemployment Rate				X	X	X	X
"Kitchen-Sink" Controls							X
Observations	550,904	550,904	550,904	550,904	550,904	550,904	550,904
Model	Logit	Logit	Logit	Logit	Probit	OLS	OLS
R-squared						0.157	0.177

Notes: Data source: 1971-1986 March CPS data. 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, probit, 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. "Kitchen-sink" controls 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, as well as annual inflation interacted with low education (<12 years), having kids and number of kids, and married, and finally state-year measures of manufacturing employment interacted with low education, having kids, and married. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.3: Heterogeneous and Subgroup Treatment Effects of the 1975 EITC on Employment

Subgroup:	All	Married				Education	"High-Impact"	Single Men
Description:	Larger Response Among Unmarried Mothers	All Married Women	Spouse Earning Below EITC Limit	Placebo: Spouse Earning Above EITC Limit	Response Negatively Correlated With Spousal Earnings	Larger Response Among Lower-Education Mothers	Larger Response Among "High-Impact" Group	No Response Among Men
Mean Dependent Variable:	0.65	0.61	0.54	0.62	0.61	0.65	0.65	0.78
Mean Dep Var for 1975 Treat. Group:	0.53	0.51	0.46	0.52	0.51	0.53	0.53	0.85
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mom x Post1975	0.027***	0.016	0.049***	0.007	0.062***	-0.002	0.021**	
	(0.007)	(0.011)	(0.018)	(0.012)	(0.012)	(0.010)	(0.009)	
Mom x Post1975 x Unmarried	0.115***							
	(0.008)							
Mom x Post1975 x Spousal Earnings					-0.009***			
(10,000s of 2013 $)					(0.001)			
Mom x Post1975 x (<12 Yrs Ed)						0.051***		
						(0.006)		
Mom x Post1975 x (12-15 Yrs Ed)						0.043***		
						(0.005)		
Mom x Post1975 x High-Impact							0.038***	
							(0.007)	
Dad x Post1975								0.002
								(0.020)
Observations	550,904	321,147	55,313	265,834	321,147	550,904	550,904	231,087

Notes: Data source: 1971-1986 March CPS data. All samples limited to 16 to 45 year olds. Binary dependent variable employment equals 1 for 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* implicitly refers to *Mom x Post1975 x Married* in column 1, *Mom x Post1975 x (>16 Yrs Ed)* in column 6, and *Mom x Post1975 x Non-High-Impact Sample* in column 7. "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.

Table 1.4: Triple Differences Corroborates Difference in Differences Results

DDD Comparison Group:	Comparing High-Impact and Placebo Women (Table 3 Columns 5 and 10)	Comparing High-Impact Women and Single Men (Table 3 Columns 5 and 11)
Variables	(1)	(2)
Mom x Post1975 x EITC Eligible	0.045***	
	(0.011)	
Parent x Post1975 x Woman		0.044**
		(0.019)
Observations	550,904	787,230

Note: Data source: 1971-1986 March CPS data. Samples limited to 16 to 45 year olds. Binary dependent variable employment equals 1 for positive earnings. High-impact women from Table 3 column 10, placebo women from Table 3 column 5, and single men from Table 3 column 11. Equation (3), CPS weights, full set of controls used along with interactions of each control with being a high-impact women, 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. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.5: The EITC Effect on Annual Work Hours and Earnings (Intensive + Extensive Margins)

Dependent Variable:	Annual Work Hours			Annual Earnings (2013 $)		
Sample:	"High-Impact" Group	All	EITC-Ineligible Placebo Group	"High-Impact" Group	All	EITC-Ineligible Placebo Group
Mean Dependent Variable:	972	765	743	15,149	12,826	13,145
Mean Dep. Var. 1975 Mothers:	769	592	530	11,872	10,001	9,395
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Mom x Post1975	73.7***	43.1***	-5.6	1,556.6***	964.8***	450.9
	(14.9)	(11.7)	(19.1)	(255.9)	(226.3)	(476.2)
Observations	230,399	550,904	265,834	230,399	550,904	265,834
R-squared	0.269	0.215	0.156	0.346	0.257	0.184

Notes: Data source: 1971-1986 March CPS data. 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 $18,000 (2013 dollars). 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. Weekly work hours refers to the week prior to the March CPS interview. Similar results for the three groups using imputed hourly wage (annual earnings divided by annual work hours; zero assigned if annual work hours equals zero, even if reported annual earnings is positive): 0.94 (.20), 0.41 (.016), and 0.02 (0.33), which represent percent increases of 13, 6, and 0 for 1975 mothers. 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 1.6: Maternal Employment Led to an Increase in Gender-Equality Preferences

Controls:	Demographic Changes						Changes in Other Social Attitudes		
	Educ.	Married	Race	Working	Earnings	Mom Worked	Fraction Democrat	Racial-Equality	Too Much Welfare
Panel A: Controlling for Pre1975-Post1975 State-Trait Changes									
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
EITC-Led Increase in	0.022***	0.017***	0.019***	0.018***	0.016**	0.019***	0.023***	0.019***	0.019***
Working Mothers	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
(in Percentage Points)									
Observations	32	32	32	32	32	32	32	32	32
R-squared	0.452	0.200	0.183	0.191	0.237	0.183	0.261	0.193	0.186
Panel B: Controlling for Pre1975 State-Trait Levels									
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
EITC-Led Increase in	0.012*	0.019***	0.018***	0.018***	0.016**	0.019***	0.022***	0.017***	0.019***
Working Mothers	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
(in Percentage Points)									
Observations	32	32	32	32	32	32	32	32	32
R-squared	0.381	0.188	0.196	0.190	0.222	0.181	0.233	0.198	0.209

Notes: 1972-1985 restricted GSS data with state-level identifiers. Gender-equality preferences constructed from the GSS variable *fework* which asks respondents whether married women should work. GSS sample reflects males 18 to 60 years old in 32 states. 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. Additional results shown shown in Panels A and B: average age 0.017 (0.007) and 0.017 (0.006); mother's education: 0.019 (0.006) and 0.019 (0.006); fraction religious: 0.020 (0.007) and 0.019 (0.006). Education measured in years, working denotes labor-force participation (working full-time, working part-time, temporarily laid off, or unemployed), earnings denotes real log earnings. Mom worked and mom education constructed from GSS variables *mawk16* and *maeduc*, democrat from *partyid*, racial equality from *racpres*, religious from *reliten,* and too much welfare from *natfare*. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2

The Long-Term Impact of the Earned Income Tax Credit on Children's Education and Employment Outcomes (with Kathy Michelmore)

2.1 Introduction

The Earned Income Tax Credit (EITC) is one of the largest cash-transfer programs in the United States. In 2013, the EITC distributed over $66 billion to 28 million low-income families and lifted more than 3 million children out of poverty (Center on Budget and Policy Priorities 2014). Several decades of research on the EITC indicates that it has had a substantial impact on low-income families, increasing labor force participation of single mothers (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Bastian 2017), raising pre-tax earnings (Dahl, DeLeire, and Schwabish 2009), lifting families out of poverty (Hoynes and Patel 2015), and improving health (Evans and Garthwaite 2014).[1]

Recent work has shown that children from low-income households benefit from the EITC as well. Hoynes, Miller, and Simon (2015) find that the EITC led to a reduction in low birth weight, Dahl and Lochner (2012, 2017) find that the program led to an increase in childhood test scores, and Maxfield (2014) and Manoli and Turner (2014) find positive effects of the EITC on college-going. While these papers all find positive contemporaneous effects of the EITC on children, little is known about the long-term effects of the EITC on children's outcomes once they reach adulthood. This is an important question, as it has implications for the intergenerational transmission of poverty. This paper builds on prior research by analyzing how exposure to the EITC in childhood affects subsequent educational attainment and labor-market outcomes in adulthood.

[1] See Nichols and Rothstein (2015) for a recent summary of the EITC.

The EITC has the potential to affect the long-run outcomes of children through multiple pathways. First, the EITC provides additional resources to low-income households. This "income effect" could lead to improvements in long-term outcomes. Second, mothers may increase their labor supply (often on the extensive margin) as a response to the EITC.[2] Although household earnings may rise substantially, this "substitution effect" may result in more time spent working and less time spent with children at home. The overall effect of the EITC on children's long-term outcomes is therefore theoretically ambiguous. As we will demonstrate empirically, the EITC leads to large increases in household resources, while we find little evidence that mothers spend less time with their children as a function of EITC generosity.

Since there are no limits on the number of years that households can claim the EITC, this program has the potential to impact children from birth until college.[3] Family resources have been shown to affect the human capital and long-run outcomes of young children (Cunha and Heckman 2007; Chetty, Friedman, and Rockoff 2011; Currie and Almond 2011) as well as the outcomes of older children (Dynarski and Scott-Clayton 2013; Manoli and Turner 2014; Carneiro et al. 2015; Cohodes et al. 2016). This is the first study to our knowledge to use the longitudinal nature of the EITC to causally estimate when in a child's life the EITC (and family income) matters most for later-life outcomes.

To conduct this analysis, we use data from the 1968 to 2013 waves of the Panel Study of Income Dynamics. For each individual of interest we construct annual measures of "EITC exposure" in childhood, defined as the maximum federal and state EITC benefits that a child's family could receive given the year, state of residence, and number of children residing in the household.[4] This measure of exposure is used rather than actual EITC eligibility based on actual family income due to concerns of endogeneity of own EITC eligibility with respect to education outcomes. We then analyze the impact of EITC exposure in childhood on educational attainment and employment when these children reach adulthood. Four decades of variation in EITC generosity, as well as panel data on individuals from several birth cohorts allows for the estimation of heterogeneous treatment effects by age of exposure. EITC exposure is then used as

[2] The EITC has consistently been shown to increase the labor supply of single mothers, but may reduce the average labor supply of married mothers (Eissa and Hoynes 2004), although married women with lower-earning spouses respond positively as well (Bastian 2017; Eissa and Hoynes 2006).
[3] Children can be claimed on the EITC until they turn 19, or 24 if they remain full-time students.
[4] There is little evidence that the EITC affects fertility, reducing concerns of endogeneity of family size (Baughman and Dickert-Conlin 2009). Likewise, the EITC has at most a minor impact on marriage and divorce rates (Dickert-Conlin and Houser 2002; Herbst 2011; Michelmore Forthcoming).

an instrument for family income to investigate how exogenous shocks to family income in childhood affect education and employment outcomes in adulthood. [5]

This paper makes the following contributions: First, we extend the literature on the impact of the EITC on children of EITC recipients by estimating its effect on education, employment, and earnings when children reach adulthood. Second, this is the first analysis to exploit four decades of policy-induced changes to EITC generosity at both the federal and state level. Third, we contribute to the broader literature on how household resources affect educational attainment. Finally, we examine when in a child's life the EITC (and family income) matters most for later-life outcomes.

Results indicate that a policy-induced increase in EITC generosity in childhood has a positive effect on educational attainment and employment in adulthood. These effects are driven by EITC exposure during the teenage years, which is consistent with a growing body of research that finds positive education outcomes for children exposed to additional resources in their teenage years (e.g. Carneiro et al. 2015; Cohodes et al. 2016; Manoli and Turner 2014). Reduced-form estimates of the impact of EITC exposure on education and employment outcomes suggest that a $1,000 (dollars throughout are real 2013 dollars) increase in EITC exposure between ages 13 and 18 leads to a 1.3-percent increase in high school graduation, a 4.2-percent increase in college graduation, and a 1.0-percent increase in employment in adulthood.

Our instrumental-variables analysis reveals that EITC exposure in childhood leads to large increases in pre-tax family earnings, which leads to increases in educational attainment and employment in adulthood. A $1,000 increase in the maximum EITC when a child is 13 to 18 years old increases family income over that age range by approximately $12,000 (or $2,000 per year). A $1,000 increase in family income generated by EITC exposure between ages 13 and 18 leads to a 0.2-percent increase in the likelihood of completing high school and a slight increase in total years of schooling. These education gains also translate into labor market gains. A $1,000 increase in family income generated by EITC exposure between ages 13 and 18 leads to a 0.1-percent increase in the probability of being employed between ages 22 and 27 and a $57 (or 0.2-percent) increase in annual earnings. Conditional on exposure at older ages, we find no significant positive effects of EITC exposure prior to age 13 on education outcomes, though in

[5] Family income is defined as reported earnings plus the imputed EITC benefits a family is eligible for given their state, year, family size, marital status, and earnings.

many cases, estimates are too noisy to draw firm conclusions regarding the relationship between exposure to the EITC in early childhood and subsequent education outcomes.

These results provide further evidence that, in addition to the positive economic impacts the EITC has on its adult recipients, the EITC also has positive effects on children growing up in low-income households and these effects persist into adulthood. These findings have important implications for intergenerational mobility, suggesting that the EITC improves the financial well-being of children growing up in disadvantaged households. These results also have implications for intergenerational income inequality, as we find that effects are largest among individuals from the lowest-income households.

2.2 Federal and State EITC Policy Changes over Time

The EITC is a refundable tax credit that provides an annual earnings subsidy to low-income working parents. Benefits are determined by the state of residence, marital status, and number of children of the taxpayer, and were worth as much as $8,462 in 2013 for households earning between $13,430 and $22,870.[6] The EITC structure contains a phase-in region, where benefits increase as a function of earnings; a plateau region, where benefits do not change as a function of earnings; and a phase-out region, where benefits decrease as a function of earnings. The slopes of these regions vary by the number of children living in the household. The structure of the federal EITC for the 2013 tax year is presented in Figure 2.1.

Since its inception, the EITC has undergone several changes at both the federal and state level. The federal EITC began in 1975 as a 10 percent earnings subsidy for low-income parents. By 1986, the phase-in rate had increased to 14 percent, and in 1991, a larger benefit was introduced for households with two or more children. The largest federal expansion occurred in the years leading up to welfare reform in the 1990s. By 1996, the phase-in rate for households with two or more children was 40 percent, while for households with one child it was 34 percent. This resulted in a difference of up to $2,000 (2013 dollars) in annual EITC benefits between families with one and families with at least two children. In 2003, the plateau region of the EITC was extended for married couples, and in 2009, a larger credit was introduced for households

[6] For a married family with three or more children living in a state with an EITC worth 40 percent of the federal EITC.

with three or more children that phased in at a rate of 45 percent. Over this period, maximum federal EITC benefits grew from $1,700 in 1975 to $6,000 in 2013 (2013 dollars).

In addition to the federal EITC, 24 states and the District of Columbia had their own EITC by 2013. In general, these policies supplemented federal EITC benefits by a fixed rate, ranging from 3.5 to 40 percent of federal EITC benefits. States began implementing their own EITCs in the late 1980s, but many did so in the late 1990s. After welfare reform in 1996, states were given federal block grants to reduce welfare caseloads, with flexibility in determining how that money was spent. Many states used these block grants to establish EITCs. Table B1.1 lists the states that have ever implemented EITCs as well as the generosity of state EITCs as of 2013. States with EITCs can be found in all regions of the country and across the political spectrum[7] (for example, Washington, Pennsylvania, and Alabama do not have an EITC while Oregon, New York, and Louisiana do). The year that a state enacted an EITC and the generosity of state EITC benefits are sources of *between*-state variation. There is also *within*-state variation in EITC benefits as states expanded (and occasionally reduced) their programs over time.

Numerous policy expansions in federal EITC benefits and dozens of state-level changes interact to provide substantial identifying variation. Figure 2.2 presents variation in the federal and state maximum potential EITC a household could receive for each of the states that have their own EITCs from 1975 to 2014. Figure 2.2 illustrates the three sources of plausibly exogenous variation in EITC generosity: variation in the generosity of the federal credit over time, variation based on the implementation and expansions of state EITCs over time, and variation based on the number of children residing in the household. All values are presented in 2013 dollars, adjusted using the Consumer Price Index. In 1986, just one state had its own EITC, and by 2014, there were 25 states with their own EITCs. In 1986, the difference between the most generous and least generous state in terms of the maximum potential EITC was quite small. By 2014, the difference was more than $2,000.

Using state EITCs yields more variation than federal EITC policy expansions alone and allows for more precise estimation of the impact of the EITC on the outcomes of interest. This variation also implicitly controls for national events that may have coincided with federal EITC expansions, such as recessions or welfare reform. Much of the early research on the EITC relied

[7] The federal EITC has been expanded by every president since it began under President Ford. State EITCs have also been created and expanded by Republican and Democrat governors alike.

on a one-time expansion to the federal EITC during the mid-1990s, a time when the federal economy was booming and traditional welfare was undergoing a significant reform. It is not clear whether results based on this federal expansion are generalizable under different economic circumstances. Using state policy variation in EITC benefits helps address this concern and takes advantage of policy changes that occurred over several decades and across the business cycle.

In using state-level variation, we assume that changes in state EITC generosity are uncorrelated with other state-level policies or economic conditions that may also affect education and employment outcomes. For instance, if states are more likely to increase their EITC benefits when the unemployment rate is high, and high unemployment induces more individuals to stay in school, then changes in educational attainment will reflect not only higher state EITC benefits, but also the effect of high unemployment.

To test whether state EITC generosity is correlated with other state policies or macroeconomic events, we regress the maximum federal and state EITC on several state-by-year characteristics such as GDP, unemployment rate, the top marginal income-tax rate, minimum wage, welfare generosity, higher-education spending, and total state tax revenue. We also include lags of each of these controls. Results in Table B1.2 indicate a negative relationship between state EITC generosity and spending on higher education (significant at the 10-percent level). States with more generous EITC benefits tend to spend less on higher education, which would bias our results towards zero if spending on higher education is positively correlated with educational attainment. Although we cannot reject that all 14 covariates are statistically zero in a joint F-test[8] (p-value 0.32), these state-year variables are included as controls throughout the analysis to help alleviate concerns that state-year policies and economic conditions might be correlated with both EITC generosity and long-run outcomes. We also control for state-specific time trends to account for additional policies or conditions that vary by state over time. We also explicitly test the sensitivity of results to the inclusion of state EITC variation; results are robust to excluding state EITC variation and relying on federal variation alone.

2.3 Previous Research

A few studies have investigated the relationship between the EITC and children's

[8] This is also true when the sample is restricted to states that ever had an EITC (Table B1.2 column 2).

education outcomes. Using a federal expansion of the EITC for two-child households in the 1990s, Dahl and Lochner (2012, 2017) show that a $1,000 increase in family income generated by the EITC increased math and reading test scores on the Peabody Individual Achievement Test (PIAT) by 4 percent of a standard deviation. It is not clear whether these positive effects persist or whether these test-score improvements lead to gains in long-run educational attainment or labor-market outcomes. Many policy interventions that improve the short-run test scores of children have been shown to fade out after the intervention ends (Currie and Thomas 2000; Kane and Staiger 2008; Jacob, Lefgren, and Sims 2010). However, some policies have been shown to improve longer-term outcomes such as educational attainment or labor-market outcomes (Krueger and Whitmore 2001; Ludwig and Miller 2007; Deming 2009; Heckman et al. 2010; Chetty et al. 2011a). This paper assesses whether the test score gains found in Dahl and Lochner (2012, 2017) translate into improvements in longer-term outcomes.

Manoli and Turner (2014) examine the impact of the EITC on college-going among high school seniors. The authors exploit variation in family income generated by kinks in the EITC benefit structure and the tax code more generally to analyze the impact of an increase in family income during tax time on college enrollment the following fall. The authors find a 0.4 to 0.7 percentage point (or about 1.9 to 3.4 percent from a base of 20.5 percent) increase in college enrollment associated with a $1,000 increase in household income during one's senior year of high school. The authors exploit sources of variation that differ from those exploited here, and utilize different populations for comparison groups. Manoli and Turner (2014) primarily focus on the sample of individuals located at different kink points along the EITC benefit schedule, comparing individuals on either side of the kink points. The extent to which there are heterogeneous treatment effects by family income, the Manoli and Turner (2014) results may not be generalizable to the full population, or the full EITC-eligible population. Additionally, Manoli and Turner (2014) examine college-going patterns for individuals who receive an exogenous shock to family income in their senior year of high school, while our analysis tests whether there are differential effects of family income received at different points in childhood. The extent to which the results presented here align with those of Manoli and Turner (2014) will shed light on potential mechanisms through which the EITC affects children's educational attainment.

Chetty, Friedman, and Rockoff (2011b) also analyze the impact of the EITC on later-life outcomes of children. Chetty et al. (2011b) examine how the EITC impacts long-run outcomes through its impact on childhood test scores. They find that a $1,000 increase in tax credits leads to a 6 percent of a standard deviation increase in childhood test scores. This increase in childhood test scores is calculated to have a 0.3 percentage point increase in college-going by age 20. The authors also find significant increases in earnings in adulthood as a result of test score increases in childhood. Chetty et al. (2011b) express education and earnings gains in adulthood in terms of a single-year gain in test scores generated by the EITC. If exposure to the EITC has a cumulative effect on outcomes in adulthood, we would expect to find larger gains in education outcomes than earlier studies.

Our analysis builds on prior work by estimating the impact of exposure to the EITC throughout childhood on children's education and employment outcomes when they reach adulthood. Four decades of both federal and state EITC variation enables estimation that is not limited to a specific point in time or segment of the income distribution. Panel data on EITC exposure across time, birth cohorts, and age allows for the estimation of heterogeneous effects of the EITC by age of exposure. An analysis of how the timing of EITC exposure affects children's long-run outcomes is an area of particular interest, as prior work linking the EITC to children's education outcomes has not explored whether there are differential impacts of the EITC on education outcomes by age of exposure. Finally, while we estimate the reduced-form effect of EITC exposure on long-term outcomes, we also use an instrumental variables approach to show that the primary mechanism through which the EITC impacts children's outcomes is through increasing family income.

2.4 Data and Sample

Data come from the 1968 to 2013 waves of the Panel Study of Income Dynamics (PSID). The PSID is a nationally-representative household survey that has followed households and their offspring since 1968. The original sample contained information on approximately 5,000 households and over 70,000 individuals have participated in the PSID as of the 2013 wave (McGonagle et al. 2012). All individuals residing in the household are interviewed, and any individual born into the household is then followed for life even if they subsequently leave the household.[9] Households were interviewed annually until 1997, after which they were

[9] Any individual that marries into a PSID household is observed while residing in that household, but is not followed

interviewed biannually.[10] The PSID contains a rich set of information regarding household and individual characteristics, facilitating the calculation of annual EITC exposure during childhood.

We limit the sample to individuals observed in at least one year between each of the age intervals in which EITC exposure is measured: 0 to 5, 6 to 12, and 13 to 18.[11] We also restrict our sample to individuals whom we can observe until at least age 18. These restrictions produce a sample of 3,495 individuals born between 1967 and 1995 (see the data appendix for a more detailed description of the sample and variable definitions).[12]

Summary statistics are presented in Table 2.1, weighted using childhood PSID weights averaged across the years from birth to age 18. All dollar values are adjusted for inflation using the Consumer Price Index and reported in 2013 dollars. Most of the individuals in the sample lived with parents who had completed high school, and about half had at least one parent who completed some college. The maximum federal and state EITC in the year individuals turned 18 was approximately $4,600, while the cumulative maximum EITC benefits from a child's birth to age 18 were worth approximately $77,000.[13] On average, individuals in our sample were exposed to larger EITC benefits between the ages of 13 and 18 ($29,300) than between birth and age 5 ($16,600), which is partially explained by the fact that much of the expansions to the EITC occurred over the last two decades, when much of our sample was older than five. Only the youngest individuals in our sample would have been five or younger during the large, federal expansions of the early 1990s. Demographically speaking, children under the age of six are also less likely to have other siblings in the household compared to older children, which may also explain why EITC exposure is lower during that age range.[14]

if they later separate from the household. See the 2013 PSID Main Interview User Manual for more details.

[10] For non-interview years, we impute family income and EITC benefits by averaging income and EITC benefits from the interview years just before and after the non-interview year.

[11] Results are robust to restricting the sample to individuals present at least three years of each of the age ranges.

[12] Results are similar if we exclude all individuals born before 1975 and focus only on the sample with 18 years of exposure to the EITC.

[13] Discounting EITC exposure by 3 percent a year from age 18 results in a cumulative amount of about $36,000.

[14] Approximately a quarter of 0 to 5 year olds in our sample have no other siblings in the household, compared to just 11 percent of 6 to 12 year olds. 13 to 18 year olds are equally likely to have no other siblings in the household as 0 to 5 year olds, but are more likely to have at least two siblings in the household.

2.5 Empirical Method

To analyze how the EITC affects education and employment outcomes, we create measures of EITC exposure during childhood defined as the maximum potential federal and state credit a child's family could receive given their state of residence, family size, and tax year; independent of own family income or parental marital status. For each individual in the analysis, EITC exposure is cumulated from an individual's birth until the year they turn 18, or the last year they reside in their parents' household, whichever comes first.[15] The value of EITC exposure changes over time for an individual based on federal and state policy changes to the EITC, as well as changes to the individual's family size or movements across states, the latter of which is relatively rare.[16] For instance, an individual who is the first-born child of a household will be assigned the maximum federal and state EITC available for a one-child household in the year they were born, in their state of birth. If a second child enters the household a year later, both siblings in the household will then be assigned the maximum federal and state EITC available for a two-child household in that state after the birth of the second child. Once the first-born child turns 19, the second-born child will be assigned the maximum federal and state EITC available for a one-child household for the remaining years until she turns 19 (assuming no other children enter the household). Variation in annual EITC exposure stems from three primary sources: the year the individual was born (reflecting the generosity of the federal credit), the state the individual lives in (reflecting state EITC benefits), and the number of children in the household (reflecting larger EITC benefits available for larger households).

We use this measure rather than own EITC benefits due to concerns of endogeneity of family income and own EITC benefits with respect to education outcomes. Families eligible for the EITC must have income below a certain threshold, which was $51,567 in the 2013 tax year,[17] and thus EITC-eligibility is negatively correlated with income. Individuals with higher levels of EITC benefits during childhood were also likely to be disadvantaged in other ways (e.g. poor neighborhoods and schools, single parenthood, poor nutrition) that may have affected their educational attainment. A direct analysis of the impact of own EITC benefits on education

[15] Parents cannot claim children for the EITC unless the children reside in the household for at least half the year. Only 38 observations in the sample do not live with a parent at age 18; all results are robust to dropping them.
[16] About 1.6 percent of individuals move across state lines in a given year; while 20 percent of individuals ever move across state lines between birth and age 18. All results are robust to excluding these individuals.
[17] This threshold was lower for unmarried families, or families with fewer than three children.

outcomes generates a negative correlation, as higher family EITC benefits are an indicator of economic hardship during childhood.[18] Using EITC exposure rather than actual EITC eligibility captures plausibly exogenous policy variation and excludes endogenous variation in own EITC eligibility.

To reflect our interest in measuring how the timing of income affects education outcomes, we parse cumulative EITC exposure throughout childhood into three age intervals: 0 to 5, 6 to 12, and 13 to 18.[19] The 0 to 5 category represents the cumulative maximum potential EITC benefits a child was exposed to from birth to age 5, while the 6 to 12 category represents cumulative EITC exposure from age 6 to age 12, and the 13 to 18 category represents cumulative EITC exposure from age 13 to age 18.

Figure 2.3 simulates the variation in EITC exposure a child could have received from birth to age 18 by birth cohort, state, and number of children residing in the household—our treatment variable of interest. This figure is constructed by cumulating the maximum federal and state EITC benefits available in each year over 19 years (age 0 to 18) for each of the states that have ever implemented EITCs, as well as the federal EITC alone (the bottom line in each panel) for one-child, two-child, and three-plus child households. The figure simulates what EITC exposure would look like for individuals born between 1957 and 1995 if they lived in a one-child, two-child, or three-child household over their entire childhoods. For simplicity, the simulations assume a fixed state of residence over the entire period of childhood, as well as a fixed household size. The results of this simulation illustrate the substantial variation in exposure to the EITC depending on the year of birth, household size, and state of residence. Individuals born in 1975 would have been exposed to roughly $30,000 (2013 dollars) in EITC benefits from birth to age 18, while individuals born 10 years later could have received up to $100,000 (2013 dollars) in EITC benefits during childhood. For the birth cohort range of our sample, cumulative EITC exposure ranges from $15,000 for those born in 1967 to over $145,000 for those born in 1995.

Figure B1.1 illustrates how the potential variation in Figure 2.3 translates to EITC exposure between birth and age 18 for our sample. Figure B1.1 reveals more than 1,400 unique

[18] In our sample an additional $1,000 in own EITC eligibility at age 18 is correlated with a 1.6 percentage point decrease in the probability of finishing high school.

[19] We also examined the effects of the cumulative EITC exposure between birth and age 18, as well as the average annual EITC exposure. These results proved to be largely redundant.

values of EITC exposure for our sample of 3,495 individuals and reflects any changes in the number of children in the household as well as any cross-state moves an individual experiences between birth and age 18. Consistent with the variation presented in Figure 2.3, Figure B1.1 illustrates the substantial variation in EITC exposure among individuals in our sample, ranging from less than $15,000 for individuals born in 1967, to over $145,000 for individuals born in 1995. In addition to the refundable tax credits that these children could have received from the EITC, they also experienced an increase in household earnings as the EITC encouraged their parents to go to work over this time-period as well. As a result, the EITC had a large impact on household resources for many children growing up in low-income families over this period.

2.5.1 EITC Exposure in Childhood Increases Educational Attainment

To analyze the impact of the EITC on educational attainment, we first estimate the reduced-form effect of increasing EITC exposure during childhood on subsequent education outcomes. Parsing EITC exposure into the three age intervals (0 to 5, 6 to 12, and 13 to 18) allows us to test two hypotheses regarding the timing of income and children's education outcomes.[20] One hypothesis posits that the main mechanism through which the EITC increases educational attainment is by improving the home environment, non-cognitive skills, or academic preparedness of young children. If this hypothesis prevails, we would expect to find relatively larger effects of EITC exposure on education when individuals are young. This would be consistent with evidence suggesting that early childhood interventions are effective in improving the life outcomes of children growing up in poverty (Cunha and Heckman 2007; Ludwig and Miller 2007; Deming 2009; Chetty et al. 2011a; Currie and Almond 2011; Caucutt and Lochner 2012). If instead, the main reason that the EITC improves educational attainment is by alleviating credit constraints in paying for college, then we would expect to find larger effects of EITC exposure when individuals were adolescents. This would support the hypothesis that the household cash on hand in the years leading up to college decisions has a significant impact on college enrollment (Dynarski 2003; Belley and Lochner 2007; Kane 2007; Lovenheim 2011; Manoli and Turner 2014). These two hypotheses are not mutually exclusive. It is possible that

[20] We also modeled the impact of EITC exposure at age 18 on subsequent education outcomes as a proxy for childhood EITC exposure. Results were consistent, see Table B1.3.

each of these channels contributes to improvements in education outcomes. To test these hypotheses, we first model the reduced-form impact of EITC exposure on children's educational attainment as follows:

$$Y_i = \beta_1 EITC_{i(0\text{-}5)} + \beta_2 EITC_{i(6\text{-}12)} + \beta_3 EITC_{i(13\text{-}18)} + \eta \boldsymbol{X}_i + \psi \boldsymbol{V}_{s,t} + \lambda \boldsymbol{Z}_s + \pi \boldsymbol{W}_t + \varepsilon_i \qquad (1)$$

where i indexes individuals, s indexes states, and t indexes years. Y_i is the outcome variable of interest: high school completion, college attendance, college completion, years of schooling, employment, and earnings. Outcomes are evaluated as of age 20 for high school completion, age 24 for college attendance, age 26 for college completion and years of schooling, and between the ages of 22 and 27 for employment and earnings. For each outcome of interest, each individual is represented in the sample exactly one time. Coefficients of interest are β_1, β_2, and β_3, which represent the impact of an additional $1,000 of EITC exposure when the focal child is 0 to 5 years old, 6 to 12 years old, and 13 to 18 years old on subsequent education and employment outcomes.

$\boldsymbol{X}_i$ is a vector of personal characteristics that includes cohort fixed effects, an age cubic, indicators for black, Hispanic, female, ever-married parents, number of siblings at age 18 fixed effects, and whether the child's mother and father finished high school and at least some college, as well as interactions between black, Hispanic, and female indicators with state and birth year. These controls account for changes in educational attainment over time that vary by race, gender, and state, as well as family characteristics that correlate with educational attainment. Interactions allow for a more flexible model and control for differential trends by race and gender that vary by state and across years.

$\boldsymbol{V}_{s,t}$ includes state-by-year policy and economic indicators discussed in section I: per-capita GDP, the unemployment rate, top marginal income-tax rate, minimum wage, maximum welfare benefits, spending on higher education, tax revenue, and a state-specific quadratic time trend. These controls are measured at age 18 and are included to address concerns that various state-by-year factors may confound the relationship between EITC generosity and education outcomes. State-specific time trends control for further-unaccounted for policies or conditions that vary by state across time. $\boldsymbol{Z}_s$ and $\boldsymbol{W}_t$ are state and year fixed effects, and ε_i is an idiosyncratic-error term. To account for unobserved correlation of the error terms within states, standard errors are clustered at the state level.[21]

Table 2.2 presents estimates of β_1, β_2, and β_3 for various education and employment outcomes for equation (1). Each column represents a separate regression. Results are weighted using the PSID childhood weights, averaging the weights across childhood years.[22]

Results suggest that the largest impact of the EITC on education outcomes occurs when a child is 13 to 18 years old. Columns 1 through 4 show that conditional on EITC exposure at younger ages and the full set of controls, a $1,000 increase in EITC exposure when a child was between 13 and 18 years old leads to a 1.2 percentage point (or 1.3 percent) increase in the likelihood of completing high school, a 1.3 percentage point (or 4.2 percent) increase in the likelihood of completing college, and 0.08 more years of schooling (or 0.6 percent). For employment outcomes, columns 5 and 6 show that a $1,000 increase in EITC exposure between ages 13 and 18 leads to a 0.8 percentage point (or 1.0 percent) increase in the likelihood of being employed in adulthood, and a $564 (or 2.2 percent) increase in annual earnings.

We find little evidence that EITC exposure prior to age 13 has an impact on education outcomes, though estimates are noisy and, in most cases, not significantly different from the coefficients on EITC exposure between ages 13 and 18. Coefficients for each age range are significantly different, however, for the high school graduation outcome (p-value on F-test of coefficient equality: 0.006). This implies that exposure to expansions in the EITC when a child is 13 to 18 years old yields a significantly larger effect on the likelihood of graduating high school than exposure to expansions in the EITC at younger ages. These results could be interpreted as the EITC having little impact on the long-term outcomes of children under 12, although we cannot rule out small positive effects due to a lack of identifying variation for the youngest age range. We discuss these hypotheses in more detail below.

These results are largely consistent with the 'cash-on-hand' hypothesis that household resources around the time of college-going are important for college enrollment (Lovenheim 2011; Manoli and Turner 2014). In comparison to other estimates in the literature, though insignificant, we find a similar point estimate for college going (proxied by having completed at

[21] Models that progressively add these controls are presented in Table B1.4; results are robust to the inclusion of additional fixed effects for family size and marital status (and their interactions with state and year fixed effects) as well as fixed effects that isolate the impact of federal and state EITC exposure (see Table B1.5).
[22] Table B1.6 shows results are robust to alternate weighting choices, including unweighted.

least some college) as that found in Manoli and Turner (2014) as well as Chetty et al. (2011b). Our estimate captures the effect of EITC exposure from ages 13 to 18, while Manoli and Turner (2014) focus on exposure in a student's senior year of high school. In Table B1.3, we present results of regressing education outcomes on EITC exposure at age 18 (rather than parsing exposure into age intervals). This exercise reveals that a $1,000 increase in EITC exposure at age 18 increases the likelihood of completing at least some college by 3.5 percentage points (or 3.8 percent), which is much larger than the single-year estimates generated by Manoli and Turner (2014) and Chetty et al. (2011b). However, our measure of EITC exposure at age 18 can be interpreted as a proxy for EITC exposure throughout childhood: in results not shown, we find that a $1,000 increase in EITC exposure at age 18 is correlated with a roughly $5,000 increase in EITC exposure between ages 13 to 18. Dividing the 3.5 percentage point effect by 5 suggests that a $1,000 increase in annual EITC exposure leads to a 0.7 percentage point increase in the likelihood of completing at least some college, which is consistent with both the Manoli and Turner (2014) and the Chetty et al. (2011b) estimates.

To ensure that results are not driven by children from higher-earning families that were not eligible for the EITC, we use the full set of controls and estimate the relationship between EITC generosity and high school graduation as we restrict the sample to families with lower earnings (see Figure 2.4). We illustrate the results using EITC exposure at age 18 for simplicity in presenting coefficients.[23] When the sample is restricted to families earning below $60,000, $50,000, $40,000, etc., the estimated effect of an additional $1,000 of EITC exposure on the likelihood of graduating high school grows larger. In other words, there is an inverse relationship between the magnitude of the EITC's effect on education and the upper bound on family income. Children from the lowest-income households benefit the most from expansions to the EITC.[24]

The effect of the EITC on educational attainment is also larger for subgroups of academically at-risk youth. Table 2.3 shows results for various subgroups: black youth, males and females separately, black males, and children with unmarried parents. Using the full set of controls, estimates show the effect of an additional $1,000 of EITC exposure at various points in

[23] As discussed above concerning results in Table B1.3, for this analysis we use equation (1) except we replace variables for EITC exposure at age 0-5, 6-12, and 13-18 with EITC exposure at age 18 (which can be interpreted as a proxy for EITC exposure throughout childhood: empirically, a $1,000 increase in EITC exposure at age 18 is correlated with a roughly $5,000 increase in EITC exposure between ages 13 to 18).

[24] As a placebo test, we conducted an analysis where we restricted the sample to children from families earning above $50,000 (or $60,000, $70,000, etc.) and found no significant effect of the EITC on educational attainment. Results not shown but available upon request. See Table 3 column 8 for a similar placebo test.

childhood on the likelihood of graduating from high school using the full set of controls. Results indicate that the effect of EITC exposure between ages 13 and 18 are larger for black children (2.4 percentage points) and children in single-parent households (1.7 percentage points), despite similar levels of EITC exposure across these groups (roughly $34,000 between birth and age 18). Effects are of similar magnitude for boys and girls (0.9 and 1.1 percentage points), and effects are largest among black boys (2.7 percentage points).

Finally, the last two columns of Table 2.3 illustrate differences by parental education. Children with the least educated parents (defined as neither parent having attended college) are 2.6 percentage points more likely to complete high school when maximum EITC benefits increase by $1,000 during the child's teenage years. In contrast, we find no significant relationship between the EITC and high school completion among children with the highest educated parents (defined as at least one parent having completed college). This final column serves as a placebo test—we expect that children with highly educated parents would not be affected by policy expansions to the EITC in childhood. In fact, we estimate that the relationship between EITC exposure in childhood and high school completion for this group is very close to zero for all three age coefficients.

2.5.2 Mechanisms

Results above indicate that the EITC has a positive impact on educational attainment and employment of individuals exposed to the EITC in childhood. We next explore several mechanisms through which the EITC could affect these outcomes. Table 2.4 tests whether the EITC affects imputed family EITC benefits,[25] pre-tax family earnings, maternal labor supply, standardized test scores, and daily time parents spend with their children. For these results, each outcome is measured in the same year as EITC exposure and can be interpreted as the contemporaneous effect of the EITC on the outcome of interest. The sample for each outcome is restricted to individuals in the main sample in Table 2.1 that also have non-missing data for each outcome.

[25] Actual family EITC benefits are not reported in the PSID, so we impute them based on the federal and state EITC rules for a given family based on the year, state, number of household children, family income, and marital status in the current year. This should be interpreted as EITC eligibility rather than EITC receipt, although take-up rates of the EITC tend to be higher than other social programs at around 80 percent (Scholz 1994; Currie 2004)

All of the family earnings and maternal labor supply outcomes are measured at each interview wave between the focal child's birth and age 18. Test scores and time with parents is measured in the 1997, 2002, and 2007 PSID Child Development Supplement. Test scores are observed for individuals between ages 5 and 18; time with parents is observed for individuals between ages 2 and 18. All regressions include the full set of controls. Panel A shows regressions pooling all years and child-ages and evaluating an overall effect of EITC exposure on the outcomes of interest, while Panel B illustrates how EITC exposure varies by the child's age by interacting EITC exposure with the age of the child in the year the outcome was observed.

Results in Table 2.4 indicate that the EITC had a substantial impact on financial resources, primarily through increasing maternal labor supply. Reflecting the fact that few individuals receive the maximum federal and state EITC, a $1,000 increase in the maximum EITC increases imputed family EITC benefits by an average of $223. We also find increases in pre-tax family earnings as a function of EITC generosity: a $1,000 increase in EITC exposure leads to a $2,750 increase in earnings (measured in 2013 dollars). This is similar to the estimates found by Dahl and Lochner (2017), who estimate that a $1,000 increase in EITC generosity increases family income by approximately $1,800 in year 2000 dollars, or roughly $2,400 in 2013 dollars.

These results imply that much of the increase in family resources generated by the EITC comes from increases in labor supply and not the receipt of the benefit itself. This is consistent with prior research indicating that much of the anti-poverty impacts of the EITC are generated not by the benefit itself, but through increases in pre-tax earnings generated by the labor supply incentives associated with the EITC (Hoynes and Patel 2015). Confirming a long line of research on the EITC's impacts on maternal labor supply, we find that a $1,000 increase in the maximum EITC benefit in a given state and year leads to a 4.6 percentage point increase in the likelihood of working among the mothers of the children in our sample. Annual hours worked also increases, by about 83 hour per year.[26] These results are in line with previous research showing significant increases in maternal labor supply as a function of EITC generosity (Eissa and Liebman 1996; Meyer and Rosenbaum 2001).

Consistent with prior work by Dahl and Lochner (2012, 2017), we also find evidence that the EITC increases test scores of young children. Results indicate that a $1,000 policy-induced

[26] Annual number of weeks worked also increases by about 2 weeks, not shown.

increase in EITC exposure leads to a 12.9 percent of a standard deviation increase in contemporaneous child test scores. Since $1,000 of EITC exposure is correlated with an average increase of family earnings of about $2,700, this implies that $1,000 of family income is associated with about a 5 percent of a standard deviation increase in test scores. This aligns with previous estimates of the effect of $1,000 in family income on test scores: Dahl and Lochner (2012, 2017), Duncan, Morris, and Rodrigues (2011), and Chetty et al. (2011b) find estimates of 4, 5, and 6 to 9 percent of a standard deviation, respectively.

While we have shown that the EITC increases maternal labor supply, it may also lead to less time invested in caring for children and home production. We do not find much support for this theory: a $1,000 increase in EITC exposure leads to a statistically insignificant 10.5-minute reduction (or about 8 percent) in daily minutes that mothers spend with their children. We find virtually no effect of the EITC on time that fathers spend with their children, so overall declines in time spent with children is about 11 minutes per day. Previous research implies that this could have an adverse effect on children (Bettinger, Hægeland, and Rege 2014), although our estimates are substantively quite small. Taken together, these results support the hypothesis that the EITC has a substantial impact on maternal labor supply and earnings, and the negative consequences for time spent with children are minimal.

To reflect our interest in understanding how exposure to the EITC at different points in childhood affects educational outcomes, we also examine each of these intermediate outcomes for each of our three age ranges of interest (panel B of Table 2.4). To do this, we interact EITC exposure with an indicator for whether the individual is 0 to 5, 6 to 12, or 13 to 18 at the time the outcome was measured. Annual pre-tax family earnings and mothers' extensive margin labor supply results are consistent across the three age ranges. We find some evidence that intensive margin labor supply effects are larger for mothers with teenagers compared to younger children. A $1,000 increase in EITC exposure when a child is 13 to 18 is correlated with mothers working 86 more hours annually. The coefficients on annual hours worked for the two younger age ranges are between 75 and 80 hours worked annually; these effects are statistically distinct (p-value 0.031). Similarly, we find evidence that test-score gains are larger among children aged 13 to 18 than children aged 6 to 12 (p-value 0.038).

If maternal time declines the most for children aged 0 to 5, this may partially explain why we find no significant effects of exposure to the EITC between ages 0 and 5 on later-life outcomes—the reductions in time spent with parents may partially offset the income gains to the family.[27] We find some evidence that maternal time spent with children declines the most for children under six at the time of EITC expansions, although it is not statistically different from the decline in maternal time spent with teenage children. We find the opposite age pattern for time spent with fathers: teenagers experience the largest reductions in time spent with father as a function of EITC generosity, while children under six experience small increases in time spent with father. These coefficients are not statistically different from one another, so we cannot rule out the scenario where time reductions are uniform across the three age ranges. Reductions in time spent with either parent (column 8) are substantively quite small and suggest that declines in time spent with either parent are comparable across each age range, alleviating some concern of differential effects of EITC exposure on time spent with parents across the three age ranges.

2.5.3 Instrumental-Variables Analysis

We next use an instrumental-variables strategy to directly analyze how a $1,000 increase in family income generated through policy-induced increases in the EITC affects education outcomes. Using EITC exposure as an instrument for family income, we model three first stage equations, one for each of the age intervals:

$$I_{i,(age)} = \beta_{1,age}EITC_{i,(0\text{-}5)} + \beta_{2,age}EITC_{i,(6\text{-}12)} + \beta_{3,age}EITC_{i,(13\text{-}18)} + \gamma\boldsymbol{X}_{i,age} + \varphi\boldsymbol{V}_{s,t} + \theta\boldsymbol{Z}_s + \alpha\boldsymbol{W}_t + \varepsilon_{i,age} \quad (2)$$

where $I_{i,\,age}$ represents family income (including imputed EITC benefits) for individual i at each age interval: 0 to 5, 6 to 12, and 13 to 18 years old, and is modeled as a function of EITC exposure at each of those age intervals with the full set of controls.

Using predicted family income $\hat{I}_{i,\text{age}}$ generated from equation (2), we then estimate the impact of increasing family income on education in the following second-stage equation:

$$Y_i = \beta_0 + \beta_1\hat{I}_{i(0-5)} + \beta_2\hat{I}_{i(6-12)} + \beta_3\hat{I}_{i(13-18)} + + \gamma\boldsymbol{X}_i + \varphi\boldsymbol{V}_{S,t} + \theta\boldsymbol{Z}_S + \alpha\boldsymbol{W}_t + \varepsilon_i \quad (3)$$

[27] Prior research has shown that parental time is most important for young children (Del Boca, Flinn, and Wiswall 2014); reductions in parental time for young children might be particularly detrimental for child outcomes.

Equation (3) parallels equation (1), except $EITC_{i,(0\text{-}5)}$, $EITC_{i,(6\text{-}12)}$, and $EITC_{i,(13\text{-}18)}$ are replaced with predicted family income in each age interval, $\hat{I}_{i(0-5)}$, $\hat{I}_{i(6-12)}$, and $\hat{I}_{i(13-18)}$, generated from equation (2). Results from this analysis indicate whether exogenous shocks to family income generated by expansions to the EITC affect subsequent educational attainment of children exposed to the EITC throughout childhood.

Using EITC exposure as an instrument for family income assumes that the EITC affects children's outcomes *solely* through its impact on family income. Table 2.4 shows that the EITC may also decrease the amount of time that mothers spend with their children, which could have a negative impact on children's subsequent outcomes. This would bias the IV estimates towards zero, in favor of finding a null result. However, it is also possible that the IV estimates could be biased upwards if the benefits from the EITC are not fully captured by family income. This could happen if, in addition to the direct benefits of increased family income, children's long-run outcomes improve because of non-income related benefits such as having a working mother serve as a positive role model that inspires the child to achieve more. Although the overall bias is unknown and could be positive or negative, we provide evidence that the EITC is strongly correlated with family income and robust to a number of different specifications of the first stage. Further, estimates from the reduced-form models imply that the overall effect of expansions to the EITC on education and employment outcomes is positive.

Table 2.5 presents estimates from the first-stage regressions of family income on EITC exposure. Since we are interested in predicting family income in three age ranges, we have three endogenous regressors, with three instruments (EITC exposure in each age range) for each endogenous regressor. Table 2.5 therefore presents nine first-stage coefficients regressing family income in each age range on EITC exposure in each of the three age ranges. We present results in three panels: panel A depicts results from regressing family income between age 0 and 5 on the three measures of EITC exposure, panel B presents results for family income between ages 6 to 12, and panel C presents results for family income between ages 13 to 18. We present several different specifications of the first-stage equations to illustrate the robustness of our findings: the first specification includes state, year, and cohort fixed effects, as well as demographic controls. The second specification adds state-by-year controls discussed in section I, the third specification adds interactions of race and gender with state and year fixed effects, and the fourth specification adds state-specific quadratic time trends. Since results are fairly consistent across specifications, we use the full-set-of-controls specification in column (4) as our main first-stage equation.[28]

Results indicate that EITC exposure in childhood has a substantial impact on family income during each of the three age ranges. Increasing EITC exposure by $1,000 when a child is 0 to 5 years old leads to a $10,000 increase in family income over that age range, which implies a roughly $2,000 annual increase in family income. We do not find much evidence that EITC exposure later in childhood (ages 6 to 18) has an impact on family income from ages 0 to 5, which is expected. However, in the specification that includes state-specific quadratic time trends, we do find some evidence that exposure to the EITC from ages 13 to 18 is correlated with family income from ages 0 to 5, but this is only marginally significant and may be due to unobserved family characteristics or a spurious correlation.

We find similar results for family income between ages 6 to 12 and family income between ages 13 to 18. A $1000 increase in EITC exposure leads to a $11,500 to $12,500 increase in family income, roughly $2,000 annually. We also find that EITC exposure from ages 6 to 12 has an impact on family income between ages 13 to 18. This correlation is robust to different specifications of the first-stage equation, suggesting that this cross-age correlation may not be spurious. This is plausible if we believe that the EITC increases permanent family income, such that exposure when a child is 6 to 12 increases maternal labor supply, which in turn increases future earnings. Previous research suggests that earnings growth does increase as a function of EITC generosity (Dahl, DeLeire, and Schwabish 2009); our findings are consistent with this previous research and suggests that exposure to the EITC not only increases contemporaneous earnings, but also future year earnings.

Results from the second-stage equations are presented in Table 2.6. Consistent with results presented thus far, we find positive effects of family income on education and employment outcomes for children once they reach adulthood. In particular, a $1,000 increase in family income between ages 13 and 18 increases the likelihood of completing high school by 0.2 percentage points and increases the number of years of schooling by 0.01 years. We also find positive associations between family income and college attendance and completion, though these results are not significant at conventional levels. Since our first-stage estimates imply that a $1,000 increase in the maximum EITC increases family income by approximately $12,000 over the six-year period between age 13 and 18 (roughly $2,000 per year), this implies a 2.5

[28] Second-stage results for different specifications presented in Appendix Table B1.7.

percentage point increase in high school completion and a 0.12 increase in years of schooling.

We do not find much evidence that family income between birth and age 12 leads to increases in educational attainment, although F-tests indicate that we cannot reject that coefficients for the three age ranges are statistically identical (Table 2.6). In one case, we find a marginally significant, negative association between family income from ages 6 to 12 and high school completion. Since our first-stage estimates revealed a correlation between EITC exposure between ages 6 to 12 and family income between ages 13 to 18, the coefficients on family income between ages 6 to 12 in Table 2.6 should be interpreted with some caution. The coefficients from Table 2.5 imply that the increase in family income between ages 13 to 18 as a function of EITC exposure between ages 6 to 12 is roughly half of the increase in income between ages 6 to 12. This would imply that a $1,000 increase in family income between ages 6 to 12 generated by EITC exposure between ages 6 to 12 would also increase family income between ages 13 to 18 by roughly $5000. The total effect should then be calculated by summing the coefficient on family income between ages 6 to 12, and half of the coefficient on family income between ages 13 to 18, roughly -0.0007. The coefficients on family income in these two age ranges thus partially offset each other in the case where a child is exposed to larger EITC benefits between ages 6 to 12 but not between ages 13 to 18. As the EITC is increasing over our time frame, children who are exposed to larger EITC benefits between ages 6 to 12 are also likely exposed to larger EITC benefits between ages 13 to 18.[29]

Turning to the results for employment and earnings, the IV estimates suggest that a $1,000 increase in after-tax family income leads to a 0.1 percentage point increase in the likelihood of being employed and a $57 increase in annual earnings in adulthood. We also find a significant relationship between family income between birth and age 5 and annual earnings in adulthood—a $1,000 increase in family income generated by the EITC leads to a $118 increase in annual earnings between ages 22 and 27. We find no clear impact of EITC generosity between birth and age twelve on any of the other education or employment outcomes in adulthood. Overall, our results imply that, conditional on exposure in early childhood, EITC generosity during the teenage years has the largest impact on education and employment outcomes in adulthood.

[29] In results not shown, we find that a $1,000 increase in EITC exposure between ages 6 to 12 is associated with a $500 increase in EITC exposure from ages 13 to 18.

2.6 Conclusion

This paper analyzed the long run effects of childhood EITC exposure on education and employment outcomes among individuals born between 1967 and 1995. Using variation in federal and state EITC benefits over time by family size, results indicate that the EITC significantly improves a number of outcomes for children, and these improvements persist into adulthood. After a policy-induced increase in EITC exposure of $1,000 between ages 13 and 18, we find that individuals are subsequently 1.3 percent more likely to complete high school by age 20 and 4.2 percent more likely to complete a college degree by age 26. These education gains also translate into increases in employment and earnings in adulthood. Estimates suggest that a $1,000 increase in EITC exposure from ages 13 to 18 leads to a 1.0-percent increase in the likelihood of being employed between ages 22 and 27, and a $560 (or 2.2 percent) increase in average annual earnings.[30]

We find little evidence that EITC exposure prior to age 13 affects education and employment outcomes in adulthood. This suggests that the EITC increases the educational attainment of children growing up in low-income households by increasing the cash on hand that a family has when their child is approaching college age. This is consistent with other recent findings in the literature linking social benefit programs to positive education outcomes for children (e.g. Cohodes et al. 2015). However, results for younger ages are noisily estimated and, for many outcomes, we are unable to rule out positive impacts of EITC exposure between birth and age 12 on subsequent educational attainment and employment outcomes. Given the large range of birth cohorts included in the sample, we have more variation in EITC exposure for the 13 to 18 age range than for earlier age ranges, which may also explain why we are only able to estimate significant positive effects for later ages. Since we include individuals born as early as 1967, we only observe significant changes in EITC exposure for these earlier cohorts when they were 13 to 18 years old. Results are robust to excluding earlier cohorts from our analysis, for instance, focusing on those born in 1975 or later. Further, since much of the federal and state policy variation in the EITC occurred over the last two decades, children born in the youngest cohorts of our analysis (1990-1995) may not be old enough to have fully realized their

[30] In results not shown, we also found that these individuals were more likely to report better health and to have earnings above 100, 150, and 200 percent of the federal poverty line as adults.

educational attainment and employment outcomes. More time may be required before we are fully able to identify the impacts of exposure to the EITC between birth and age 5 on education and employment outcomes in adulthood.

In examining the mechanisms that explain how increasing EITC generosity improves child education outcomes, we find that the EITC has a substantial impact on pre-tax family earnings. While a $1,000 increase in the maximum federal and state EITC increases imputed family EITC benefits by only $200, it increases pre-tax family earnings by $2,700. This increase in family income is accompanied by significant increases in maternal labor supply (women are 4.6 percentage points more likely to be working) and minor reductions in time spent with children (11 minutes per day, although not statistically significant). This implies that the primary channel through which the EITC affects educational and employment outcomes is through increases in family income. In using EITC exposure as an instrument for family income, we find that a $1,000 increase in family income between ages 13 and 18 leads to a 0.2-percent increase in the likelihood of completing high school by age 20 and a 0.1-percent increase in the probability of being employed between ages 22 and 27.

This analysis has shown that, in addition to lifting millions of households out of poverty each year, the EITC also improves a number of long-term outcomes for children from economically-disadvantaged households. The EITC is a wide-reaching program that distributed an average of $2,400 per household to nearly 28 million families in the United States in 2013 (IRS 2013). Recent estimates suggest that fully half of all households with children will claim the EITC at some point over an 18-year period (Horowitz and Dowd 2011). This analysis has shown that the EITC also helps children finish high school and complete college, which supports previous research linking the EITC to higher test scores among low-income children (Dahl and Lochner 2012, 2017). These gains also translate into increases in employment and annual earnings once these individuals reach adulthood. Together, these results provide further evidence that the EITC not only works to lift families out of poverty for the current generation, but also provides hope of upward mobility for future generations of children growing up in economically-disadvantaged households.

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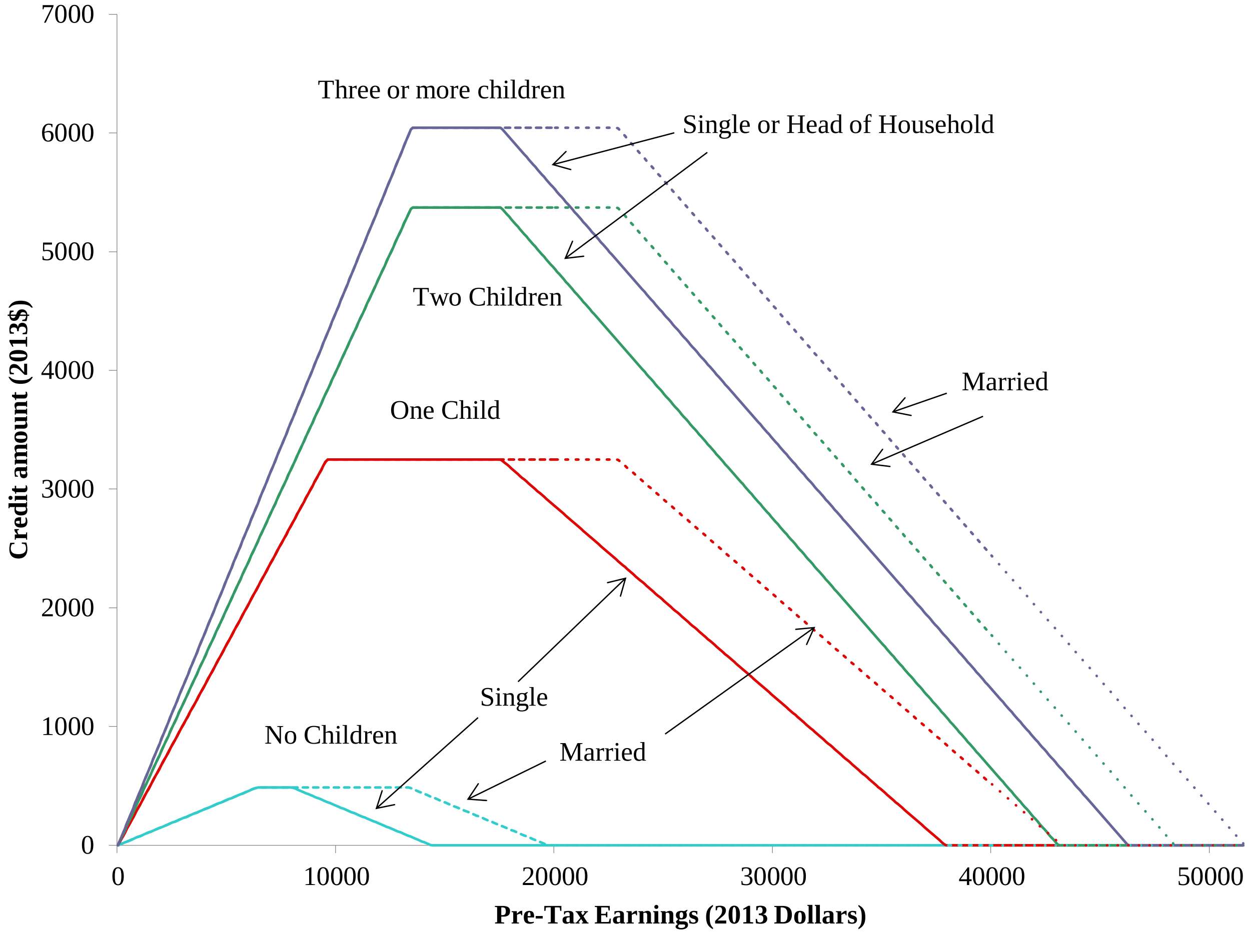
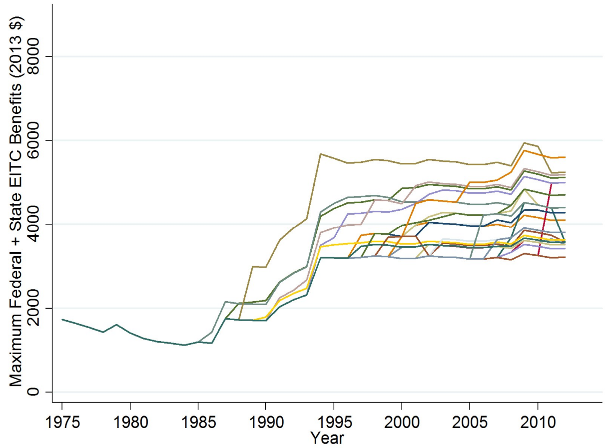
2.8 Figures and Tables

Figure 2.1: 2013 EITC Schedule by Number of Children and Marital Status

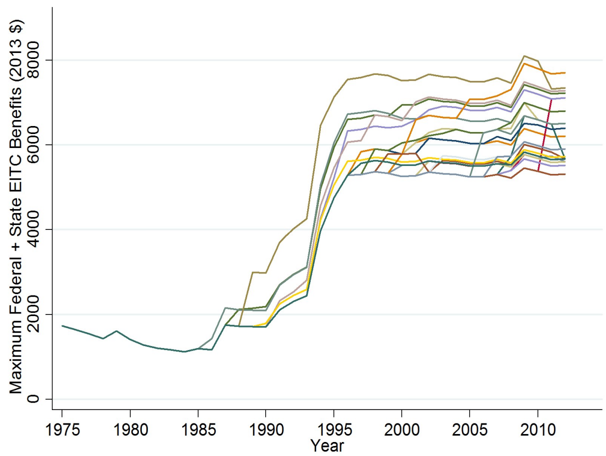


Source: Authors' calculations from EITC parameters in 2013.

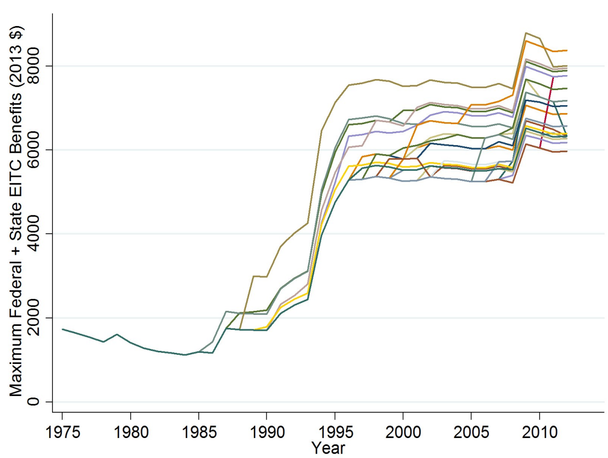
Figure 2.2: Federal and State EITC Exposure by Year and State



Panel A: One Child



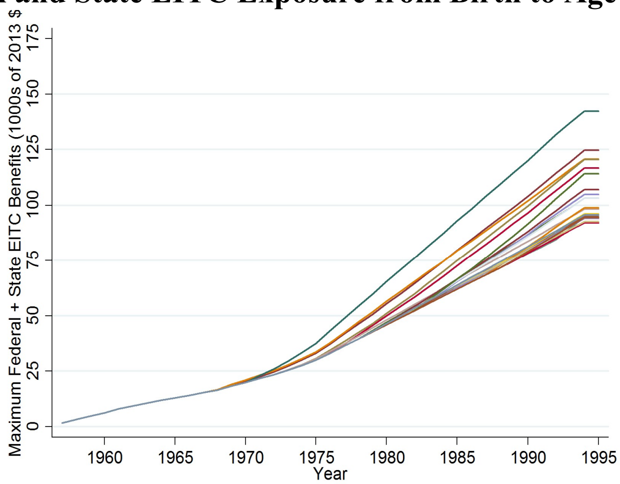
Panel B: Two Children



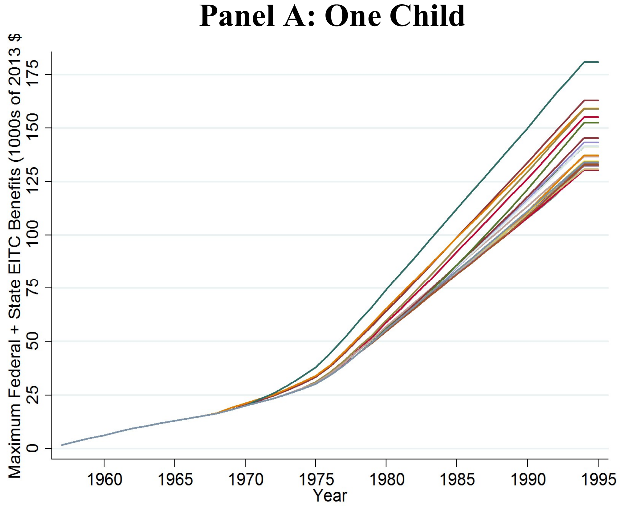
Panel C: Three (or More) Children

Source: Authors' calculations. Notes: EITC exposure defined as the maximum potential federal and state EITC an individual could receive in a given year and state for a one- two- or three-child household. Lowest line denotes federal EITC for states with no state EITC.

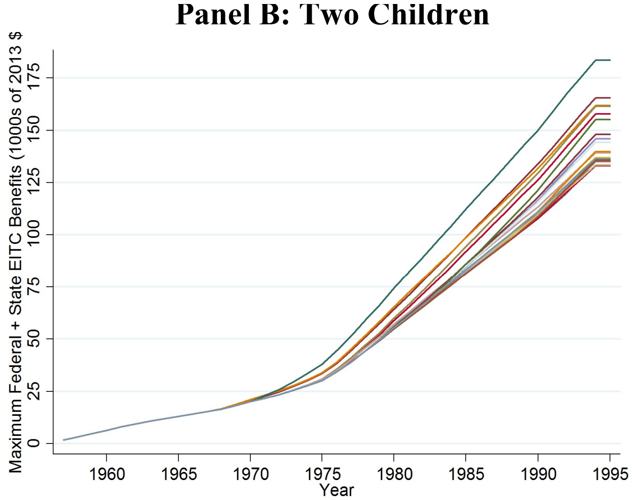
Figure 2.3: Federal and State EITC Exposure from Birth to Age 18 by Cohort and State



Panel A: One Child



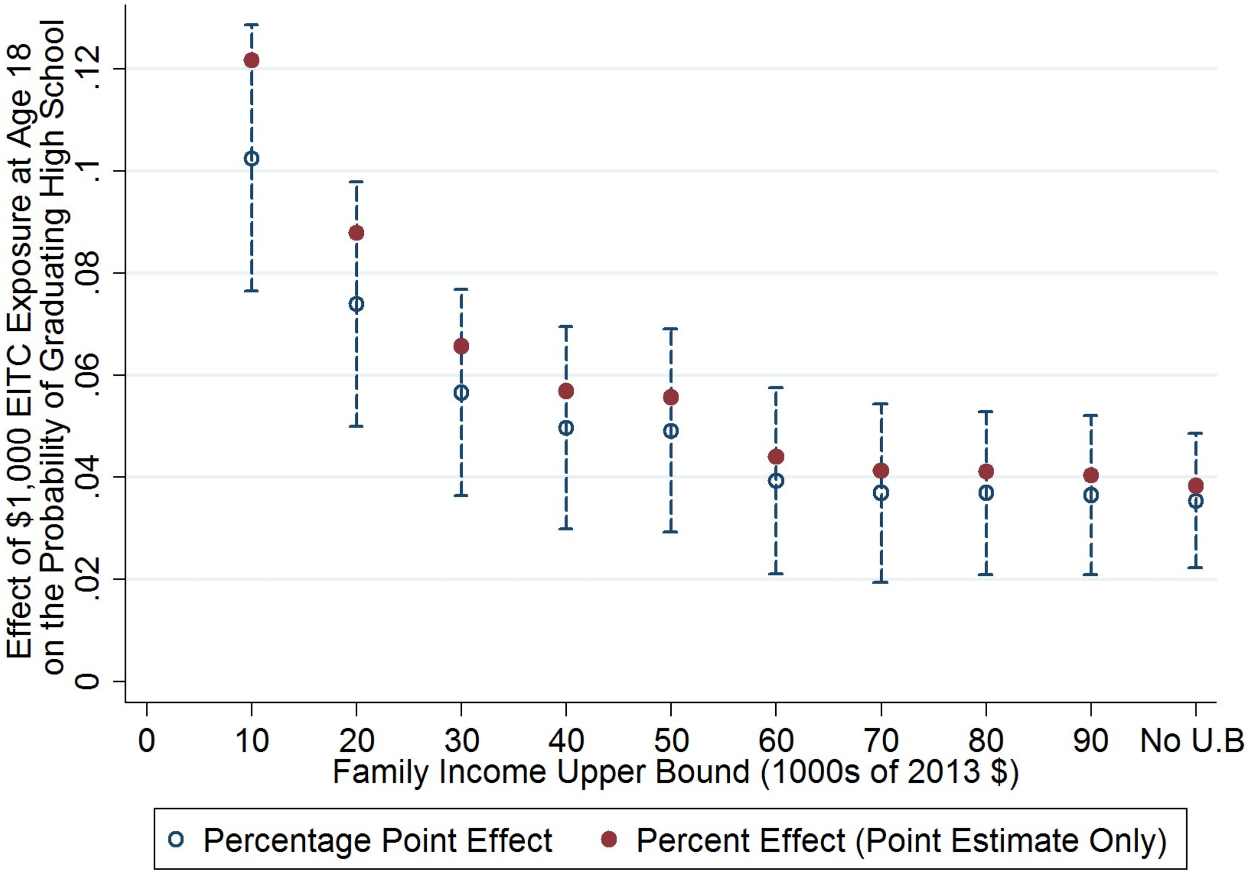
Panel B: Two Children



Panel C: Three (or More) Children

Source: Authors' calculations. Notes: EITC exposure defined as the maximum potential federal and state EITC an individual could receive in a given year and state for a one- two- or three-child household. Lowest line denotes federal EITC benefits for states with no state EITC.

Figure 2.4: EITC Exposure Has Largest Benefits for Lowest-Income Families



Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: Each point represents the estimate of $1,000 of EITC exposure on the probability of high school graduation from a separate OLS regression. EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. Regressions include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Estimates comes from an estimating equation similar to (1) except that EITC exposure at age 18 is used instead of EITC exposure at 0-5, 6-12, and 13-18. All values weighted by average childhood PSID weights. 95% confidence intervals represented by vertical bars.

Table 2.1: Descriptive Statistics of Sample

Variable	Mean	Standard Deviation
Female	0.49	0.50
Black	0.18	0.38
Hispanic	0.01	0.08
Siblings	2.06	1.35
Ever-Married Parents	0.88	0.32
Mom Finished High School	0.95	0.21
Mom Attended Some College	0.69	0.46
Dad Finished High School	0.86	0.34
Dad Attended Some College	0.60	0.49
EITC Exposure between 0 and 5 ($1,000s)	16.62	7.96
EITC Exposure between 6 and 12 ($1,000s)	31.12	8.87
EITC Exposure between 13 and 18 ($1,000s)	29.28	6.41
EITC Exposure between 0 and 18 ($1,000s)	77.01	18.09
EITC Exposure at Age 18 ($1,000s)	4.60	1.58
Family Income at Age 18 ($1,000s)	77.76	80.84
Observations	3,495	

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: Children born between 1967 and 1997 that meet the following criteria: observed between age 0-5, age 6-12, and age 13-18, observed at or after age 18, and either finished or dropped out of high school. All means weighted by average childhood PSID weight. All dollar measures are in 2013 real dollars. Average family income is high due to a few wealthy households: the median is 43.1 and the mean falls to 61.6 without the three millionaire-household children. EITC exposure is defined as the maximum potential federal and state EITC a household could receive and is a function of year, state, and number of household children.

Table 2.2: Effect of EITC Exposure on Education, Employment, and Earnings (OLS Results)

Dependent Variable:	High School Graduate	At Least Some College	College Graduate	Highest Grade Completed	Employed	Annual Earnings (2013 $)
Mean dependent variable:	0.92	0.52	0.31	13.7	0.817	25,391
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
EITC Exposure from Age 0 to 5	-0.005	-0.000	-0.007	-0.024	0.021	646.1
(in Real $1,000s)	(0.005)	(0.006)	(0.019)	(0.071)	(0.022)	(818.3)
EITC Exposure from Age 6 to 12	-0.003	0.002	0.009	0.008	-0.002	42.4
(in Real $1,000s)	(0.003)	(0.005)	(0.006)	(0.022)	(0.007)	(415.1)
EITC Exposure from Age 13 to 18	0.012***	0.006	0.013**	0.081***	0.008*	564.0**
(in Real $1,000s)	(0.003)	(0.007)	(0.005)	(0.025)	(0.004)	(244.9)
P-value: F-Test Identical Estimates	0.006	0.837	0.513	0.108	0.562	0.649
Observations	3,495	3,309	2,506	2,506	1,758	1,758
R-squared	0.331	0.420	0.380	0.470	0.310	0.426

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20; some college evaluated by age 24; college graduation and highest grade completed evaluated by age 26. Employment and earnings measured between age 22 and 27; average value used if observed more than once during these ages. Results reflect estimating equation (1) and include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Results in columns 1 and 2 are similar if the sample is restricted to those in columns 3 and 4. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Effect of EITC Exposure on the High-School Graduation of Subgroups (OLS Results)

								Placebo Test
Subgroup:	All	Black	Male	Female	Black-Male	Single-Parent Household	Lower-Ed. Parents	Higher-Ed. Parents
Mean Dependent Variable:	0.92	0.91	0.92	0.93	0.90	0.85	0.85	0.95
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EITC Exposure from Age 0 to 5	-0.005	-0.004	-0.005	-0.002	-0.006	-0.003	-0.004	0.007
(in Real $1,000s)	(0.005)	(0.007)	(0.006)	(0.008)	(0.011)	(0.008)	(0.013)	(0.007)
EITC Exposure from Age 6 to 12	-0.003	-0.007	-0.003	-0.003	-0.011*	-0.009**	-0.006	-0.003
(in Real $1,000s)	(0.003)	(0.005)	(0.005)	(0.003)	(0.006)	(0.004)	(0.008)	(0.003)
EITC Exposure from Age 13 to 18	0.012***	0.024***	0.009**	0.011***	0.027**	0.017***	0.026*	0.002
(in Real $1,000s)	(0.003)	(0.008)	(0.005)	(0.004)	(0.011)	(0.004)	(0.014)	(0.003)
P-value: F-Test Identical Estimates	0.006	0.011	0.050	0.040	0.007	0.003	0.091	0.444
Mean EITC Exposure (from Age 0 to	34.9	34.9	36.9	33.2	38.0	34.6	33.2	35.6
Observations	3,495	1,503	1,681	1,814	706	1,468	632	1,846
R-squared	0.331	0.359	0.390	0.417	0.480	0.537	0.654	0.444

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. Results reflect estimating equation (1) and include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Lower-ed. parents defined as both parents have at most 12 years of education. High-ed. parents defined as at least one parent has 16 years of education. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Effect of EITC Exposure on Intermediate Outcomes (OLS Results)

Dependent Variable:	EITC Benefits (in Real $1000s)	Family Earnings (in Real $1000s)	Mother Working	Mother's Annual Hours Worked	Standardized Test Scores	Daily Minutes Spent with Mother	Daily Minutes Spent with Father	Daily Minutes Spent with Either Parent
Mean dependent variable:	0.60	64.3	0.48	840.4	0.00	151.6	73.5	225.0
Panel A: Contemporaneous Effect of EITC Exposure (All Ages Pooled)								
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EITC Exposure	0.223***	2.749**	0.046***	82.7***	0.129**	-10.53	-0.78	-11.31
(Contemporaneous, Real $1,000s)	(0.025)	(1.269)	(0.009)	(17.6)	(0.057)	(7.08)	(5.58)	(9.62)
Observations	48,623	48,623	48,623	48,623	4,533	4,975	4,975	4,975
Unique Observations	3,495	3,495	3,495	3,495	2,489	2,539	2,539	2,539
R-squared	0.195	0.300	0.195	0.178	0.396	0.276	0.189	0.285
Panel B: Contemporaneous Effect of EITC Exposure Varies by Age of Child								
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EITC Exposure x (Age 0 to 5)	0.224***	2.531*	0.043***	80.2***	--	-10.14	1.85	-8.29
	(0.030)	(1.409)	(0.011)	(20.4)	--	(11.49)	(7.12)	(12.60)
EITC Exposure x (Age 6 to 12)	0.224***	2.503**	0.043***	74.4***	0.125**	-11.05	-3.08	-14.13
	(0.027)	(1.210)	(0.010)	(18.4)	(0.057)	(6.65)	(5.58)	(10.11)
EITC Exposure x (Age 13 to 18)	0.223***	2.854**	0.048***	85.7***	0.278***	-2.06	-10.21	-12.27
	(0.025)	(1.322)	(0.009)	(17.4)	(0.089)	(9.23)	(8.13)	(16.03)
P-value: F-Test Identical Estimates	0.970	0.440	0.256	0.031	0.038	0.577	0.410	0.874
Observations	48,623	48,623	48,623	48,623	4,533	4,975	4,975	4,975
Unique Observations	3,495	3,495	3,495	3,495	2,489	2,539	2,539	2,539
R-squared	0.195	0.300	0.195	0.178	0.396	0.276	0.190	0.285

Source: 1968 to 2013 PSID and the PSID's 1997, 2002, and 2007 Child Development Survey (CDS). Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. All regressions include full set of controls: demographic controls, state-year controls, state, cohort, and year fixed effects, along with state-specific quadratic time trends. EITC benefits are imputed by authors and a function of year, state, marital status, number of household children, and household earnings. Family earnings are pre-tax sum of parental earnings. Outcomes in columns 1-4 measured between 1975 and 2013 when individuals in the main sample are between 0 and 18 years old; outcomes in columns 5-8 measured in the 1997, 2002, and 2007 CDS. Column 2 also controls for a cubic in lagged family income following Dahl and Lochner (2012; 2017). Heteroskedasticity-robust standard errors clustered at the household level are in parentheses. Results weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: First Stage Estimates (Effect of EITC on Family Income at Different Ages)

VARIABLES	(1)	(2)	(3)	(4)
Panel A: Dependent Variable Family Income from Age 0 to 5 (in Real $1,000s)				
EITC Exposure from Age 0 to 5	9.39**	9.55***	10.42***	10.00**
(in Real $1,000s)	(3.59)	(3.46)	(3.67)	(4.00)
EITC Exposure from Age 6 to 12	-0.77	-1.12	-2.16	-3.22
(in Real $1,000s)	(2.25)	(2.21)	(2.45)	(2.70)
EITC Exposure from Age 13 to 18	3.66	3.11	3.75	5.18*
(in Real $1,000s)	(2.69)	(2.67)	(2.74)	(2.87)
Panel B: Dependent Variable Family Income from Age 6 to 12 (in Real $1,000s)				
EITC Exposure from Age 0 to 5	-3.96	-3.96	-2.59	-3.76
(in Real $1,000s)	(5.55)	(5.58)	(5.52)	(5.97)
EITC Exposure from Age 6 to 12	11.31**	11.60**	10.73**	11.56**
(in Real $1,000s)	(4.95)	(5.00)	(4.64)	(5.37)
EITC Exposure from Age 13 to 18	2.42	1.83	1.54	3.52
(in Real $1,000s)	(4.77)	(4.51)	(4.60)	(4.33)
Panel C: Dependent Variable Family Income from Age 13 to 18 (in Real $1,000s)				
EITC Exposure from Age 0 to 5	-3.60	-3.75	-2.38	-1.96
(in Real $1,000s)	(4.31)	(4.27)	(3.66)	(3.43)
EITC Exposure from Age 6 to 12	7.07**	7.11***	6.63**	5.78*
(in Real $1,000s)	(2.66)	(2.54)	(2.68)	(3.03)
EITC Exposure from Age 13 to 18	11.24***	10.72***	10.46**	12.48***
(in Real $1,000s)	(4.04)	(3.89)	(3.95)	(3.61)
Controls				
State, Cohort, Year Fixed Effects	X	X	X	X
Demographic Controls	X	X	X	X
State-Year Controls		X	X	X
Interaction Controls			X	X
State-Specific Quadratic Time Trends				X
Observations	3,495	3,495	3,495	3,495
Kleibergen-Paap rk LM Stat.	8.1	9.5	7.5	7.0
Kleibergen-Paap rk Wald F Stat.	4.0	5.1	3.7	3.2

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. Each column in each panel represents a separate regression reflect estimating equation (2). Full set of controls in column (4). EITC exposure and family income are discounted at a 3 percent annual rate from age 18 (Chetty, Friedman, and Rockoff 2011). Stata's ivreg2 combines the three first stage regressions for the three instruments into one Kleibergen-Paap rk LM statistic (Kleibergen and Paap 2006) and one Kleibergen-Paap rk Wald F statistic (Bound and Jaeger 1995; Stock and Yogo 2005). Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Effect of EITC Exposure on Education, Employment, and Earnings (IV Results)

Dependent Variable:	High School Graduate	At Least Some College	College Graduate	Highest Grade Completed	Employed	Annual Earnings (2013 $)
Mean dependent variable:	0.92	0.52	0.31	13.7	0.817	25,391
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Family Income from Age 0 to 5	-0.0001	0.0002	0.0004	0.0024	0.0017	117.5*
(in Real $1000s)	(0.0013)	(0.0009)	(0.0018)	(0.0061)	(0.0016)	(68.6)
Family Income from Age 6 to 12	-0.0017*	0.0001	0.0015	-0.0009	-0.0008	-26.6
(in Real $1000s)	(0.0010)	(0.0014)	(0.0015)	(0.0059)	(0.0007)	(32.4)
Family Income from Age 13 to 18	0.0021*	0.0009	0.0013	0.0101**	0.0011***	57.2*
(in Real $1000s)	(0.0011)	(0.0014)	(0.0012)	(0.0045)	(0.0004)	(32.4)
Implied "Total" (Sum of Coefficient	0.0003	0.0012	0.0032	0.0116	0.002	148.1
P-value: F-Test Identical Estimates	0.123	0.938	0.864	0.551	0.146	0.253
Observations	3,495	3,309	2,506	2,506	1,758	1,758

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20; some college evaluated by age 24; college graduation and highest grade completed evaluated by age 26. Employment and earnings measured between age 22 and 27 and average value used if observed more than once during these ages. All regressions reflect estimating equation (3) and include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. EITC exposure and family income are discounted at a 3 percent annual rate from age 18 (Chetty, Friedman, and Rockoff 2011). Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

CHAPTER 3

Unintended Consequences? More Marriage, More Children, and the EITC

3.1 Introduction

There has long been a concern that public assistance programs may discourage marriage among low-income households. This is an important policy question since marriage is associated with positive health and economic outcomes for adults (Waite 1995; Sawhill 2014) and their children (McLanahan and Sandefur 1994; Chetty et al. 2014). Although welfare may have led to small decreases in marriage (Murray 1993; Moffitt 1998; Grogger and Bronars 2001)[1], the overall effect of the Earned Income Tax Credit (EITC) is theoretically ambiguous since it provides a financial incentive for some couples to marry and a disincentive for others to do so. Similarly, the effect of the EITC on fertility is theoretically ambiguous: on one hand, the EITC is only available to working parents and may encourage fertility up to a point; on the other hand, since a household must work to benefit from the EITC, this increase in labor supply may discourage fertility (Killingsworth and Heckman 1986; Angrist and Evans 1998). This paper uses individual-level panel data to explore the effect of the EITC on both marriage and fertility.

After welfare reform in the 1990s, the Earned Income Tax Credit (EITC) replaced AFDC as the largest U.S.-cash-transfer program. In 2013, the EITC redistributed $66 billion to over 28 million low-income households and lifted 9.4 million individuals out of poverty. EITC benefits are available to low-income working adults with children[2] and has encouraged millions of

[1] Before welfare reform in the 1990s, AFDC was largely conditional on being a single parent. AFDC-UP was available to two-parent households but only if one parent was unemployed with a significant work history and the household had income and assets below a low state-specific threshold. One survey of empirical estimates (Moffitt 1998, p.74) shows that a 25 percent reduction in welfare benefits would have reduced nonmarital births by as much as 30 percent or by as little as 4 percent. However, Hoynes (1997) and Schoeni and Blank (2000) find that welfare reform did not affect marriage rates. On the other hand, it is possible that welfare encouraged marriage since welfare reform encouraged mothers to work and led to lower marriage rates (Bitler, Gelbach, Hoynes, and Zavodny 2004).
[2] A small EITC for adults without children became available in 1994.

mothers to join the labor force since 1975 (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Bastian 2017a).

Intuition and economic theory would suggest that the EITC may affect household fertility and marriage decisions. Households may increase fertility to maximize EITC benefits, but also may decrease fertility in order to work and receive EITC benefits. In a simple model, I illustrate how the EITC encourages fertility for some households and discourages fertility for others. Regarding marriage, the EITC provides a marriage bonus for some couples and a marriage penalty for others. The relative sizes of these two groups is not clear and has changed over time (Acs and Maag 2015). In addition to tax-related incentives, the EITC may also strengthen existing marriages by increasing household resources and reducing financial insecurity (Mendenhall et al. 2012; Jones and Michelmore 2016).

Heterogeneous responses to the EITC – and changes to the EITC program over time – could help explain why existing evidence on how the EITC affects fertility and marriage has been mixed.[3] Evidence regarding fertility is inconclusive and largely limited to the context of the 1990s and most evidence regarding marriage shows that the EITC has had small negative effects (detailed in section 3.2).

This paper uses a novel approach to identify the effect of the EITC on both fertility and marriage. Using 30 years of household-level panel data and state-by-year variation in state EITC rates, I control for lagged fertility and marital status to identify the effects of state EITC expansions on the decisions of households to marry and have children. Multiple observations of each household and state-by-year variation in EITC generosity enable relatively precise estimation of the EITC's impact on fertility and marriage.

Regarding fertility, estimates suggest that a 10-percentage-point increase in state EITC rates led to a 1.2-percentage-point increase in the likelihood of having an additional child – on average – in the following year. Fertility results are robust to various sets of controls and do not simply reflect a shift in birth timing since effects are still evident several years later. Heterogeneous analysis shows that fertility responses are concentrated among households that would be expected to respond to the incentives of the EITC. For example, households that would be eligible for additional EITC benefits if they had another child increase their fertility, but

[3] The EITC marriage penalty was decreased in 2003; studies before and after this may find different overall effects of the EITC on marriage.

placebo households not eligible for additional EITC benefits do not. Additionally, fertility responses are only found among younger household heads still in their childbearing years and able to respond to such incentives.

Regarding marriage, estimates suggest that a 10-percentage-point increase in state EITC rates led to a 1.1-percentage-point (or 2.4 percent) increase in the probability of being married in the following year. Marriage results are robust to various sets of controls, are largest for previously unmarried adults, and may also be positive for currently married adults. This suggests that, on average, the EITC encourages new marriages and may help married couples stay married. Corroborating the positive average effect of the EITC on marriage rates, I also find that the EITC decreases nonmarital cohabitation: a 10-percentage-point increase in state EITC rates led to a 1.7-percentage-point decline in cohabitation. These results give pause to concerns that the EITC has a negative effect on marriage and arguments against additional expansions of the EITC because of potential marriage disincentives (Rachidi 2015).

These fertility and marriage results may also have implications for other EITC papers that condition on marital status or use difference-in-differences (DD), where one of the differences is determined by number of children. If number of children is endogenous with EITC expansions, then the stable-group-composition condition required for DD may not be met. If individuals are endogenously switching from the control to the treatment group, this could bias DD estimates. Furthermore, many studies restrict the sample to unmarried women, and if the EITC is affecting the composition of married women, this could also lead to biased results.

3.2 Federal and State EITC Expansions and Effects of the EITC

As of 2013, the EITC was worth up to $8,462 per household, redistributed a total of $66 billion to over 28 million low-income households, and was one of the most important programs in the U.S. safety net.[4] The EITC is an earnings subsidy with a phase-in region, a plateau region, and a phase-out region (Figure 3.1). Perhaps the best-documented effect of the EITC has been to

[4] The EITC is also administratively efficient. About 0.3 percent of the EITC budget goes to state and federal overhead administrative costs, less than Medicaid (4.6 percent), SNAP (5.4 percent), public housing vouchers (9.1 percent), SSI (7.2 percent), and subsidized school lunch (2.5 percent). Source CBPP (2012)

encourage low-income households to increase labor supply on the extensive margin.[5] The EITC began in 1975 as a 10 percent earnings subsidy for low-income working parents, and did not depend on number of children (only that there was at least one), marital status (though benefits were a function of household income), or state of residence. Since 1975, the EITC has been expanded numerous times, including a phase-in rate increase in 1986, additional benefits for families with at least two children in 1991, a large phase-in rate increases between 1993 and 1996, extending the plateau region for married couples in 2003 to decrease the "marriage penalty", and additional benefits for families with at least three children in 2009.

Beginning with Rhode Island in 1986, states began implementing their own EITCs and by 2013 half of all states had one in place. State EITCs are generally a fixed percentage of federal EITC benefits (between 3.5 and 40 percent in 2013). States with an EITC can be found all over the country and cannot be stereotyped as liberal or conservative. For example, Washington, Pennsylvania, and Alabama do not have an EITC, while Oregon, New York, and Louisiana do (as of 2016). Figure 3.2 shows the year that each state EITC was implemented and the 2013 rate. There is substantial cross-state variation in when these state policies were enacted and how generous they were. Additionally, there is considerable within-state variation in EITC generosity, reflecting two sources. One source comes from federal EITC expansions: state EITCs that are a fixed fraction of the federal EITC automatically become more generous when the federal EITC is expanded. A second source of variation comes from changes in state EITC rates (usually increases but occasionally decreases or even complete phase outs). Figure 3.3 shows a histogram of the number of times that each state changed their EITC rate (as of 2013). The zeros in this figure represent the half of all states that do not have an EITC and never did. After enacting an EITC, five states never adjusted their rates, but nine states adjusted their rates between three and eight more times.

The EITC has transformed the U.S. by raising maternal employment (Eissa and Liebman 1996; Meyer and Rosenbaum 2001; Bastian 2017a), earnings (Dahl, DeLeire, and Schwabish 2009), and health (Evans and Garthwaite 2014), as well as decreasing rates of poverty (Hoynes and Patel 2015), and decreasing the social stigma against working women (Bastian 2017a). The EITC has also helped children of EITC recipients by improving health (Hoynes, Miller, and

[5] Households may also have an incentive to decrease (or increase) labor supply on the intensive margin, but such results are small and hard to detect empirically (Meyer, 2002; Saez, 2002; Eissa and Hoynes, 2006a).

Simon 2013; Averett and Wang 2015), childhood test scores (Chetty, Friedman, and Rockoff 2011; Dahl and Lochner 2012), and longer-run outcomes such as educational attainment (Manoli and Turner 2014; Bastian and Michelmore 2017) and employment and earnings when these children grow up (Bastian and Michelmore 2017). The EITC may also have led to lower pre-tax wages of low-skill workers (Leigh 2010; Rothstein 2010) and higher tax revenue for local, state, and federal governments (Bastian 2017b). See Nichols and Rothstein (2015) or Hoynes and Rothstein (2017) for recent a review of the EITC literature.

Since EITC benefits depend on household number of children and marital status, the EITC also likely affects fertility and marriage decisions. Regarding fertility, evidence has been mixed: Baughman and Dickert-Conlin (2003) finds an increase in the rate of first births for nonwhite women; Baughman and Dickert-Conlin (2009) finds a decrease in higher-order births for white women; and Duchovny (2001) finds an increase in second children for white-married and nonwhite-unmarried women. These studies use aggregate birth-records data or cross-sectional observations and are limited to the context of the 1990s.

The average effect of the EITC on marriage is theoretically ambiguous. The marriage incentives of the EITC are couple-specific and depend on the distribution of earnings between the two adults and how they claim custody of their children on their taxes (Acs and Maag 2015). These marriage incentives have also changed over time, especially after 2003 when the plateau region of the EITC schedule was extended for married couples in an attempt to reduce the marriage penalty. Some studies show that, on average, the EITC provides a marriage penalty (Alm, Dickert-Colin, and Whittington 1999; Holtzblatt and Rebelein 2000; Ellwood 2000; Lin and Tong 2012), while others show that the EITC provides a marriage bonus (Acs and Maag 2015). In addition to this financial incentive, the EITC could strengthen existing marriages by increasing household resources and reducing financial insecurity (Mendenhall et al. 2012, Jones and Michelmore 2016). Evan among studies looking at contexts where the EITC leads to marriage penalties on average, lower observed marriage rates are not always observed empirically (Ellwood 2000). Still, with few exceptions,[6] there appears to be an emerging

[6] Eissa and Hoynes (2004) find that the EITC increases marriage rates for very low-income tax payers.

consensus that the EITC leads to small decreases in marriage (Dickert-Colin and Houser 2002; Acs and Maag 2005; Fisher 2011; Herbst 2011; Michelmore 2015).[7]

3.3 Theoretical Framework: The Effect of the EITC on Fertility and Marriage

3.3.1 Fertility

The EITC is available to working parents and is based on household earnings, providing a financial incentive for some families to adjust fertility and marriage decisions. Fertility responses may occur at the extensive or intensive margin (Aaronson, Lange, and Mazumder 2014). When the EITC began in 1975, it was available to parents but did not provide additional benefits for having more than one child. In this scenario, the EITC subsidized childbearing on the extensive margin but not the intensive margin. In 1991 and 2009 the EITC was expanded so that women with at least two children and three children received additional EITC benefits. In these scenarios, the EITC subsidized childbearing on the extensive margin and (to a point) the intensive margin, by increasing after-tax household earnings. However, the EITC may also discourage higher-order births due to increased opportunity costs and childcare costs associated with higher labor supply. How the EITC affects individual households depends on preferences, whether the mother works, and various costs associated with working.

The following discrete-choice model illustrates how the EITC encourages some households to *increase* their fertility, but encourages others to *decrease* fertility.[8] For simplicity, I assume that households consist of one adult, working is a binary decision, and number of kids is 0, 1, or 2.

$$V_i(l,k) = \max_{\mathrm{l,k}} \left[\alpha \log\left(1_{\{l=1\}}w_i + n_i - \gamma_i 1_{\{l=1\}}k + 1_{\{k=1,l=1\}}E_1 + 1_{\{k=2,l=1\}}E_2\right) + \beta_i k\right] \quad (1)$$

Each household i maximizes her utility by chooses whether to work ($l = 1$) or not ($l = 0$) and how many kids to have (k can be 0, 1, or 2). If a household works she earns w_i; independent of whether she works she receives non-labor income n_i; γ_i is the per-child cost (e.g. childcare) to household i if she works; β_i represents the utility that household i derive from each child she

[7] Popular press example: http://www.usnews.com/opinion/economic-intelligence/2015/11/12/we-penalize-marriage-for-low-income-couples-and-it-might-be-getting-worse.

[8] Of course, fertility decisions in this model should be thought of as expected number of future children.

has; and E_1 are EITC benefits earned if the household works and has exactly one child; and E_2 are EITC benefits earned if the household works and has exactly two children.[9] If the household has no children, then she receives zero EITC benefits.

The following illustrates how increases in the EITC could lead some household types to increase fertility and others to decrease fertility. Assume an EITC expansion (resembling the early 1990s EITC expansions) where $E_1^{new} > E_1^{old}$ and $E_2^{new} > E_2^{old}$. One type of household that would *decrease* their fertility in response to this EITC expansion would satisfy the following two conditions.

$$V_i\left(l = 1, k = 1 \mid E_1^{old}, E_2^{old}\right) < V_i\left(l = 0, k = 2 \mid E_1^{old}, E_2^{old}\right) \tag{2}$$
$$V_i(l = 1, k = 1 \mid E_1^{new}, E_2^{new}) > V_i(l = 0, k = 2 \mid E_1^{new}, E_2^{new})$$

Such households would choose to not work and have two children under the old EITC and choose to work and have one child under the new EITC. Normalizing $\alpha = 1$, the two inequalities in equation (2) imply the following:

$$\log\left(w_i + n_i - \gamma_i + E_1^{old}\right) < \log(n_i) + \beta_i \tag{3}$$
$$\log(w_i + n_i - \gamma_i + E_1^{new}) > \log(n_i) + \beta_i$$

which implies that:

$$E_1^{old} < \mathrm{n} * \mathrm{e}^{\beta_\mathrm{i}} + \gamma_i - w_i - n_i < E_1^{new}. \tag{4}$$

Households satisfying equation (4) would *decrease* fertility in response to the EITC expansion.

On the other hand, one type of household that would *increase* their fertility in response to the same EITC expansion would satisfy the following two conditions.

$$V_i\left(l = 1, k = 2 \mid E_1^{old}, E_2^{old}\right) < V_i\left(l = 1, k = 1 \mid E_1^{old}, E_2^{old}\right) \tag{5}$$
$$V_i(l = 1, k = 2 \mid E_1^{new}, E_2^{new}) > V_i(l = 1, k = 1 \mid E_1^{new}, E_2^{new})$$

[9] Without specifying the distribution of $(w_i, n_i, \beta_i, \gamma_i)$, I assume that combinations of $(w_i, n_i, \beta_i, \gamma_i)$ exist such that each of the six combinations of l and k are chosen by at least some types of households.

Such households would choose to work and have one child under the old EITC and choose to work and have two children under the new EITC. Normalizing $\alpha = 1$, these inequalities imply that:

$$\frac{w_i+n_i+E_2^{old}-2\gamma_i}{w_i+n_i+E_1^{old}-\gamma_i} < e^{\beta_i} < \frac{w_i+n_i+E_2^{new}-2\gamma_i}{w_i+n_i+E_1^{new}-\gamma_i}. \tag{6}$$

Households satisfying equation (6) would *increase* fertility in response to the EITC expansion.

The simple illustration above shows that in response to one type of EITC expansion (resembling the early 1990s EITC expansions) some households would choose to increase fertility and others would choose to decrease fertility.[10] This is partly due to the tradeoff households face from having more children to maximize EITC benefits and the higher costs associated with working with more children. The overall average effect of EITC expansions on fertility will depend on the relative size of each household type in the population, which could vary by state and change over time.

3.3.2 Marriage

The EITC also likely affects marriage decisions, and as with fertility, the EITC will encourage marriage for some families and discourage it for others. For example, since EITC benefits are only available for working adults with children, a non-working adult with children would have a financial incentive to marry a working partner and file joint taxes. On the other hand, two working adults with children may have a disincentive to marry (and file taxes separately) if their combined earnings would put them beyond the EITC income limit. The incentive to marry or not depends on the intrahousehold distribution of earnings and children. Figure 3.4 shows how marriage is financially rewarded or penalized conditional on household earnings for different types of households. As with fertility, the overall effect of the EITC on marriage will depend on the relative size of each household type in the population, which could vary by state and change over time.

[10] Similar results could be shown for other types of EITC expansions.

3.4 Fertility and Marriage Trends, PSID Data Summary Statistics, Empirical Strategy

Fertility has steadily declined over the last 50 years, except for a slight uptick between the mid-1980s and the mid-1990s (Figure 3.5A). The fraction of married adults has also been decreasing over time while the fraction of never married adults has been increasing (Figure 3.5B). In this context of declining fertility and marriage, I look at the impact of three decades' worth of state EITC expansions on marriage and fertility rates.

Data from the 1980 to 2013 Panel Survey of Income Dynamics (PSID) is used and observations are at the household-head-by-year level. The sample contains 89,978 observations reflecting 13,577 unique household heads between 21 and 39 years old. Table 3.1 shows summary statistics for this sample both with and without PSID weights. On average these household heads have 1.2 kids, 13.9 years of education, annual earnings of $25,340 (2013 dollars), and total household earnings of $51,060 (2013 dollars). Over half are married (57 percent), most work (86 percent), and 27 percent are female. One of the few traits that the PSID weights affect is race: 40 percent of the unweighted sample is Black, but only 16 percent of the weighted sample is. The treatment variable of interest in this paper is state EITC rates, which average 27 percent across the sample. In Table C.2, I show a transition matrix describing year-to-year changes in number of household children and marital status; as expected most households do not change marital status (92 percent) or number of children (90 percent) in adjacent years.

To estimate the effect of state EITCs on fertility and marriage, the identification strategy in this paper looks at how changes in state EITC rates affect household number of children and household head marital status in the *following year*.[11] Panel data allows me to control for current number of children and marital status, and the treatment variable of interest is changes in state EITC rates.[12] This approach differentiates this paper from previous studies that have looked at how the EITC affects the decision to have children or marry.

I use OLS to estimate the following equation.

$$y_{i,s,t+1} = \beta_1 \Delta StateEITC_{(t)-(t-1)} + \beta_2 X_{its} + \delta_t + \gamma_s + \epsilon_{its} \tag{7}$$

[11] I look at outcomes in the following year since fertility decisions tend to take a year or so to materialize. Marital decisions may also occur with a lag. Analysis looking at these outcomes in an event-study framework are shown in Figure 3.6 and 3.7 and shows the contemporaneous response, lagged responses years later, and placebo responses before state EITCs are expanded.

[12] Changes in state EITC rates largely reflect policy changes, but could also reflect moving across state lines.

y is the binary outcome of interest (having an additional child or being married in the following year), $\Delta StateEITC$ is the annual change in the state EITC rate – measured as a fraction of the federal EITC – measured in 10-percentage-point units, δ_t are year fixed effects, γ_s are state fixed effects, and ϵ is an idiosyncratic error term. I control for state and year fixed effects to account for shocks coming from state-level institutions and social norms, or annual events (e.g. recessions or welfare reform) affecting everyone across the country. X is a set of household-head level controls including birth year fixed effects, race, state by race, an age cubic, and education. i, t, and s denote individual household heads, years, and states. PSID weights are used throughout to help correct for the oversampling of poor and minority households in the PSID (see Table 3.1). β_1 is the coefficient of interest and measures (in percentage points) the increased likelihood of having an additional child or being married in the year after a 10-percentage-point state EITC increase.[13]

Figures 3.1 and 3.2 (discussed in section 3.2) show substantial variation in state EITC rates over time, both within and across states, which yields relatively precise estimates of the effect of the EITC on marriage and fertility. One potential threat to identification would be if state EITCs were created or expanded in response to economic conditions or changes in other polices that also affect fertility and marriage behavior. For example, if state EITCs are expanded during economic expansions and if people are more likely to have children or marry during such periods, then estimates of the effect of state EITC expansions on fertility and marriage would be biased upward. On the other hand, if state EITCs are a substitute for other public assistance programs that encourage fertility and are expanded when other programs are cut, then estimates of the effect of state EITC expansions on fertility and marriage could be biased downward.

I formally test whether state EITC rates are correlated with other state policies or economic conditions in Table C.1. State EITC rates are regressed on various state-by-year characteristics such as GDP, minimum wage, unemployment, welfare generosity, marginal income-tax rates, higher-education spending, total tax revenue, and lags of each of these variables along with state and year fixed effects. Column 1 uses state EITC rate as the dependent variable and column 2 uses state EITC rate interacted with a binary for whether a state ever had an EITC as of 2013. The only covariates significant at the 10 percent level is lagged state unemployment (in column 1) and higher education spending (in column 2). Each of these

[13] A similar approach would separately control for $StateEITC_t$ and $StateEITC_{t-1}$ and yields similar results.

estimates are negative, providing weak evidence that state EITCs are lower the year after high unemployment and lower in years with higher education spending. However, with 14 covariates one or two are likely to be significant at the 10-percent level by random chance. Joint F-tests (excluding state and year fixed effects) yield p-values of 0.35 and 0.37 and I cannot reject the null hypothesis that none of these state-by-year variables are correlated with state EITC rates. However, it may still be important to control for these state-by-year policies and economic conditions if they are correlated with marriage and fertility, and I include these seven state-by-year covariates as controls, in X_{ist} in equation (7).[14]

3.5 Fertility Results

3.5.1 Average Contemporaneous Effects

I first look at the average effect of state EITCs on fertility and how these estimates vary across sets of controls. Columns in Table 3.2 progressively add controls and show the effect of a 10-percentage-point increase in the state EITC rate on household number of children in the following year. Column 1 includes no controls. Column 2 controls for number of children. Column 3 adds controls for the household head's gender, marital status, and race. Column 4 adds controls for the household head's age (cubic) and fixed effects for year and birth year to account for declining birth rates across cohorts and the age-varying probability of having children.[15] Column 5 controls for state and state-by-race fixed effects. Column 6 controls for education: both as a linear in years as well as fixed effects for finishing high school, having some college, and completing college. Finally, column 7 adds state-by-year economic policies and conditions discussed in section 3.4 that may be correlated with fertility or marriage decisions. Column 7 contains the full set of controls that is used for all subsequent analysis.

Estimates in Table 3.2 are positive and consistent across sets of controls, ranging from 1.4 to 2.5 percentage points, with the full set of controls indicating that a 10-percentage-point increase in state EITC benefits leads to a 1.4-percentage-point increase in the average likelihood

[14] These state-year controls do not have much of an effect on the estimates. About a third of states with an EITC (9 states) implemented it between 1997 and 2000, just after the major reform to welfare in 1996. During this time, states were given federal block grants to reduce welfare caseloads, and many states used these grants to establish EITCs. Therefore, welfare and the EITC may be substitutes at the state level, and controlling for welfare generosity will help account for this negative correlation.

[15] To deal with the collinearity of age, year, and birth year, I round age up to even numbers.

of having an additional child in the following year.[16] Although this is a fairly large effect, a back of the envelope calculation suggests that the implied elasticity is similar to those found in the AFDC-fertility literature.[17]

3.5.2 Event Study Approach

In addition to affecting fertility in the following year, the EITC may also affect fertility in other ways. For example, the effect may grow over time as households learn about state EITC expansions. On the other hand, these positive effects may simply reflect changes in birth timing (Grogger and Bronars 2001; LaLumia, Sallee, and Turner 2013; Meckel 2015) and not changes in completed fertility. I test for these possibilities in an event-study framework where I re-estimate equation (7) with the full set of controls from Table 3.2 column 7. Instead of just using y_{t+1} as the dependent variable, my event-study approach uses y_{t+j} where $j \in \{-5, -4, \ldots -1, 1, 2, \ldots 5\}$. Since y_t is still used as a control, $j = 0$ is not used and each estimate can be interpreted as the probability of having an additional child relative to the year of the state EITC expansion.[18] If the estimates in Table 3.2 reflect simply a timing adjustment and not a permanent fertility adjustment, then the point estimates should shrink and approach zero over time for $j > 1$. However, if the estimates in Table 3.2 reflect a permanent change in household fertility, then the estimates should remain positive and perhaps even grow over time. An event-study approach also provides a number of placebo tests, since EITC expansions should not affect past fertility.

[16] Results are similar when the roughly two-thirds of observations that do not have a state EITC are dropped.
[17] Moffitt (1998, p.74) cites empirical studies showing that a 25 percent reduction in welfare benefits would have reduced non-marital births by as much as 30 percent (Fossett and Kiecolt 1993; Hill and O'Neill 1993; Rosenzweig 1995). Since average household AFDC benefits were worth around $600 (2013 dollars) during the 1980s and early 1990s (in 1994, nominal state AFDC benefits ranged from $120 to $923 (Page and Lamer 1997)), a 25 percent decrease represents an $1800 annual income decrease (2013 dollars). In my sample, a 10 percent increase in the state EITC rate is worth around $200 of imputed EITC benefits, but the largest effect of the EITC is actually on labor supply and pre-tax earnings. Estimates in Dahl and Lochner (2017, Table 3) and Bastian and Michelmore (2017, Table 4) suggest that $200 of EITC benefits is associated with a $2400 to $2800 increase in pre-tax earnings (2013 dollars). Therefore, a 10 percent increase in state EITC rates leads to an average of $2600 in after-tax earnings and a 14 percent increase in births (based on estimates in Table 3.2). Although my implied elasticity is smaller than those cited by Moffitt (1998), my results are averaged over the whole population, not just non-marital births. The fraction of births to an unmarried mother has steadily risen from below 20 percent in 1980 to about 40 percent in 2010 (https://www.cdc.gov/nchs/data/databriefs/db162.htm) and scaling my estimates by a factor of three yields elasticities similar to those found by Fossett and Kiecolt (1993), Hill and O'Neill (1993), and Rosenzweig (1995). It is also possible that these results are biased upwards: Fairlie and London (1997) claim to find a spurious correlation between welfare and fertility.
[18] To be clear, this will not only reflect fertility, but also children moving in or out of each household for various reasons. See Table C.2 for a transition matrix for household number of children.

Event-study results in Figure 3.6 show that the effect on fertility is positive in the year after a state EITC expansion (as shown in Table 3.2) and grows larger the second and third year after a state EITC expansion. However, in the fourth and fifth year after an EITC expansion, the point estimates fall and become statistically insignificant (although they do remain positive). These results are consistent with both a birth-timing hypothesis, where births occur sooner but overall fertility does not increase, and an increased-fertility hypothesis, where births occur soon and remain higher than they otherwise would have been. Figure 3.6 also shows that there appears to be no response in the years leading up to state EITC changes, providing evidence that the positive fertility responses do not simply reflect state-level trends unrelated to the EITC.

3.5.3 Heterogeneous Fertility Responses

As discussed in section 3.3.1, the EITC likely had different fertility effects on different types of households. Table 3.5 uses equation (7) but interacts state EITC changes with EITC eligibility, education, race, and age to investigate heterogeneous responses and whether fertility responses only exist among households that should have responded to the EITC.

In column 1, I divide the sample into households that would and would not receive additional EITC benefits if they had another child. Households that would not receive EITC benefits with another child had at least one child before 1991, at least two children between 1991 and 2008, and at least three children after 2008 (see Figure 3.1). Interacting the treatment variable with this binary variable shows that a 10-percentage-point increase in state EITC rates increased the likelihood that households eligible for additional EITC benefits increased their fertility (1.8 percentage points) but households not eligible for additional EITC benefits did not see a statistically significant increase.

In column 2, I divide the sample into three categories by years of education (less than 12, 12 to 15, and at least 16 years of education). Interacting the treatment variable with these three variables shows that a 10-percentage-point increase in state EITC rates had the strongest effect on the middle-education group (1.9 percentage points), no apparent effect on the lowest-education group, and puzzlingly, perhaps an effect on the highest educated group, although this result is only significant at the 10-percent level.

In column 3, I divide the sample by race (white and nonwhite), interact the treatment variable with these variables, and find a positive response from white and nonwhite household heads (1.3 and 1.6 percentage points), although only the result for white heads is statistically significant. Previous empirical literature is not consistent on this dimension: Duchovny (2001) finds effects for white adults, but Baughman and Dickert-Conlin (2003; 2009) but effects for nonwhite adults.

Finally, in column 4 I divide the sample into those younger and older than age 40. If the EITC increases household fertility, then the responses should be larger for younger household heads in their prime childbearing years and near zero for older household heads. I find that a 10-percentage-point increase in state EITC rates had a positive effect on younger adults (1.6 percentage points) and a small effect on older adults (-0.7 percentage points) significant at the 10 percent level. This makes sense since younger adults are more likely to be in their childbearing years and able to respond to incentives to adjust fertility.

3.6 Marriage Results

3.6.1 Average Contemporaneous Effects

To estimate whether state EITCs affected marriage rates, I estimate equation (7) where y_{t+1} is a binary variable for whether the household head is married. Table 3.3 progressively adds controls – exactly as in Table 3.2 – and shows the effect of a 10-percentage point increase in the state EITC rate on the probability that a household head is married in the following year. Panel data allows me to control for current marital status and estimate the effect of the EITC on being married in the following year. Estimates in Table 3.3 are positive and consistent across sets of controls, ranging from 0.6 to 1.2 percentage points, with the full set of controls indicating that a 10-percentage-point increase in state EITC benefits leads to a 1.1-percentage-point (or 2.4 percent) increase in the average likelihood of being married in the following year.[19]

[19] Results are unlikely to reflect manipulation of tax-filing status (Tach and Halpern-Meekin 2014), since there is no financial incentive to misreport marital status to the PSID. Although see Hurst, Li, and Pugsley (2014) on misreporting on survey data.

3.6.2 Event Study Approach

In addition to affecting marriage in the following year, the EITC may also affect marriage in other ways. As discussed in section 3.5.2, this positive effect may grow over time as households learn about state EITC expansions. On the other hand, perhaps the effect decreases over time as households adjust to their new standard of living. I test for these possibilities in an event-study framework where I re-estimate equation (7) with the full set of controls from Table 3.3 column 7 (see section 3.5.2 for further discussion).

Event-study results in Figure 3.7 show that the effect on marriage is positive in the year after a state EITC expansion (as shown in Table 3.3) and then remains positive but falls over time, becoming less statistically significant. Figure 3.7 also shows that there appears to be no response in the years leading up to state EITC changes, providing evidence that the positive marriage responses do not simply reflect state-level trends unrelated to the EITC.

3.6.3 Heterogeneous Marriage Responses

Table 3.4 shows how the effect of state EITCs on marriage varies by current marital status. Using the full set of controls, the estimate in column 1 is identical to Table 3.3 column 7 and uses the full sample, while columns 2 to 4 split the sample into household heads currently married, never married, and currently divorced. Results show that for these three groups, a 10-percentage-point increase in state EITC benefits is associated with a 0.5-, 1.9-, and 1.8-percentage-point increase in the probability of being married in the following year. The largest effects are for adults never previously married, although the EITC seems to increase the average marriage rate for each of these groups. Another way to estimate the effect of the EITC on these subgroups is to use the whole sample and interact the treatment variable (change in state EITC rate $\Delta StateEITC_{(t)-(t-1)}$) with marital status; column 5 does this and shows results similar to columns 2 to 4: results are largest for never-married adults, positive for previously married adults, and statistically insignificant for married adults. Perhaps one reason that the effect on currently married adults is near zero is that 95 percent of adults married in year t are still married in year $t + 1$, and therefore there is little room for growth.

In Table 3.5, I investigate how heterogeneous responses varied by EITC eligibility, education, race, and age and test whether marriage responses only exist among households that should have responded to the EITC. Columns 5 to 8 follow the approach in columns 1 to 4 that show fertility responses by subgroup.

In column 5, I divide the sample into households that would or would not be EITC eligible if they were married and interact the treatment variable with this binary variable. A 10-percentage-point increase in state EITC rates increased the likelihood that EITC-eligible households married (1.7 percentage points) but had no effect on EITC-ineligible households (0.2 percentage points). In column 6, I divide the sample into three categories by years of education (less than 12, 12 to 15, and at least 16 years of education). Interacting the treatment variable with these three variables shows that a 10-percentage-point increase in state EITC rates had the strongest effect on the middle-education group (2.0 percentage points) and no detectable effect on the lowest- or highest-education groups. In column 7, I divide the sample by race (white and nonwhite), interact the treatment variable with these two variables, and find a positive response from white and nonwhite household heads (1.2 and 0.8 percentage points), although only the result for white heads is statistically significant.

Finally, in column 8 I divide the sample into those younger and older than age 40 and find that a 10-percentage-point increase in state EITC rates had a positive effect on younger adults (1.2 percentage points) and an insignificant effect on older adults (-0.3 percentage points). This makes sense since younger adults are less likely to be married and more able to respond to incentives to adjust fertility.

One reason that the EITC may lead to more stable marriages and less divorce, is that the EITC reduces financial insecurity and stress. Evidence for this is provided by Mendenhall et al. (2012), Jones and Michelmore (2016), and Bastian and Michelmore (2017) that show the EITC increases savings and household resources. Marriages could also be strengthened by improving children's outcomes and reducing parental stress (Chetty, Friedman, Rockoff 2011; Dahl and Lochner 2012; Hoynes, Miller, and Simon 2013; Manoli and Turner 2014; Averett and Wang 2015; Bastian and Michelmore 2017).

3.7 Cohabitation Results Corroborate Marriage Results

If state EITC expansions led to increases in marriage, then the likelihood of couples cohabitating without getting married should decrease. Cohabitation is directly measured in the PSID's Transition to Adulthood sample between 2005 and 2013. This sample includes 2,567 individuals, 7,114 individual-year observations, young adults 17 to 27 years old, and is largely a separate set of individuals than the main sample used so far (see Table C.3 for sample statistics; 1,021 of the individuals are in both samples). Using a different set of individuals and a distinct (but related) outcome would provide corroborating evidence that the EITC (on average) encourages marriage.[20]

Using equation (7) and the same identification strategy used to estimate the effect of state EITCs on fertility and marriage, Table 3.6 estimates the effect of state EITC expansions on the likelihood that a young adult is cohabitating.[21] Table 3.6 progressively adds controls (following the approach in Tables 3.2 and 3.3) and shows that across various sets of controls, the EITC reduces cohabitation. The full set of controls shows that a 10-percentage-point increase in state EITC rates leads to a 1.7-percentage-point reduction in the probability of cohabitating in the following year.

Subgroup analysis in Table 3.7 shows that this effect holds for both currently cohabitating and currently non-cohabitating adults (column 1), although the effect is larger for the latter group. This reduction in cohabitation is largest for individuals most affected by the EITC – those with the lowest education – and is statistically insignificant for those least affected by the EITC – those with the most education. Similar to the marriage and fertility results in Table 3.5, Table 3.7 column 3 also shows that the largest decrease in cohabitation occurs among white adults.[22]

Reductions in cohabitation provides additional evidence that the EITC encourages couples to get – and stay – married. Most estimates of the EITC on cohabitation are opposite of those found here (Eissa and Hoynes 2000; Ellwood 2000; Dickert-Colin and Houser 2002). One

[20] I did not include this sample of young adults in the main sample because I was focusing on household heads. Many PSID variables are only available for household heads and wives. Previous results are all very similar if I add the 1546 people in this sample not in the main sample.

[21] With this smaller sample, I observe much less variation in $\Delta StateEITC_{(t)-(t-1)}$ and use the related variable $StateEITC_t$ as the treatment variable of interest.

[22] Results for nonwhite adults are not statistically significant, but the point estimate is positive (0.009).

possible reason is that this analysis uses data from 2005 to 2013, after the 2003 reduction of the EITC marriage penalty; another reason could be the way that individual-level panel data accounts for incentives in a context where, overall, marriage rates are declining and cohabitation is increasing.

3.8 Less Parametric Approaches: Fertility and Marriage Results

Figures 3.8 and 3.9 show results from a locally weighted, double-residual regression (Cleveland 1979). In this approach, two sets of residuals are created: one set comes from the regression of binary y_{t+1} (for $y = another\ kid$ and $y = married$) on the full set of controls (except for $\Delta StateEITC$), and the second set comes from the regression of $\Delta StateEITC$ on the full set of controls. Regressing the first set of residuals on the second set mechanically reproduces the main OLS estimate for fertility (in Table 3.2 column 7) and marriage (in Table 3.3 column 7).[23] Figures 3.8 and 3.9 compare the linear OLS estimate with a locally weighted regression fit through the residuals (averaged into 50 quantile bins for visual purposes). Although the boundary estimates are noisy, the interior local linear slope is positive and suggests that the linear OLS fit does a decent job of describing the average effect of state EITC increases on fertility and marriage.

3.9 Implications for the Stable-Group-Composition Assumption in Other EITC Studies

If the EITC affects both fertility and marriage, results in this paper may have implications for other EITC research. Many EITC papers use a difference-in-differences (DD) approach, with one of the differences being determined by number of children.[24] However, if number of children and EITC expansions are endogenous, then the stable-group-composition condition required for DD may not be met. Positive fertility effects would imply that individuals are endogenously switching from the control to the treatment group. Furthermore, many EITC papers restrict the sample to unmarried women to focus on the population most affected by the EITC. However, if

[23] This follows from the Frisch-Waugh-Lovell theorem (Frisch and Waugh 1933; Lovell 1963).
[24] One commonly used approach compares women with one versus two children during the 1993 EITC expansion.

the EITC is affecting marriage, then comparing single women before and after EITC expansions may be problematic, and again, the stable-group-composition condition may not hold.

If the stable-group-composition condition does not hold, it is not obvious whether this would bias estimates in previous EITC studies up or down. This will depend on whether women endogenously switching from control to treatment groups have higher or lower values of the outcome variable of interest than women previously in the treatment group, as well as whether controls can account for this difference. For example, if the outcome is labor supply, and if women endogenously switching into the treatment group are more likely to work (since they are response on the fertility margin, perhaps they are also responsive on other margins) then this could result in an overestimate of the effect of the EITC on labor supply.[25]

3.10 Future Work

There are a few factors and robustness checks that merit further work. One, with some effort I may be able to put together an annual PSID panel. This will help reconcile the annual pre-1997 PSID waves with the biannual post-1997 PSID waves. Two, think more about the dependent variable and use other measures of fertility to ensure that I am indeed measuring fertility and new household children. Three, use alternate specifications instead of 10 percent increases in state EITC rates: use maximum EITC exposure such as in Bastian and Michelmore (2017) or use maximum EITC divided by the required household earnings to proxy for the fraction of earnings that these EITC benefits represent. Four, show intermediate outcomes resulting from state EITC increases, comparable to Bastian and Michelmore (2017) Table 4. Five, think more about magnitudes. It is possible that annual data, alternate dependent variables, or intermediate outcomes will help make sense of the large implied fertility elasticities.[26]

[25] A back of the envelope calculation suggests that a 10 percent increase in fertility could mean that 10 percent of the control group selected into the treatment group. For a hypothetical difference-in-differences estimate of 4 percentage points, this could imply a bias of around 0.4 percentage points.

[26] I am grateful to my dissertation committee for pointing out these issues during my dissertation defense.

3.11 Discussion and Summary

There has long been a concern that public assistance programs may discourage marriage among low-income households. This is an important policy question since marriage is associated with positive health and economic outcomes for adults (Waite 1995; Sawhill 2014) and children (McLanahan and Sandefur 1994; Chetty et al. 2014). Although the EITC provides incentives for some couples to marry and others to not, and some couples to have more children and others to have less, I find a positive effect of the EITC on these outcomes over a 30 year period.[27]

Heterogeneous responses to the EITC – and numerous changes to the EITC program over time – could help explain why existing evidence on how the EITC affects fertility and marriage has been mixed. Previous literature relating the EITC and fertility has been mixed: Baughman and Dickert-Conlin (2003) finds an increase in the rate of first births for nonwhite women; Baughman and Dickert-Conlin (2009) finds a decrease in higher-order births for white women; and Duchovny (2001) finds an increase in second children for white-married and nonwhite-unmarried women.[28] Regarding marriage, small negative effects are found by Dickert-Colin and Houser (2002), Acs and Maag (2005), Fisher (2011), Herbst (2011), and Michelmore (2015). Null effects are found by Ellwood (2000) and small positive effects among low-income households are found by Eissa and Hoynes (2004). However, the marriage penalty has decreased over time and the number of low-income couples that face a marriage bonus may outnumber those that face a marriage penalty (Acs and Maag 2015).[29]

This paper uses a novel approach to identify the effect of the EITC on both fertility and marriage. Using 30 years of household-level panel data and state-by-year variation in state EITC rates, I control for lagged fertility and marital status to identify the effects of state EITC expansions on the decisions of households to marry and have children.

Estimates suggest that a 10-percentage-point increase in state EITC rates led to 1.2-percentage point increase in the likelihood of having an additional child in the following year. Fertility results are robust to various sets of controls, do not simply reflect a shift in birth timing since effects are still evident several years later, and heterogeneous analysis shows that fertility

[27] Policy implications should be based on how the EITC affects households on the margin of marrying or not.
[28] Although, Fairlie and London (1997) claim to find a spurious correlation between welfare and fertility.

[29] Though Lin and Tong (2012) show that among cohabitating couples, 48 percent face a marriage penalty and 38 percent face a marriage bonus.

responses are concentrated among households that would be expected to respond to the incentives of the EITC.

Regarding marriage, estimates suggest that a 10-percentage-point increase in state EITC rates led to a 1.1-percentage-point increase in the probability of being married in the following year. Marriage results are robust to various sets of controls, are largest for currently unmarried adults, and may be positive for currently married adults too. These results suggest that, on average, the EITC encourages new marriages and helps married couples stay married. In addition to this financial incentive, the EITC could strengthen existing marriages by increasing household resources and reducing financial insecurity (Mendenhall et al. 2012, Jones and Michelmore 2016). These results give pause to growing concerns that the EITC has a negative effect on marriage and arguments against additional expansions of the EITC because of potential marriage disincentives.

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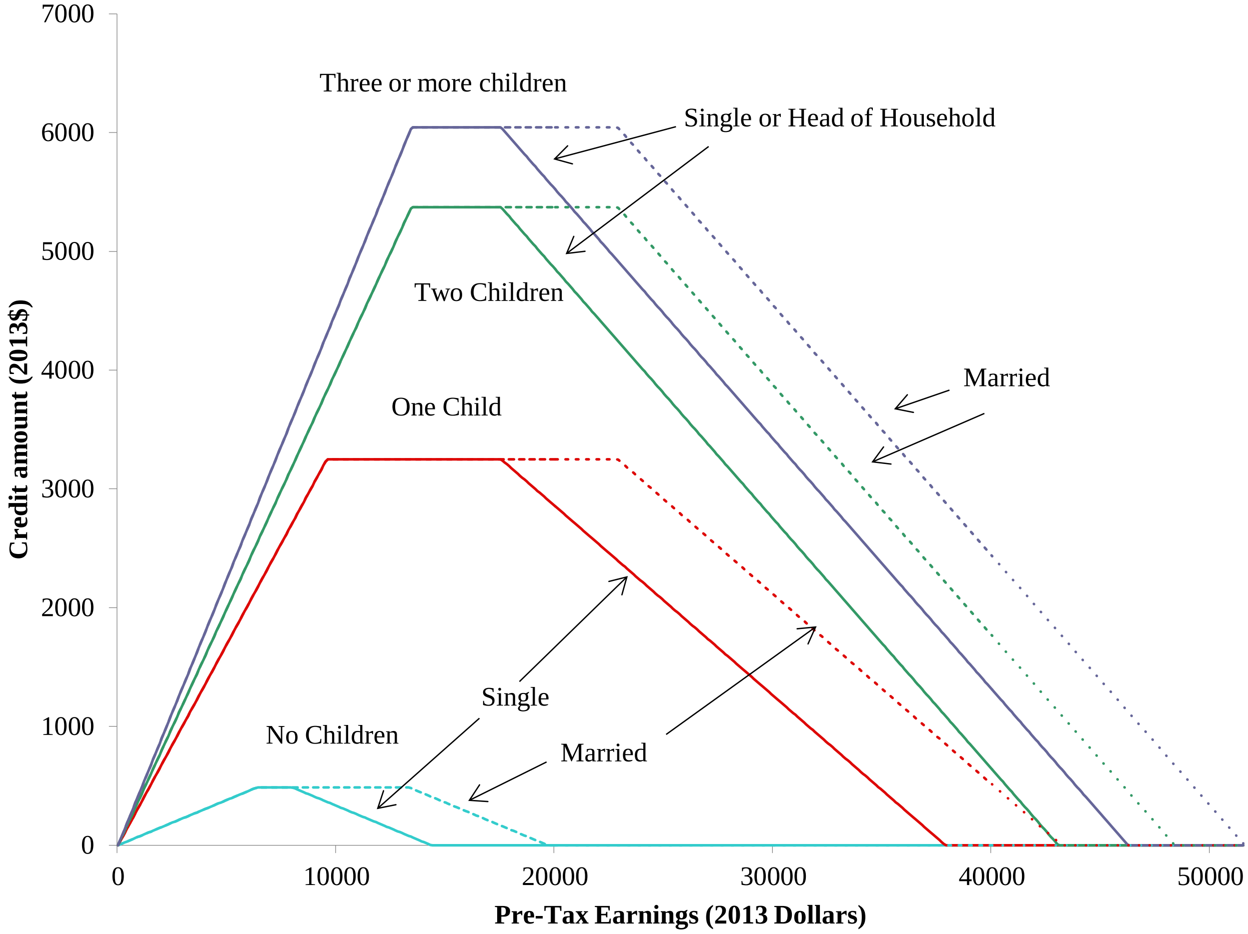
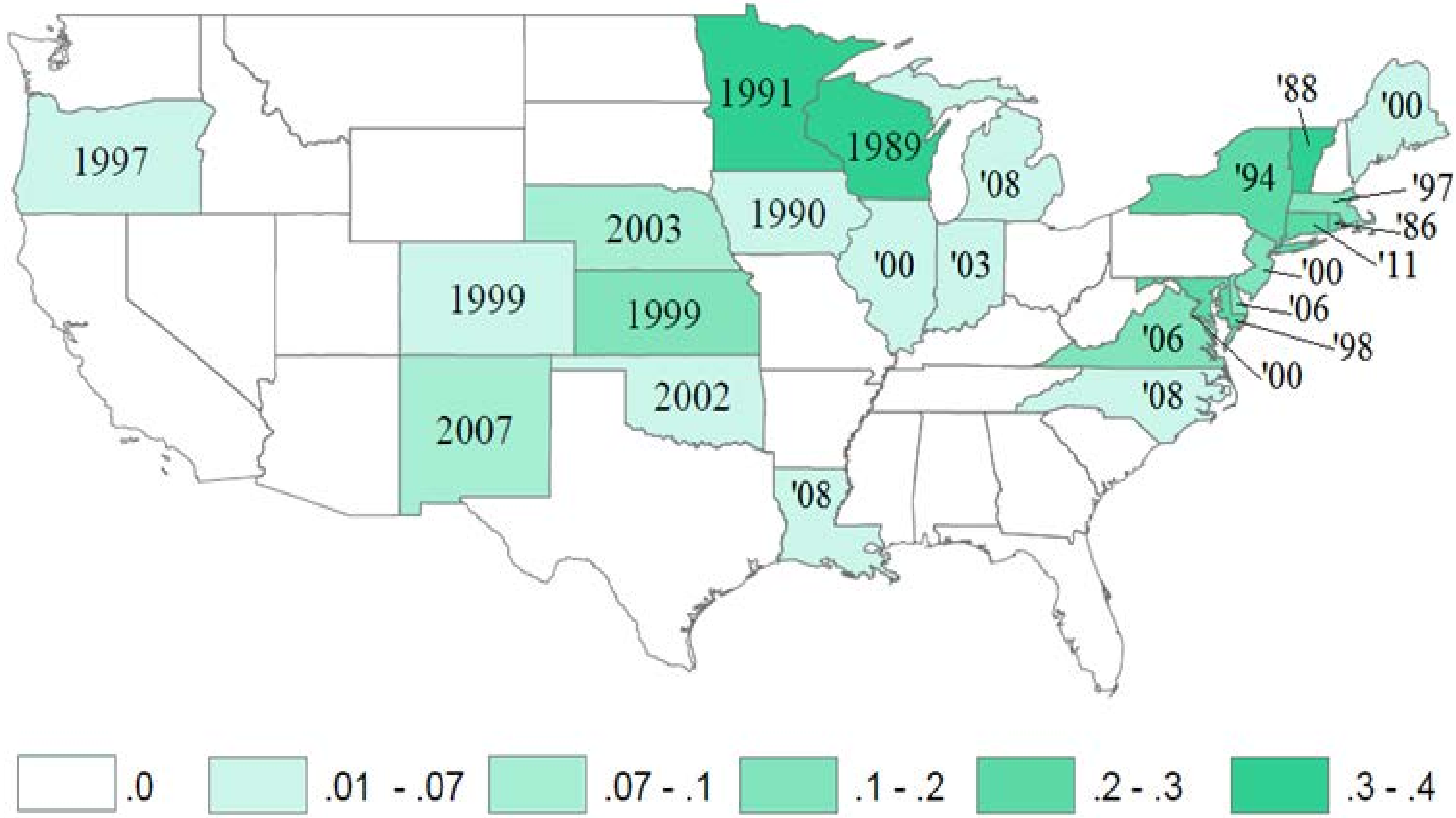
3.13 Figures and Tables

Figure 3.1: 2013 EITC Schedule by Number of Children and Marital Status



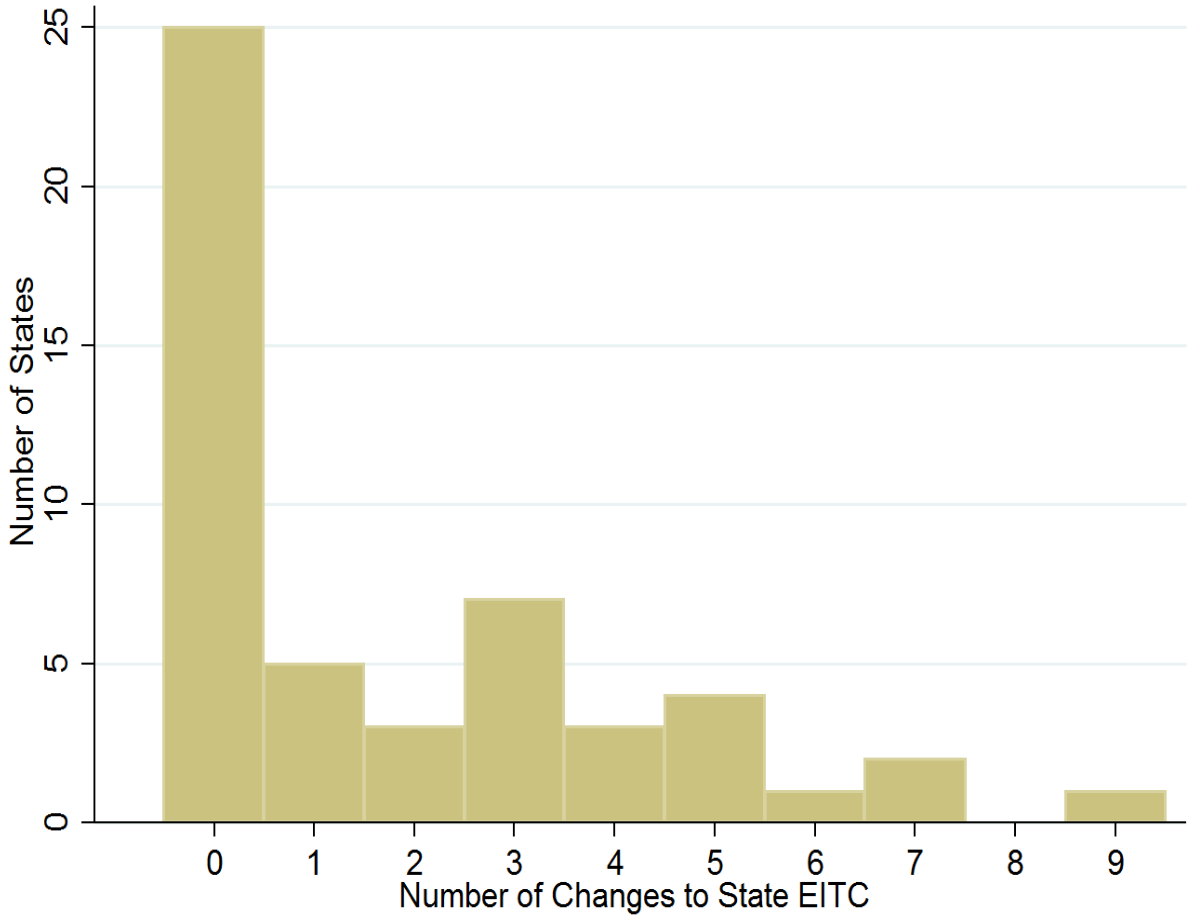
Source: Bastian and Michelmore (2017).

Figure 3.2: State EITC Benefits in 2013 (as a Fraction of Federal EITC) and Year of Enactment



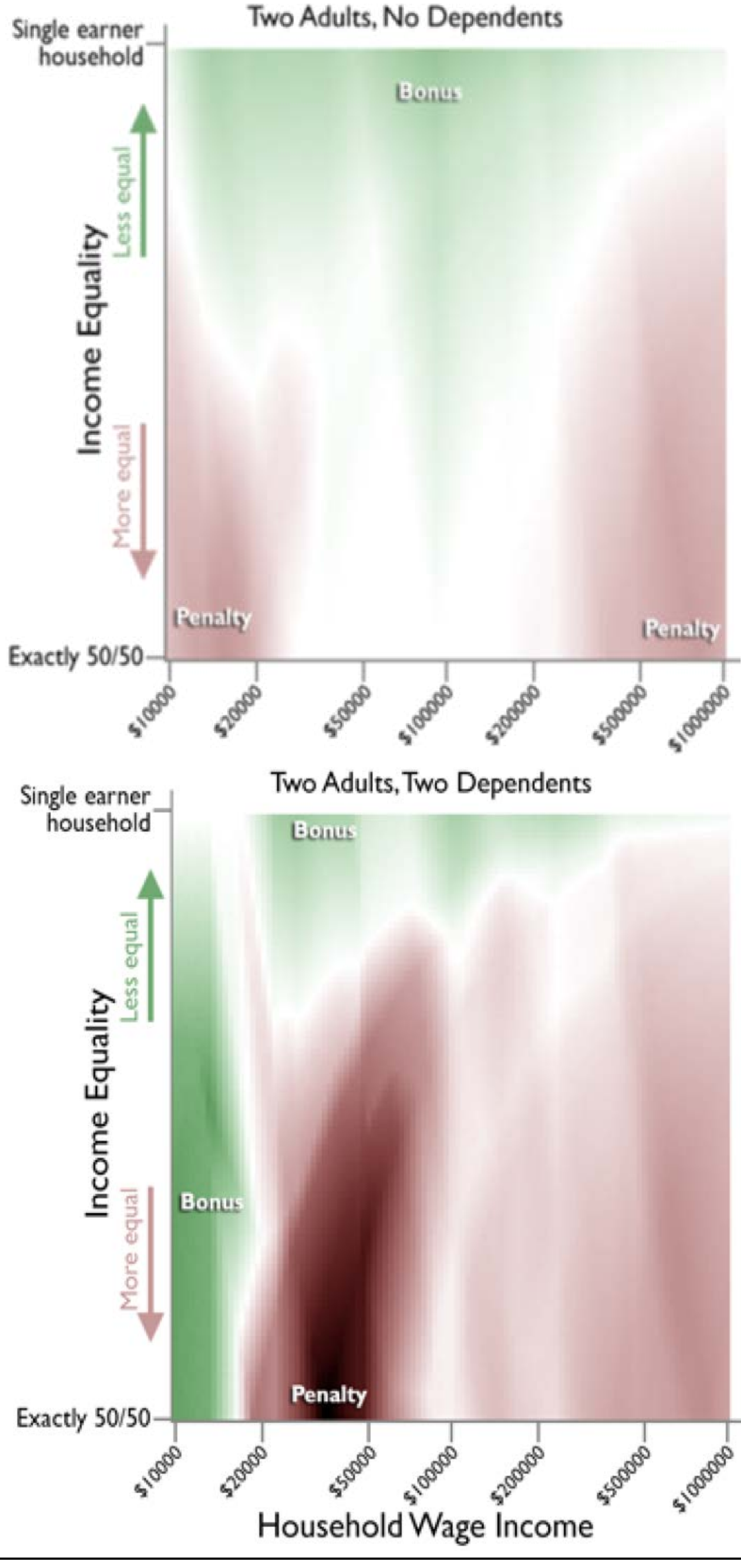
Source: Author's calculations. IRS data.

Figure 3.3: Number of Times that States Have Changed Their EITC Program



Source: Author's calculation. IRS data.

Figure 3.4: Marriage Tax Bonus or Penalty Depends on Distribution of Earnings within a Couple



Source: https://taxfoundation.org/marriage-penalties-and-bonuses-families-children-edition/ accessed 1/25/2017.

Figure 3.5A: Number of Children has Been Declining for 50 Years

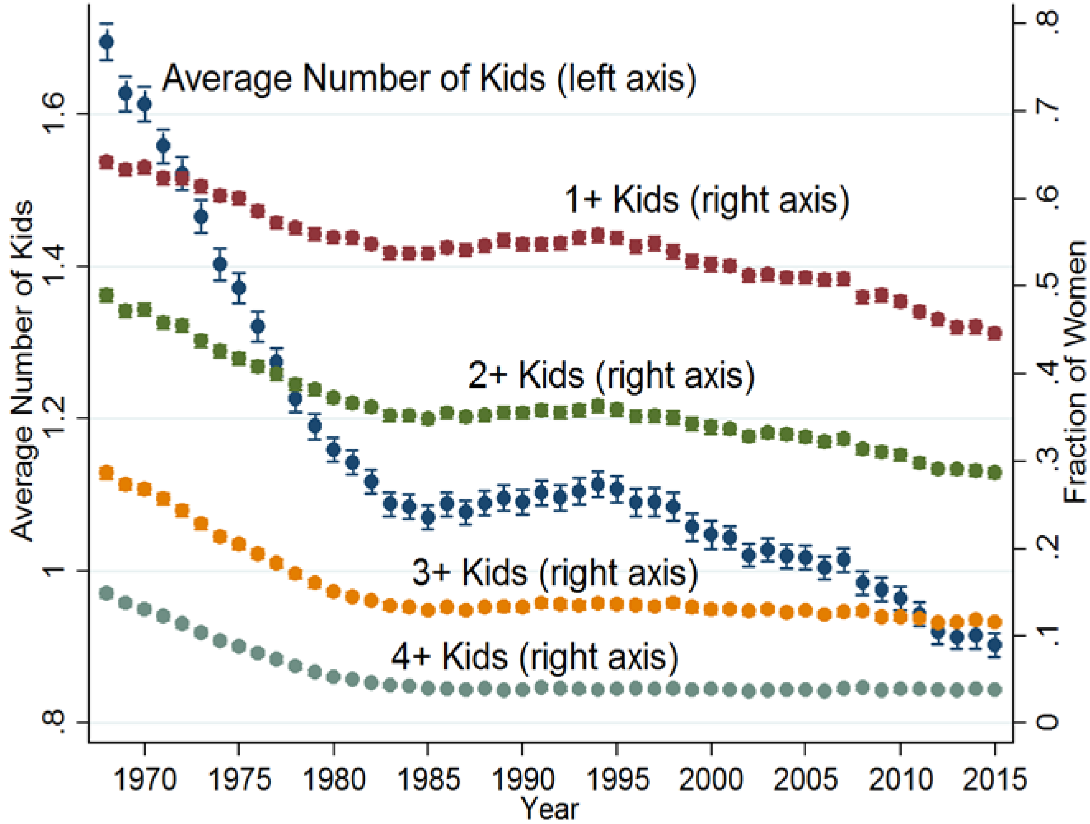
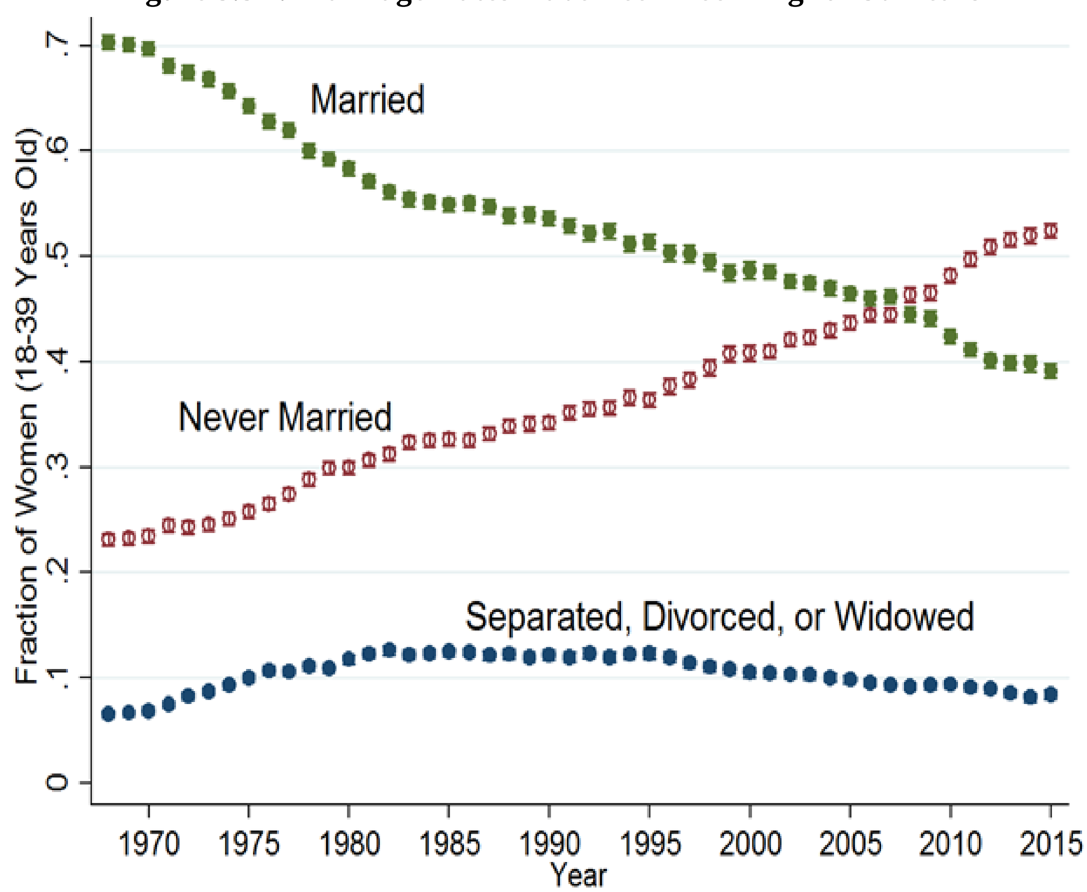
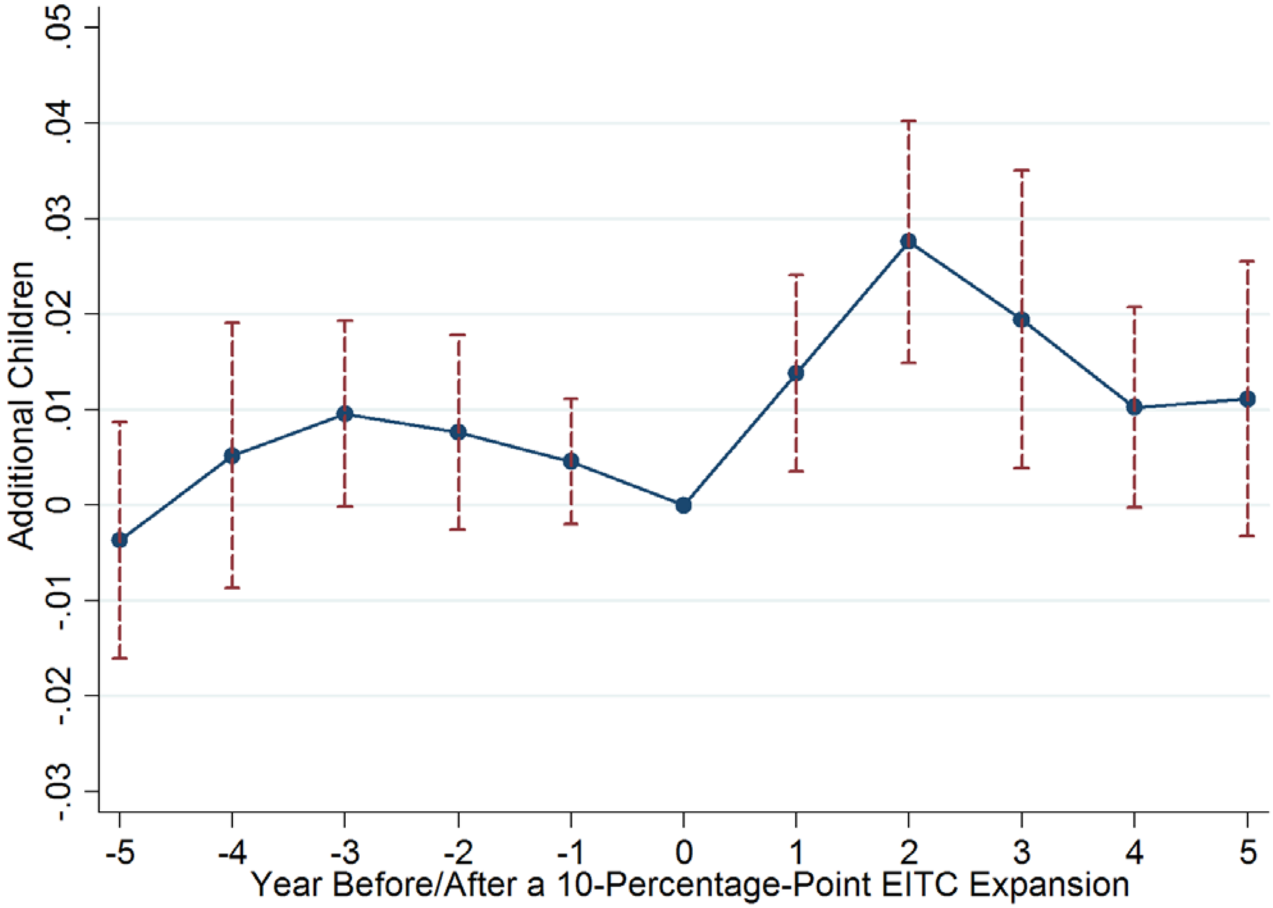


Figure 3.5B: Marriage Rates Have Been Declining for 50 Years



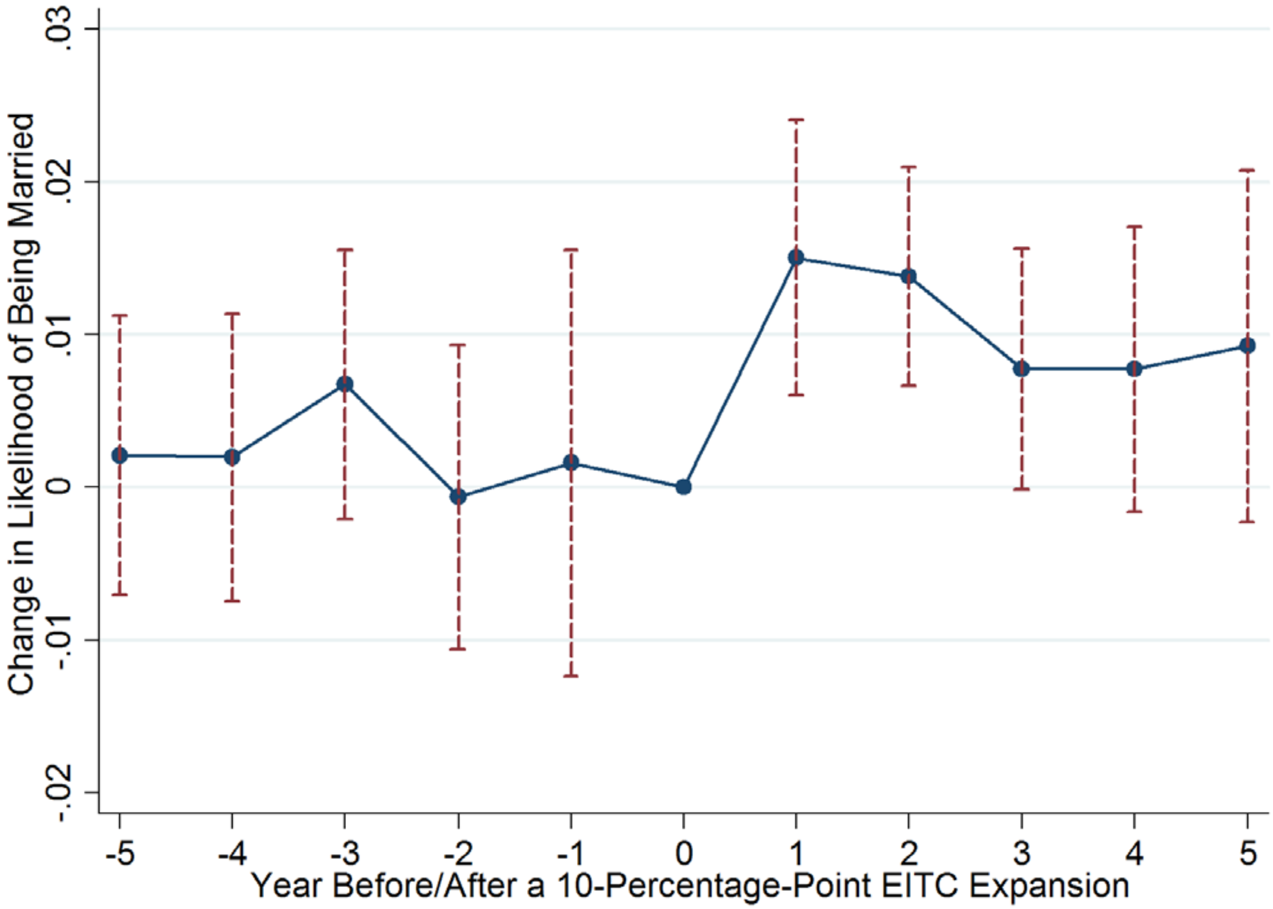
Source: 1968 to 2015 March Current Population Survey. 95% confidence interval shown for robust standard errors clustered at the state level. Observations are 18 to 40 year-old women.

Figure 3.6: Effect of State EITCs on Household Number of Children, Event Study



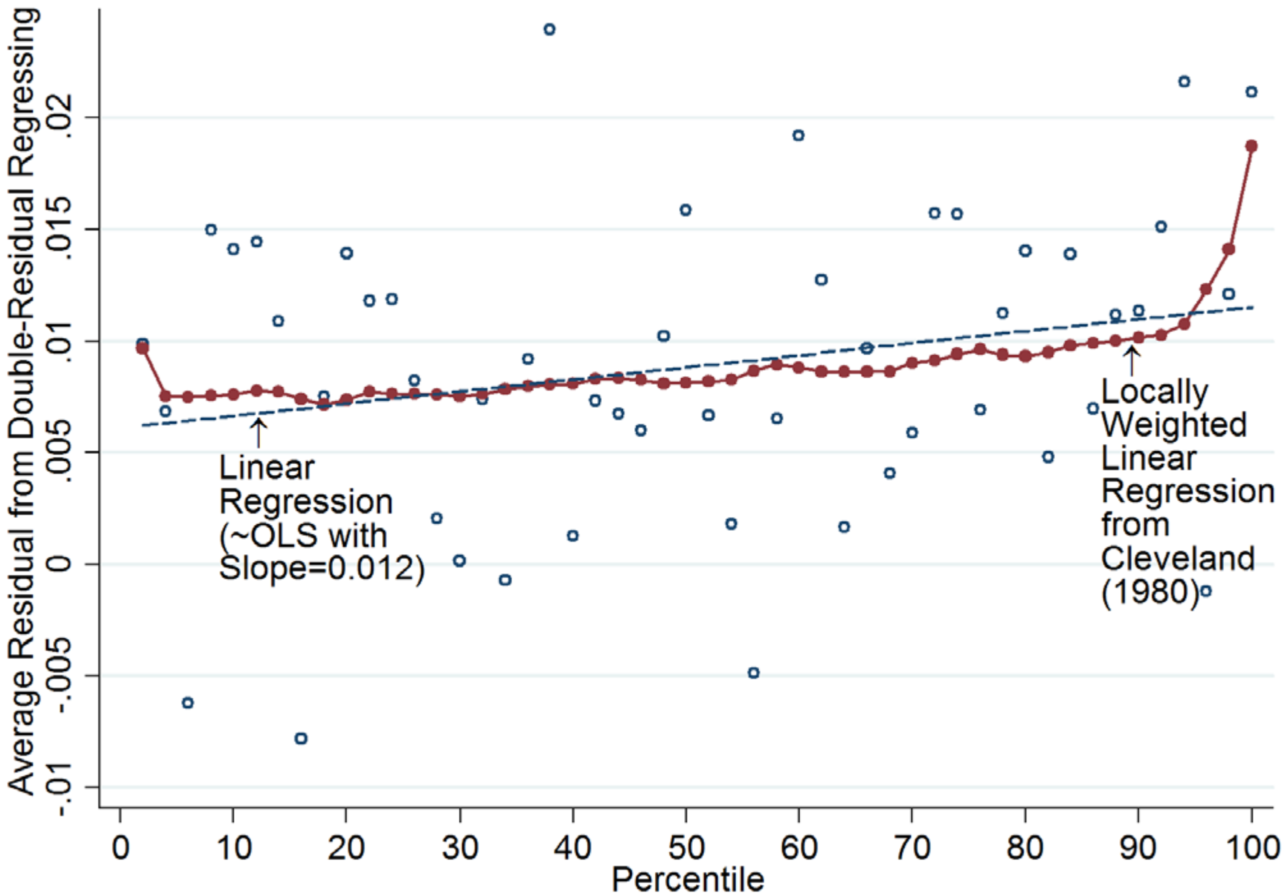
Source: 1980 to 2013 PSID. Observations are 21 to 39 year-old household heads. PSID weights used. 95% confidence interval shown for robust standard errors clustered at the state level. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the number of children observed in the household, before or after the EITC rate change. I set to missing the observations that appear to gain at least 4 children in adjacent years, this only affects 31, 45, 55, 60, 62, 55 observations and has little effect on the results. Mean dependent variables (from t = -5 to t = 5) are -0.672, -0.583, -0.484, -0.376, -0.258, 0.000, 0.115, 0.256, 0.372, 0.479, and 0.576, which means that the results as percentage effects are 2.8, 5.1, 4.6, 0.2, 0.3, 0.0, 8.9, 16.7, 12.1, 8.8, and 10.3 percent.

Figure 3.7: Effect of State EITCs on Marriage, Event Study



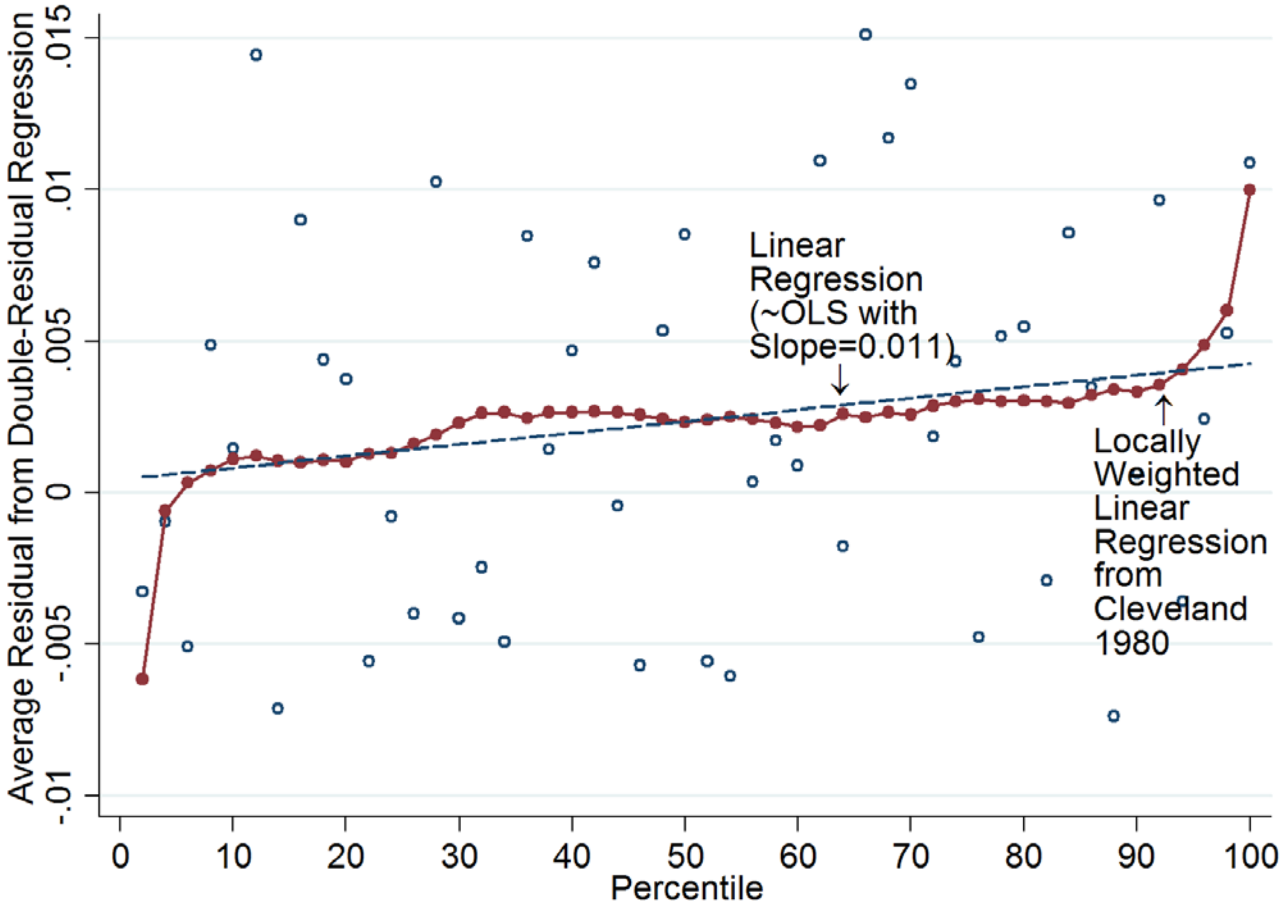
Source: 1980 to 2013 PSID. Observations are 21 to 39 year-old household heads. PSID weights used. 95% confidence interval shown for robust standard errors clustered at the state level. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the number of children observed in the household, before or after the EITC rate change. I set to missing the observations that appear to gain at least 4 children in adjacent years, this only affects 31, 45, 55, 60, 62, 55 observations and has little effect on the results. Mean dependent variables (from t = -5 to t = 5) are -0.672, -0.583, -0.484, -0.376, -0.258, 0.000, 0.115, 0.256, 0.372, 0.479, and 0.576, which means that the results as percentage effects are 2.8, 5.1, 4.6, 0.2, 0.3, 0.0, 8.9, 16.7, 12.1, 8.8, and 10.3 percent.

Figure 3.8: Double-Residual Regression Shows Fertility Effect Similar to OLS



Notes: 1980 to 2013 PSID. Observations are 21 to 39 year-old household heads. PSID weights used. Residuals are averaged into 50 quantiles. The linear best fit line and locally weighted linear regression uses all the original residuals. Locally weighted regression is from Cleveland (1979) and differs from regular local polynomial regression as it downweights observations with large residuals. A bandwidth of 0.8 is used, running-line least squares smoothing, and Cleveland (1979)'s tricube weighting function. The non-parametric regression shown approximates linear OLS estimate quite closely except for the highest and lowest values of state EITC benefits. However, boundary bias is a common issue with these methods.

Figure 3.9: Double-Residual Regression Shows Marriage Effect Similar to OLS



Notes: 1980 to 2013 PSID. Observations are 21 to 39 year-old household heads. PSID weights used. Residuals are averaged into 50 quantiles. The linear best fit line and locally weighted linear regression uses all the original residuals. Locally weighted regression is from Cleveland (1979) and differs from regular local polynomial regression as it downweights observations with large residuals. A bandwidth of 0.8 is used, running-line least squares smoothing, and Cleveland (1979)'s tricube weighting function. The non-parametric regression shown approximates linear OLS estimate quite closely except for the highest and lowest values of state EITC benefits. However, boundary bias is a common issue with these methods.

Table 3.1: Summary Statistics

Variable	Unweighted Mean	Unweighted Standard Deviation	Using PSID Weights Mean	Using PSID Weights Standard Deviation
Female Head	0.26	0.44	0.27	0.44
Number of Kids	1.49	1.35	1.24	1.28
State EITC Rate	0.23	0.75	0.26	0.81
Years of Education	13.45	2.38	13.87	2.49
White	0.55	0.50	0.77	0.42
Black	0.40	0.49	0.16	0.37
Single Head	0.44	0.50	0.43	0.49
Married Head	0.56	0.50	0.57	0.49
Birth Year	1962.8	10.4	1963.0	10.4
Age	30.61	5.01	30.64	5.04
Employed	0.82	0.38	0.86	0.35
Earned Income of Head (1000s of 2013 dollars)	22.43	31.10	25.34	34.77
Family Earned Income (1000s of 2013 dollars)	46.65	53.56	51.06	58.71
Number of Observations	89,978			
Unique Observations	13,577			

Source: 1980 to 2013 PSID. Observations are 21 to 39 year-old household heads. PSID weights used.

Table 3.2: State EITCs Increase Likelihood of Having Another Child Next Year: Similar Effect Across Controls

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State EITC Rate Change: Year *t-1 to Year t*	0.025***	0.025***	0.026***	0.016***	0.014***	0.014**	0.014**
(10-Percentage-Point Units)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Controls							
Number of Kids in Year *t*		X	X	X	X	X	X
Head Gender, Married, Race			X	X	X	X	X
Year FE, Birth Year FE, Age Cubic				X	X	X	X
State FE, State-Race FE					X	X	X
Education						X	X
State-Year Policies and Economic Conditions							X
Observations	89,978	89,978	89,978	89,978	89,978	89,978	89,978
Unique Observations	13,577	13,577	13,577	13,577	13,577	13,577	13,577
R-squared	0.000	0.002	0.016	0.038	0.042	0.044	0.044
Mean Dependent Variable (Having an Additional Children in Year *t+1*) = 0.103							

Source: 1980 to 2013 PSID. Robust standard errors clustered at the state level in parentheses. Observations are 21 to 39 year-old household heads. PSID weights used. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the likelihood of the household having an additional child in the following year. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.3: State EITCs Increase Likelihood of Being Married Next Year: Similar Effect Across Controls

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
State EITC Rate Change: Year *t-1 to Year t*	0.006	0.012***	0.010***	0.010***	0.011***	0.012***	0.011***
(10-Percentage-Point Units)	(0.010)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Controls							
Marital Status in Year *t*		X	X	X	X	X	X
Head Gender, Kids, Race			X	X	X	X	X
Year FE, Birth Year FE, Age Cubic				X	X	X	X
State FE, State-Race FE					X	X	X
Education						X	X
State-Year Policies and Economic Conditions							X
Observations	89,978	89,978	89,978	89,978	89,978	89,978	89,978
Unique Observations	13,577	13,577	13,577	13,577	13,577	13,577	13,577
R-squared	0.000	0.778	0.785	0.786	0.787	0.787	0.787
Mean Dependent Variable (Married in Year *t+1*) = 0.46							

Source: 1980 to 2013 PSID. Robust standard errors clustered at the state level in parentheses. Observations are 21 to 39 year-old household heads. PSID weights used. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the likelihood of the household head being married in the following year. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4: State EITC Expansions Lead to More Marriages Among Currently Not Married Adults

Sample of Household Heads:	All	Currently Married	Never Married	Currently Divorced	All
VARIABLES	(1)	(2)	(3)	(4)	(5)
State EITC Rate Change: Year *t-1 to Year t*	0.011***	0.005	0.019**	0.018**	
(10-Percentage-Point Units)	(0.003)	(0.004)	(0.009)	(0.008)	
State EITC Rate Change x Currently Married					-0.000
					(0.006)
State EITC Rate Change x Never Married					0.026**
					(0.011)
State EITC Rate Chnage x Previously Married					0.014*
					(0.008)
Observations	89,978	44,756	29,253	9,477	89,978
Unique Observations	13,577	7,444	6,378	2,955	13,577
R-squared	0.787	0.046	0.077	0.108	0.787
Mean Dependent Variable (Married in Year *t+1*)	0.46	0.95	0.06	0.07	0.46

Source: 1980 to 2013 PSID. Robust standard errors clustered at the state level in parentheses. Observations are 21 to 39 year-old household heads. PSID weights used. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the likelihood of the household head being married in the following year. Previously married includes divorced, widowed, and separated. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Effect of State EITCs on the Fertility and Marriage of Various Subgroups

Outcome:	Fraction EITC	Fertility				Married			
VARIABLES	Eligibile	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible	0.00	0.008				0.002			
		(0.012)				(0.004)			
State EITC Rate Change x EITC Eligible	1.00	0.018**				0.017***			
		(0.008)				(0.006)			
Education	0.28		0.005				0.004		
			(0.005)				(0.006)		
Education	0.21		0.019***				0.020***		
			(0.006)				(0.006)		
Education	0.08		0.022*				0.011		
			(0.012)				(0.009)		
State EITC Rate Change x Nonwhite	0.35			0.016				0.008	
				(0.017)				(0.008)	
State EITC Rate Change x White	0.16			0.013***				0.012***	
				(0.005)				(0.004)	
State EITC Rate Change x Under Age 40	0.25				0.016***				0.012***
					(0.004)				(0.003)
State EITC Rate Change x Over Age 40	0.15				-0.007*				-0.003
					(0.003)				(0.003)
Observations		89,978	89,978	89,978	153,700	89,978	89,978	89,978	153,700
Unique Observations		13,578	13,578	13,578	17,486	13,571	13,571	13,571	17,486
R-squared		0.044	0.044	0.044	0.053	0.787	0.787	0.787	0.832
P-Value from F-Test for Identical Estimates		0.753	0.166	0.93	0.001	0.073	0.261	0.007	0.004
Mean Dependent Variable		0.10	0.10	0.10	0.08	0.46	0.46	0.46	0.53

Source: 1980 to 2013 PSID. Robust standard errors in parentheses. Observations are 21 to 39 year-old household heads, except in columns 4 and 8 where 21 to 59 year-old heads are used. PSID weights used. EITC eligible are households that would receive additional EITC benefits if they had another child. Each regression represents the effect of a 10 percentage point increase in state EITC benefits on the likelihood of the household head having an additional child or being married in the following year. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: State EITCs Decrease Likelihood of Cohabitating Next Year: Similar Effect Across Controls

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State EITC Rate	0.012	0.003	-0.029***	-0.028***	-0.019***	-0.019***	-0.019***	-0.017**
(10-Percentage-Point Units)	(0.008)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.007)
Controls								
Cohabitation Status in Year *t*		X	X	X	X	X	X	X
State FE			X	X	X	X	X	X
Head Gender, Kids, Race				X	X	X	X	X
Year FE, Birth Year FE, Age Cubic					X	X	X	X
State-Race FE						X	X	X
Education							X	X
State-Year Policies and Economic Conditions								X
Observations	7,114	7,114	7,114	7,114	7,114	7,114	7,114	7,114
Unique Observations	2,567	2,567	2,567	2,567	2,567	2,567	2,567	2,567
R-squared	0.001	0.269	0.279	0.281	0.286	0.294	0.296	0.297
Mean Dependent Variable (Cohabitating in Year $t+1$) = 0.163								

Source: 2005 to 2013 PSID. Robust standard errors clustered at the state level in parentheses. Sample contains all individuals in the PSID Transition to Adulthood sample (17 to 27 years old). PSID weights used. Each regression represents the effect of an additional 10 percentage points of state EITC benefits on the likelihood of cohabitation in the following year. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Effect of State EITCs on Cohabitation for Various Subgroups

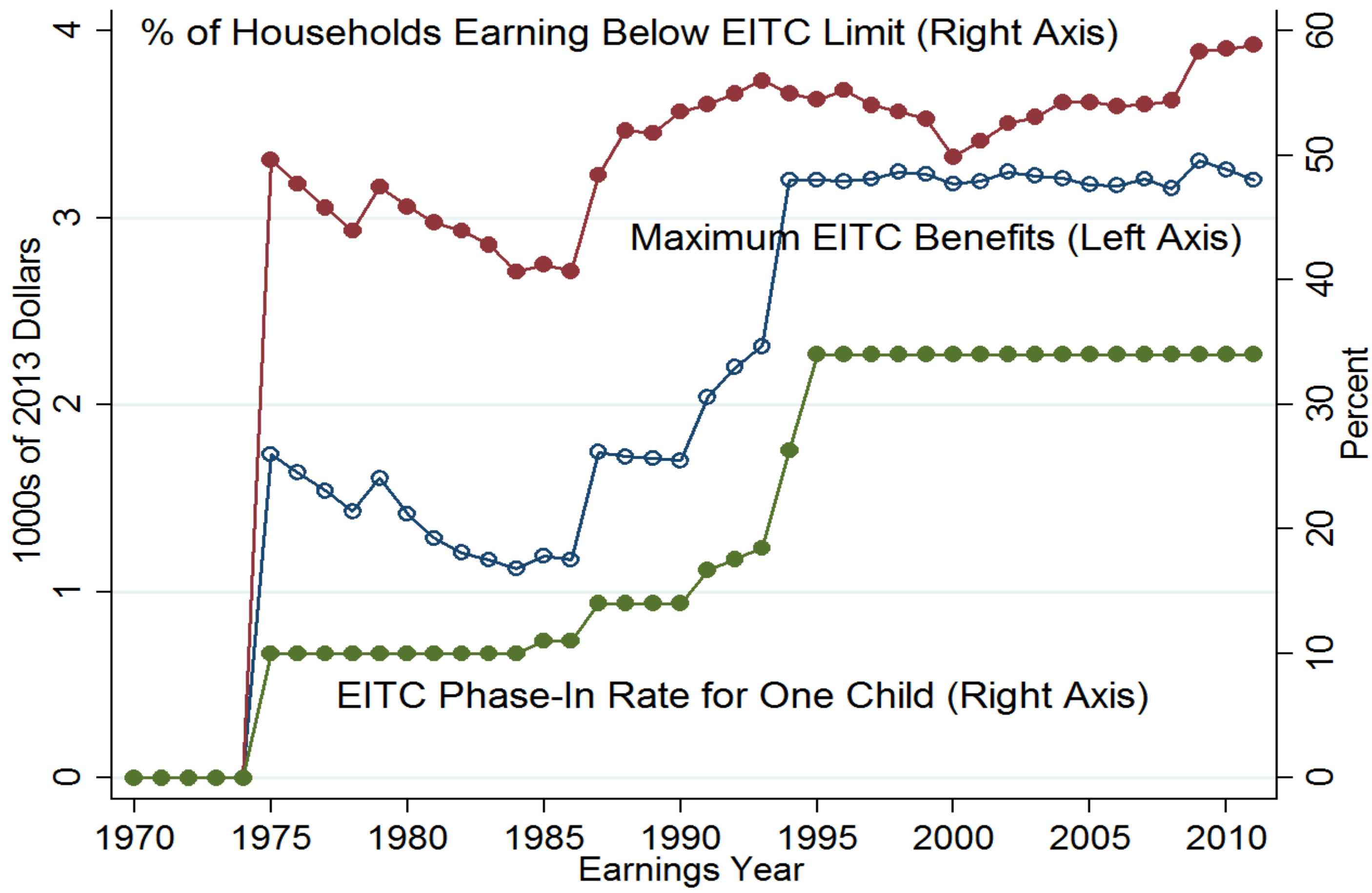
Subgroup:	Current Cohab Status	Education	Race
VARIABLES	(1)	(2)	(3)
State EITC Rate x Currently Cohab.	-0.013**		
	(0.006)		
State EITC Rate x Currently Not Cohab.	-0.047**		
	(0.021)		
State EITC Rate x Low Ed.		-0.022***	
		(0.008)	
State EITC Rate x Med. Ed.		-0.020*	
		(0.011)	
State EITC Rate x High Ed.		0.001	
		(0.011)	
State EITC Rate x Nonwhite			0.009
			(0.020)
State EITC Rate x White			-0.020***
			(0.006)
Observations	7,114	7,114	7,114
Unique Observations	2,567	2,567	2,567
R-squared	0.299	0.298	0.298
P-Value from F-Test for Identical Estimat	0.089	0.064	0.128
Mean Dependent Variable (Cohabitating in Year *t+1*) = 0.163			

Source: 2005 to 2013 PSID. Robust standard errors clustered at the state level in parentheses. Sample contains all individuals in the PSID Transition to Adulthood sample (17 to 27 years old). Of the 7114 person-year observations, 994 are cohabitating and unmarried, of which 976 are never married but only 18 are previously married. PSID weights used. Each regression represents the effect of an additional 10 percentage points of state EITC benefits on the likelihood of cohabitating in the following year. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A: Extra Material for Chapter 1

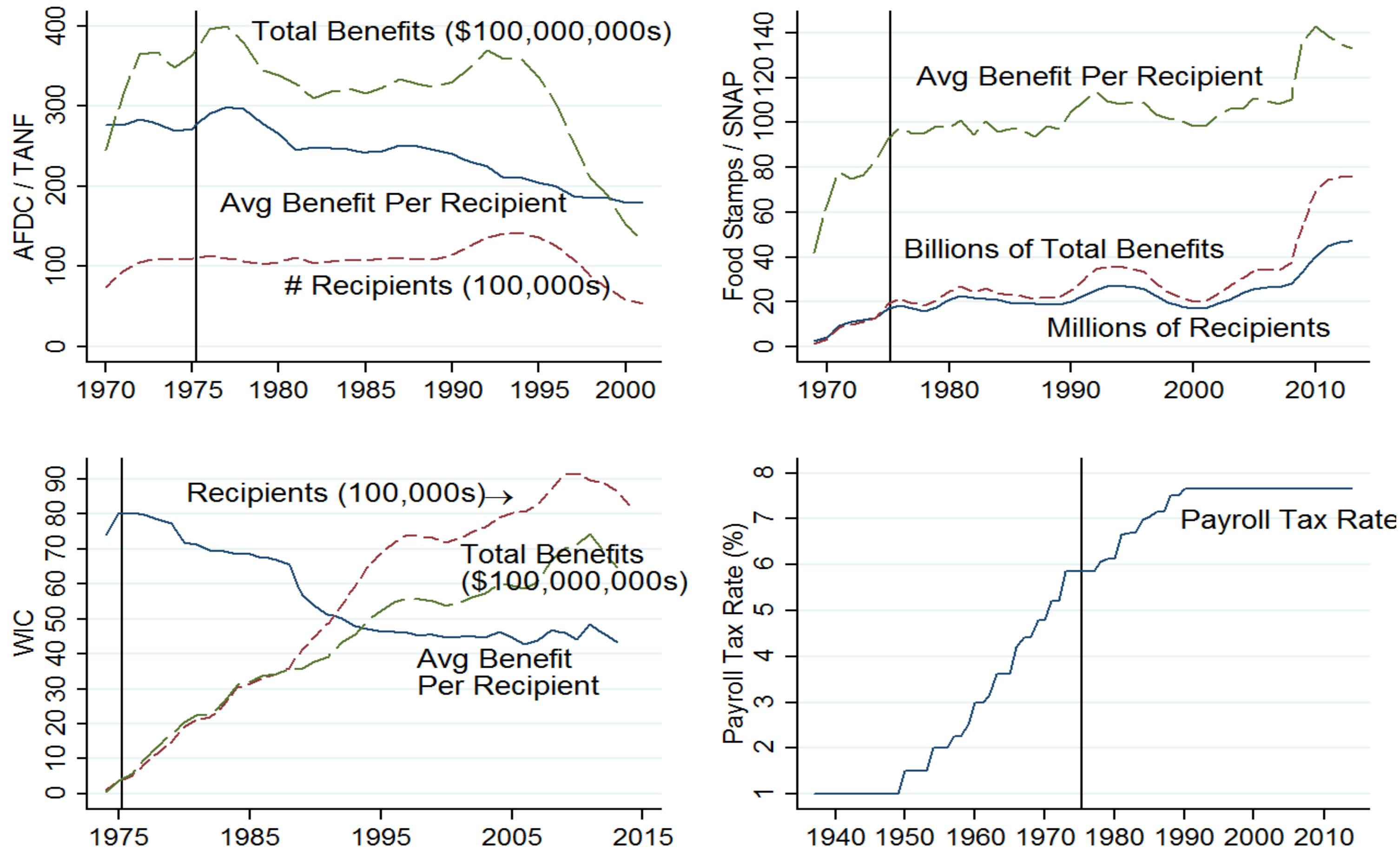
Appendix A1: Additional Figures and Tables

Figure A1.1: Trends in EITC Income Eligiblity Limit and Max Potential Benefits



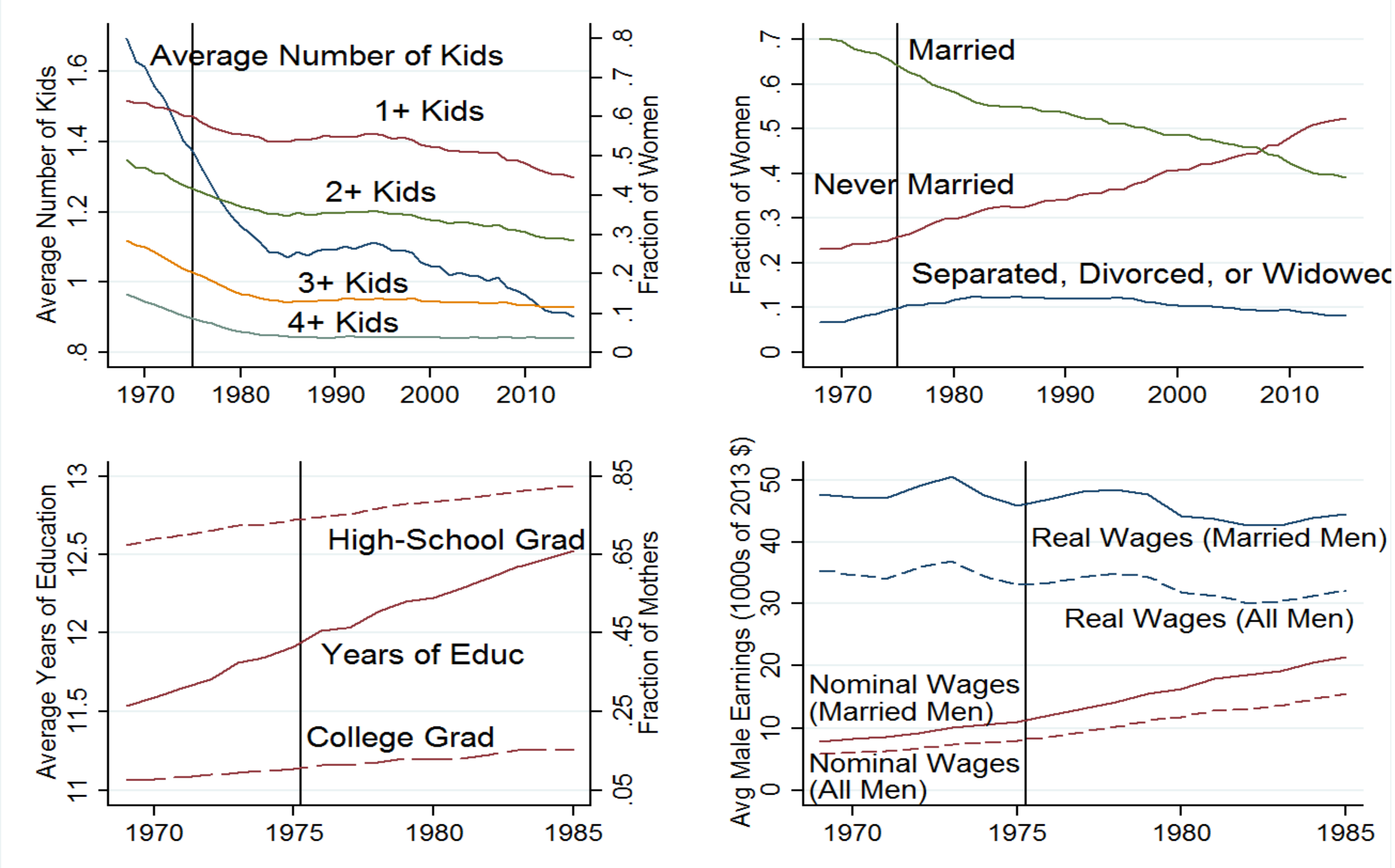
Notes: Author's calculation from IRS EITC data and 1975-2013 March CPS data on household earned income (adding each spouses earnings).

Figure A1.2: Ruling Out Confounding Policies (Tax Rate, WIC, AFDC, Food Stamps)



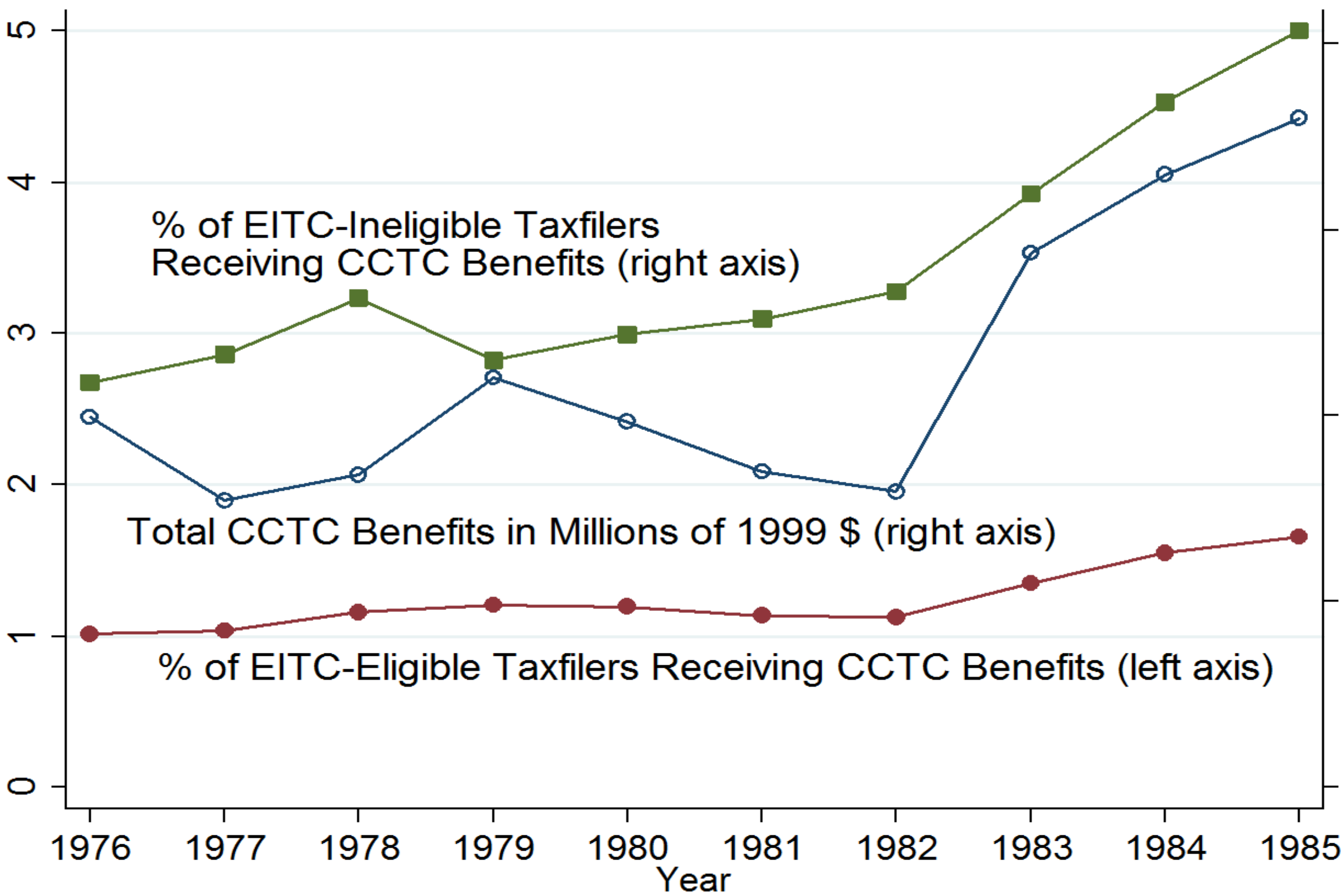
Notes: Author's calculation from AFDC/TANF data, Food Stamps/SNAP data, WIC data, and payroll tax data.

Figure A1.3: Ruling Out Confounding Trends (Fertility, Marriage, Education, Male Earnings)



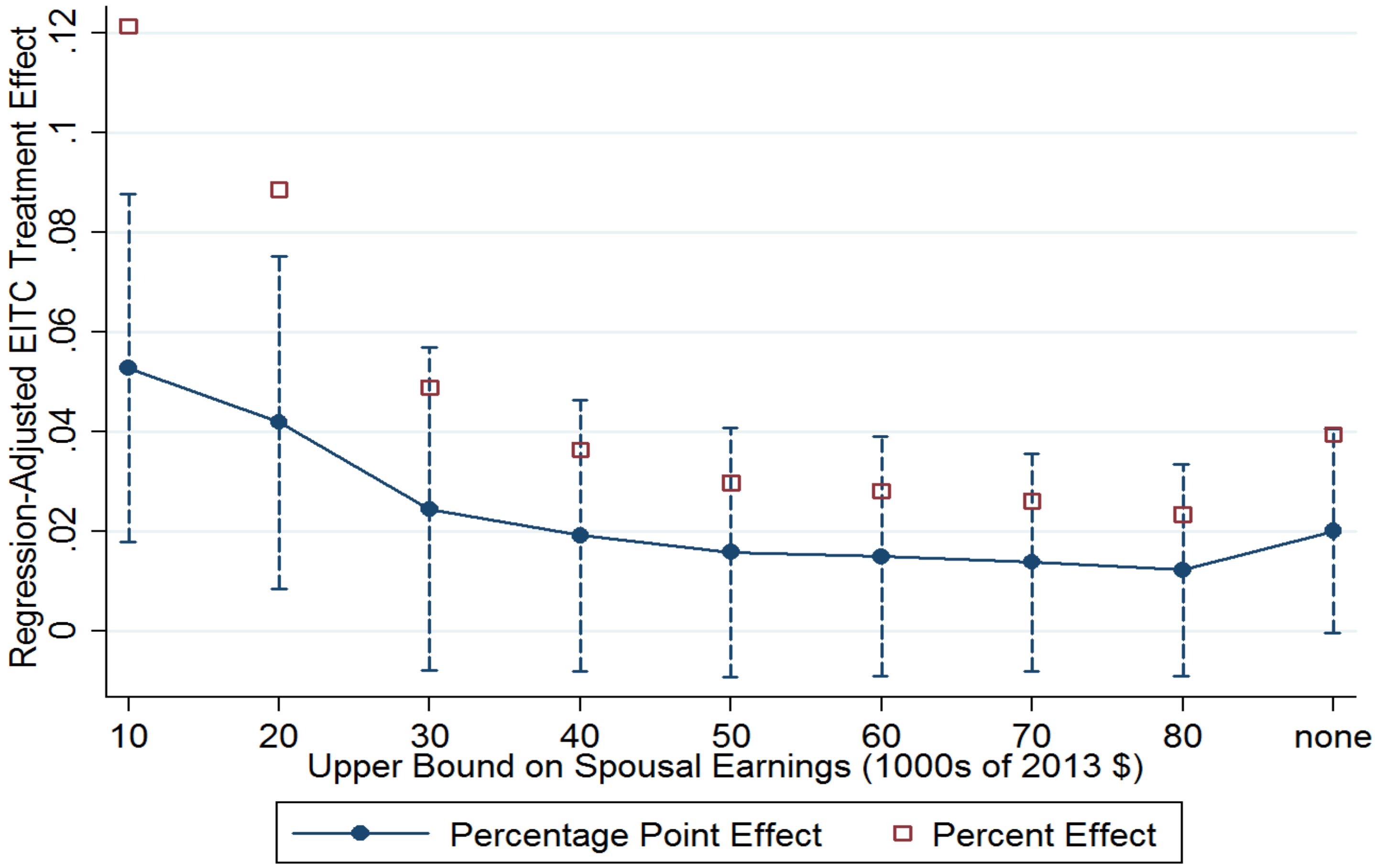
Notes: Author's calculation from 1968 to 2015 March CPS. Sample includes individuals 16 to 45 years old and refer to women unless men is specified.

Figure A1.4: 1976 Child Care Tax Credit Affects a Small Number of EITC-Eligible Taxfilers



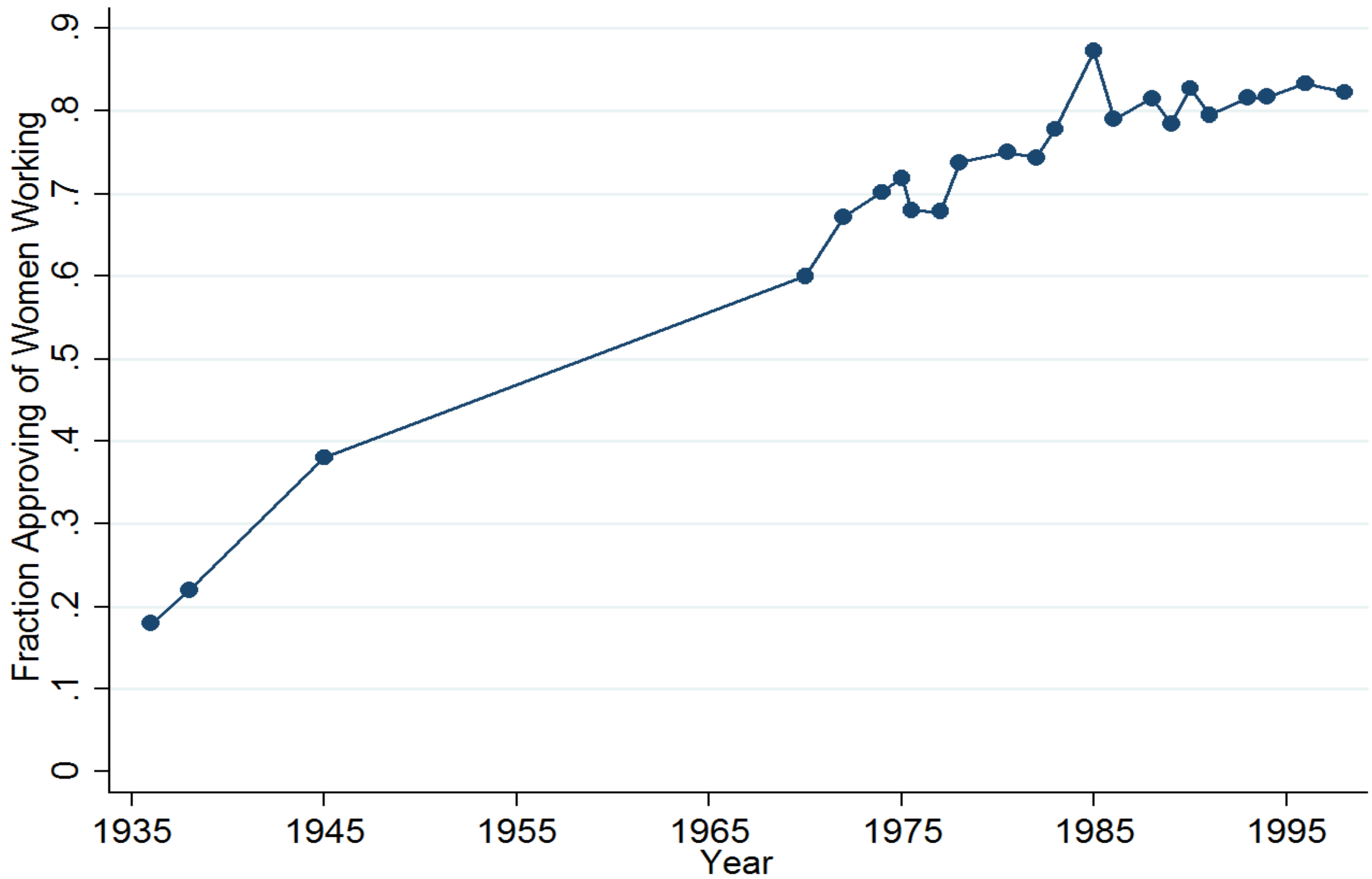
Notes: Author's calculations from 1976-1985 IRS Statistics of Income Public Use data files. Sample restricted to taxfilers with earned income or business income; this eliminates taxfilers with only dividend, interest, capital gains, pensions, farm, and alimony income. EITC eligibility imputed to taxfilers 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.

Figure A1.5: Among Married Mothers, EITC Response Negatively Correlated with Spousal Earnings



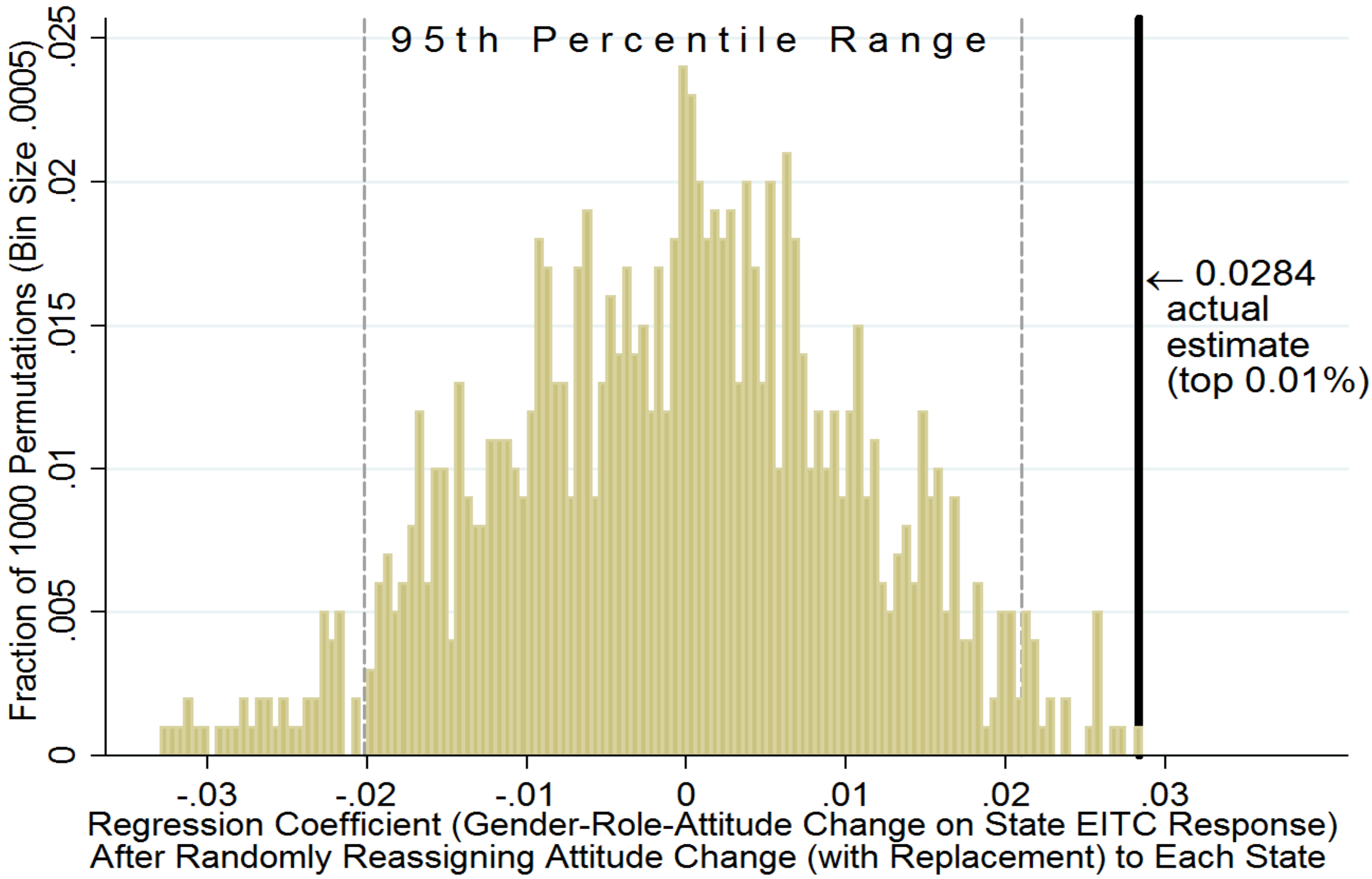
Notes: 1971-1986 March CPS data. Spousal earnings in 1000s of 2013 dollars. Married women with missing spousal earnings dropped, as in the rest of the analysis. Each estimate is from a separate logit regression that uses the set of controls from Table 2 column 4 and the sample of all married women with spouses earning below each specified amount. Treatment effects are estimates of *Mom x 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-earnings cutoff 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.

Figure A1.6: Gender-Equality Preferences Over Time



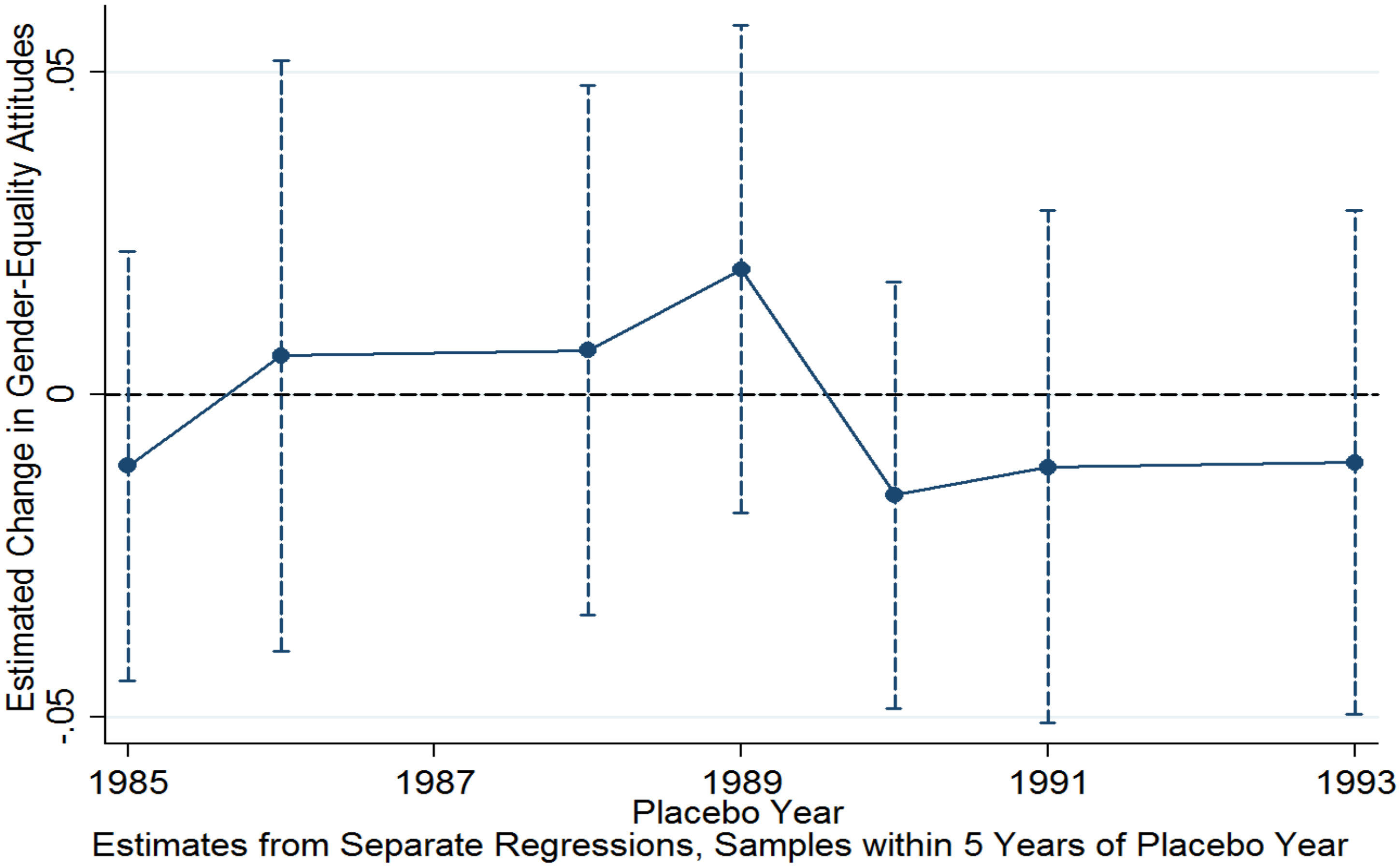
Notes: Gender-equality preferences constructed from the binary survey question, "Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?" Data sources include the restricted state-identified 1972-1998 General Social Survey (GSS) data and datasets obtained from the Roper Center (http://ropercenter.cornell.edu/CFIDE/cf/action/ipoll/index.cfm) including Gallup data (USGALLUP.111536.R01A, USAIPO1938-0136, USAIPO1945-0360, USAIPO1970-0808, USAIPO1975-0936) and Connecticut Mutual Life Insurance Company data (CMLIC Poll # 1980-AMVAL). GSS weights used to construct annual averages. Other datasets are unweighted. Respondents include male and female adults of all ages.

Figure A1.7: Permutation Test: Randomly Reassigning Attitude Changes to Each State



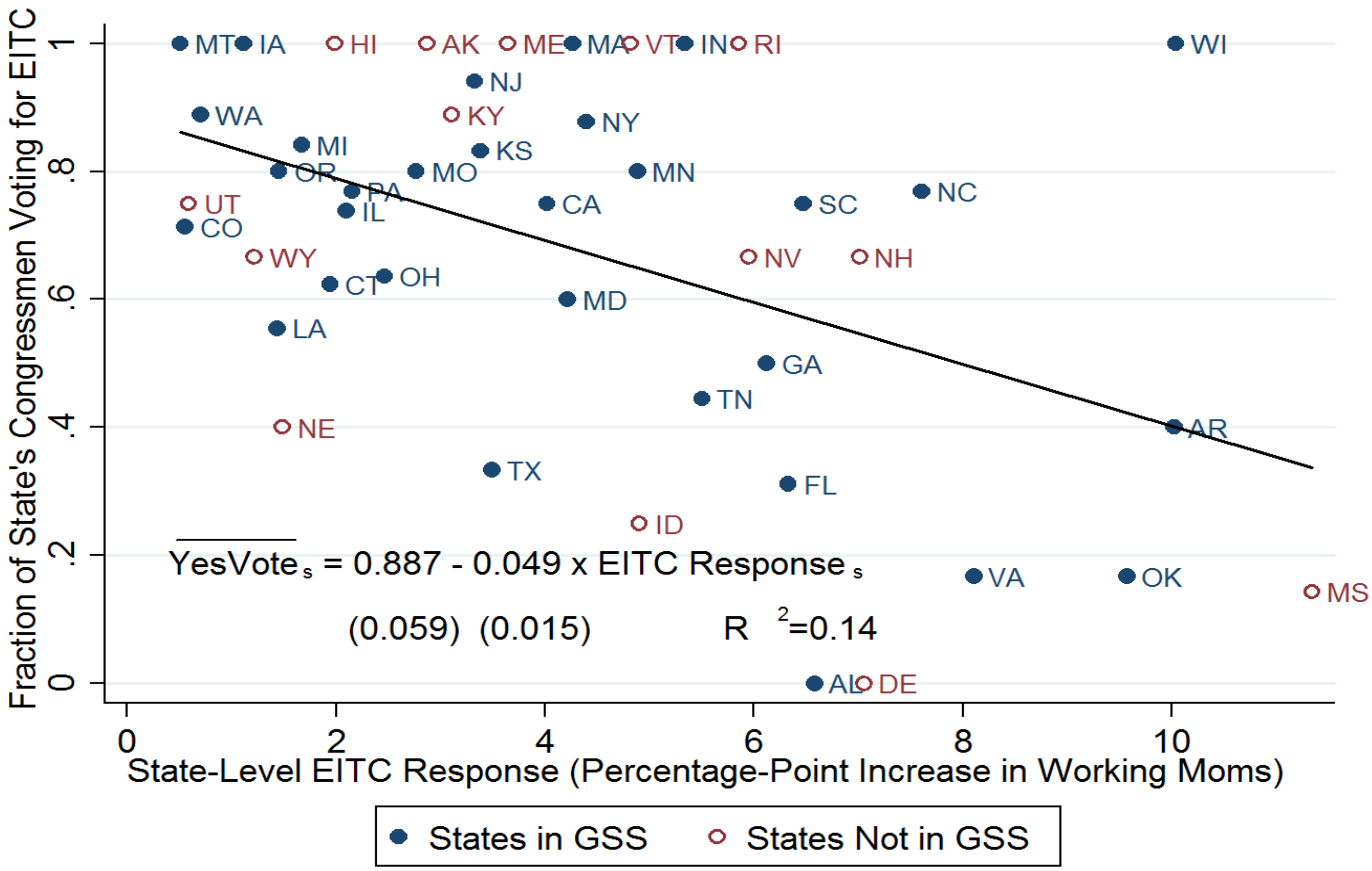
Notes: Permutation test consists of randomly reassigning (with replacement) state attitude changes to each state and re-regressing attitude changes on EITC response. 1000 iterations. This is a modified version of the modified Fisher permutation in Buchmueller, DiNardo, and Valletta (2011). The actual point estimate is .0284 and is in the top 0.01 percent of these permutations. This suggests that the bivariate regression in Figure 16a is likely not due to chance.

Figure A1.8: Placebo Test: EITC Response Uncorrelated with Placebo Year Attitude Changes



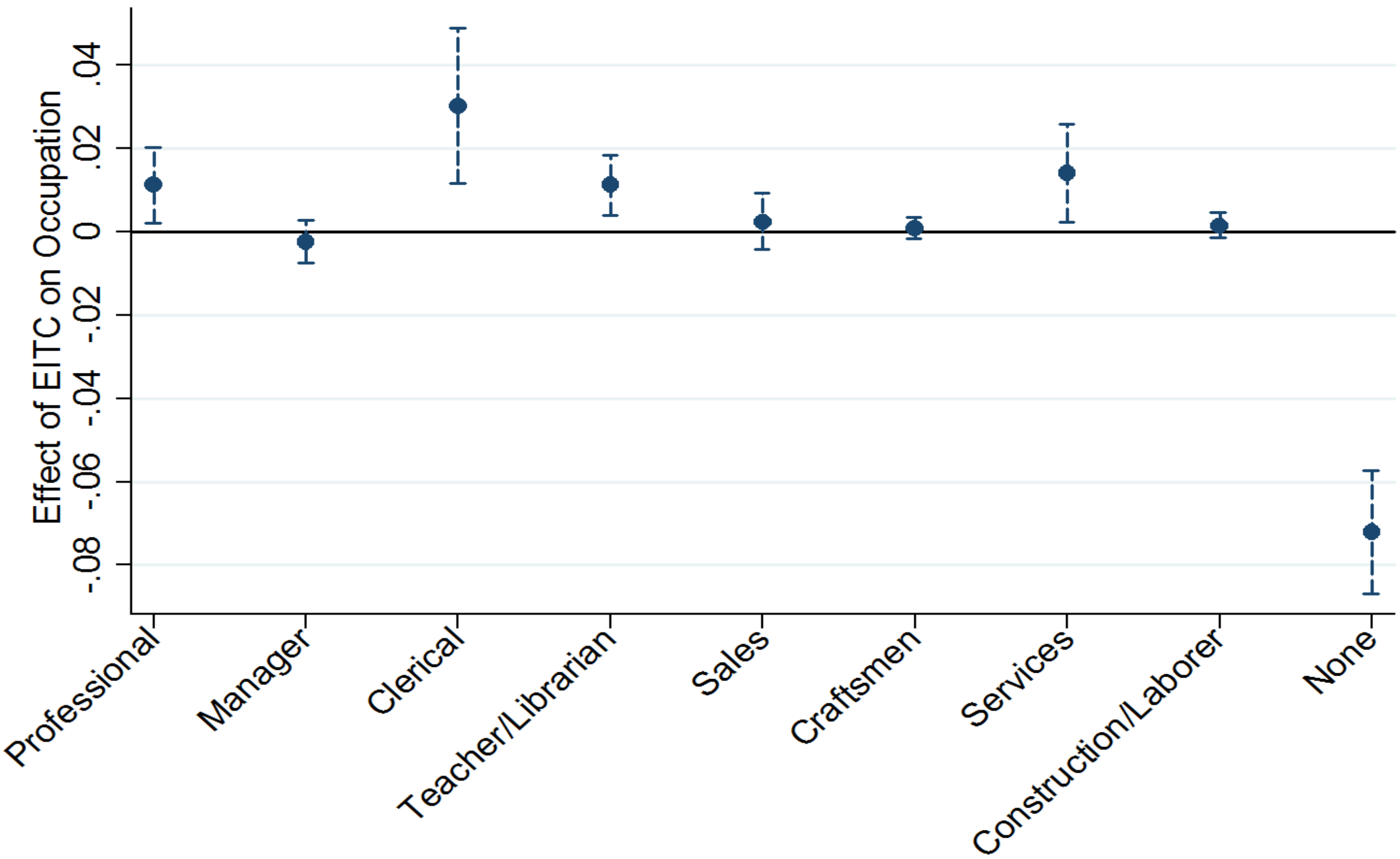
Notes: 1972-1985 restricted GSS data with state-level identifiers. Each estimate is from the bivariate regression of the change in attitudes (difference between average state attitudes in the five years after each cutoff minus the average state attitude in the five years before each cutoff, using GSS sample weights) on state response to the 1975 EITC. 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). Bootstrapped standard errors computed from 1000 replications with replacement.

Figure A1.9: State EITC Response Negatively Correlated with Voting for 1975 EITC



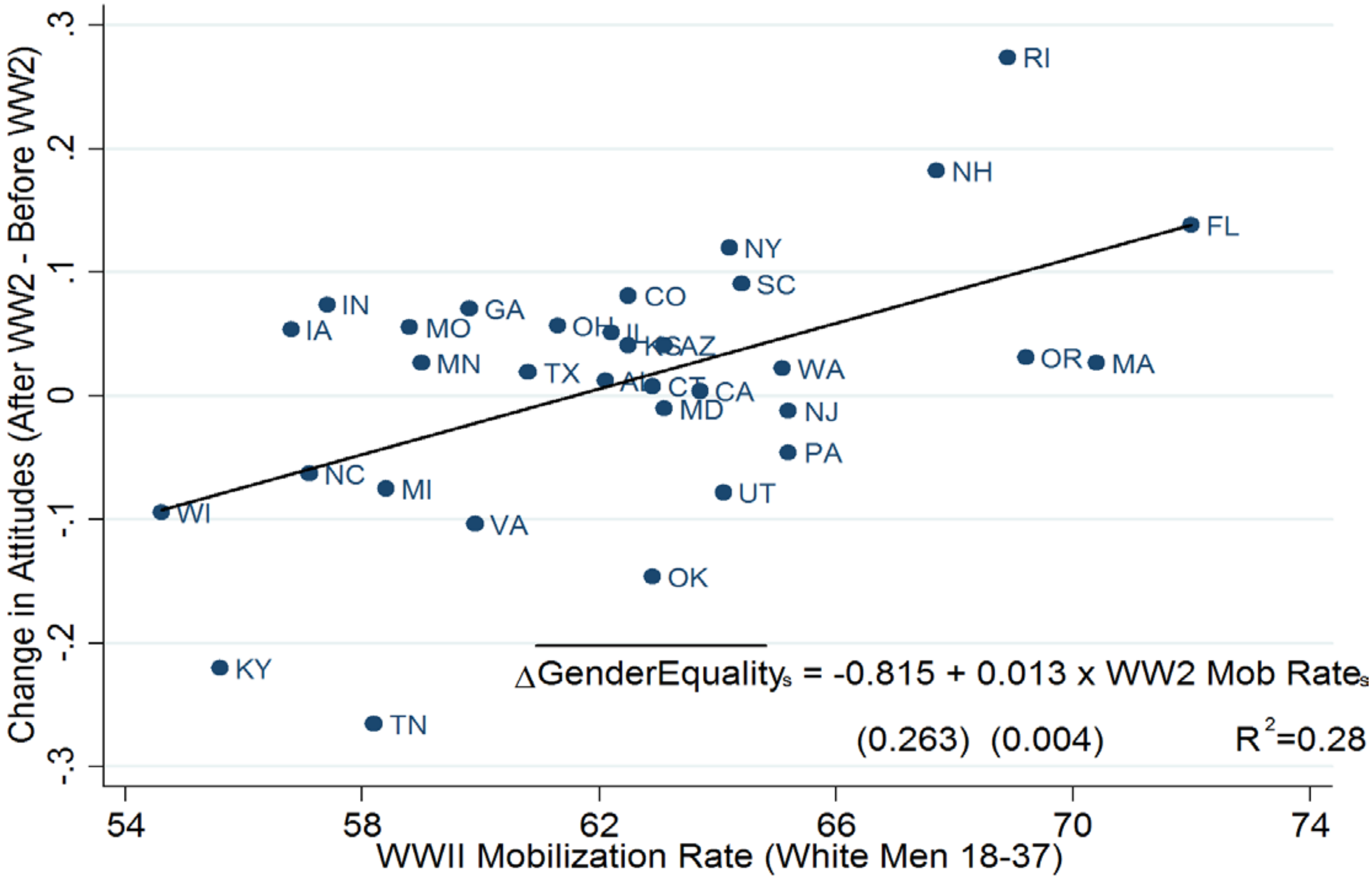
Notes: Data on House of Representatives voting from https://www.govtrack.us/congress/votes/94-1975/h67 and data on Senate voting from https://www.govtrack.us/congress/votes/94-1975/s112. Congressmen include House Representatives and Senators pooled. State EITC response comes from equation (3). Red, hollow 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.

Figure A1.10: Which Occupations Did These 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.

Figure A1.11: 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). The following Gallup datasets by archive number and survey questions were used: USAIPO1937-0062, USAIPO1937-0063, USAIPO1937-0064: Are you in favor of permitting women to serve as jurors in this state?" USAIPO1937-0066: "Would you vote for a woman for President if she qualified in every other respect?" USAIPO1938-0136: "Do you approve of a married woman earning money in business or industry if she has a husband capable of supporting her?" USAIPO1939-0165: "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?" USAIPO1945-0360: "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 WW2 - Before WW2) created by, first, coding each binary response so that 1 represents egalitarian gender attitudes and 0 represents non-egalitarian 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 WW2" and average the six 1937-1939 questions at the state level to create "Before WW2." Mobilization rates from Goldin and Olivetti (2013) Table A1. Attitudes restricted to white adults to match the mobilization rate of white men. I focus on white adults here because WW2 mobilization likely had a larger effect on white women. As Goldin and Olivetti (2013) state, "black women's [labor force] participation was high before the war and many were in agricultural occupations." The slope estimate is 0.013 (0.004), p-value 0.003. Unfortunately, it is not possible to compare exact questions before and after WW2 but estimates are quite 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).

Table A1.1: Treatment Effect Robust to Alternate Definitions of Working

Definition of Working:	Earnings >$0 (2013 $)	Earnings >$1000 (2013 $)	Earnings >$5000 (2013 $)	Work Weeks >0	Work Weeks >25	Labor-Force Partic-ipation	Unemp-loyed
Mean Dependent Variable:	0.651	0.611	0.501	0.682	0.491	0.604	0.054
Mean Dependent Variable for Mothers in 1975:	0.530	0.502	0.417	0.563	0.395	0.495	0.045
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mom x Post1975	0.040***	0.038***	0.035***	0.035***	0.032***	0.031***	0.009***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.003)
Observations	556,143	556,143	556,143	556,143	556,143	556,143	556,143
Implied Extensive Margin Labor Supply Elasticity:	0.734	0.736	0.813	0.608	0.786	0.613	--

Note: Data source: 1971-1986 March CPS data. Sample includes all women 16 to 45 years old. Binary dependent variable. 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 A1.2: Results Robust to Alternate Sample Age Ranges

Age Lower Bound (Top Row) and Age Upper Bound (Left Column)	16	18	21	25	30
	0.046***	0.040***	0.039***	0.030***	0.017
35	(0.009)	(0.008)	(0.007)	(0.010)	(0.014)
	407,261	361,199	296,011	210,668	108,059
	0.040***	0.035***	0.034***	0.028***	0.023***
45	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
	550,904	504,842	439,654	354,311	251,702
	0.038***	0.035***	0.037***	0.035***	0.033***
55	(0.007)	(0.006)	(0.005)	(0.006)	(0.006)
	683,053	636,991	571,803	486,460	383,851

Note: Data source: 1971-1986 March CPS data. Regression identical to Table 2 column 4 regression except for the sample age range. Results larger for younger mothers but results are consistently positive and statistically significant for various age ranges. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.3: Married Women with Lower-Earning Spouses Also Responded to the EITC in the 1980s and 1990s

Sample:	1986 EITC Expansion				1993 EITC Expansion			
Dependent Variable:	Employed		Annual Work Hours		Employed		Annual Work Hours	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mom x Post EITC Expansion	0.017	0.056***	24.442	130.824***	-0.002	0.033***	4.625	92.780***
	(0.012)	(0.011)	(17.199)	(18.885)	(0.007)	(0.007)	(14.758)	(18.249)
Mom x Post EITC Expansion x		-0.009***		-24.473***		-0.007***		-17.573***
Spousal Income (1000s of 2013 $)		(0.002)		(2.830)		(0.001)		(1.757)
Observations	115,194	115,194	115,194	115,194	173,241	173,241	173,241	173,241
R-squared	--	--	0.135	0.137	--	--	0.095	0.098

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 Hoynes (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). Heterogeneous responses to the EITC by married women also found by Eissa and Hoynes (2004) Table 8 and Eissa and Hoynes (2006). *** p<0.01, ** p<0.05, * p<0.1.

Table A1.4: Additional Heterogeneous Effects of the 1975 EITC on Employment

Subgroup:	Age	Age of Child	Race
Description:	Larger Response Among Younger Mothers	Larger Response Among Moms with Younger Kids	Similar Response for White and Nonwhite Mothers
Mean Dependent Variable = 0.65			
Mean Dep Var for 1975 Treat. Group = 0.53			
Variables	(1)	(2)	(3)
Mom x Post1975	0.092***	0.051***	0.038***
	(0.009)	(0.009)	(0.009)
Mom x Post1975 x Age	-0.003***	-0.002***	
	(0.0002)	(0.0003)	
Mom x Post1975 x White			0.002
			(0.008)
Observations	550,904	550,904	550,904

Notes: Data source: 1971-1986 March CPS data. All samples limited to 16 to 45 year olds. Binary dependent variable employment equals 1 for positive earnings. CPS weights 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 full set of controls from Table 2 column 4. Column 1 uses equation (2) and adds *Mom x Post1975 x (Age-16)*. There are at least two reasons to expect younger mothers to be more responsive to the EITC. First, younger women are more flexible, with smaller adjustment costs of choosing to work. Second, since earnings increase with age, younger workers are more likely to earn below the EITC limit and be eligible for EITC benefits. (Although increased earnings over the life cycle is largely attributed to increased experience, among two women with no experience (one younger, one older), a younger woman should still be more likely to respond to the EITC because even if each began earning the same amount, the younger woman could expect a higher return to lifetime earnings. 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. Column 2 uses equation (2) and adds *Mom x Post1975 x (Age of Youngest Kid)*. Whether the EITC had a larger effect on mothers with younger or older children is not obvious. Mothers with very young children had lower employment rates than mothers with older children and therefore had more room for growth, however, the opportunity and childcare costs associated with working were higher for mothers with very young children. The treatment effect was 5.1 percentage points for mothers with newborn infants and this effect decreased by 0.2 percentage points for every year older her youngest child was. This result suggests that the EITC may help explain why the U.S. has long had such a high number of new mothers that work despite few childcare subsidies or parental-leave policies. Column 3 uses equation (2) and adds *Mom x Post1975 x White*. 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 were more likely to already be working before 1975 (55 percent compared to 49 percent) and more likely to have non-labor welfare income (16 percent compared to 4 percent). However, reasons to suspect that nonwhite mothers were more affected by the EITC are that nonwhite mothers had lower household earnings before 1975 (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 of the EITC. White and nonwhite mothers had statistically identical responses to the EITC of about 3.8 percentage points. This result may reflect the context of the 1970s and not generalize to other EITC expansions. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.5: Individual Traits Correlated with Gender- and Racial-Equality Preferences

Panel A: Gender-Equality Preferences

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age / 10	-0.049***								
	(0.004)								
Year / 10		0.071***							
		(0.010)							
Years of Education			0.029***						
			(0.002)						
Married				-0.013					
				(0.010)					
Married, Wife Works					0.092***				
					(0.010)				
Non-White						-0.046***			
						(0.013)			
Log Income							0.002***		
							(0.000)		
Mother Worked								0.035*	
								(0.020)	
Racial-Equality Attitudes									0.110***
									(0.012)
Controls									
State FE	X	X	X	X	X	X	X	X	X
Year FE	X		X	X	X	X	X	X	X
Observations	8,512	8,512	8,512	8,512	8,512	8,512	8,512	1,773	8,512
R-squared	0.074	0.030	0.089	0.036	0.037	0.053	0.046	0.043	0.044

Panel B: Placebo Outcome: Racial-Equality Preferences

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age / 10	-0.021***								
	(0.002)								
Year / 10		0.039***							
		(0.007)							
Years of Education			0.017***						
			(0.001)						
Married				0.002					
				(0.008)					
Married, Wife Works					0.037***				
					(0.008)				
Non-White						0.145***			
						(0.020)			
Log Income							0.001***		
							(0.000)		
Mother Worked								0.033*	
								(0.017)	
Gender-Equality Attitudes									0.078***
									(0.008)
Controls									
State FE	X	X	X	X	X	X	X	X	X
Year FE	X		X	X	X	X	X	X	X
Observations	8,512	8,512	8,512	8,512	8,512	8,512	8,512	1,773	8,512
R-squared	0.046	0.031	0.062	0.036	0.052	0.042	0.038	0.042	0.044

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. Gender-equality preferences constructed from the GSS variable *fework*, which asks respondents whether married women should work; positive values represent egalitarian attitudes. Racial-equality preferences comes from the GSS variable *racpres*, which asks respondents whether they would vote for a black president. Robust standard errors clustered at the state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A1.6: Summary Statistics (State-Level GSS Data)

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

Table A1.7: Predicted State EITC Response (from Pre-1975 State Traits) Leads to Gender-Equality Preferences

Variable Used to Predict EITC Response	State White Single Moms	State Nonwhite Single Moms	State Fraction Female	State Male Earnings	State Fraction Men Not Working	State Occupational Structure: Teachers, Librarians	House-keepers, Cleaners	Bakers, Food Makers	Constr-uction (Men)
Panel A: Predicting EITC Response and Change in Preferences (without Region FE)									
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted State-Level Increase in Maternal Employment due to EITC (in Percentage Points)	0.031**	0.060	0.039	0.059	0.032*	0.036**	0.037***	0.076**	0.040
	(0.015)	(0.035)	(0.024)	(0.035)	(0.018)	(0.016)	(0.010)	(0.029)	(0.026)
Observations	32	32	32	32	32	32	32	32	32
R-squared	0.066	0.114	0.049	0.141	0.073	0.138	0.126	0.115	0.031
Panel B: Predicting EITC Response and Change in Preferences (with Region FE)									
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted State-Level Increase in Maternal Employment due to EITC (in Percentage Points)	0.023**	0.023**	0.022**	0.025***	0.021**	0.023**	0.022**	0.017	0.021**
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.011)	(0.009)
Observations	32	32	32	32	32	32	32	32	32
R-squared	0.120	0.119	0.118	0.172	0.104	0.121	0.117	0.073	0.106

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. State-level EITC response estimated from equation (3). Implicit behind each estimate is the four-panel scatterplot in Figure 12 and each estimate corresponds to Figure 12 Panel C. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix A2: Additional Robustness Checks

A2.1 Model Choice and Sample Period

Figure A2.1 shows that the treatment effect is robust to a probit, logit, or OLS model, and robust to when the sample ends after 1975. As would be expected from Figure 1.1B, the treatment effect grows and flattens out as more years after 1975 are included. OLS results are consistently a bit larger.[1]

A2.2 Larger Response from Mothers Eligible for More EITC Benefits

Conditional on year and spousal earnings in 2013 dollars (if any), I calculate maximum potential EITC benefits ($MaxEITC$) and run a regression identical to equation (2) except with the additional variable $Mom\ x\ Post1975\ x\ MaxEITC$. For mothers with non-earning spouses and unmarried mothers, $MaxEITC$ 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 1.3 column 5, $MaxEITC$ was zero; for married women with a spouse earning below the EITC kink point, $MaxEITC$ was equal to 10 percent of the difference between the EITC kink point and her spouse's earnings. For example, a mother with spousal earnings of $10,000 and an EITC kink point of $16,000 would have a $MaxEITC$ value of $600. Table A2.1 column 1 shows that a $1,000 increase in $MaxEITC$ is associated with a 4.5-percentage-point increase in maternal employment and carries out the placebo test from Table 1.3 column 5 in a different way: the estimate of $Mom\ x\ Post1975$ 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 $Mom\ x\ Post1975\ x\ MaxEITC$.

[1] In part because OLS 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.

A2.3 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 mothers 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 use two sets of weights: one set is constructed from the approach in DiNardo, Fortin, and Lemieux (1996) ("DFL" weights) and the other set is inverse propensity weights ("IP" weights). To construct these weights, I first I use a logit[2] and a parsimonious set of traits – 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 from a year before 1975.[3]

$$P(Pre75_{i,s}) = f(\beta_1 Age + \beta_2 Ed + \beta_3 State_{i,t} + \beta_4 Marr + \beta_5 Race + \beta_6 Mom + \epsilon) \qquad (1)$$

For notational ease, I omit i, s, and t subscripts. Each observation is assigned a probability p of being from a year before 1975; I create DFL and IP weights by assigning each observation a weight of $\frac{p}{1-p}$ and $\frac{1}{p}$.[4] Women are weighted less if their observed characteristics are less likely to be from a year before 1975 and weighed more if their characteristics are more likely to be from a year before 1975 (e.g. lower education or higher fertility). Figure A2.2 verifies that the characteristics of women before and after 1975 overlap sufficiently and have common support (Busso, DiNardo, and McCrary 2013). Re-estimating equation (2) with these new weights yields estimates of 4.2 and 3.7 percentage points (Table A2.1 columns 2 and 3), similar to the baseline DD estimate of 4.0.

[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.
[3] DiNardo, Fortin, and Lemieux (1996) utilize a parsimonious set of controls with 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.
[4] Weights are multiplied with the CPS sample weights and normalized to add up to 1 (Butcher and DiNardo 2002).

A2.4 March CPS Imputations

In 1975 the Census changed its hot deck procedure[5] for imputing missing earnings (Bound and Freeman 1992) and could affect the results in Tables 1.2, 1.3, and A2.1 since I define employment as having positive earnings (although Table A1.1 shows similar estimates for other binary definitions of working). 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, and 1.0. In Table A2.2, 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 (2) and a logit to predict the probability than an observation has missing earnings data (to account for data missing not at random), create DFL and IP weights (in the way described in the previous section), and re-estimate equation (2) with these weights. Columns 5 and 6 reflect estimates from a bounding exercise – similar to Manski bounds Manski (1990) – where I assign all observations with missing earnings data to be working or not working. Across each regression, the DD estimate is stable between 3.9 and 4.7 percentage points, similar to the baseline estimate of 4.0.

A2.5 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 (2) and accounts for any differential impact on employment from having at least J kids.

$$P(E) = f(\beta_1 Post1975 + \sum_{k=1}^{J} [\beta_{2,k} Mom^k + \beta_{3,k} Mom^k \; x \; Post1975] + \beta_4 X + \epsilon) \qquad (2)$$

For notational ease, I omit i, s, and t subscripts. Table A2.3 columns 1 to 3 show results of this regression for $J = \{1,2,3\}$. Column 1 replicates the baseline estimate where $J = 1$, but

[5] Where people with missing information are matched with similar people based on sex, race and ethnicity, household relationship, education, 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: https://usa.ipums.org/usa/voliii/80editall.shtml\#note1.

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 (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, when I restrict the sample to year before 1979 I still find a positive estimate on $\beta_{3,k=2}$ (Table A2.3 column 4) and conclude that increased exemptions is not driving the results in this context.

Another potential explanation is that mothers with multiple children were more likely to have completed their fertility. If mothers that had completed their fertility were more receptive to working – especially when their children reached school age – then with cross-sectional CPS data there could be a mechanical relationship between having multiple children and EITC response. I test this hypothesis in Table A2.3 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 EITC response from mothers with at least two children (relative to mothers with one child) converges to zero, while the estimated response of mothers with exactly one child ($\beta_{3,k=1}$) 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 find the same pattern for the 1986 and 1993 EITC expansion as well (results omitted) and conclude that the additional employment increase for women with at least two children may be explained by mothers that had completed their fertility. (This may also be why Eissa and Liebman (1996) find the same pattern.)

A2.6 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. First, many non-working individuals do not file taxes, so detecting an extensive margin response is not easy. Second, *household* income is reported, so it is not possible to determine whether one or two spouses worked. Third, IRS SOI data include few demographic variables so it is not possible to determine the gender, age, race, or education of the tax filer, whether they have children – dependents are not necessarily children – or child's age.[6]

Constrained by the IRS SOI data, I find evidence that the EITC affected the composition of tax filers. Using 1968 to 1985 IRS SOI data, I show that the fraction of unmarried EITC-eligible tax filers – section 1.5.2.1 shows that single mothers were relatively more affected by the EITC – increased in the years after 1975 (Figure A2.3). The pattern closely resembles Figure 1.1B: flat before 1975, a quick rise between 1975 and 1980, and relatively flat again after 1980. Without knowing tax filer gender or whether dependents denote children, this is only suggestive evidence that the EITC affected the employment of single mothers.[7]

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 A2.4 shows that the number of EITC-eligible households and aggregate EITC benefits – that I calculate from reported 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[8] due to unintentional tax filer 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.[9] The growing

[6] Marital status is available. Number of children available after 1977, otherwise only in 1970 and 1975. See http://users.nber.org/~taxsim/taxsim-ndx.txt for annual available IRS SOI variables.

[7] It is difficult to determine whether the number of tax filers increased, since one million working mothers over a four year period (Figures 1.1A and 1.1B) corresponds to about 250,000 mothers per year, small in comparison to the 80 million households, 100 million adults in the labor force, and 95 million tax filers in the U.S. by 1980 (source: CPS, BLS, IRS SOI). As a result, I am not able to detect an aggregate rise in tax filers 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 tax-filing household.

[8] https://www.eitc.irs.gov/Tax-Preparer-Toolkit/faqs/fraud

[9] This is related to the infamous event where millions of children “disappeared” when taxpayers had to begin reporting the Social Security number of all dependents in 1987 (LaLumia and Sallee 2013).

gap between CPS and IRS data in Figure A2.4 suggests that tax filer error may have increased between 1975 and 1985.

Observing less (imputed) EITC benefits in the CPS than the aggregate IRS numbers suggest that my employment estimates from the CPS are not overestimates of the actual working response to the EITC. Although Figure A3.1 shows evidence of misreporting self-employed income to take advantage of EITC benefits, this represents a relatively small number of the million mothers that begin working in response to the EITC.

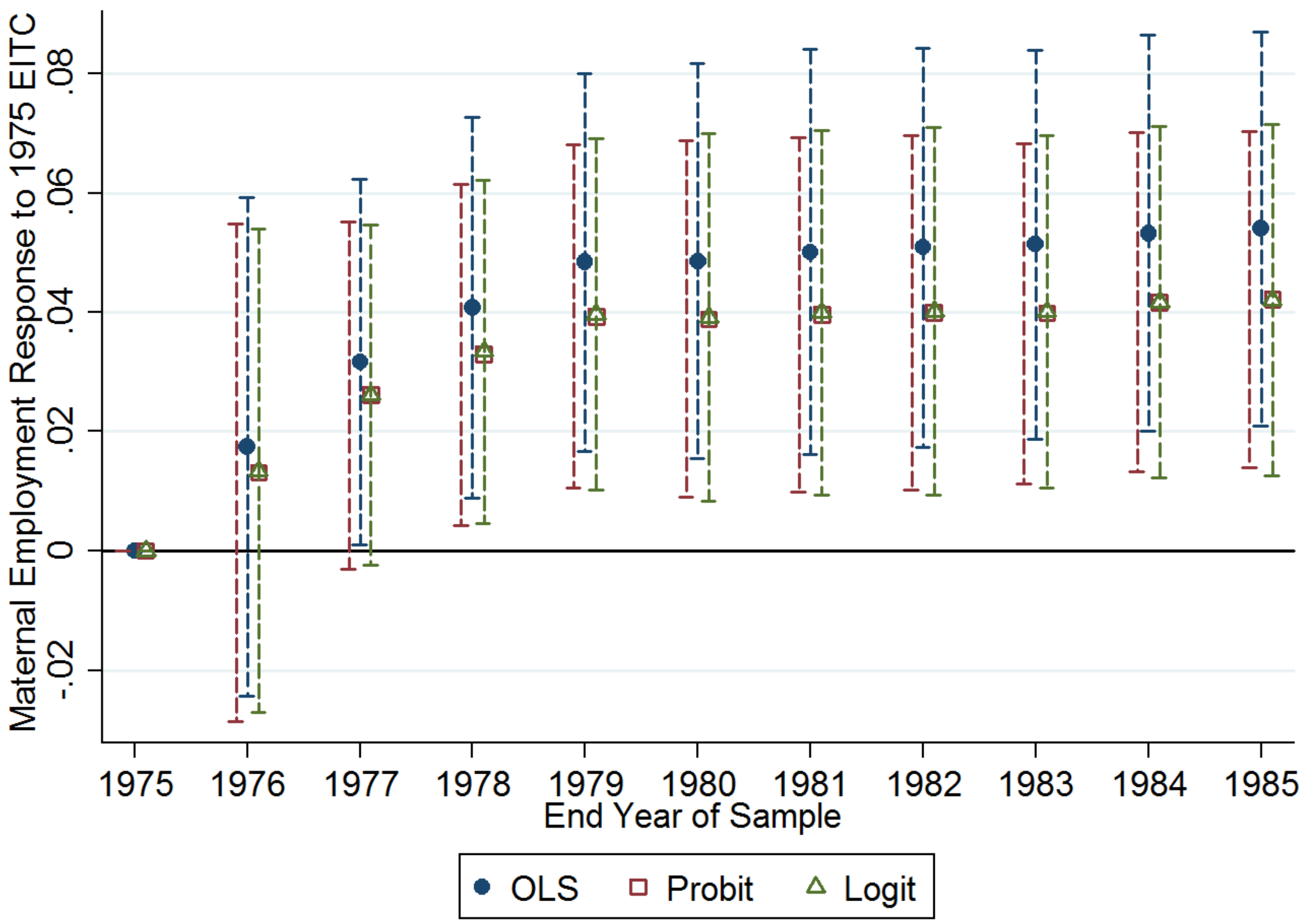
Aggregate IRS data also reveal a puzzle in light of estimates in Table 1.2: the number of EITC recipients and the aggregate EITC benefits remained roughly constant between 1975 and 1985 (Figure A2.5). One way to reconcile the positive maternal employment response to the EITC and flat EITC benefits is 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, but most recipients seem to have already been working since Figures 1.1A and 1.1B suggest that the 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" (Saez 2003).[10] The increase in EITC-eligible working mothers (Table 1.2) and no-longer-EITC-eligible households may have 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 1974 SOI earnings distribution (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 tax filers that were EITC-*eligible* in 1975 but EITC-*ineligible* in 1976, 1977, or 1978 due to rising nominal income.[11] Figure A2.6 illustrates that by 1976, 1977, and 1978, 0.6, 1.0, and 1.6 percent of tax filers eligible for the EITC in 1975 would bracket-creep out of EITC eligibility, corresponding to 700,000, 1,200,000, and 1,800,000 tax filers.[12] Even though the stock of EITC recipients remained roughly constant in the decade after 1975, there was substantial flow in and out of

[10] This nominal limit was $8,000 through 1978 and $10,000 through 1984.
[11] Assuming constant real earnings – rising real wages would yield even more “bracket creep".
[12] 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 tax filers 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 200,000 to 600,000 additional EITC recipients.

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.

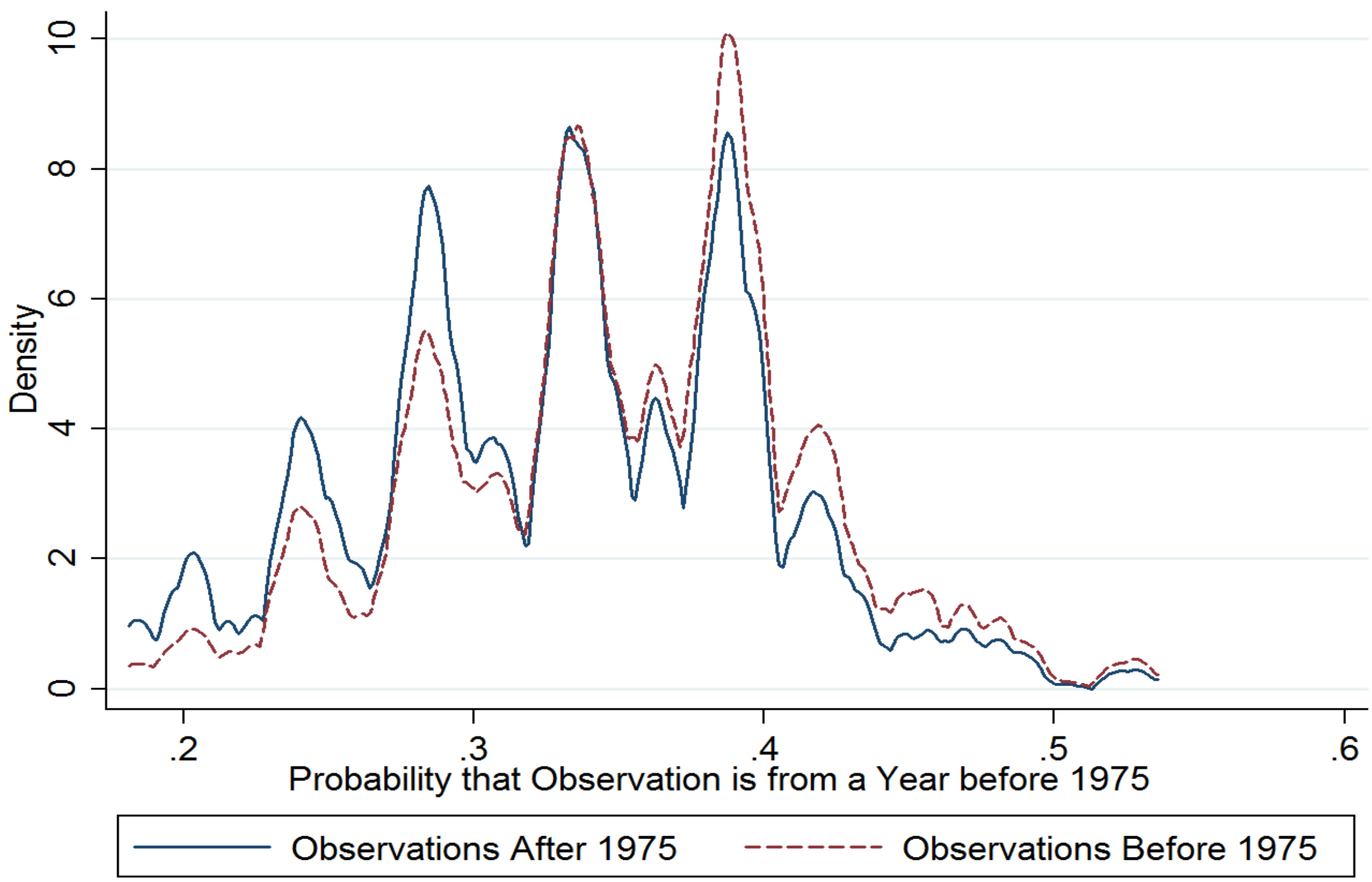
A2.7: Figures and Tables

Figure A2.1: DD Estimate Robust to Model Choice and Year that Sample Period Ends



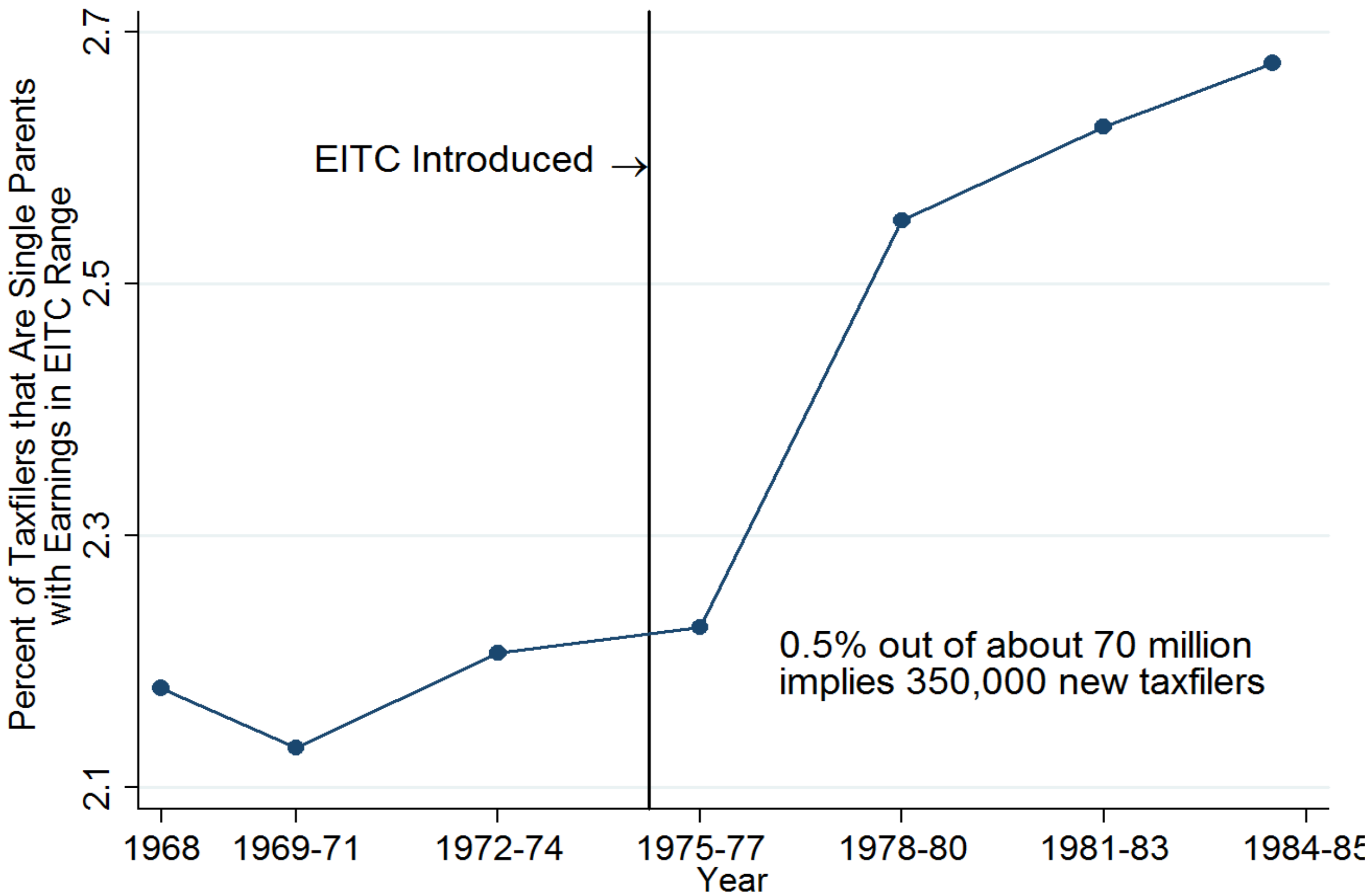
Notes: Data source: 1971-1986 March CPS data. Dependent variable is binary employment defined as having positive earnings. CPS weights, logit, equation (2), and controls from Table 2 column 4 used. Average-marginal effects shown. Standard errors are computed by the delta method, are robust to heteroskedasticity, 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 x Post1975* in equation (2) where *Post1975* starts in 1976 and extends through the year specified on the x-axis.

Figure A2.2: Kernel Density Plot for Women Before and After 1975



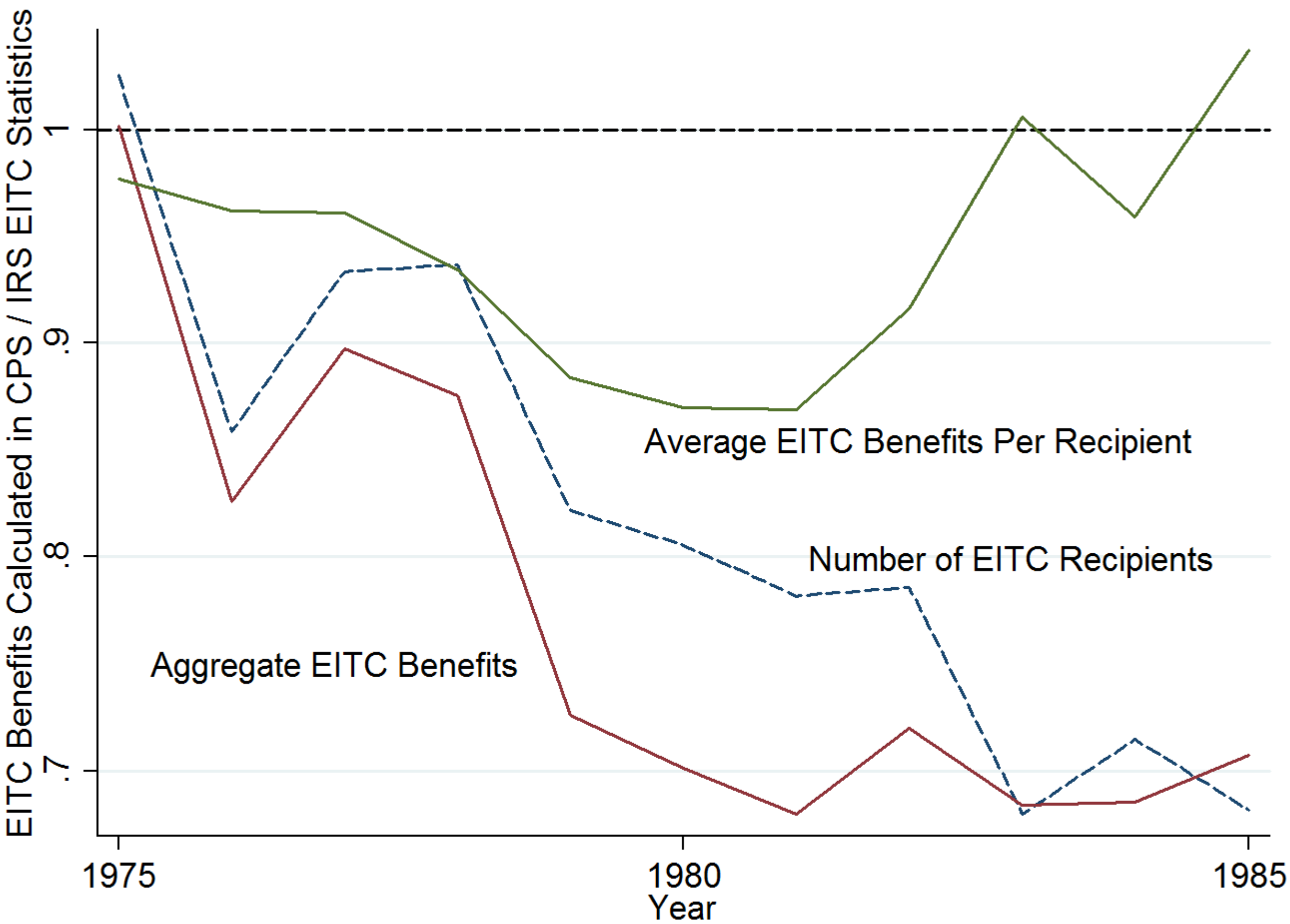
Notes: Data source: 1971-1986 March CPS data (Ruggles et al. 2015). Equation (B2) used to predict the probability of each observation being observed in a year before 1975. Following DiNardo, Fortin, and Lemieux (1996), I choose a parsimonious set of observable characteristics X, which includes six age bins, three education bins, married and nonwhite dummy variables, and 21 state bins for a total of 1512 cells.

Figure A2.3: The 1975 EITC May Have Affected the Composition of Taxfilers



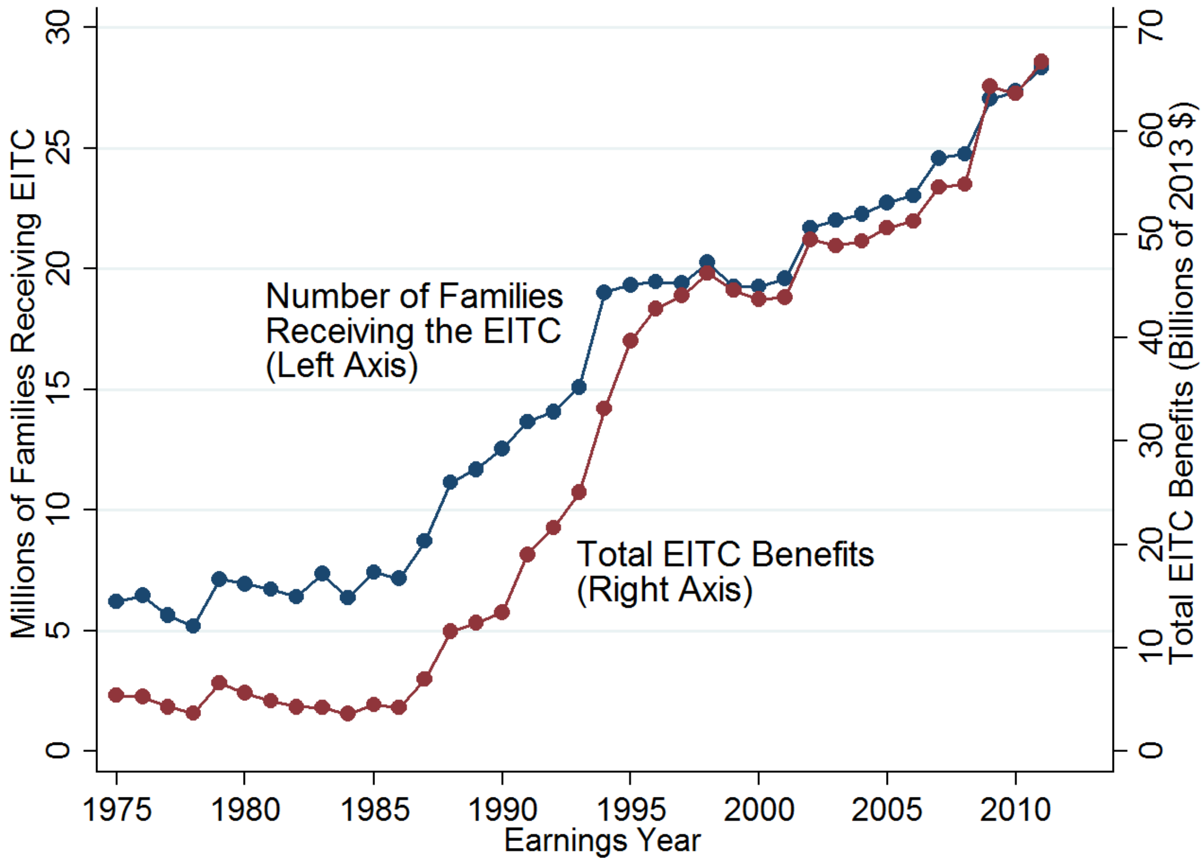
Notes: Author's calculations from 1968-1985 IRS Statistics of Income Public Use data files. Sample restricted to taxfilers with earned income or business income; this eliminates taxfilers with only dividend, interest, capital gains, pensions, farm, and alimony income. Refundable portion of the EITC is 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.

Figure A2.4: Comparing EITC Recipients and Benefits (CPS / IRS Ratio)



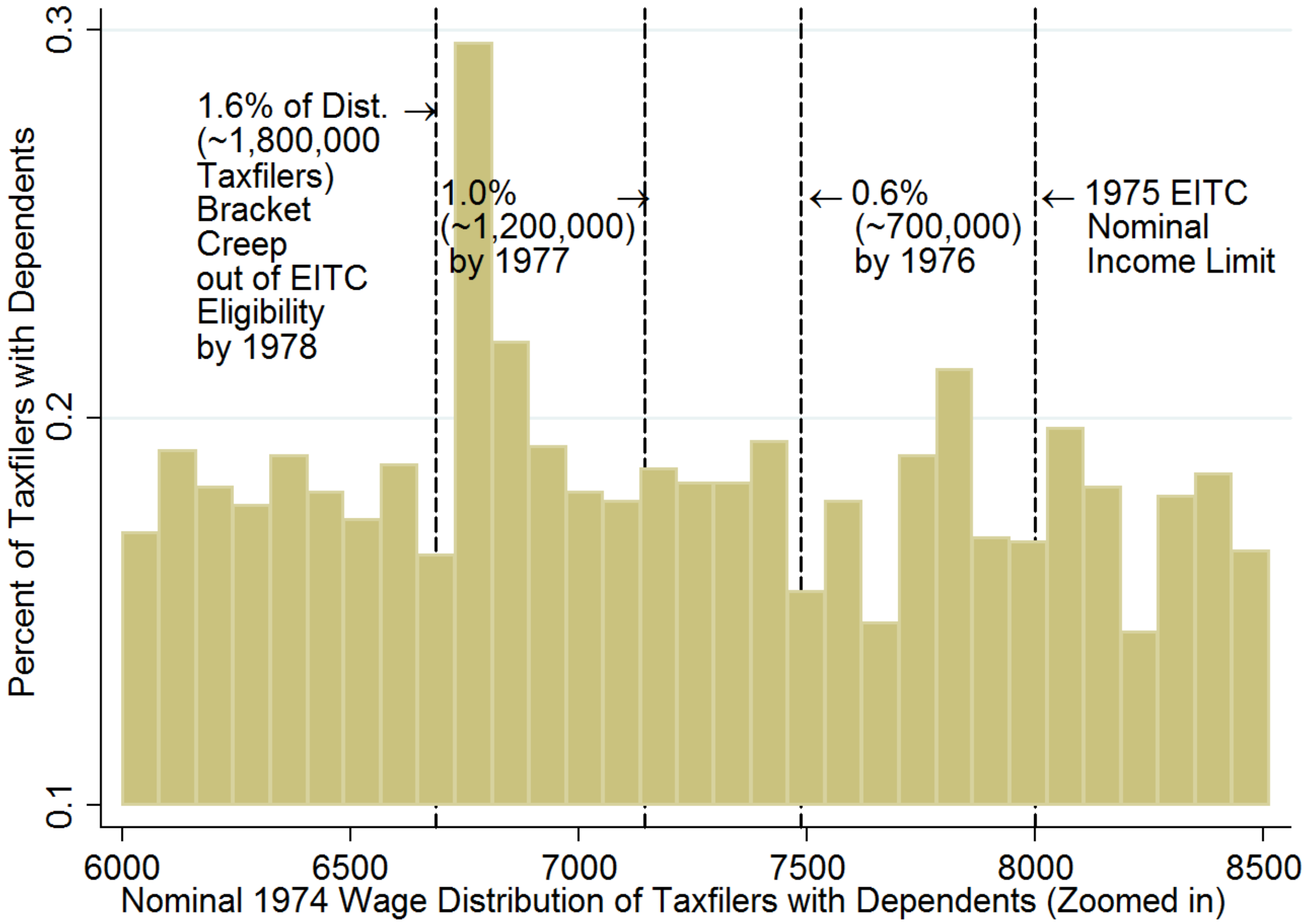
Notes: Author's calculation from 1976-1986 March CPS data (Ruggles et al. 2015) and published aggregate EITC recipients and benefits (http://www.taxpolicycenter.org/statistics/eitc-recipients). I calculate EITC recipients and benefits based on household earnings, the annual EITC schedule, and whether the household has any children.

Figure A2.5: Trends in EITC Benefits and Recipients



Notes: Author's calculations from IRS data.

Figure A2.6: Bracket Creep, Nominal EITC Schedule Reconciles Table 2 and Figure B5



Notes: Author's calculations from 1974 IRS SOI data and CPI. Sample includes taxfilers with wage earnings or business income.

Table A2.1: Robustness Checks: *MaxEITC* and Reweighting

	Larger Response from Moms Eligible for More Potential Max EITC Benefits	Reweighting Post-1975 Moms to Look Like Pre-1975 Moms	
		DFL Weights	IP Weights
Variables	(1)	(2)	(3)
Mom x Post1975	0.003	0.042***	0.037***
	(0.008)	(0.010)	(0.008)
Mom x Post1975 x MaxEITC	0.045***		
	(0.005)		
Observations	550,904	550,904	550,904

Note: Data source: 1971-1986 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 logit regression are shown. Reweighting discussed in Appendix B section 11.3. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.2: Alternate Ways to Treat Imputed CPS Observations

	Baseline: Using CPS Imputations	Drop Imputed Obs.	Using DFL Weights	Using IPW	Assigning 0 to all Imputed Obs	Assigning 1 to all Imputed Obs
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Mom x Post1975	0.040***	0.042***	0.039***	0.040***	0.047***	0.040***
	(0.009)	(0.009)	(0.008)	(0.010)	(0.009)	(0.009)
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 employment for positive earnings. 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 4.

Table A2.3: 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 Exemption Explanation	DD for 2+ Children Approaches Zero for Mothers Likely to Have Completed Fertility				More
Restricting Mothers by Age of Youngest Child	No	No	No	No	> 1	> 2	> 3	> 4	> 5
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mom x Post1975	0.040***	0.029***	0.029***	0.013	0.029***	0.029***	0.031***	0.036***	0.037***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.008)	(0.008)	(0.009)	(0.009)
Mom of 2+ Kids x Post1975		0.018***	0.023***	0.020***	0.016***	0.014**	0.013**	0.009	0.002
		(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Mom of 3+ Kids x Post1975			-0.011**						
			(0.005)						
Observations	550,904	550,904	550,904	281,746	488,063	460,007	436,009	415,305	396,508

Note: Data source: 1972-1981 March CPS. Binary dependent variable employment. CPS weights used and average marginal effects from logit 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 4. 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 5 to 9 drop mothers in the treatment group with a youngest child below the specified age. *** p<0.01, ** p<0.05, * p<0.1.

Appendix A3: Calculating Elasticities

A3.1 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 piece 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 1.4B). I report compensated (Hicksian) elasticities.

In Table A3.1 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 A3.1 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.

A3.2 Elasticities from Bunching of Self-Employed Workers

Following Saez (2010), I use IRS Statistics of Income Public Use Data (SOI) to look for bunching among self-employed tax filers. Figure C1 shows bunching at the EITC kink point among EITC-eligible tax filers with positive self-employment income (business schedule C), both for the 1975-1978 EITC schedule and the expanded 1979-1984 EITC schedule. Figure C1 shows no bunching among EITC-ineligible tax filers (claiming zero children) with positive self-employment income. Figure C2 shows no bunching among wage earners (with no self-employment income), both for EITC-eligible and EITC-ineligible tax filers. There is only evidence of bunching among EITC-eligible tax filers with positive self-employed income. This bunching likely reflects income misreporting since there is no third-party reporting for self-employed workers (LaLumia 2009, Saez 2010, Kuka 2013). Following the approach in Saez (2010), I calculate the implied bunching elasticity and find similar results.[1]

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. The first order condition is $z = n(1-t)^e$ and the bunching elasticity can be estimated by solving the following for e:

$$B = \frac{z^*}{2}\left[\left(\frac{1-t_0}{1-t_1}\right)^e - 1\right]\left[h(z^*)_- + \frac{h(z^*)_+}{\left(\frac{1-t_0}{1-t_1}\right)^e}\right] \tag{1}$$

Where z^* is the kink threshold, $\frac{1-t_0}{1-t_1}$ 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 δ, B is equal to the density of tax filers 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 A3.1 for nominal EITC schedules), and bandwidths of \$1000, \$1500, and \$2000 to calculate the implied bunching elasticity.

For 1975-1978 and δ=\$1000: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.2, B=0.0114, $h(z^*)_-$=0.0000582, and $h(z^*)_+$=0.0000788. Yielding e=0.23.

[1] Using a quasi-linear and iso-elastic utility function, the excess bunching density in the earnings distribution, and bandwidths of \$1000, \$1500, and \$2000, I calculate elasticities of taxable income of 0.23, 0.52, and 0.77 in 1975-1978 and 0.58, 1.28, and 2.22 in 1979-1985. The nominal EITC schedule was slightly modified in 1979. 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).

For 1975-1978 and δ=\$1500: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.2, B=0.0173, $h(z^*)_-$=0.0000388, and $h(z^*)_+$=0.0000525. Yielding e=0.52.

For 1975-1978 and δ=\$2000: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.2, B=0.0191, $h(z^*)_-$=0.0000291, and $h(z^*)_+$=0.0000394. Yielding e=0.77.

For 1979-1984 and δ=\$1000: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.1, B=0.0204, $h(z^*)_-$=0.0000642, and $h(z^*)_+$=0.0000838. Yielding e=0.58.

For 1979-1984 and δ=\$1500: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.1, B=0.0299, $h(z^*)_-$=0.0000428, and $h(z^*)_+$=0.0000559. Yielding e=1.28.

For 1979-1984 and δ=\$2000: z^*=\$4000, $\frac{1-t_0}{1-t_1}$=1.1, B=0.0388, $h(z^*)_-$=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.

A3.3: Figures and Tables

Figure A3.1A: Bunching Among Self-Employed EITC-Eligible Taxfilers

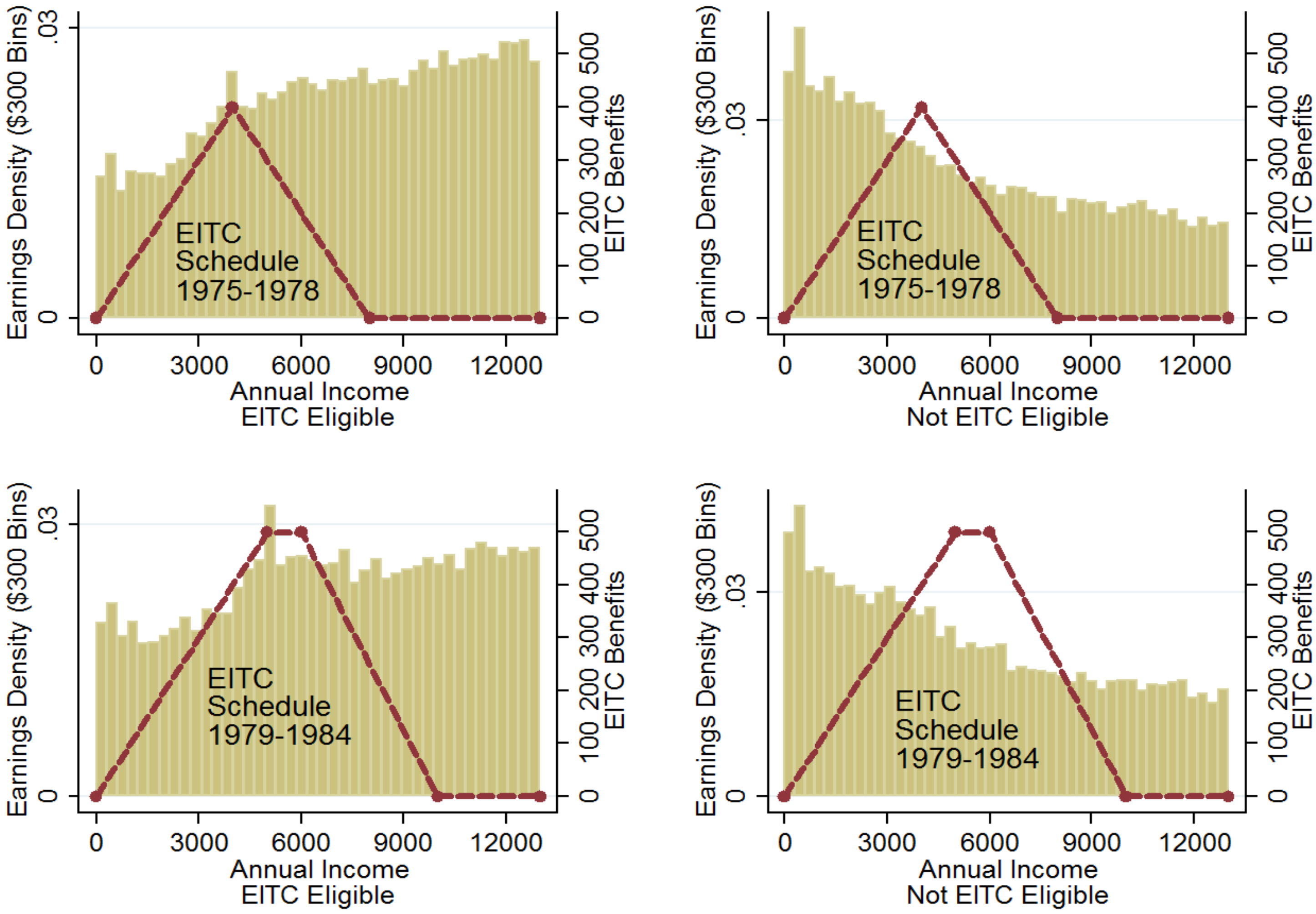
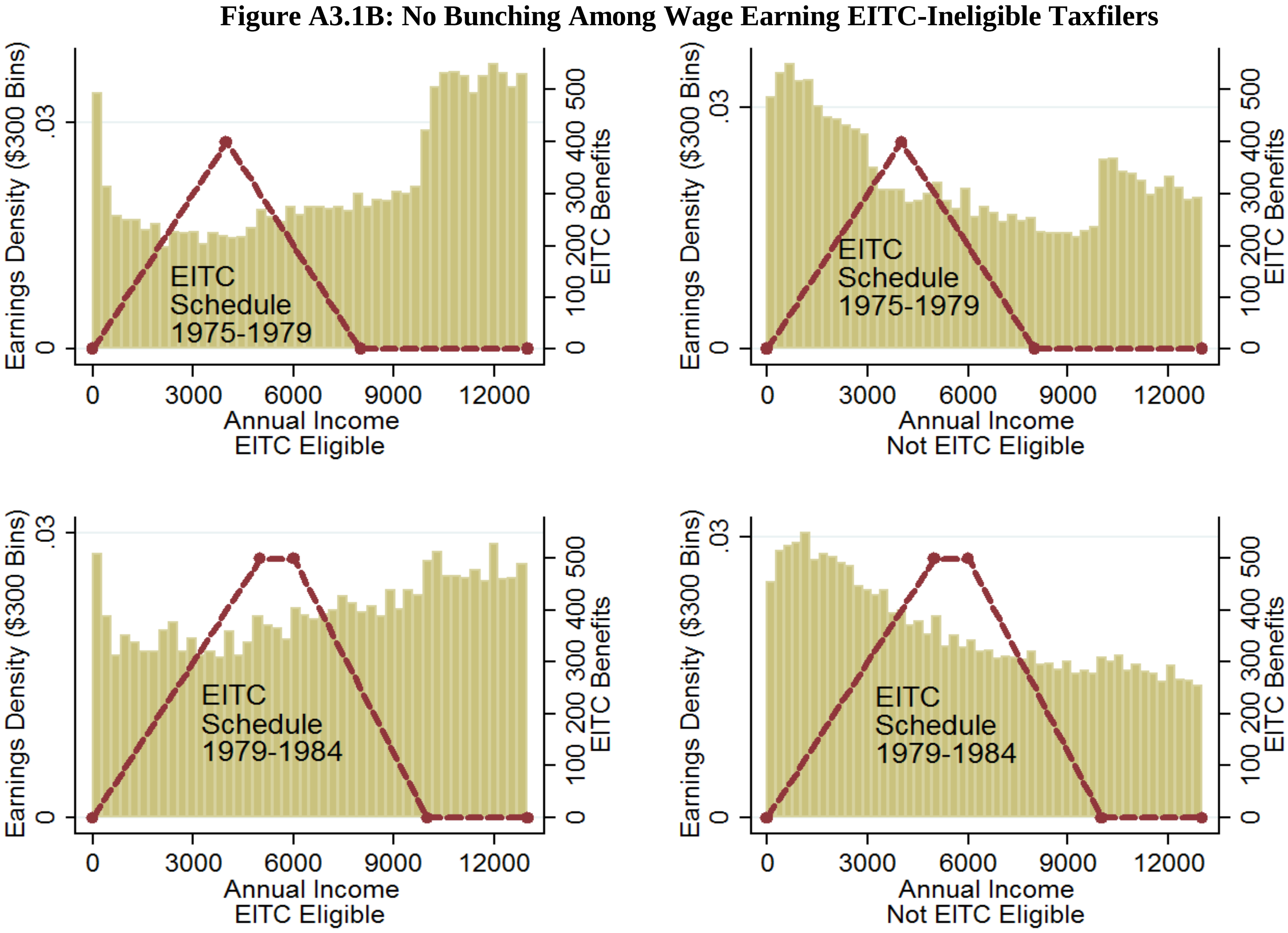


Figure A3.1B: No Bunching Among Wage Earning EITC-Ineligible Taxfilers



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.

Table A3.1: Calculating Labor Supply Elasticity for a Representative Unmarried Mother with Median Earnings

	Nominal Dollars														Real 2013 $
Year	Nominal Earnings (worth $19,160 in 2013 $, the Median Earnings in CPS Sample)	Dependent Exemption	Taxable Income with One Child	Payroll Tax Rate	Payroll Tax Paid	EITC Benefits	Income Taxes Owed	Total Taxes Owed	Monetary Return to Working	Avg. Annual Food Stamp Benefits	Avg. Annual WIC Benefits	Avg. Annual AFDC Benefits	Monetary Return to Not Working	Difference in Return to Work vs. Not Work	Difference in Return to Work vs. Not Work
1970	3195	625	2570	4.8	123	0	360	483	2712	127	0	552	679	2033	**12193**
1971	3330	675	2655	5.2	138	0	372	510	2820	163	0	576	739	2081	**11977**
1972	3439	750	2689	5.2	140	0	376	516	2922	162	0	612	774	2149	**11972**
1973	3654	750	2904	5.85	170	0	407	576	3077	175	0	636	811	2266	**11884**
1974	4056	750	3306	5.85	193	0	473	666	3390	211	188	684	1083	2307	**10896**
1975	4427	750	3677	5.85	215	357	530	388	4039	257	223	756	1236	2803	**12132**
1976	4683	750	3933	6.05	238	332	570	476	4207	287	235	852	1374	2832	**11589**
1977	4985	750	4235	5.85	248	301	0	-54	5039	297	250	936	1482	3557	**13670**
1978	5366	750	4616	6.05	279	263	0	16	5350	321	264	996	1581	3769	**13458**
1979	5970	1000	4970	6.13	305	403	0	-98	6068	367	289	1044	1700	4368	**14019**
1980	6776	1000	5776	6.13	354	322	0	32	6744	414	305	1128	1847	4897	**13848**
1981	7479	1000	6479	6.65	431	252	94	273	7206	474	334	1152	1960	5246	**13441**
1982	7939	1000	6939	6.7	465	206	205	464	7475	470	346	1236	2052	5423	**13087**
1983	8190	1000	7190	6.7	482	181	264	565	7625	516	355	1272	2143	5482	**12824**
1984	8548	1000	7548	7	528	145	345	728	7820	513	367	1320	2200	5620	**12596**
1985	8850	1000	7850	7.05	553	127	385	812	8038	540	380	1344	2264	5774	**12500**

Return to Work (1970-1974 Average) = 11,784 — Return to Work (1975-1985 Average) = 13,024

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 intensive + 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.

Appendix A4: Attitude Changes, Less Parametric Approaches

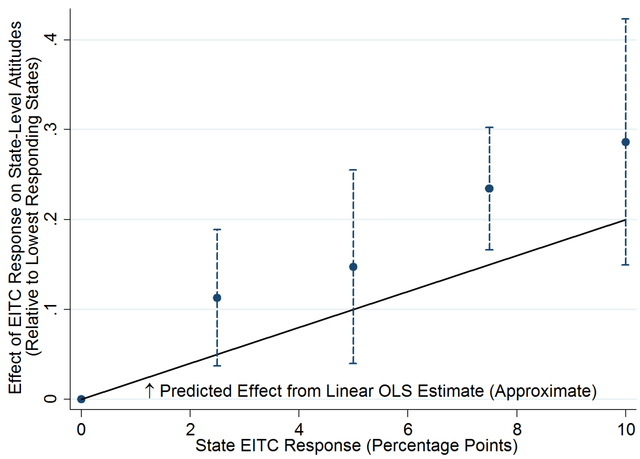
Results in Figure 1.7 show that each percentage-point increase in EITC response led to a 1.7 percentage point increase in positive state gender-equality attitudes. However, if this relationship is not linear — such as with decreasing marginal treatment effects — an OLS specification could be a poor approximation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

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 A4.1 show estimates from a regression resembling equation (5), but with four binary variables instead of the continuous variable *EITC Response*. 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 A4.1 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 A4.2 shows that when the regression behind Figure 1.7 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 or perhaps the influx of working mothers actually had an increasing, then stable, then increasing again, effect on attitudes.

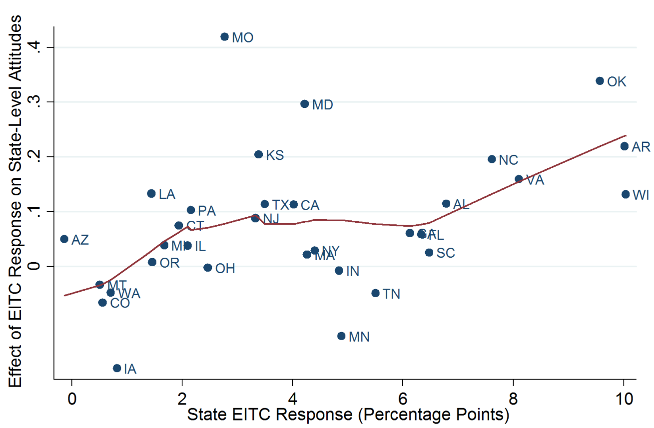
Appendix A4: Attitude Changes, Less Parametric Approaches

Figure A4.1: Comparing Categorical EITC Response with Constant, Linear Effect



Notes: Results come from a single regression resembling equation (5) except that *EITC Response* 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 *EITC Response* between 0 and 0.9 percentage points) are 5, 7, 15, 2, 3.

Figure A4.2: Locally Weighted Regression



Notes: Locally weighted regression (Cleveland 1979). State EITC response and attitude changes. Stata command lowess, default setting: running-line least squares, tricube weighting function, bandwidth 0.8.

Appendix A5: Data Appendix

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

A5.1 March Current Population Survey Data

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. Dropping women with missing spousal earnings (15,126 observations), males (837,755), and women over 45 (388,951 observations), yields the 550,904 women used in the main analysis.

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 age cubed are 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 come 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). When state-level measures pertain to these multi-state groups, I weight the variable by annual state population. This data source begins in 1969. Dollars adjusted to real dollars (when specified) using the Consumer Price Index. Occupations detailed in Figure A1.10 notes.

A5.2 IRS Statistics of Income Public Use Files

Analysis behind Figures A3.1A and A3.1B and bunching elasticities calculated in Appendix A3 use 1975 to 1984 SOI data. Sample restricted to tax filers 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. Variable availability in SOI data found here: http://users.nber.org/~taxsim/taxsim-ndx.txt.

Analysis behind Figure A1.4 use 1976 to 1985 SOI data. EITC-eligible tax filers defined those with wage earnings or business schedule C income below the EITC income limit with a child dependent. Child and Dependent Care Tax Credits given by SOI variable *data64*.

Section A2.6 uses 1968 to 1985 SOI data. Marital status given by SOI variable *data2*. Number of tax filers in Figure B3 determined from SOI weight *data1*.

A5.3 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 real 1000s. Democrat defined as *partyid* values between 0 and 2, religious defined as *reliten* values of 1 or 3, too much welfare defined from *natf* are, 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 all years.

Figure A1.6 includes adults of all ages (18+) and pools men and women. All other GSS analysis is restricted to adults ages 18-60. This upper bound 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 1.6 Panel B), all analysis is also restricted to only the attitudes of men.

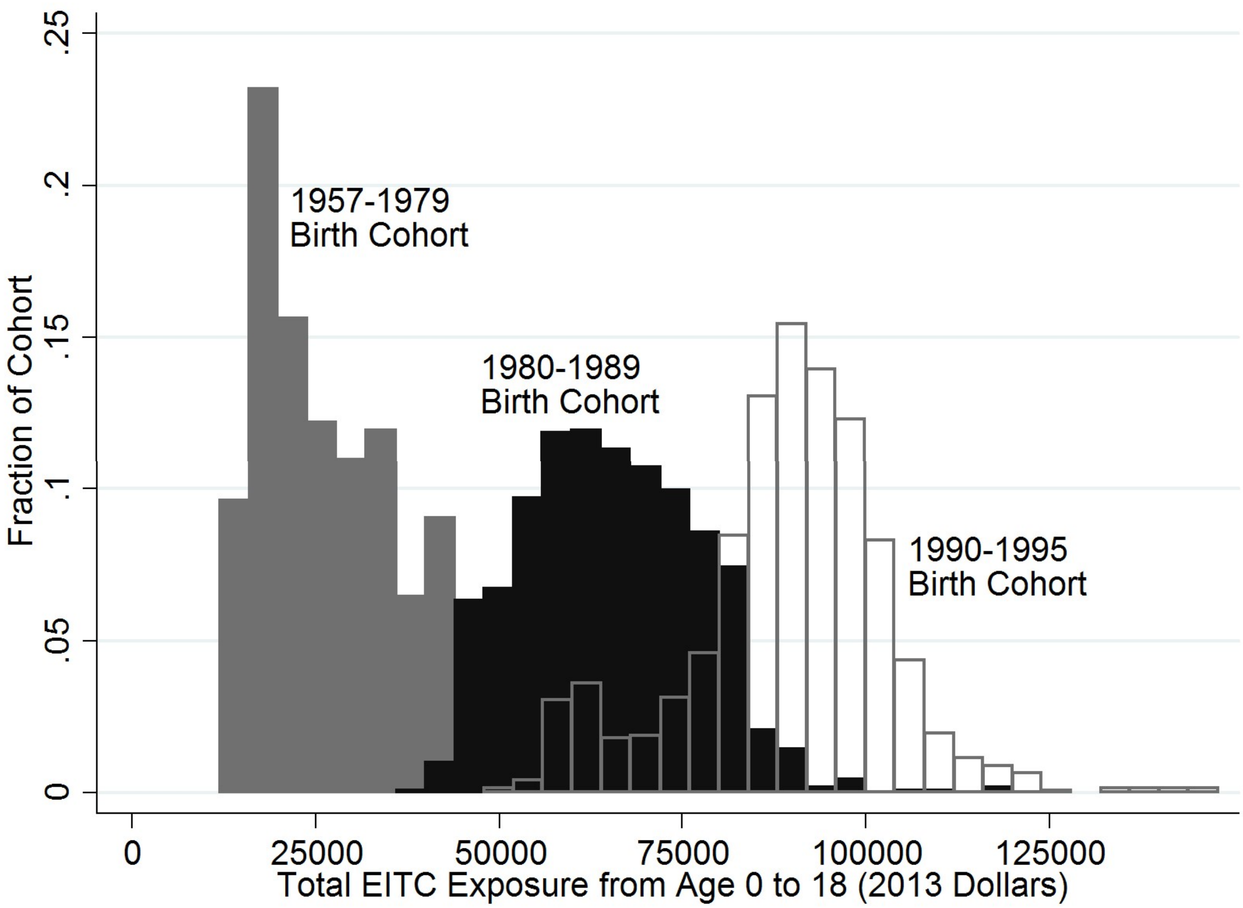
Results define the post1975 period through 1985 and include years 1977, 1978, 1982, 1983, and 1985. The other questions do not have the outcome variable of interest. Results are similar if 1985 (or if 1983 and 1985) is excluded. As would be expected from the employment trends in Figures 1.1A and 1.1B, the effect on attitudes is larger if 1977 is excluded from the post1975 period.

A5.4 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 A1.11. Gallup (1937c), Gallup (1937a), and Gallup (1937b): "Are you in favor of permitting women to serve as jurors in this state?" Gallup (1937b): "Would you vote for a woman for President if she was 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?"

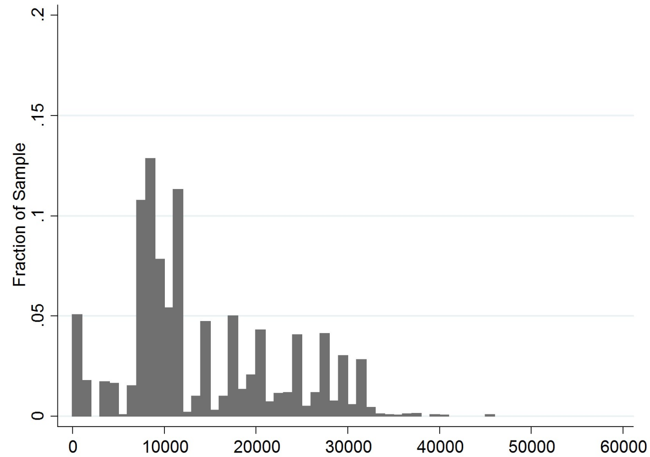
Appendix B1: Additional Figures and Tables

Figure B1.1: Distribution of EITC Exposure from Birth to Age 18

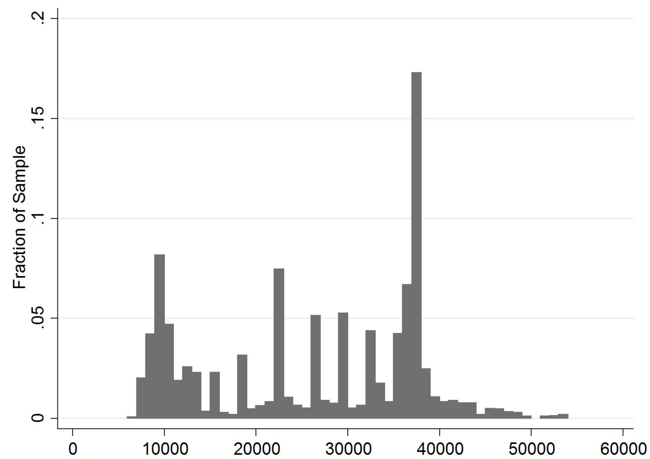


Notes: Authors' calculations from 1968 to 2013 Panel Study of Income Dynamics. EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. Histogram reflects 1454 unique values. Main sample of 3495 observations used. PSID data covers only odd years after 1997, for even years between 1998 and 2012 EITC exposure is imputed as average value of adjacent years.

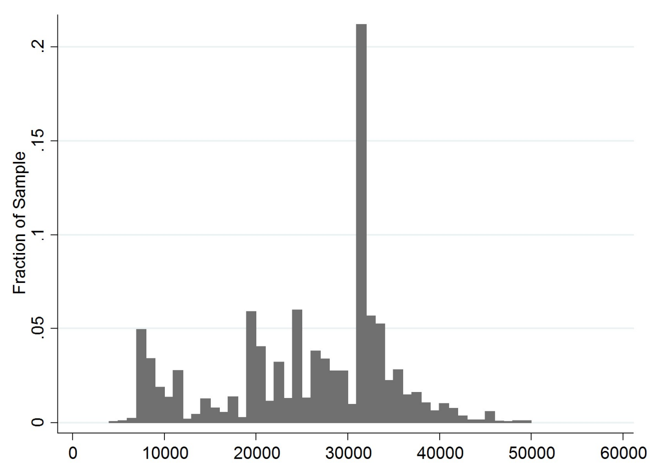
Figure B1.2: Cumulative EITC Exposure During Various Age Ranges (in 2013 Dollars)



Panel A: EITC Exposure from Age 0 to 5



Panel B: EITC Exposure from Age 6 to 12



Panel C: EITC Exposure from Age 13 to 18

Source: Authors' calculations. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children.

Table B1.1: State EITC Details

	Year of Implementation	EITC (as percent of federal) in 2013
Rhode Island	1986	25%
Vermont	1988	32%
Wisconsin[1]	1989	34%
Iowa	1990	14%
Minnesota[2]	1991	33%
New York	1994	30%
Massachusetts	1997	15%
Oregon	1997	6%
Kansas	1998	17%
Maryland	1998	25%
Colorado	1999	0%
DC	2000	40%
Illinois	2000	5%
Maine	2000	5%
New Jersey	2000	20%
Oklahoma	2002	5%
Indiana	2003	6%
Nebraska	2003	10%
Delaware	2006	20%
Virginia	2006	20%
New Mexico	2007	10%
North Carolina	2008	4.5%
Michigan	2008	6%
Louisiana	2008	3.5%
Connecticut	2011	25%
Ohio	2014	0%

Source: Tax Policy Center http://www.taxpolicycenter.org/taxfacts/displayafact.cfm?Docid=293. Footnote 1: Wisconsin has a system based on the number of children in the household. Rate shown here is for households with 3 or more children. Footnote 2: Minnesota has a system based on whether there are any children living in the household, and after 1997, household earnings. Rate shown here is average.

Table B1.2: Testing Exogeneity of State EITC Benefits

Dependent Variable:	Max Federal + State EITC Benefits	Max Federal + State EITC Benefits x State Ever Had an EITC
Mean Dependent Variable (in 1000s of 2013 $):	5.36	2.77
VARIABLES	(1)	(2)
State GDP per Capita	0.313	1.057
(in 1000s of 2013 $)	(0.312)	(1.042)
Lagged State GDP per Capita	0.401	0.548
(in 1000s of 2013 $)	(0.347)	(1.118)
Unemployment Rate	-0.010	-0.077
	(0.024)	(0.052)
Lagged Unemployment Rate	-0.038	0.029
	(0.024)	(0.063)
Top Marginal Income Tax Rate	0.043	0.089
	(0.031)	(0.081)
Lagged Top Marginal Income Tax Rate	0.020	0.090
	(0.019)	(0.096)
Real Minimum Wage	-0.023	-0.004
	(0.028)	(0.061)
Lagged Real Minimum Wage	-0.013	-0.000
	(0.022)	(0.048)
Max Monthly Welfare Benefits, Family of 3	0.024	-0.037
(in 100s of 2013 $)	(0.035)	(0.118)
Lagged Max Monthly Welfare Benefits, Family of 3	-0.038	0.005
(in 100s of 2013 $)	(0.039)	(0.089)
Higher Education Spending	-0.062*	-0.143*
	(0.035)	(0.083)
Lagged Higher Education Spending	-0.000	0.000
	(0.001)	(0.002)
State Tax Revenue	-0.007	0.004
(in 1,000,000s of 2013 $)	(0.005)	(0.014)
Lagged State Tax Revenue	0.001	0.001
(in 1,000,000s of 2013 $)	(0.002)	(0.008)
State and Year Fixed Effects	X	X
P-Value from Joint F-Test (Excluding FE)	0.316	0.357
Observations	1,071	1,071
R-squared	0.963	0.956

Source: State-level data from 1992-2013. Unemployment rates from BLS. GDP from BEA regional data. Tax data from the NBER. Minimum wage from the Tax Policy Center's Tax Facts. Spending on higher education from the State Higher Education Executive Officers. Welfare benefits from the Urban Institute's Welfare Rules Database. Tax revenue from the Annual Survey of State Government Tax Collections. Robust standard errors clustered at the state level in parentheses. All dollars are 2013 dollars. *** p<.01 ** p<.05 * p<.10.

Table B1.3: EITC Exposure at Age 18 on Education, Employment, and Earnings (Reduced Form)

Dependent Variable:	High School Graduate	At Least Some College	College Graduate	Highest Grade Completed	Employed	Annual Earnings (2013 $)
Mean dependent variable:	0.92	0.52	0.31	13.7	0.817	25,391
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
EITC Exposure at Age 18	0.035***	0.031**	0.044***	0.235***	0.023**	643.0
(in Real $1,000s)	(0.007)	(0.014)	(0.011)	(0.055)	(0.010)	(546.3)
Observations	3,495	3,309	2,506	2,506	1,758	1,758
R-squared	0.334	0.422	0.376	0.471	0.309	0.420

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. Regressions include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Estimates comes from an estimating equation similar to (1) except that EITC exposure at age 18 is used instead of EITC exposure at 0-5, 6-12, and 13-18. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table B1.4: Different Sets of Controls Show Positive Effect of EITC on High-School Graduation

Dependent Variable:	High School Graduate (Mean=0.92)					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
EITC Exposure from Age 0 to 5	-0.006	-0.003	-0.002	-0.002	-0.005	-0.009
(in Real $1,000s)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)
EITC Exposure from Age 6 to 12	0.001	-0.003	-0.003	-0.003	-0.003	0.001
(in Real $1,000s)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
EITC Exposure from Age 13 to 18	0.002	0.012***	0.011***	0.011***	0.012***	0.011**
(in Real $1,000s)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Controls						
State, Cohort, Year Fixed Effects	X	X	X	X	X	X
Demographic Controls		X	X	X	X	X
State-Year Controls			X	X	X	X
Interaction Controls				X	X	X
State-Specific Time Trends Quadratic					X	X
Family Fixed Effects						X
P-value: F-Test Identical Estimates	0.240	0.003	0.004	0.003	0.006	0.183
Observations	3,495	3,495	3,495	3,495	3,495	3,495
R-squared	0.151	0.229	0.237	0.291	0.331	0.676

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. Results reflect estimating equation (1). Full set of controls in column (5). Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table B1.5: Results Robust to Controlling for Additional Fixed Effects

Dependent Variable:	High School Graduate (Mean=0.92)				
Variables	(1)	(2)	(3)	(4)	(5)
EITC Exposure from Age 0 to 5	-0.002	-0.006	-0.007*	-0.002	-0.006
(in Real $1,000s)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)
EITC Exposure from Age 6 to 12	-0.003	-0.003	-0.002	-0.004	-0.003
(in Real $1,000s)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
EITC Exposure from Age 13 to 18	0.011***	0.013***	0.011***	0.015***	0.011***
(in Real $1,000s)	(0.004)	(0.003)	(0.003)	(0.005)	(0.003)
Controls					
Full controls from Table A4 Column 5 (Main Specification)	X	X	X	X	X
State x Year FE		X		X	
State x Siblings at 18 FE			X	X	
Year x Siblings at 18 FE			X	X	
State x Married Parents at 18 FE					X
Year x Married Parents at 18 FE					X
P-value: F-Test Identical Estimates	0.006	0.018	0.006	0.010	0.004
Observations	3,495	3,495	3,495	3,495	3,495
R-squared	0.402	0.369	0.412	0.468	0.373

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Table B1.6: Results Robust to Different Specification of Weights

Weights Dependent Variable:			High School Graduate		
Mean Dependent Variable:	0.924	0.924	0.931	0.928	0.928
Weight Used:	None	PSID Avg	PSID Sum	1990 State	2000 State
Variables	(1)	(2)	(3)	(4)	(5)
EITC Exposure from Age 0 to 5	-0.004	-0.005	-0.005	-0.006*	-0.006*
(in Real $1,000s)	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)
EITC Exposure from Age 6 to 12	-0.001	-0.003	-0.003	-0.003	-0.003
(in Real $1,000s)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
EITC Exposure from Age 13 to 18	0.007**	0.012***	0.011***	0.009***	0.009***
(in Real $1,000s)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
P-value: F-Test Identical Estimates	0.033	0.006	0.008	0.001	0.001
Observations	3,495	3,495	3,495	3,495	3,495
R-squared	0.225	0.331	0.318	0.222	0.223

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. All regressions reflect estimating equation (1) and include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B1.7: High School Graduation Results Robust to Instrumental-Variables Analysis

VARIABLES	(1)	(2)	(3)	(4)
Family Income from Age 0 to 5	0.0006	0.0007	0.0007	-0.0001
(in Real $1,000s)	(0.0012)	(0.0012)	(0.0012)	(0.0013)
Family Income from Age 6 to 12	-0.0018**	-0.0018**	-0.0018**	-0.0017*
(in Real $1,000s)	(0.0009)	(0.0008)	(0.0009)	(0.0010)
Family Income from Age 13 to 18	0.0019**	0.0018**	0.0018**	0.0021*
(in Real $1,000s)	(0.0007)	(0.0007)	(0.0008)	(0.0011)
Controls				
State, Cohort, Year Fixed Effects	X	X	X	X
Demographic Controls	X	X	X	X
State-Year Controls		X	X	X
Interaction Controls			X	X
State-Specific Quadratic Time Trends				X
P-value: F-Test Identical Estimates	0.048	0.041	0.046	0.128
Observations	3,495	3,495	3,495	3,495

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. All regressions reflect estimating equation (3). Full set of controls in column (4). EITC exposure and family income are discounted at a 3 percent annual rate from age 18 (Chetty, Friedman, and Rockoff 2011). Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table B1.8: Effect of Family Income at Age 18 on High-School Graduation (OLS and IV Results)

	OLS		IV				
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Family Income at Age 18	0.0005***	0.0002**	0.0047*	0.0044***	0.0046***	0.0047***	0.0046***
(in Real $1,000s)	(0.0001)	(0.0001)	(0.0026)	(0.0014)	(0.0014)	(0.0013)	(0.0013)
Controls							
State, Cohort, Year Fixed Effects		X	X	X	X	X	X
Demographic Controls		X		X	X	X	X
State-Year Controls		X			X	X	X
Interaction Controls		X				X	X
State-Specific Quadratic Time Trends		X					X
Observations	3,495	3,495	3,495	3,495	3,495	3,495	3,495
R-squared	0.020	0.321	--	--	--	--	--
First-Stage Estimates							
EITC Exposure at Age 18	--	--	2.8	7.4	7.2	7.0	7.6
(in real $1,000s)			(1.2)	(1.7)	(1.8)	(1.6)	(1.6)
Kleibergen-Paap rk LM Stat.	--	--	4.5	11.6	11.2	12.4	12.5
Kleibergen-Paap rk Wald F Stat.	--	--	5.7	18.0	17.0	20.4	22.1

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. All regressions reflect estimating equation (2) and (3) except that EITC exposure at age 18 is used instead of EITC exposure at 0-5, 6-12, and 13-18. Full set of controls in column (7). Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B2: Additional Robustness Checks

In the following, we discuss a number of robustness checks conducted to illustrate that our results are not sensitive to concerns of sample attrition, the linear specification of the treatment variable, and variation in the effect of the EITC on education outcomes over time.

B2.1 Sample Attrition

One possible concern with the main analysis is differential non-response among PSID respondents. Attrition from the PSID over time is unlikely to be random. In our sample, about 11 percent of individuals are not observed in the last PSID wave of 2013. To account for potentially endogenous attrition, a logistic regression and a parsimonious set of covariates are used to predict the probability that an individual leaves the sample. Figure B2.1 shows sufficient overlap between the kernel-density plots of attriters and non-attriters. Using the predicted probability of attrition, DiNardo-Fortin-Lemieux weights (following the approach in DiNardo, Fortin, and Lemieux 1996) and inverse-propensity-score weights are constructed. Table B2.1 uses these two sets of weights and verifies that estimates are very similar to those in Table 2.2. The effects of the EITC on education and employment outcomes are robust to reweighting and potentially endogenous sample attrition.

B2.2 Non-Linear Specifications

A linear specification restricts the impact of a $1,000 increase in the EITC to be constant over the range of EITC exposure. It is possible that the effect of $2,000 and $5,000 in EITC eligibility are both positive, but indistinguishable. Another possibility is that there is a decreasing marginal treatment effect with each subsequent EITC expansion. A more flexible approach is to divide up

EITC exposure into a number of binary variables indicating EITC exposure within various ranges. Figure B2.2 shows estimates from a regression resembling equation (1) – where EITC exposure at age 18 is used instead of EITC exposure at 0-5, 6-12, and 13-18 for simplicity in presenting coefficients – but with five binary variables instead of the continuous variable EITC exposure at 18. The excluded group represents individuals with the lowest EITC exposure (below $1,500 in 2013 dollars) and the other five bins cover values between $1,500 and $9,000. Figure B2.2 shows that the effect of EITC exposure on education steadily increases and roughly approximates the predicted effect from a linear specification. This semi-parametric approach shows that the linear specification closely approximates the effect of the EITC on intergenerational outcomes and may suggest constant marginal treatment effects with each subsequent EITC expansion over the last four decades.

B2.3 Treatment Effect Size Over Time

A related but separate question is whether the effect of $1,000 of EITC exposure has changed over time. Figure B2.3 presents results from a regression that replaces $EITC_i^{13-18}$ in equation (1) with three continuous variables that interacts $EITC_i^{13-18}$ with binary for being born between 1967 and 1982, 1983 and 1989, and 1990 and 1995. This restricts the effect of EITC exposure at age 0 to 5 and age 6 to 12 to be the same over time, as shown in equation (B1).[1]

$$Y_i = \beta_1 EITC_i^{0-5} + \beta_2 EITC_i^{6-12} + \sum_{c=[1,3]} \beta_{3c} EITC_{i,c}^{13-18} + \eta X_i + \psi V_{st} + \lambda Z_s + \pi W_t + v_i \text{ (B1)}$$

These three birth cohorts contain 997, 1281, and 1217 observations (adding up to the 3495 people in the main sample). Figure B2.3 shows that the effect of the EITC on teenagers has a positive, statistically identical effect across birth cohorts. The fact that $1,000 of EITC exposure has a similar effect on educational attainment over time may suggest constant marginal treatment effects as federal and state EITCs have expanded over the last four decades.

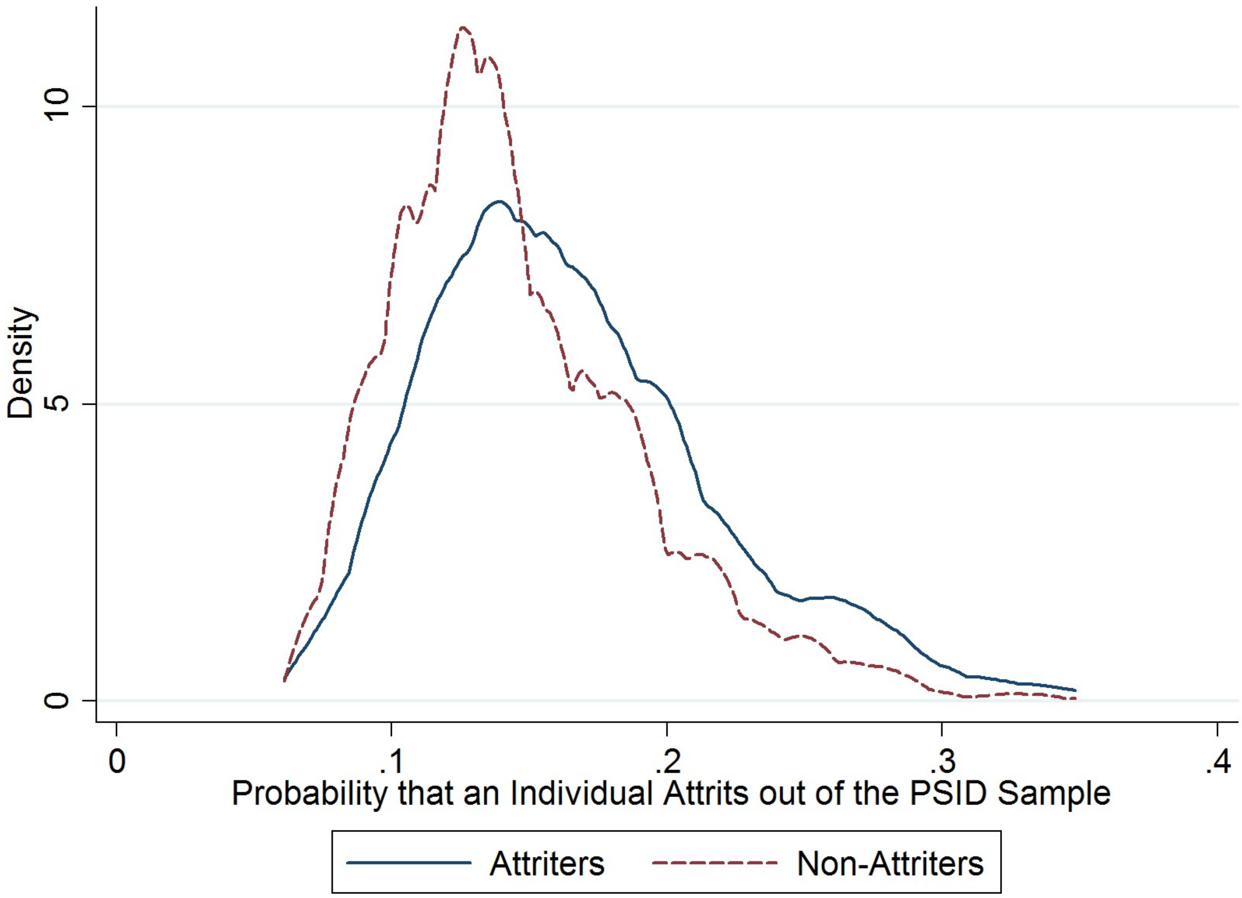
[1] Results are similar, but less precise, if EITC exposure at ages 0-5 and 6-12 are also allowed to vary by cohort.

Table B2.1: Accounting for PSID Attrition by Reweighting

Dependent Variable:	High School		At Least Some		College Graduate		Highest Grade		Employed		Annual Earnings	
Mean Dependent Variable:	0.92		0.52		0.31		13.7		0.817		25,391	
Weight Used:	DFL	IPW	DFL	IPW	DFL	IPW	DFL	IPW	DFL	IPW	DFL	IPW
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EITC Exposure from Age 0 to 5	-0.006	-0.003	0.001	-0.002	-0.006	-0.007	-0.020	-0.025	0.031	0.014	762.8	517.8
(in Real $1,000s)	(0.005)	(0.005)	(0.006)	(0.006)	(0.019)	(0.018)	(0.073)	(0.070)	(0.025)	(0.018)	(847.7)	(792.0)
EITC Exposure from Age 6 to 12	-0.003	-0.003	0.001	0.003	0.007	0.010*	0.001	0.016	-0.004	-0.000	66.9	34.8
(in Real $1,000s)	(0.003)	(0.002)	(0.005)	(0.005)	(0.006)	(0.006)	(0.023)	(0.022)	(0.008)	(0.007)	(397.5)	(426.2)
EITC Exposure from Age 13 to 18	0.012***	0.011***	0.006	0.005	0.012**	0.014***	0.079***	0.080***	0.008*	0.008**	526.4*	594.0**
(in Real $1,000s)	(0.003)	(0.003)	(0.007)	(0.007)	(0.005)	(0.005)	(0.026)	(0.024)	(0.004)	(0.004)	(264.7)	(239.3)
P-value: F-Test Identical	0.005	0.009	0.890	0.713	0.567	0.460	0.100	0.143	0.442	0.668	0.700	0.611
Observations	3,495	3,495	3,309	3,309	2,506	2,506	2,506	2,506	1,758	1,758	1,758	1,758
R-squared	0.354	0.310	0.437	0.407	0.395	0.371	0.500	0.445	0.330	0.301	0.448	0.417

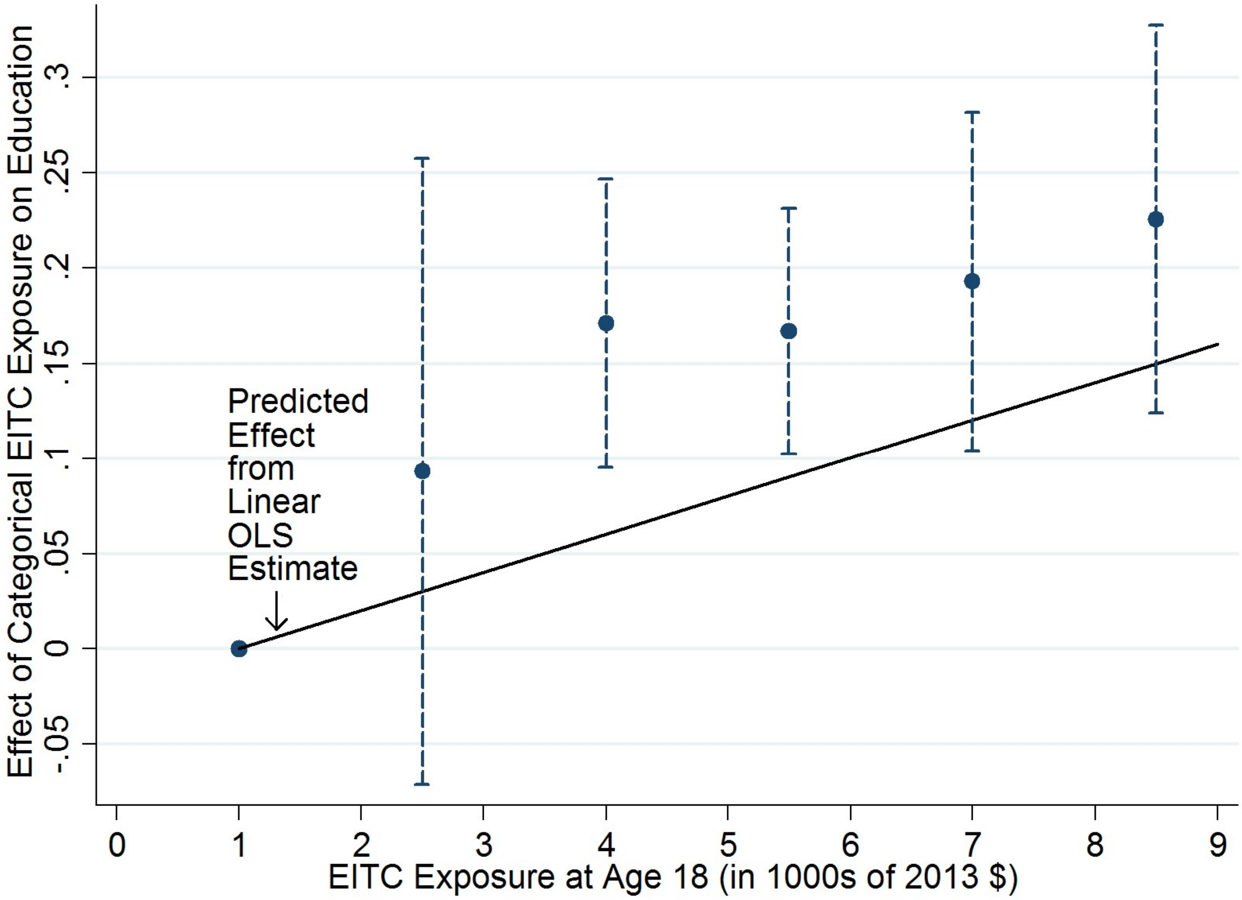
Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20; some college evaluated by age 24; college graduation and highest grade completed evaluated by age 26. Employment and earnings measured between age 22 and 27; average value used if observed more than once during these ages. Results reflect estimating equation (1) and include full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Creation of DFL and IPW weights discussion in Appendix B. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure B2.1: Kernel Density Overlap Plot (PSID Attriters and Non-Attriters)



Source: 1968 to 2013 Panel Study of Income Dynamics. Note: Kernel density overlap plot verifies that there are similar types of individuals (based on a parsimonious set of covariates) that attrit and do not attrit out of the PSID sample. Reweighting accounts for endogenous attrition from the PSID. About 11 percent of individuals in the sample attrit out of the PSID before 2013. The graphs plots the predicted probability of attriting out of the PSID for those that attrit and those that do not. These predicted probabilities are created by using a logitistic regression of binary attrition on black, hispanic, gender, whether mom and dad finished high school or attended at least some college, and three birth year categories (1967-1975, 1976-1985, and 1986-1995). The main sample of 3495 individuals is used.

Figure B2.2: Categorical EITC Exposure Reveals a Relatively Constant Marginal Treatment



Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. Each point represents the estimated effect of having a value of EITC exposure at age 18 (as shown in Table A3) on the probability of high school graduation. A single OLS regression with five indicator variables was used. Full set of controls used: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. The omitted group is those with EITC exposure under $1,500 and the other five bins are created from cutoffs at $2200, $4000, $6000, $7600.

Figure B2.3: The Positive Effect of the EITC on Teenagers Finishing High-School is Similar Over

Source: 1968 to 2013 Panel Study of Income Dynamics. Notes: EITC exposure in thousands of 2013 dollars and defined as the maximum potential federal and state EITC a household could receive given the year, state, and number of children. High school graduation evaluated by age 20. Regressions reflect estimating equation (B1) and include the full set of controls: demographic controls, state-year controls at age 18, state, cohort, and year fixed effects, along with state-specific quadratic time trends. Heteroskedasticity-robust standard errors clustered at the state level are in parentheses. All results are weighted by average childhood PSID weights. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B3: Description of Data and Variables

Sample restrictions:

All individuals born between 1958 and 1995 are included in the analysis. For results presented by age group, individuals must have been present in at least one year for each of the three age periods evaluated: 0-5, 6-12, and 13-18 to be included in the analysis. For results presented using EITC exposure at age 18, individuals need only be present in the household at age 18. However, since the PSID switches to bi-annual interviewing after 1997, individuals not observed at age 18 are assigned their EITC at age 17, or age 16 if not observed at 18 or 17. This affects 1939 and 40 individuals, respectively, while 3894 individuals are observed at age 18.

EITC exposure is measured each year between birth and age 18 as the maximum potential EITC a household could receive given the state, year, and number of children residing in the household in each year. EITC exposure is independent of household earnings or parental marital status.

For years in which the PSID does not survey (even-numbered years after 1997), we impute family income (and EITC exposure) by averaging family income (or EITC exposure) in the two adjacent interview years.

A few individuals (26 in total) leave the household and move out on their own prior to age 18. For these individuals, we observe family income and EITC exposure until the individual leaves the household, and then assign zero family income and EITC exposure for the years after the individual leaves the household (up to age 18). Results are quite similar if these individuals are excluded from the analysis entirely.

All analyses incorporate sample weights provided by the PSID. Weights are constructed by averaging the child sample weights in each year between birth and age 18. Results are not sensitive to this particular specification of the weight. Similar results are obtained when using the PSID weight at age 18, the sum of the PSID weights between 0 and 18, or in the year the outcome was observed. Table A6 shows results for alternate weighting options, including additional weighting based on state populations.

Glossary of terms:

EITC exposure: This is a measure of the maximum potential federal and state EITC benefit the household could receive based on the year, state, and number of children residing in the household. This is not dependent on the family's income or marital status. For measures of EITC exposure at age 18, the maximum EITC is evaluated in the year the respondent is 18 based on the state, year, and number of children in the household. For measures of EITC exposure from age 0 to 5, age 6 to 12, and age 13 to 18, the maximum EITC at each age is calculated and then summed up within each age category. Thus, EITC exposure reflects birth year, cross-state moves, and changes in the number of children residing in the household.

Imputed household EITC: The PSID does not contain information on receipt of EITC benefits. We therefore calculate household EITC benefits based on the family income, the number of children residing in the household, the state and year, and the marital status of the parents of the focal child. Variation in this term is generated by household characteristics (marital status, income, number of children in the household), as well as policy variation over time and state.

High school graduate: Dummy variable evaluated between ages 18 and 20. Set to one if respondent has graduated from high school by age 20, zero otherwise. Individuals observed up to age 17 or less are assigned a missing value for high school graduate and are not included in the sample.

At least some college: Dummy variable evaluated between ages 19 and 24. Individuals are considered to have some college if they have completed at least one year of post-secondary schooling by age 24, zero otherwise. Individuals observed up to age 18 or less are assigned a missing value for at least some college.

College graduate: Dummy variable evaluated between ages 22 and 26. Set to one if the respondent has completed a college degree by age 26, zero otherwise. Individuals observed up to age 21 or less are assigned a missing value for college graduate.

Highest grade completed: Continuous variable evaluated between ages 22 and 26. Represents the highest grade that the respondent has completed by age 26.[1] Individuals observed up to age 21 or less are assigned a missing value for highest grade completed.

Employed: Continuous variable taking on the values between 0 and 1 and evaluated between ages 22 and 27. Represents the fraction of years employed between ages 22 and 27 as the fraction of years worked divided by the number of years observed in the PSID over that age range. This narrow age range allows for measurement of employment at roughly the same age for all cohorts included in the analysis, is comparable with young adult earnings shown in Chetty et al. (2011a), and increases sample size – beyond what a single age would produce – to account for bi-annual PSID waves after 1997. Employment created from PSID variable ER34216 (and related annual variables).

Annual earnings: Continuous variable evaluated between ages 22 and 27. Individuals observed more than once during this period are assigned their average annual earnings value. Put into real 2013 dollars based on the Consumer Price Index. Earnings created from PSID variables ER54309 and ER53935 (and related annual variables). Same sample used for employed and earnings outcomes.

Family has savings account

Binary outcome. Data on savings account available beginning in 1999 (see PSID variable ER54660). Question asks, “Not including employer-based pensions or IRAs, do (you/you or anyone in your family living there) have any money in any of the following: Checking or savings accounts, Money market funds, Certificates of deposit, Government bonds, or Treasury bills?”

[1] Since the PSID switched to bi-annual interviewing in 1997, some respondents will not be interviewed at age 20, 24, or 26. Outcomes in these cases are determined by age 19, 23, or 25.

Family Savings Amount

Data on savings account available beginning in 1999 (see PSID variable ER54661). Question asks, “If you added up all such accounts (for all of your family living there) about how much would they amount to right now?” Observations kept that report whether they have a savings account and report a family savings amount. Results not sensitive to this sample restriction. Observations are at the child-year level and to increase power some children are used more than once: we observe 3775 unique children and 15074 child-year observations. As described in Table 4 notes, results are robust to including an upper bound on savings accounts (to help ensure results are not driven by very wealthy families) or using median regression.

Standardized test scores:

Data on test scores from the 1997, 2002, and 2007 child development survey (CDS). Each test score is a standardized (mean zero, standard deviation one) aggregate score of four tests: applied problem solving (*Q24APSS, Q34APSS, and Q3AP_SS*), reading (*Q24BRSS, Q3BRE_SS, and Q34BRSS*), math (*Q3BMA_SS, MATH02, and MATH07*), and passage comprehension (*Q3PC_SS, Q24PCSS, and Q34PCSS*). Each test is standardized and then averaged within each year. 2002 CDS merged with 2001 PSID. Sample includes children ages 4 to 18 that took at least one of these tests in at least one of these years. More details can be found here: http://psidonline.isr.umich.edu/cds/questionnaires/cds-i/english/child.pdf.

Daily minutes spent with mother

Data on time spent with mother from the 1997, 2002, and 2007 child development survey time diaries. Sample includes children ages 0 to 18. Observations on weekends dropped. PSID variables COLG_B, COLGB_02, and COLHB_07.

Family income + EITC benefits

Family income measured in the PSID as the sum of the head and wife’s earnings. EITC benefits imputed as the amount the household would be eligible for given the number of children living in the household, marital status, state, year, and family income. Note that the PSID does not contain information on actual EITC benefits received, therefore this measure of EITC benefits should be interpreted as eligibility rather than receipt. Imputed EITC benefits are then added to reported family income.

Description of control variables

Most demographic variables are self-explanatory: black, Hispanic, siblings at age 18, whether mom or dad finished high school or some college, female, age cubic, whether parents were ever married, and fixed effects for state, year, cohort, cohort x black, cohort x Hispanic, cohort x female, state x black, state x Hispanic, and state x female.

The following state by year economic and policy variables are used (with their source): state GDP per capita (Bureau of Economic Analysis), unemployment rate (Bureau of Labor Statistics), top marginal income tax rate (National Bureau of Economic Research), minimum wage (Tax Policy Center), maximum welfare benefits for a family with three children (Urban Institute), per capita spending on higher education (State Higher Education Executive Officers), total tax revenue (Annual Survey of State Government Tax Collections), and average college tuition (Delta Cost Project).

Appendix C: Additional Material for Chapter 3

Appendix C1: Additional Figures and Tables

Table C.1: Testing Exogeneity of State EITC Rates

Weight:	None	Sum of PSID Weights in Main Sample	
		State-Year Cells	State Cells
Mean Dependent Variable:	0.053	0.053	0.052
VARIABLES	(1)	(2)	(3)
State GDP per Capita	0.008	0.038	0.035
(in $1000s)	(0.063)	(0.099)	(0.099)
Lagged State GDP per Capita	0.058	0.100	0.107
(in $1000s)	(0.067)	(0.082)	(0.086)
Unemployment Rate	-0.001	-0.007	-0.006
	(0.004)	(0.006)	(0.006)
Lagged Unemployment Rate	-0.008*	-0.009*	-0.009*
	(0.004)	(0.005)	(0.005)
Top Marginal Income Tax Rate	0.005	0.008	0.009
	(0.006)	(0.006)	(0.006)
Lagged Top Marginal Income Tax Rate	0.002	0.001	0.001
	(0.003)	(0.004)	(0.004)
Real Minimum Wage	-0.005	-0.010	-0.009
	(0.005)	(0.007)	(0.007)
Lagged Real Minimum Wage	-0.004	-0.001	-0.003
	(0.004)	(0.004)	(0.004)
Maximum Monthly Welfare Benefits for a	0.009	0.008	0.010
Family of 3 (in $100s)	(0.007)	(0.009)	(0.010)
Maximum Monthly Welfare Benefits for a	-0.010	-0.008	-0.007
Family of 3 (in $100s)	(0.007)	(0.008)	(0.009)
Higher Education Spending	-0.007	-0.014	-0.012
	(0.007)	(0.009)	(0.009)
Lagged Higher Education Spending	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
State Tax Revenue	-0.001	-0.001	-0.002
(in millions of 2013 dollars)	(0.001)	(0.001)	(0.001)
Lagged State Tax Revenue	0.000	0.000	0.000
(in millions of 2013 dollars)	(0.000)	(0.001)	(0.001)
State and Year Fixed Effects	X	X	X
P-Value from Joint F-Test (Excluding FE)	0.352	0.141	0.200
Observations	1,071	1,071	1,071
R-squared	0.834	0.803	0.806

Source: Dependent variable is state EITC rates as a fraction of federal EITC (between 0 and 1). Observations are at the state-year level and span 1992 to 2013. Unemployment rates from BLS. GDP from BEA regional data. Tax data from the NBER. Minimum wage from the Tax Policy Center's Tax Facts. Spending on higher education from the State Higher Education Executive Officers. Welfare benefits from the Urban Institute's Welfare Rules Database. *** p<.01 ** p<.05 * p<.10

Table C.2: Transition Matrix, Fertility and Marrital Status

Panel A: Number of Children

# Kids in Year t	# Kids in Year t+1: 0	1	2	3	4+
0	29,489	3,179	606	195	65
1	1,254	14,509	3,161	234	46
2	706	1,080	18,509	1,737	141
3	278	153	852	8,402	727
4+	117	46	119	471	3,902

Panel B: Marital Status

Marital Status in Year t	Marital Status in Year t+1: Married	Not Married	Widowed	Divorced	Separated
Married	39,304	2,300	84	1,214	1,854
Not Married	2,990	25,039	52	559	613
Widowed	71	85	428	14	18
Divorced	1,483	1,179	23	6,404	388
Separated	908	650	29	1,286	3,003

Source: Author's calculation from 1980 to 2013 PSID using the main sample of 89,978 household heads 21 to 39 year-old.

Table C.3: Summary Statistics (PSID Transition to Adulthood Sample)

Variable	Unweighted Mean	Unweighted Standard Deviation	Using PSID Weights Mean	Using PSID Weights Standard Deviation
Female Head	0.53	0.50	0.51	0.50
Number of Kids	0.73	1.03	0.63	0.99
State EITC Rate	0.06	0.10	0.06	0.11
Years of Education	12.9	3.3	13.2	3.4
White	0.51	0.50	0.74	0.44
Married	0.16	0.37	0.16	0.37
Cohabitating (and Unmarried)	0.14	0.35	0.12	0.32
Birth Year	1988.7	2.9	1988.7	2.8
Age	21.0	2.6	21.0	2.6
Number of Observations	7,114			
Unique Observations	2,567			

Source: 2005 to 2013 PSID. Observations are 17 to 27 year-old individuals in the PSID Transition to Adulthood sample. PSID weights used.