The Effects of the Earned Income Tax Credit on Child Achievement and Long-Term Educational Attainment*

Michelle Maxfield†
Michigan State University
Job Market Paper
November 14, 2013

ABSTRACT: The Earned Income Tax Credit (EITC) is a significant source of government assistance to low income families. Total outlay reached over $50 billion in 2008, with more than 97 percent of aid received by families with children (Internal Revenue Service 2011). Despite its size and pro-child goals, relatively little is known about how the EITC affects children directly. Until recently, studies focused only on indirect measures of child well-being such as poverty, parental labor supply, marriage, fertility, and consumption. This study directly links EITC receipt throughout all ages of childhood to both contemporaneous achievement and long-run educational attainment. I take advantage of both Federal tax code changes and state EITC adoptions, which result in large variation in EITC generosity across state, time, and family size. Using the 1979 National Longitudinal Survey of Youth, I find that EITC expansions improve both contemporaneous and long-run educational outcomes of children. An increase in the maximum EITC of $1,000 (2008 dollars) in a given year significantly increases math achievement by about 0.072 nationally normed standard deviations. This change in EITC generosity during childhood also increases the probability of graduating high school or receiving a GED at age 19 by about 2.1 percentage points and increases the probability of completing one or more years of college by age 19 by about 1.4 percentage points. Estimated effects are larger for boys and minority children, and I find evidence that an expansion in the EITC is more effective at improving educational outcomes for children who are younger during the expansion. An increase in the maximum EITC of $1,000 also results in other changes in the household, including an increase in net family income inclusive of EITC and welfare payments of about $888 and an increase in maternal labor force participation.

* The author is grateful to Stacy Dickert-Conlin, Leslie Papke, Jeffrey Wooldridge, Steven Haider, and the participants of the 2013 Association for Public Policy Analysis and Management Conference for their guidance and comments. The research presented here is partially supported by the Institute of Education Sciences, U.S. Department of Education, through grant R305B090011 to Michigan State University. The opinions expressed are those of the author and do not represent the views of the Institute or the U.S. Department of Education.
† Department of Economics, Michigan State University, 110 Marshall Adams Hall, East Lansing, MI 48824, maxfiel7@msu.edu

1 Introduction

The Earned Income Tax Credit (EITC) is a significant source of government aid to needy families and has grown dramatically since its inception in 1975. Total outlay reached over $50 billion in 2008, with more than 97 percent of aid received by families with children (Internal Revenue Service 2011). The largest expansion in the EITC came as part of President Bill Clinton's Omnibus Budget Reconciliation Act (OBRA) in 1993. At that time, more than 12 million children, one in every four, were living in poverty, making up about one third of the total population living in poverty in the United States (Mink 1993). The EITC gained support from many child advocates, including the National Commission on Children (1993), as the president's plan ensured that no family with a parent working full-time would live below the poverty line (Stupak 1993). In a congressional session addressing children's initiatives, Congresswoman Karen Shepard (1993) stated: "If you believe that work should be rewarded and that children deserve security, you should support expanding the Earned Income Tax Credit. Plain and simple."

Despite the size and pro-child goals of the EITC, relatively little is currently known about how the credit affects children directly. Until recently, studies focused only on indirect measures of child well-being such as poverty, parental labor supply, marriage, fertility, and consumption (see Hotz and Scholz 2003 and Eissa and Hoynes 2006 for reviews of the literature). Without knowing the direct impacts of the EITC on child outcomes such as physical and mental health, cognition, and long-run economic sufficiency, it is difficult to accurately assess the performance of the program. This paper looks to address the effects of the EITC on both contemporaneous and long-term educational outcomes.

Only three studies I am aware of directly examine the effects of the EITC on child cognitive outcomes. Dahl and Lochner (2012) use the EITC as an exogenous source of income variation to determine the effects of family income on child achievement in math and reading. Chetty et al. (2011a) first analyze the effects of the EITC and the Child Tax Credit on math and reading scores. They then use the finding that the credits improve test scores to consider possible long-term effects by examining how test score gains from being assigned a more effective teacher affect outcomes such as college attendance and earnings. Michelmore (2013) examines state EITCs as an unexplored source of financial aid to determine effects of income on educational attainment.

Using the National Longitudinal Survey of Youth (NLSY), which follows mothers and their children over time, I am able to directly estimate the impact of exposure to a more generous EITC during childhood on both contemporaneous achievement and long-run educational attainment for children of all ages. These data also allow me to estimate changes to family resources following EITC expansion, giving some insight into the mechanisms behind the effects on child outcomes. I take advantage of both Federal tax code changes and state EITC adoptions, which result in large variation in EITC generosity across state, time, and family size.

I find that the EITC is an effective policy for improving both contemporaneous and long-run educational outcomes of children. I estimate that OBRA 1993, which increased the Federal maximum EITC payment by about $3,000 (2008 dollars), had large, significant effects on children. For an elementary-aged child in a family with 2 or more children, OBRA 1993 increased math achievement by about 0.215 nationally normed standard deviations, increased probability of graduating high school or receiving a GED at age 19 by about 7.2

percentage points, and increased probability of completing one or more years of college by age 19 by about 4.8 percentage points. Along with changes to educational outcomes, OBRA 1993 resulted in other changes in the household, including an increase in net family income inclusive of EITC and welfare payments of about $2,664 and an increase in maternal labor force participation.

In the following section, I review the institutional details of the EITC. Section 3 outlines how the EITC might affect children and reviews the previous literature on this topic. Section 4 describes the NLSY data and presents summary statistics for my sample. Section 5 details my empirical strategy, and Section 6 presents the results. I summarize the findings and conclude in Section 7.

2 Institutional Details of the EITC

The EITC began in 1975 with modest credits for low income families with children as a way to offset payroll taxes. Since then, the Federal government expanded the EITC multiple times in an effort to create an anti-welfare, anti-poverty, and pro-work tool (Ventry 2000). The credit is refundable and only available to families who work. It is based on a family's earned income, number of children, and state of residence. Table 1 shows the Federal EITC parameters for the years I examine, 1987 to 2000. As the table illustrates, there is an initial "phase-in" range and rate, where the credit is equal to the subsidy rate times the family's earned income until the maximum credit is reached. The family then receives the maximum credit during the "flat" range. Once a family reaches a certain level of income, they enter a "phase-out" range, where the credit is reduced at the phase-out rate. Thus, only families below a certain level of income are eligible for the credit in each

year. Families are given the option to receive the credit with periodic payments throughout the year as opposed to a one-time lump sum. However, less than five percent of families exercised this option during the time frame I study (Friedman 2000). Thus, the vast majority of families receive their EITC credit as a lump sum upon filing their tax returns, with over 80 percent of families receiving the credit by the end of March (LaLumia 2013).

In addition to Federal funding of the credit, many states have their own credits that typically "piggyback" onto the Federal credits – meaning some states will increase the Federal EITC credit by a given percentage. The states vary substantially on the generosity of their add-ons, whether they offer it to families without children, and whether the credit is refundable. Table 2 contains the state EITC parameters from 1987 to 2000, the time period covered by the data in this paper. As seen in the table, the state add-ons range from 4 to 75 percent in this time frame, and, by 2000, fifteen states enacted their own EITCs.

Figure 1 plots the real (2008 dollars) value of the maximum Federal EITC credit by tax year and number of children from 1987 to 2000. Two main law changes, the 1990 and 1993 enactments of the OBRA, resulted in real expansions in the Federal maximum credit. OBRA 1993 changes were quite substantial and also increased the Federal maximum EITC differentially by number of children. For example, a family with two or more children and real earnings of $12,000 in 1993 and 1996 would receive the maximum Federal EITC payment in both years of $2,251 and $4,880, respectively. Thus, the EITC increases income for this family by about 19 percent before OBRA 1993 and by about 41 percent after the law change is fully phased in. Figure 2 plots the real maximum Federal EITC credit for a family with three children and the combined Federal and state EITC maximum values for New York and Wisconsin from 1987 to 2000. The figure illustrates that the state EITC add-

ons can be quite large as well. For example, the maximum credit in Wisconsin increased from about $1,600 to nearly $7,000 over this time period, while the Federal credit increased from about $1,600 to about $5,000.

3 The EITC and Child Outcomes

3.1 How the EITC Affects Children

The EITC changes the home environments of children in two main ways – changes in the labor supply decisions of their mothers and changes in family income. The structure of the credit provides incentives for altering child bearing and marriage decisions as well, but previous studies have found no effect of the EITC on these outcomes (Eissa and Hoynes 2000; Ellwood 2000; Dickert-Conlin and Houser 2002; Hotz and Sholz 2003; and Baughman and Dickert-Conlin 2003 and 2009). Thus, I focus this discussion on the effects of maternal labor supply and family income.

The structure of the EITC creates different labor supply incentives depending upon the taxable income of the family. Assuming leisure is a normal good and the mother is the sole earner in the family, an EITC expansion creates an unambiguously positive incentive to enter the labor force, as it increases the potential wage of those not participating in the labor force.[1] For those mothers already participating, the incentive depends upon her income and the EITC parameters in a given year. If the mother is working and her income falls in the "phase-in" range of the EITC, there is a substitution effect away from leisure since the EITC-induced wage increase makes leisure more expensive, and there is an

[1] Technically you must also assume that the substitution effect dominates the income effect for a nonzero number of women. If the income effect dominates, the response is to stay out of the labor force.

income effect to consume more leisure. Thus, the overall effect on hours worked is ambiguous. By similar reasoning, women in the "flat" or "phase-out" range have an unambiguous incentive to work less. Women with family income above the cutoff to be eligible for the EITC (end of the phase-out range) may also have an incentive to work less depending on their preferences and how close they are to the end of the phase-out range. As the EITC is based on family income, mothers filing jointly with a wage-earning husband will be more likely to fall in the flat or phase-out range of the EITC schedule, so these women are likely to be induced to decrease their hours worked, or possibly even leave the labor force altogether (see Hotz and Scholz 2003 for a more detailed theoretical discussion of labor supply responses to the EITC).

Previous work confirms these labor supply predictions. First, EITC expansions substantially increase the labor force participation (LFP) of single mothers (Dickert et al. 1995; Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2000 and 2001; Nuemark and Wascher 2001; Grogger 2003; Hotz et al. 2006; Rothstein 2007; and Adireksombat 2010). If anything, the credit *modestly* decreases the LFP of married mothers (Dickert et al. 1995; Ellwood 2000; and Eissa and Hoynes 2004).

Evidence on the effects on hours worked for those women already in the labor force is mixed, with some studies finding no effect (Eissa and Liebman 1996; Liebman 1998; Meyer and Rosenbaum 1999; and Rothstein 2007) and others finding a slight decrease in hours worked following an EITC expansion (Dickert et al. 1995; Nuemark and Wascher 2001; and Saez 2010). These mixed results likely stem from evidence that EITC recipients are not well informed of the kinked structure of the EITC (Olson and Davis 1994; Smeeding et al. 2000; Ross-Phillips 2001; Romich and Weisner 2000; Maag 2005; and Chetty and Saez

2013). Supporting this, Chetty and Saez (2013) and Chetty et al. (2013) find that there is more "bunching" of incomes at kink points in the EITC when recipients live in neighborhoods with higher levels of knowledge about the EITC.

It is not clear ex-ante how maternal labor supply itself affects children, but two main hypotheses arise in the literature. The first is that maternal LFP could be harmful, as the mother spends less time with the child. This is likely most important at very young ages of a child's life. You could also posit that less time spent with children could be beneficial, depending on the quality of the alternative care, such as other family members or daycare centers. The second hypothesis is that a working mother might provide a better example for children, changing future career expectations or aspirations, especially for girls (Goldberger at al. 2008 and Brooks-Gunn et al. 2011).

There is an expansive literature examining the relationship between maternal labor supply and child behavioral and cognitive outcomes. However, the endogeneity of maternal labor supply offers challenges to establishing causal relationships between working and child outcomes. Mothers who work have very different (more favorable) observable characteristics than those who do not work. Thus, it is likely that there is something unobservable about these mothers, like ability, intelligence, and motivation, which influence their decision to participate in the labor force (Hill et al. 2005).

Much of the literature suggests that maternal labor supply may be harmful during early childhood, increasing behavioral issues and decreasing achievement (see Goldberger at al. 2008 and Brooks-Gunn et al. 2011 for a current review of this literature). However, the literature suggests that maternal LFP may be beneficial to child cognition beyond the first few years of a child's life. Using the NLSY, James-Burdumy (2005) uses family fixed

effects and instruments for maternal labor supply using the percent of the county labor force employed in services. She finds that maternal employment in the first year of a child's life has very small negative effects on math and reading scores and that weeks worked by the mother in the third year of a child's life positively affect math scores.

As with maternal labor supply, the literature on the effects of parental income on child development is also plagued by similar endogeneity issues. As a result, most studies are correlational in nature with mixed results. Using longitudinal data from Norway, Løken et al. (2012) address the endogeneity of family income using sibling fixed effects as well as by instrumenting for income using a dummy for whether a family lived in a county that experienced an unexpected economic boom following an oil discovery. Using a quadratic specification of family income and the instrumental variables approach, they find a sizable effect of income on education attainment, high school dropout rates, and adult IQ later in life with both approaches. For a family with about $8,500 (1999 U.S. dollars) in average family income during ages one through 11, an increase in average family income of about $1,600 increases years of education by about 0.1 and decreases the probability of being a high school drop out by about 0.07.

It is possible that the income increases induced by the EITC are different from a general increase in income, as EITC payments are typically received as a lump sum once a year. Romich and Weisner (2000) provide qualitative evidence that EITC recipients in Wisconsin spend EITC funds differently than typical work income. Recipients spend the credit on housing, cars or car-related expenses, childcare, children's clothing and educational items, or paying off bills. Smeeding et al. (2000) confirms this hypothesis using data from Chicago, finding that the large majority of recipients make purchases with their

EITC payment that they would not be able to if they did not receive the EITC. They find that about 80 percent of recipients expected to pay a bill or make a commodity purchase, 50 percent expected to save at least some of their refund, 16 percent planned to pay tuition, and 22 percent planned to use some funds for a car-related expenditure. The authors argue that such expenditures may improve the social mobility of these families, which could improve child development.

For a large portion of EITC recipients, earnings that lead to increased EITC receipt result in lower cash welfare and food assistance benefits, where incentives vary by state. Figure 3, a partial reprint from Meyer and Rosenbaum (2000), depicts the 1996 annual total Aid to Families with Dependent Children (AFDC) welfare cash benefits plus annual food stamps benefits for Alabama, Mississippi, and Pennsylvania. I added the Federal EITC schedule (none of these states had their own EITC this year). For example, in the phase-in region of the EITC schedule, an increase in earned income leads to an increase in EITC payments but a decrease in combined AFDC and food stamps benefits. In the absence of the EITC, the implicit tax rate on earnings is near 100 percent for the majority of AFDC and food stamps recipients (Blank 2002). The EITC helps offset this tax rate. Although income loss from welfare receipt decline would most likely harm child development, prior research suggests that welfare receipt itself might negatively impact children due to the social stigma related to receipt (Levine & Zimmerman 2005).

The above discussion illustrates that the effect of the EITC on child development is an open empirical question, as the effects of changes to maternal employment and income caused by changes in EITC generosity could be contradictory. As mentioned above, relatively little work exists on the direct effects of the EITC on child outcomes, with existing

studies focusing mainly on child health. I review the existing literature below as well as some findings from related government programs.

3.2 Evidence on the Effects of the EITC and Related Programs on Child Outcomes

Taking advantage of the large differential expansions in the EITC with respect to the number of children from OBRA 1993, Hoynes et al. (2012) and Baker (2008) employ difference-in-difference (DiD) techniques to estimate the effect of the EITC expansion on infant health. They both find that being exposed to a more generous EITC schedule during pregnancy reduces the likelihood of low birth weight. Strully et al. (2010) find that living in a state with an EITC supplement also increases birth weight. Baughman and Duchovny (2012) find that an increase in the maximum state EITC raises the probability that children ages 6 to 11 are in better health, but find no effects on the health outcomes of younger children.

Using the NLSY, Dahl and Lochner (2012) estimate the effects of current family income on achievement for children age 5 to 15. They instrument for changes in income using predicted changes in income based on lagged pre-tax income and changes to the Federal EITC schedule (with a flexible control function for lagged pre-tax income included as well). They find that a $1,000 (2000 dollars) increase in income leads to an increase in combined math and reading achievement of about 0.061 SD, with largest effects for reading comprehension (0.036 SD for reading recognition, 0.061 SD for reading comprehension, and 0.058 SD for math). They find larger effects for single mothers and minority children. They also find larger effects for children under age 12 compared to older children (0.077

SD and 0.052 SD, respectively) and much larger effects for boys compared to girls (0.088 SD and 0.040 SD respectively).

Chetty et al. (2011a) use the Internal Revenue Service income tax data and administrative data from a large anonymous school district to estimate the long-term effects of the EITC and Child Tax Credit (CTC). They use non-linearity in the schedule of the two tax credits to identify contemporaneous effects of tax credits on child test scores in grades three through eight (grades that are tested for accountability purposes), but their identification comes mainly from changes in the EITC. The tax data are only available beginning in 1996, so they are unable to utilize the largest changes in EITC generosity to date resulting from OBRA 1993. Also, data constraints do not allow them to directly link changes in the EITC to long-term outcomes. They proceed in two steps – first estimating the effect of tax credits on contemporaneous child test scores and then estimating the effect of test score gains on long-run outcomes using teacher assignment as exogenous variation in test scores.

They find that a $1,000 (2010 dollars) increase in tax credits in a single year raises combined math and reading achievement by about 0.080 SD, with greater effects for math than reading (0.093 SD compared to 0.062 SD). Estimated effects are larger in middle school (0.085 SD) than in elementary (0.073 SD). Since they cannot estimate the long-term effects of tax credits directly, they use the finding that tax credits improve test scores to consider possible long-term effects by examining how test score gains from being assigned a more effective teacher affect outcomes. They find that a one SD increase in test scores in a single grade raises the probability of college attendance at age 20 by about 5 percentage points (sample mean of 37 percent), improves the quality of college attended, and raises

earnings at age 28 by about 9 percent. They also find that higher test scores are associated with reductions in the probability of teen pregnancy and an increase in 401(k) savings. However, as the authors point out, to make any causal inferences on the effects of tax credits on long-run outcomes you must assume that the effects of higher scores resulting from being assigned a better teacher are the same as those that would result from receiving a higher tax credit. There are many reasons these could differ including teacher cheating or teaching students only material that will be tested (i.e. "teaching to the test").

In concurrent work, Michelmore (2013) looks at the effects of state EITCs on educational attainment of children whose parents have a high school education or less (likely eligible for the EITC). She uses a DiD approach comparing 18 to 23 year olds in states with an EITC to those without an EITC, and a triple-difference specification using children from more educated households as the control group to account for state-level trends in educational outcomes. Using the Survey of Income and Program Participation, Michelmore finds that a $1,000 (2011 dollars) increase in the combined state and Federal EITC maximum increases years of schooling by 0.11, increases the probability of completing high school by 2.0 percentage points, and increases the probability of ever enrolling in college by 2.5 percentage points (sample means are 11.97, 70 percent, and 41 percent, respectively). Using the triple-difference approach, she finds the same change in the EITC maximum increases the probability of college enrollment by 0.7 percentage points and increases the likelihood of completing a bachelor's degree by 0.3 percentage points (sample means are 26 percent and 3 percent, respectively). Estimated effects are larger for girls and black children. She finds much larger effects for children who were less than 12 at

the time of the state EITC implementation, with no effects for children who were college aged.

Milligan and Stabile (2011) examine the effects of the expansion of child tax benefits on child development in Canada using the National Longitudinal Study of Children and Youth and the Survey of Labour and Income Dynamics (SLID). They study two main policies – the Canada Child Tax Benefit and the National Child Benefit program. These programs provide cash assistance based on the number of children and are phased out after a certain level of income. These programs differ significantly from the EITC in two ways. They do not require the parents to work to receive the benefits, and the programs cover a much larger proportion of the population (85 percent of the sample from the SLID receive the Federal benefits). Using a simulated benefits instrumental variables approach, which exploits variation across time, province, and family size, the authors find that increased benefit levels increase achievement. A $1,000 (2004 dollars) increase in benefits increased math scores by 0.069 SD for children ages 6-10 and increase vocabulary test scores by 0.149 SD (though not statistically significant) for children ages 4-6, with much larger effects for boys on both measures. They also find the tax benefits decrease child aggression and hunger, and reduce maternal depression.

The Welfare-to-Work (WTW) experiments in the 1990s were designed to increase employment and reduce welfare receipt with two main types of programs. The first encouraged work by providing earnings supplements and the second through mandatory employment services and time limits on welfare receipt. The literature generally suggests that programs designed to increase both employment as well as income through income supplements improve child outcomes, while those without income supplements do not

have much impact. Existing research only finds evidence of improved outcomes for very young or elementary-aged children, with no positive impacts on adolescents (Morris et al. 2001 and Smolensky and Gootman 2003). Pooling achievement reports across 13 WTW programs, Morris et al. (2005) find that assignment to a WTW program with an earnings supplement increases achievement for children ages 2 to 3 by about 0.070 SD, increases achievement for children ages 4 to 5 by 0.100 SD, and actually decreases achievement for children ages 10-11 by 0.112 SD, with no effects for other programs or ages. For reference, these WTW experiments increased total annual income, which includes earnings, earnings supplements, and AFDC and food stamp benefits, by about $1,750 (2001 dollars).

4 Data

I use the restricted geocode data from the NLSY 1979 cohort and the corresponding child file. This data set is a sample of 12,686 young men and women who were age 14 to 20 on December 31, 1978. Individuals are surveyed annually through 1994 and every other year thereafter. Beginning in 1986, children of the mothers in the NLSY are also interviewed every other year. After 1994, children of the NLSY over age 15 are no longer assessed as children and are given a "young adult" survey with questions similar to those asked of the mothers. The NLSY contains extensive information on both the mothers and children, including information on family income and labor market participation and multiple child achievement assessments.

The longitudinal nature of the data allows for direct estimation of long-term effects of EITC expansions on child outcomes that is not possible using a repeated cross section. From the NLSY, I know which state a child lives in as well as family size and income

measures each survey year. This allows me to determine state and Federal EITC parameters as well as changes in welfare generosity throughout childhood. Using the ninth version of the National Bureau of Economic Research's TAXSIM program (Feenberg and Coutts 1993),[2] I am able to estimate a family's tax liability each year, including its state and Federal EITC eligibility and payments. Using these estimates along with welfare receipt reported in the NLSY, I can calculate changes to home resources following EITC expansions, providing some insight into the pathways through which the EITC affects children. Also, the NLSY allows for estimation of models with family fixed effects, as all children in surveyed families are assessed.

I use data on children linked to their mothers for all available years from 1988 through 2000, covering all major Federal expansions of the EITC. The young adult survey provides long-term outcomes for the children that span from 1994 through 2010. Following Dahl and Lochner (2012), I do not include families with mothers who are in the military, in school full-time, or disabled,[3] as these women could have much different labor supply responses to tax changes than other women. To target those families who are actually affected by changes to the EITC, I include those children in the analyses whose family income *ever* fell into the range where they would be eligible to receive the EITC in a given year.[4] I also only include those children who have a sibling in the estimation sample

[2] The program can be accessed at http://nber.org/taxsim.
[3] This restriction eliminates about 3 percent of child-year observations.
[4] Taxable income isn't explicitly given in the NLSY, so I use family earned income (from salary, wages, and tips) to estimate a family's tax liability. Earned income may underestimate taxable income, but the two measures are likely very close for low income families.

since my preferred estimates include family fixed effects.[5] This sample contains 14,607 child-year observations, with 3,720 children born to 1,424 mothers.

I analyze the effects of the EITC on contemporaneous child achievement and long-term educational attainment. To measure achievement, I use the Peabody Individual Achievement Test (PIAT) in math and reading comprehension.[6] The math test measures achievement in mathematics as taught in mainstream education, and the reading test measures a child's ability to derive meaning from sentences read silently. The PIAT is one of the most widely used assessments in child achievement research with demonstrated reliability and concurrent validity (Center for Human Resource Research 2004). The tests are administered to children age 5 and older and are normed by age to have a national mean of zero and SD of one. Long-term outcomes include whether a child has a high school diploma or GED, whether he or she has completed one or more years of college, and highest grade completed at age 19.[7] These educational attainment measures are fairly standard in the literature, making comparison to previous studies straightforward.

Table 3 contains summary statistics for this "ever-EITC-eligible" sample of children. About 39 percent of the children are black and 23 percent Hispanic.[8] The average real earned income is $26,332 (2008 dollars) and 41 percent of the sample falls below the poverty line. The average real maximum combined state and Federal EITC value is $2,855 and the average estimated EITC receipt (using TAXSIM) is $929, with receipt ranging from

[5] Including children without siblings in the analysis would attenuate the long-run results since the outcomes do not vary for each child. About 83% of the "ever-EITC-eligible" children have a sibling in the sample. This restriction results in a slightly more disadvantaged sample. Summary statistics and regression results for the full "ever-EITC-eligible" sample are available upon request.
[6] There is also a PIAT in reading recognition that I do not examine because it initially had issues that invalidated scores for young children (Center for Human Resource Research 2004).
[7] As the children of the NLSY are only interviewed every other year, these long-term variables are actually measured when a child is either 19 or 20 in order to include all children in the analysis.
[8] The NLSY oversamples poor black and Hispanic households.

$0 to $7,052. There are about 2.85 children in each family, with the average age of the mother at birth being just over 24. About 31 percent of the children in this sample have mothers with less than a high school education. Child achievement scores are below the national average for PIAT math and reading comprehension at -0.20 and -0.12, respectively (The means in the full NLSY sample are -0.04 and 0.05, respectively). At age nineteen, 75 percent of the children have a high school diploma or GED, 25 percent have completed one or more years of college, and the average highest grade completed is 12.07 (Full-sample means are 0.81, 0.32, and 12.28, respectively).

5 Methodology

EITC receipt depends on earned income, state, year, and number of children. As family income is likely correlated with unobservables that affect maternal labor supply and child outcomes, directly estimating the effect of the amount of EITC receipt will yield biased results. Thus, I exploit exogenous variation in EITC generosity across time, number of children, and state resulting from Federal policy changes and the timing of state adoption of their own EITCs. EITC generosity, as measured by the maximum possible credit a family is eligible for, is generally increasing over time (but not linearly), and the variation across state and number of children can be quite large as discussed in Section 2.

I estimate the following model:

$$y_{ijst} = \alpha + MaxEITC_{jst}\beta_1 + TwoChildren_{jst}\beta_2 + ThreePlusChildren_{jst}\beta_3 + \boldsymbol{Welfare_{st}\beta_4} + PPE_{st}\beta_5 + \boldsymbol{X_{ijst}\beta_6} + \delta_t + \gamma_s + \theta_j + \varepsilon_{ijst} \quad (1)$$

where i indexes child, j indexes mother (family), s indexes state, t indexes year, and ε_{ijst} is an idiosyncratic error term. y_{ijst}, the outcome of interest, can be either a contemporaneous or long-run outcome. $\boldsymbol{X_{ijst}}$ is a row vector of controls including age of the child and its square age, mother's score on the Armed Forces Qualification Test (AFQT), indicators for race, sex, interview month, birth order, and birth year of the child, mother's age and its square, and indicators for mother's marital status including whether she was recently married or divorced since the last survey, age at the birth of the child, and highest grade completed.[9] For regressions with long-run outcomes, I also include the child's age in months and its square when the long-run outcome was measured as well as an indicator for the year you would expect the child to graduate high school based on his or her birth month and year.

$MaxEITC_{jst}$ is the maximum EITC credit possible for family j in state s and year t and varies by state, time, and number of children. $TwoChildren_{jst}$ and $ThreePlusChildren_{jst}$ are indicators for how many children are in family j in year t (one child is the omitted group). I also include state and year fixed effects (γ_s and δ_t). Standard errors are clustered at the state level in all regressions. My identification, therefore, is similar to a DiD specification comparing children in states with their own EITCs to those in states without EITCs before and after implementation as well as comparing children in families with 2 or more children to those in families with one child before and after OBRA 1993.

[9] Less than five percent of observations had missing data for mother AFQT score, mother's highest grade completed, or the child's interview month. For these variables I include an indicator for missing values in the regressions. For AFQT score, the missing value is replaced as the mean value for AFQT. Since the other variables are entered as dummy variables in the regressions, the missing values are grouped into the same dummy variable.

Between 1993 and 1996, 43 states received waivers to experiment with changes to the welfare program, Aid to Families with Dependent Children (AFDC). These waivers generally required work, set time limits for assistance, or increased work incentives (Meyer and Rosenbaum 2000). In 1996, AFDC was replaced with Temporary Assistance for Needy Families (TANF), which also increased the emphasis on work as well as gave states greater discretion in designing their programs (Rowe 2000). To address these changes in welfare policy over the period, I include the vector $\boldsymbol{Welfare_{st}}$, which contains the maximum welfare benefit in state s in year t for a family of three as well as an indicator for whether any time limits or work requirements for welfare receipt had been put in place.[10] I also include PPE_{st}, the real combined state and federal current per pupil spending on K-12 public education in state s in year t, to control for changes in government education spending during this period.[11]

As the NLSY follows a sample of women who were ages 14 to 20 at the end of 1978 and their children beginning in 1986, the age distribution of the mothers and children will change systematically over time. I control flexibly for a rich set of characteristics including age of the mother and child as well as year and state dummy variables to remove aggregate time and state effects, but other unobservable characteristics could also be changing in a way that confounds with the timing of changes in $MaxEITC_{jst}$. For example, if a mother has a second child after 1993, the maximum EITC variable increases. However, it could be the case that mothers with more desirable unobservable characteristics have children later in

[10] I obtained the welfare variables from both the Urban Institute's Welfare Rules Database (http://anfdata.urban.org/wrd/WRDWelcome.cfm) and from data used in Meyer and Rosenbaum (2001) that was generously provided by Bruce D. Meyer.
[11] I obtained the per pupil spending variable from the National Center for Education Statistics' Common Core of Data (http://nces.ed.gov/ccd/).

the sample. Therefore, $MaxEITC_{jst}$ could be picking up these differences in unobservables that affect timing of births in the NLSY rather than the actual effect of the policy.[12] To address this, I also estimate the model using family fixed effects, θ_j, which controls for constant unobservable differences across families. In the context of family fixed effects, only cross-time variation in EITC generosity within a family identifies the effect of the policy.

6 Results

I first estimate the effect of EITC generosity on contemporaneous child achievement. As the EITC is typically received through a family's tax return in February or March of the next calendar year, I use the EITC maximum from the previous calendar year as the "contemporaneous" measure compared to the current year's test scores. Table 4 contains tabulations for the interview month of the child in the NLSY, which is when he or she takes the PIAT. 99.99 percent of the children are interviewed in April or later and 92.77% in June or later. Therefore, the results should reflect the effects of any changes to maternal labor supply and earnings induced by a change in the maximum value of the EITC in the previous calendar year as well as any immediate effects of the increase in the lump sum EITC payment received with the tax return in the current year.

Table 5 presents the main Ordinary Least Squares (OLS) results from equation (1) *without* family fixed effects for both the contemporaneous achievement and long-run

[12] This discussion is abstracting from the possibility that families might react to 1993 OBRA by having a second child in order to receive a higher EITC payment. I ignore this, as previous work finds no effect of EITC changes on childbearing (Baughman and Dickert-Conlin 2003 and 2009 and Hotz and Scholz 2003).

educational attainment measures for the "ever-EITC-eligible" sample.[13] The $MaxEITC_{jst}$ variable is in thousands of real 2008 dollars. An increase in the maximum possible EITC a family can receive in a given year of $1,000 leads to an increase in math scores of 0.035 SD and increase in reading scores by 0.065 SD, with only the reading results being statistically significant at the 10 percent level. A $1,000 increase in $MaxEITC_{jst}$ in a single year increases the probability of receiving a high school diploma or GED at age 19 by 5.4 percentage points (sample mean 75 percent) and the probability of completion of one or more years of college at age 19 by 6.2 percentage points (sample mean of 25 percent), both statistically significant at the 5 percent level or lower. Though not significant, I find that highest grade completed increases by .047 (sample mean of 12.07).

Table 6 presents the analogous results with the inclusion of family fixed effects. I find that a $1,000 increase in $MaxEITC_{jst}$ in a single year increases math achievement by 0.072 SD and reading achievement by 0.039 SD, with the math result being statistically significant at the 5 percent level. The same increase in $MaxEITC_{jst}$ increases the probability of high school diploma or GED receipt by 2.1 percentage points and probability of completion of one or more years of college at age 19 by 1.4 percentage points (significant at the 5 and 10 percent level, respectively). I estimate a positive but statistically insignificant effect on highest grade completed of 0.030.

These estimates are smaller in magnitude than the estimates without family fixed effects with the exception of that for math. The change in the estimates suggests that fixed, unobservable characteristics of families are positively correlated with the maximum EITC value. As described above, a possible explanation for this is that the timing of births in the

[13] For brevity, not all regression coefficients on control variables are shown in the tables, but these regressions contain the full set of controls above.

NLSY is endogenous due to the cohort nature of the survey. Children born later in the sample have higher possible EITC payments, and the results suggest that they are also born to families with more desirable fixed unobservable characteristics. I prefer these estimates for this reason and include family fixed effects for the remaining analyses.

6.1 Heterogeneity in the Results

The above analysis assumes that the effects of an increase in EITC generosity in a given year has the same effect on the both the contemporaneous and long-run child outcomes for all children in each year of his or her childhood. However, previous EITC, income, and maternal labor supply studies find heterogeneity across subgroups, particularly by age and sex of the child. Table 7 presents results for various subgroups of the sample. These estimates are similar to above with family fixed effects, but with the $MaxEITC_{jst}$ variable interacted with indicators for the subgroups. I also include indicators for subgroup separately if this varies within family. For example, when looking separately by sex of the child, $MaxEITC_{jst}$ would be replaced with $MaxEITC_{jst} * male_i$, $MaxEITC_{jst} * female_i$, and $male_i$ (where $male_i$ and $female_i$ are dummy variables).

The first row of Table 7 reproduces the results on the full sample from Table 6. I first look at effects by age of the child, where I define preschool age as less than 4 years old, elementary age as between 4 and 11, and middle school age as between 11 and 15. Consistent with Chetty et al. (2011a), I find larger effects on contemporaneous achievement for middle school aged children compared to elementary school for both math and reading, but the estimates aren't statistically different (0.075 SD versus 0.067 SD for math and 0.045 SD versus 0.025 SD for reading, respectively).

Though not statistically different from one another, the magnitudes on the estimates for all long-run outcomes monotonically decrease as the age band increases. For example, I estimate that a $1,000 increase in $MaxEITC_{jst}$ during preschool increases the probability of high school or GED completion by about 3.6 percentage points. This same increase during middle school increases this probability by only about 1.9 percentage points. One possible explanation is that a child who is young during an EITC expansion likely receives higher EITC payments for the remainder of his or her childhood as well, whereas an older child would only benefit from the more generous EITC for a few years. Michelmore (2013) also finds larger effects of state EITCs for children who were less than 12 years old at the time of the state EITC adoption. Another possibility is that developmental malleability is much stronger for very young children (Shonkoff and Phillips 2000). Duncan et al. (1998) find that family economic circumstances before age five are more predictive of children's completed schooling than at ages 6 to 15.

Looking separately by sex of the child, I find much larger effects for boys compared to girls on all outcome measures. For math achievement, the estimated effect for boys is almost twice as large as that for girls (0.093 SD and 0.047 SD, respectively), though they are not statistically different at conventional levels. This is consistent with previous studies finding much larger effects of income via tax credits on achievement for boys (Milligan and Stabile 2011 and Dahl and Lochner 2012). I also estimate larger effects for boys on all long-run outcomes though, again, the estimates aren't statistically different than the estimates for girls.

Lastly, I estimate effects separately by race. Again consistent with Dahl and Lochner (2012), I find larger effects on math achievement for minority children (black or Hispanic)

compared to their white counterparts (0.089 SD and 0.059 SD, respectively). Estimates on long-run outcomes are fairly similar for the two groups, but I estimate a larger effect for minority children on high school diploma or GED receipt (2.3 and 1.8 percentage points, respectively). Michelmore (2013) also finds larger effects of the EITC on long-term educational attainment for minority children. In the "ever-EITC-eligible" sample, average real earned income is about $21,500 for minority families and about $34,200 for white families. As minority status is a crude proxy for income, this finding suggests that the EITC is more effective at improving educational outcomes for the most disadvantaged children.

6.2 Interpreting the Magnitudes of the Effects

The above coefficient estimates represent the effects of a $1,000 increase in the maximum EITC benefit a family is eligible for in a given year. To interpret the estimates, it is helpful to determine how this change in EITC generosity affects maternal labor supply and family income. Table 8 presents results using the same sample and methodology as above (including family fixed effects) where I regress the maximum EITC variable on various measures of family income and maternal labor supply. For the "ever-EITC-eligible" sample, a $1,000 increase in $MaxEITC_{jst}$ increases maternal labor force participation by about 6.4 percentage points and increases yearly hours worked by about 93.3. These results are consistent with previous EITC maternal LFP findings (Dickert et al. 1995; Eissa and Liebman 1996; Ellwood 2000; Meyer and Rosenbaum 2000 and 2001; Nuemark and

Wascher 2001; Grogger 2003; Eissa and Hoynes 2004; Hotz et al. 2006; Rothstein 2007; and Adireksombat 2010) as well as the labor supply incentives created by the EITC.[14]

Using the NBER's TAXSIM program and reported earnings from the NLSY, I estimate each family's tax liability and EITC payment. A $1,000 increase in $MaxEITC_{jst}$ increases average estimated EITC receipt by about $328, increases average estimated after-tax income (including EITC) by about $1,446, and decreases average tax liability (including the EITC) by about $598 in the sample. A $1,000 increase in the EITC maximum reduces AFDC/TANF receipt by about $525 and reduces food stamp receipt by about $135 (both AFDC/TANF and food stamp receipt are reported in the NLSY).

I create a net income measure using reported earned income and welfare receipt and estimated tax liability/EITC. I estimate that family net income increases by about $888 following an increase in the EITC maximum of $1,000. I therefore interpret my estimates as the effect of a net increase in income of about $888. Using this interpretation, my estimate of a 0.072 SD increase in math is very comparable in magnitude to the other EITC studies. Dahl and Lochner (2012) find that a $1,000 (2000 dollars) increase in income as a result of EITC expansions increases math achievement by 0.058 SD, and Chetty et al. (2011a) find that a $1,000 (2010 dollars) increase in EITC receipt holding earned income constant increases math achievement by 0.093 SD. Both of these studies find larger effects on reading than my estimate suggests. I estimate a 0.039 SD increase in reading, and Dahl and Lochner and Chetty et al. estimate effects of 0.061 SD and 0.062, respectively. However, note that my estimated effect for reading isn't precisely measured.

[14] Running the maternal labor force participation regressions separately by marital status yields point estimates of 0.142 for families with single mothers and -0.026 for those with married mothers.

For illustration on the economic importance of the effects, consider an elementary school aged child in a family of two after OBRA 1993 is fully phased in. In the absence of a state EITC, this child would be eligible for a maximum credit of about $5,000. Compared to the maximum credit of about $2,000 before OBRA 1993, my fixed effects estimates from Table 7 suggest that this child would have a higher math score of about 0.215 SD, an increased probability of graduating high school or receiving a GED of about 7.2 percentage points (9.4% increase from sample mean), and an increased probability of completing one or more years of college by age 19 by about 4.8 percentage points (18.5% increase).

For comparison, consider one of the most studied education experiments, the Student/Teacher Achievement Ratio (STAR) experiment in Tennessee in the 1980s aimed to determine the effects of class size in kindergarten through third grade. Krueger (1999) and Chetty et al. (2011b) find that students assigned to a small class in kindergarten (about 15 students compared to 23 students) score about 4 percentile points, or about 0.20 SD, higher on combined math and reading achievement that year. On average, students assigned to a small class spend 2.14 years longer in a small class than those assigned to a large class. Chetty et al. (2011b) find that students assigned to a small class are 1.8 percentage points more likely to attend college at age 20, a 26.4% increase in their sample. Using a comparison of means of the STAR data, Finn et al. (2004) find that four years in a small class is associated with a significantly higher graduation rate than attending full-size classes (87.8% and 76.3%, respectively, suggesting a 14% increase from the sample mean).

These studies indicate that my estimated effects of OBRA 1993 on achievement and educational attainment are fairly comparable in magnitudes to those found from STAR. For comparison, consider the costs of the two programs. Krueger and Whitmore (2001)

estimate the cost of the STAR program using average U.S. per pupil spending data. A treated student spends 2.3 years in a small class which amount to a cost of about $10,712 (2008 dollars). My estimates above for OBRA 1993 are for effects on elementary aged children. The average age of elementary children in my sample is 7.56, indicating that a child will receive this higher EITC for about 11.44 years until the child is 19. This amounts to an $11,257 increase in EITC spending and a total cost to the government, the change in tax liability over time, of $20,180. However, this cost does not take into account the changes in government spending on other welfare programs following an EITC expansion. My estimates suggest that the change in tax liability is actually smaller than the decrease in combined AFDC/TANF and food stamp receipt, indicating that this program might actually have an overall *negative* cost to the government (both state and Federal combined).

Another important input for educational outcomes is teacher quality. Rockoff (2004), Rivkin, Hanushek, and Kain (2005), and Kane and Staiger (2008) estimate that a 1 SD increase in teacher quality raises test scores by between 0.1 and 0.2 standard deviations. Thus, my estimates suggest that OBRA 1993 ($3,000 increase in EITC maximum) had a similar impact on test scores for elementary and middle school aged children as a 1 SD increase in teacher quality. Chetty et al. (2011a) find that a 0.2 SD increase in test scores in a single grade as a result of being assigned a higher quality teacher raises the probability of college attendance at age 20 by about 1.0 percentage points (sample mean of 37%). I estimate that OBRA 1993 had a similar effect on test scores, but much larger long-term gains on college attendance. I estimate that OBRA 1993 increased the probability of completing one or more years of college at age 19 by 4.8

percentage points for children in elementary school during the law change and 4.0 percentage points for children in middle school (sample mean of 25%).

6.3 Specification Checks

I check the robustness of my results to alternative specifications in the top panel of Table 9. The first line again reproduces my main results including family fixed effects from Table 6. I first estimate the model using the natural log of the maximum EITC variable. I find no difference in the patterns of the results, but have less power in identifying effects. I next estimate the model using the NLSY-provided sample weights. These weights are designed to correct for the over-sampling of low income black and Hispanic households, yielding a nationally representative sample each year of children born to mothers age 14 to 20 at the end of 1978. However, when selecting the sample using variables with missing values (in this case, earnings), the weights don't yield this nationally representative sample. Generally, using the weights provides a noisier estimate that more heavily weights the observations of white children in the sample. Using the weights, I find larger effects for reading and highest grade completed and smaller effects for the other outcomes. Lastly, I estimate the model using only the Federal maximum value of the EITC. These results are not statistically different from the original specification.

The bottom panel of Table 9 contains results for three falsification tests. The first line of estimates is that from a test in which I estimate the specification from equation (1) on the various outcomes, but on the sample of children whose families were never in the

EITC-eligible range during this time period.[15] As these children never received the EITC, they should not be affected by changes in its generosity over time. Finding an effect in this sample could indicate that my identification strategy is falsely attributing the effects of shocks that impact all children over time to changes in the maximum EITC. All estimates on the $MaxEITC_{st}$ variable for this "never-EITC-eligible" sample are statistically insignificant with the exception of the college completion estimate. This estimate is statistically significant at the 10 percent level, but the point estimate is negative. All estimates are also smaller in absolute magnitude than the original estimates except for reading. I estimate a larger, but very imprecisely measured effect on reading in this sample.

OBRA 1993 increased the EITC credit differentially for families with one child compared to those with two or more children, but, in all states except Wisconsin, the maximum EITC payment does not differ for families with 2 or more children. In the last two lines of Table 9, I conduct a falsification test for families with 2 or more children where I assign families with 2 children the maximum EITC value for a family with one child and assign the families with 3 or more children their actual EITC maximum values. I also exclude children living in Wisconsin from the estimation. Without state EITCs, this test basically amounts to a difference-in-differences estimation comparing children in families with 2 children to families with 3 or more children before and after OBRA 1993.

Finding a positive effect on this "false" maximum EITC variable could indicate that my main results are incorrectly attributing effects due to the timing of births in the NLSY as effects of changes in EITC generosity. The first line of results contains state variation in the "false" maximum EITC over time, and the last line contains only Federal variation. In both

[15] I again include only those children with a sibling in the estimation sample.

specifications, the estimates on all outcomes are much smaller in absolute magnitude and negative in most cases, none of which are close to statistical significance. These falsification tests provide strong support for the validity of my research design including family fixed effects.

7 Summary and Conclusions

I find that an increase in the generosity of the EITC has large positive impacts on both contemporaneous child achievement and long-run educational attainment. An increase in the maximum EITC of $1,000 in a given year significantly increases math achievement by about 0.072 SD for children in families who were ever eligible for EITC receipt. This change in EITC generosity during childhood also significantly increases the probability of receiving a high school diploma or GED at age 19 by about 2.1 percentage points and the probability of completing one or more years of college at age 19 by about 1.4 percentage points. I find larger effects for boys and minority children and evidence that an expansion in the EITC is more effective at improving educational outcomes for children who are younger during the expansion. Along with changes to child educational outcomes, an increase in the maximum EITC of $1,000 results in other changes in the household, including an increase in net family income inclusive of EITC and welfare payments of about $888 and an increase in maternal labor force participation.

Overall, the EITC appears to be an effective policy for improving educational outcomes of children, especially for the most disadvantaged. In the current context of work requirements and lifetime limits for TANF and with recent cuts to the food stamp program, the EITC is likely more important now for low income families than this study suggests. As

more data become available from the NLSY or other sources, additional work is needed to determine the effects of the EITC on other long-term outcomes of children such as earnings or welfare dependency in order to fully assess the performance of the program. While the NLSY contains these variables, the children are not yet old enough in the available data to analyze these outcomes.

References

Adireksomdat, Kampon (2010). "The Effects of the 1993 Earned Income Tax Credit Expansion on the Labor Supply of Unmarried Women," *Public Finance Review* 38(1): 11-40.

Baker, Kevin (2008). "Do Cash Transfer Programs Improve Infant Health: Evidence from the 1993 Expansion of the Earned Income Tax Credit," mimeo, University of Notre Dame.

Baughman, Reagan and Stacy Dickert-Conlin (2003). "Did Expanding the EITC Promote Motherhood?" *American Economic Review Papers and Proceedings* 93(2): 247-250.

Baughman, Reagan and Stacy Dickert-Conlin (2009). "The Earned Income Tax Credit and Fertility," *Journal of Population Economics* 22(3): 537-563.

Baughman, Reagan and Noelia Duchovny (2012). "State EITCs and Production of Child Health: Insurance Coverage, Utilization, and Health Status," Working Paper.

Blank, Rebecca M. (2002). "Evaluating Welfare Reform in the United States," NBER Working Paper 8983.

Brooks-Gunn, Jeanne, Pinka Chatterji, and Sara Markowitz (2011). "Early Maternal Employment and Family Wellbeing," NBER Working Paper 17212.

Center for Human Resource Research (2004). "NLSY79 Child & Young Adult Data Users Guide," The Ohio State University, Columbus, OH.

Chetty, Raj, John N. Friedman, and Jonah Rockoff (2011a). "New Evidence on the Long-Term Impacts of Tax Credits," IRS Statistics of Income White Paper, 2011

Chetty, Raj, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan (2011b). "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star," *The Quarterly Journal of Economics* 126 (4): 1593-1660.

Chetty, Raj and Emmanuel Saez (2013). "Teaching the Tax Code: Earnings Responses to an Experiment with EITC Recipients," forthcoming, *American Economic Journal: Applied Economics*.

Chetty, Raj, John N. Friedman, and Emmanuel Saez (2013). "Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings," forthcoming, *American Economic Review*.

Dahl, Gordon B. and Lance Lochner (2012). "The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit," *American Economic Review* 102(5): 1927-56.

Dickert-Conlin, Stacy, and Scott Houser (2002). "EITC and Marriage," *National Tax Journal* 60 (1): 25–39.

Dickert, Stacy, Scott Houser and John K. Scholz (1995). "The Earned Income Tax Credit and Transfer Programs: A Study of Labor Market and Program Participation," *Tax Policy and the Economy* 9: 1-50.

Duncan, Greg J., Jeanne Brooks-Gunn, W. Jean Yeung, and Judith R. Smith (1998). "How Much Does Childhood Poverty Affect the Life Chances of Children?" *American Sociological Review* 63: 406-23.

Eissa, Nada and Hilary W. Hoynes (2000). "Good News for Low Income Parents: Tax-Transfer Schemes and Marriage," mimeo, University of California, Berkeley.

Eissa, Nada and Hilary W. Hoynes (2004). "Taxes and the Labor Market Participation of Married Couples: The Earned Income Tax Credit," *Journal of Public Economics* 88(9-10): 1931-58.

Eissa, Nada and Hilary W. Hoynes (2006). "Behavioral Responses to Taxes: Lessons from the EITC and Labor Supply," *Tax Policy and the Economy* 20: 74-110.

Eissa, Nada and Jeffrey B. Liebman (1996). "Labor Supply Response to the Earned Income Tax Credit," *Quarterly Journal of Economics* 111(2): 605-637.

Ellwood, David T. (2000). "The Impact of the Earned Income Tax Credit and Social Policy Reforms on Work, Marriage, and Living Arrangements," *National Tax Journal* 43(4, part 2): 1063–105.

Feenberg, Daniel, and Elisabeth Coutts (1993). "An Introduction to the TAXSIM Model." *Journal of Policy Analysis and Management* 12(1): 189–94.

Finn, Jeremy D., Jayne Boyd-Zaharias, and Susan B. Gerber (2004). "Small Classes in the Early Grades, Academic Achievement, and Graduating from High School," *Journal of Educational Psychology* 97: 214–23.

Friedman, Pamela (2000). "The Earned Income Tax Credit," Welfare Information Network, Issue Notes.

Goldberger, Wendy A., JoAnn Prause, Rachel Lucas-Thompson, and Amy Himsel (2008). "Maternal Employment and Children's Achievement in Context: A Meta-Analysis of Four Decades of Research," *Psychological Bulletin* 134(1): 77-108.

Grogger, Jeffrey (2003). "The Effects of Time Limits, the EITC, and Other Policy Changes on Welfare Use, Work, and Income among Female-Headed Families," *Review of Economics and Statistics* 85(2): 394–408.

Hill, Jennifer L., Jane Waldfogel, Jeanne Brooks-Gunn, and Wen-Jui Han (2005). "Maternal Employment and Child Development: A Fresh Look Using Newer Methods," *Developmental Psychology* 41(6): 833-50.

Hotz, V. Joseph and John K. Scholz (2003). "The Earned Income Tax Credit," Robert Moffitt, ed., Means-Tested Transfer Programs in the United States. Chicago: University of Chicago Press.

Hotz, V. Joseph, Charles H. Mullin and John K. Scholz (2006). "Examining the Effect of the Earned Income Tax Credit on the Labor Market Participation of Families on Welfare," NBER Working Paper 11968.

Hoynes, Hilary W., Douglas L. Miller and David Simon (2012). "Income, the Earned Income Tax, and Infant Health," Working Paper.

Internal Revenue Service (2011). Statistics of Income Branch, "SOI Tax Stats - Individual Statistical Tables by Size of Adjusted Gross Income, Individual Income Tax Returns with Earned Income Credit," http://www.irs.gov/pub/irs-soi/08in25ic.xls, accessed July 2011.

James-Burdumy, Susanne (2005). "The Effect of Maternal Labor Force Participation on Child Development," *Journal of Labor Economics* 23(1): 177-211.

Kane, Thomas, and Douglas O. Staiger (2008). "Estimating Teacher Impacts on Student Achievement: An Experimental Evaluation," NBER Working Paper 14607.

Krueger, Alan B. (1999). "Experimental Estimates of Education Production Functions," *Quarterly Journal of Economics* 114(2): 497–532.

Krueger, Alan B. and Diane M. Whitmore (2001). "The Effect of Attending a Small Class in the Early Grades on College-Test Taking and Middle School Test Results: Evidence from Project STAR," *Economic Journal* 111(January): 1–28.

LaLumia, Sara (2013). "The EITC, Tax Refunds, and Unemployment Spells," *American Economic Journal: Economic Policy* 5(2): 188-221.

Leigh, Andrew (2010). "Who Benefits from the Earned Income Tax Credit? Incidence among Recipients, Coworkers and Firms," IZA Discussion Paper No. 4960.

Levine, P., & Zimmerman, D. (2005). "Children's Welfare Exposure and Subsequent Development," *Journal of Public Economics* 89: 31–56.

Liebman, Jeffrey B. (1998). "The Impact of the Earned Income Tax Credit on Incentives and Income Distribution," *Tax Policy and the Economy* 12: 83-119.

Løken, Katrine V., Magne Mogstad, and Matthew Wiswall (2012). "What Linear Estimators Miss: The Effects of Family Income on Child Outcomes," *American Economic Journal: Applied Economics* 4(2): 1–35.

Maag, Elaine (2005). "Paying the Price? Low-Income Parents and the Use of Paid Tax Preparers," New Federalism: National Survey of America's Families B-64, Urban Institute.

Meyer, Bruce D. and Dan T. Rosenbaum (1999). "Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers," NBER Working Paper 7363.

Meyer, Bruce D. and Dan T. Rosenbaum (2000). "Making Single Mothers Work: Recent Tax and Welfare Policy and Its Effects," *National Tax Journal* 53(4, Part 2): 1027–1061.

Meyer, Bruce D. and Dan T. Rosenbaum (2001). "Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers," *Quarterly Journal of Economics* 116(3): 1063–1114.

Michelmore, Katherine (2013). "The Effect of Income on Educational Attainment: Evidence from State Earned Income Tax Credit Expansions," Working Paper.

Milligan, Kevin and Mark Stabile (2011). "Do Child Tax Benefits Affect the Wellbeing of Children? Evidence from Canadian Child Benefit Expansions," *American Economic Journal: Economic Policy* 3(3): 175-205.

Mink, Patsy T. (1993). "Children's Initiatives in the Budget Reconciliation," 103rd Cong., 1st session, Congressional Record, Vol. 139, July 29, p. H5511.

Morris, Pamela A., Aletha C. Huston, Greg J. Duncan, Danielle A. Crosby and Johannes M. Bos (2001). "How Welfare and Work Policies Affect Children: A Synthesis of Research," New York, NY: MDRC.

Morris, Pamela A., Greg J. Duncan and Elizabeth Clark-Kauffman (2005). "Child Well-Being in an Era of Welfare Reform: The Sensitivity of Transitions in Development to Policy Change," *Developmental Psychology* 41: 919–32.

National Commission on Children (1993). "Children's Initiatives in the Budget Reconciliation," 103rd Cong., 1st session, Congressional Record, Vol. 139, July 29, p. H5508.

Neumark, David and William Wascher (2001). "Using the EITC to Help Poor Families: New Evidence and a Comparison with the Minimum Wage," *National Tax Journal* 54(2): 281–317.

Olson, Lynn M. and Audrey Davis (1994). "The Earned Income Tax Credit: Views from the Street Level," Northwestern University Working Paper WP-94-1.

Rivkin, Steven G., Eric A. Hanushek, and John F. Kain (2005). "Teachers, Schools and Academic Achievement," *Econometrica* 73: 417–458.

Rockoff, Jonah E. (2004). "The Impact of Individual Teachers on Student Achievement: Evidence from Panel Data," *American Economics Review* 94: 247–252.

Romich, Jennifer and Thomas Weisner (2000). ''How Families View and Use the EITC Advance Payment versus Lump Sum Delivery," *National Tax Journal* 53: 1245–66.

Ross-Phillips, Katherine (2001). "Who Knows About the Earned Income Tax Credit?" Urban Institute Policy Brief No. B-27.

Rothstein, Jesse (2007). "The Mid-1990s EITC Expansion: Aggregate Labor Supply Effects and Economic Incidence," mimeo, Princeton University.

Rowe, Gretchen (2000). "State TANF Policies as of July 1999." Report. Washington, DC: The Urban Institute.

Saez, Emmanuel (2010). "Do Taxpayers Bunch at Kink Points?" *American Economic Journal: Economic Policy* 2: 180–212.

Shepard, Karen (1993). "Children's Initiatives in the Budget Reconciliation," 103rd Cong., 1st session, Congressional Record, Vol. 139, July 29, p. H5512.

Shonkoff, Jack P. and Deborah Phillips (2000). From Neurons to Neighborhoods: The Science of Early Childhood Development. Committee on Integrating the Science of Early Childhood Development, Board on Children, Youth, and Families, Institute of Medicine, Division of Behavioral and Social Sciences and Education. Washington, D.C.: National Academy Press.

Smeeding, Timothy M., Katherine Ross-Phillips, and Michael O'Connor (2000). "The EITC: Expectation, Knowledge, Use, and Economic and Social Mobility," *National Tax Journal* 53(4, part 2): 1187–209.

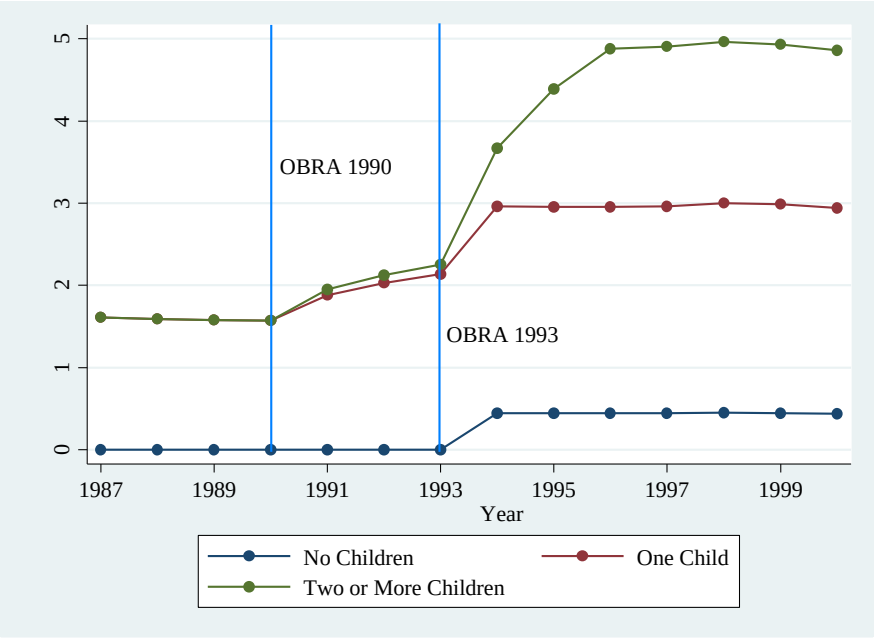
Smolensky, Eugene, and Jennifer A. Gootman (2003). Working Families and Growing Kids: Caring for Children and Adolescents. Board on Children, Youth, and Families. Division of Behavioral and Social Sciences and Education. Washington D.C.: National Academies Press.

Stupak, Bart (1993). "Children's Initiatives in the Budget Reconciliation," 103rd Cong., 1st session, Congressional Record, Vol. 139, July 29, p. H5515.

Strully, Kate, David H. Rehkopf and Ziming Xuan (2010). "Effects of Prenatal Poverty on Infant Health: State Earned Income Tax Credits and Birth Weight," *American Sociological Review* 75(4): 534-62.

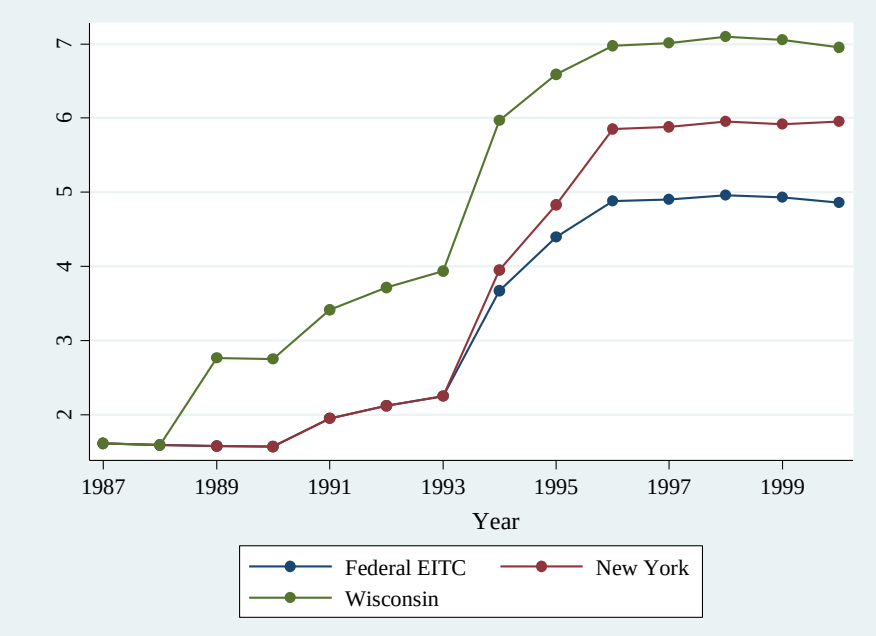
Ventry, Dennis J. (2000). “The Collision of Tax and Welfare Politics: the Political History of the Earned Income Tax Credit, 1969-1999,” *National Tax Journal* 53(2): 983-1026.

FIGURE 1
Real Maximum Federal EITC Credit by Tax Year and Number of Children (2008$)



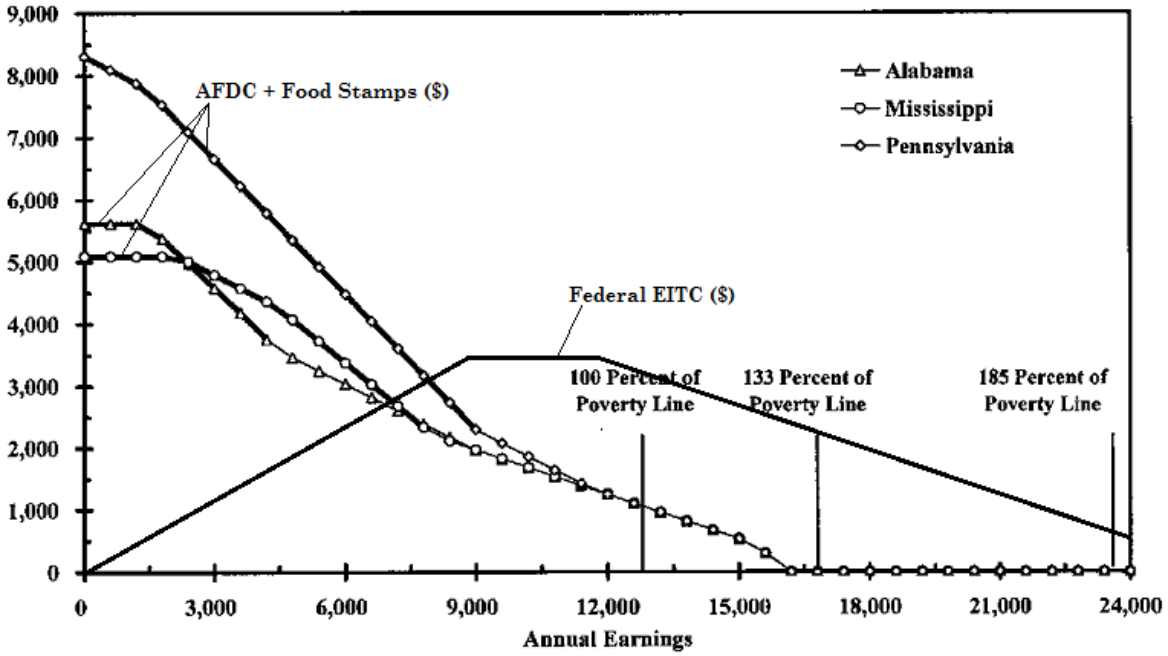
Sources: Joint Committee on Taxation, Ways and Means Committee (2004).
Formatting adopted from Hoynes et al. (2012).

FIGURE 2
Real Maximum EITC Credit by Tax Year (2008$), Family with 3 children



Sources: Joint Committee on Taxation, Ways and Means Committee (2004),
Center on Budget and Policy Priorities, and Leigh (2010).

FIGURE 3
1996 Benefit Schedule for AFDC, Food Stamps, and EITC
Mothers with Two Children, Alabama, Mississippi, and Pennsylvania



Source: Partial reprint from Meyer and Rosenbaum (2000). I have added the EITC schedule to their original graph containing only the welfare and food stamps benefits.
Notes: Women are assumed to be in their first four months of work, to have no unearned income, and to claim no child care expenses. Shelter costs per month are assumed to be at the mean for food stamp households in the given state: AL ($228), MS ($196), and PA ($322).

TABLE 1
Federal EITC Parameters, 1987-2000

Calendar year	Credit rate (%)	Min income for max credit	Max credit	Phase-out rate (%)	Phase-out range: Beginning income	Phase-out range: Ending income
1987	14	6,080	851	10	6,920	15,432
1988	14	6,240	874	10	9,840	18,576
1989	14	6,500	910	10	10,240	19,340
1990	14	6,810	953	10	10,730	20,264
1991						
One child	16.7	7,140	1,192	11.93	11,250	21,250
Two children	17.3	7,140	1,235	12.36	11,250	21,250
1992						
One child	17.6	7,520	1,324	12.57	11,840	22,370
Two children	18.4	7,520	1,384	13.14	11,840	22,370
1993						
One child	18.5	7,750	1,434	13.21	12,200	23,050
Two children	19.5	7,750	1,511	13.93	12,200	23,050
1994						
No children	7.65	4,000	306	7.65	5,000	9,000
One child	26.3	7,750	2,038	15.98	11,000	23,755
Two children	30	8,425	2,528	17.68	11,000	25,296
1995						
No children	7.65	4,100	314	7.65	5,130	9,230
One child	34	6,160	2,094	15.98	11,290	24,396
Two children	36	8,640	3,110	20.22	11,290	26,673
1996						
No children	7.65	4,220	323	7.65	5,280	9,500
One child	34	6,330	2,152	15.98	11,610	25,078
Two children	40	8,890	3,556	21.06	11,610	28,495
1997						
No children	7.65	4,340	332	7.65	5,430	9,770
One child	34	6,500	2,210	15.98	11,930	25,750
Two children	40	9,140	3,656	21.06	11,930	29,290
1998						
No children	7.65	4,460	341	7.65	5,570	10,030
One child	34	6,680	2,271	15.98	12,260	26,473
Two children	40	9,390	3,756	21.06	12,260	30,095
1999						
No children	7.65	4,530	347	7.65	5,670	10,200
One child	34	6,800	2,312	15.98	12,460	26,928
Two children	40	9,540	3,816	21.06	12,460	30,580
2000						
No children	7.65	4,610	353	7.65	5,770	10,380
One child	34	6,920	2,353	15.98	12,690	27,413
Two children	40	9,720	3,888	21.06	12,690	31,152

Source: Joint Committee on Taxation, Ways and Means Committee (2004).
Note: Dollar amounts unadjusted for inflation

TABLE 2
State EITC Supplements, 1987-2000 (%)

State	CO	DC	IA	IL	KS	MA	MD	MD	ME	MN	MN	NJ	NY	OR	RI	VT	WI	WI	WI
No. Children	0+	0+	0+	0+	0+	0+	1+	1+	0+	0	1+	1+	0+	0+	0+	0+	1	2	3+
1987							50								23				
1988							50								23	23			
1989							50								23	25	5	25	75
1990			5				50								28	28	5	25	75
1991			6.5				50			10	10				28	28	5	25	75
1992			6.5				50			10	10				28	28	5	25	75
1993			6.5				50			15	15				28	28	5	25	75
1994			6.5				50			15	15		7.5		28	25	4.4	21	63
1995			6.5				50			15	15		10		28	25	4	16	50
1996			6.5				50			15	15		20		28	25	4	14	43
1997			6.5			10	50			15	15		20	5	28	25	4	14	43
1998			6.5		10	10	50	10		15	25		20	5	27	25	4	14	43
1999	8.5		6.5		10	10	50	10		25	25		20	5	27	25	4	14	43
2000	10	10	6.5	5	10	10	50	15	5	25	33	10	23	5	26	32	4	14	43
Refundable?	Y	Y	N	N	Y	Y	N	Y	N	Y	Y	Y	Y	N	N	Y	Y	Y	Y

Sources: Center on Budget and Policy Priorities and Leigh (2010).
Notes: No. Children is the number of children required for eligibility of the state supplement. Supplement is the percentage top-up of the federal EITC payment.

TABLE 3
Summary Statistics, 1988-2000

VARIABLES	Obs.	Mean	Std. Dev.	Min	Max
PIAT Math	9908	-0.20	0.88	-2.33	2.33
PIAT Reading Comprehension	8210	-0.12	0.91	-2.33	2.33
HS Diploma or GED (at age 19)	8316	0.75	0.43	0	1
Completed One or More Years College (at age 19)	6382	0.25	0.43	0	1
Highest Grade Completed (at age 19)	7977	12.07	1.23	0	16.00
Age	14607	7.65	3.90	0	14.92
Hispanic	14607	0.23	0.42	0	1
Black	14607	0.39	0.49	0	1
Male	14607	0.50	0.50	0	1
Birth Order	14607	2.19	1.16	1	10
Mother Age at Birth	14607	24.40	4.46	13	41
Mother Married	14607	0.49	0.50	0	1
Mother AFQT Score	14096	29.40	24.03	0	99.49
Mother has less than HS Education	14607	0.31	0.46	0	1
Number of Children in Family	14607	2.85	1.19	1	9
EITC Eligible	14607	0.57	0.49	0	1
Real EITC Maximum ($1000s)	14607	2.85	1.43	1.58	7.40
EITC Payment ($1000s)	14607	0.93	1.27	0	7.05
Real Maximum AFDC Family of 3 ($1000s)	14607	6.94	3.13	1.93	16.90
Any Time Limits on AFDC Receipt	14607	0.17	0.37	0	1
Real K-12 Per Pupil Spending	14607	7.81	1.74	4.36	13.76
Mother in Labor Force	13507	0.68	0.47	0	1
Real Family Earned Income ($1000s)	14607	26.33	30.61	0	637.94
In Poverty	13202	0.41	0.49	0	1
Real AFDC Receipt	14525	1.56	3.53	0	21.85
Real Food Stamp Receipt	14462	1.41	2.32	0	17.06

Notes: Summary statistics of children in the NLSY whose estimated family income ever fell into the EITC-eligible range and who have a sibling in the sample.

TABLE 4
Tabulation of Interview Month of Child, 1988-2000

Interview Month	Obs.	Percent	Cumulative Percent
January	2	0.01	0.01
February	0	0	0.01
March	0	0	0.01
April	148	1.01	1.03
May	906	6.2	7.23
June	1,835	12.56	19.79
July	3,236	22.15	41.95
August	4,156	28.45	70.4
September	2,405	16.46	86.86
October	1,073	7.35	94.21
November	462	3.16	97.37
December	107	0.73	98.1
Missing	277	1.9	100
Total	14,607	100	-

Notes: Tabulations for children in the NLSY whose estimated family income ever fell into the EITC-eligible range and who have a sibling in the sample.

TABLE 5
Ordinary Least Squares Results, 1988-2000

	Contemporaneous		Long-Run		
VARIABLES	Math	Reading	High School Diploma or GED	Completed 1 or More Yrs. College	Highest Grade Completed
MaxEITC	0.0352	0.0651*	0.0541***	0.0616**	0.0474
	(0.0348)	(0.0364)	(0.0198)	(0.0255)	(0.0623)
Married	0.0141	0.0120	0.0464**	0.0377*	0.1420***
	(0.0290)	(0.0339)	(0.0198)	(0.0200)	(0.0506)
Two Children	0.0064	-0.0534	-0.0192	-0.0315	0.2150**
	(0.0687)	(0.0855)	(0.0359)	(0.0352)	(0.0822)
Three Plus Children	0.0263	0.0157	-0.0011	0.0107	0.0373*
	(0.0175)	(0.0236)	(0.0066)	(0.0067)	(0.0216)
Welfare Max Benefit	-0.0206	-0.0872	-0.0266	-0.0361	0.0632
	(0.0581)	(0.0728)	(0.0350)	(0.0407)	(0.0826)
Time Limits on Welfare	-0.0186	-0.0097	0.0112	-0.0315	-0.0900
	(0.0472)	(0.0368)	(0.0126)	(0.0280)	(0.0566)
PPE	-0.0188	0.0016	0.0029	-0.0208	-0.1010**
	(0.0289)	(0.0420)	(0.0119)	(0.0170)	(0.0419)
Age	0.0492	-0.6050***	-0.0665	-0.0592	-0.5390***
	(0.0707)	(0.0666)	(0.0472)	(0.0492)	(0.119)
Age²	-0.0081***	0.0142***	-0.0000	0.0000	0.0001
	(0.0010)	(0.0016)	(0.0002)	(0.0002)	(0.0004)
Male	-0.0434	-0.1420***	-0.0960***	-0.1060***	-0.3130***
	(0.0318)	(0.0290)	(0.0242)	(0.0223)	(0.0665)
Hispanic	-0.1850***	-0.0657	0.0679***	0.0434	0.1450**
	(0.0458)	(0.0557)	(0.0247)	(0.0268)	(0.0624)
Black	-0.1630***	0.0132	-0.0194	-0.0592**	-0.0799
	(0.0393)	(0.0373)	(0.0294)	(0.0244)	(0.0781)
Mother AFQT	0.0092***	0.0104***	0.0015***	0.0004	0.0045***
	(0.0009)	(0.0010)	(0.0005)	(0.0006)	(0.0015)
Year Fixed Effects	x	x	x	x	x
State Fixed Effects	x	x	x	x	x
Family Fixed Effects	-	-	-	-	-
Observations	9,808	8,128	8,220	6,310	7,896
R-squared	0.182	0.280	0.192	0.244	0.243

Notes: Robust standard errors (clustered at the state level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each column represents a separate OLS regression for the various outcomes. Full set of controls also include indicators for interview month, birth order, and birth year of the child, mother's age and its square, and indicators for whether mother was recently married or divorced since the last survey, mother's age at the birth of the child, and mother's highest grade completed. For regressions with long-run outcomes, additional controls include the child's age in months and its square when the long-run outcome was measured as well as an indicator for expected high school graduation year.

TABLE 6
Ordinary Least Squares Results w/ Family Fixed Effects, 1988-2000

	Contemporaneous		Long-Run		
VARIABLES	Math	Reading	High School Diploma or GED	Completed 1 or More Yrs. College	Highest Grade Completed
MaxEITC	0.0717**	0.0388	0.0207**	0.0139*	0.0295
	(0.0274)	(0.0426)	(0.0099)	(0.0078)	(0.0301)
Married	0.0717*	0.0247	0.0067	0.0027	0.0177
	(0.0381)	(0.0388)	(0.0061)	(0.0041)	(0.0140)
Two Children	-0.0285	-0.0520	-0.0133	-0.0154	0.0312
	(0.0568)	(0.0707)	(0.0113)	(0.0122)	(0.0359)
Three Plus Children	-0.0318	0.0045	-0.0074	-0.0124	0.0298
	(0.0528)	(0.0737)	(0.0108)	(0.0099)	(0.0404)
Welfare Max Benefit	0.0478***	0.0046	-0.0014	-0.0019	0.0117
	(0.0162)	(0.0205)	(0.0030)	(0.0033)	(0.0143)
Time Limits on Welfare	0.0263	-0.0294	-0.0055	-0.0041	-0.0142
	(0.0417)	(0.0283)	(0.0105)	(0.0081)	(0.0238)
PPE	0.0194	0.0057	0.0101	0.0099*	-0.0246
	(0.0355)	(0.0514)	(0.0072)	(0.0057)	(0.0326)
Age	0.1810**	-0.3570***	-0.1070*	0.0099	-0.3500*
	(0.0769)	(0.0740)	(0.0605)	(0.0753)	(0.1880)
Age²	-0.0087***	0.0108***	-0.0000	-0.0000	-0.0004
	(0.0011)	(0.0018)	(0.0001)	(0.0001)	(0.0003)
Male	-0.0086	-0.1250***	-0.0838***	-0.1100***	-0.3570***
	(0.0283)	(0.0317)	(0.0271)	(0.0290)	(0.0942)
Year Fixed Effects	x	x	x	x	x
State Fixed Effects	x	x	x	x	x
Family Fixed Effects	x	x	x	x	x
Observations	9,808	8,128	8,220	6,310	7,896
R-squared	0.493	0.591	0.730	0.809	0.738

Notes: Robust standard errors (clustered at the state level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each column represents a separate OLS regression for the various outcomes. Full set of controls also include indicators for interview month, birth order, and birth year of the child, mother's age and its square, and indicators for whether mother was recently married or divorced since the last survey, mother's age at the birth of the child, and mother's highest grade completed. For regressions with long-run outcomes, additional controls include the child's age in months and its square when the long-run outcome was measured as well as an indicator for expected high school graduation year.

TABLE 7
Ordinary Least Squares Results with Family Fixed Effects by Subgroups, 1988-2000

	Contemporaneous		Long-Run		
VARIABLES	Math	Reading	High School Diploma or GED	Completed 1 or More Yrs. College	Highest Grade Completed
All	0.0717**	0.0388	0.0207**	0.0139*	0.0295
	(0.0274)	(0.0426)	(0.0099)	(0.0078)	(0.0301)
Preschool	-	-	0.0359	0.0259	0.1110
			(0.0334)	(0.0442)	(0.0798)
Elementary	0.0673**	0.0250	0.0240**	0.0161	0.0323
	(0.0334)	(0.0454)	(0.0117)	(0.0102)	(0.0371)
Middle School	0.0745***	0.0453	0.0193*	0.0132*	0.0304
	(0.0259)	(0.0421)	-0.0096	(0.0076)	(0.0279)
Boys	0.0934***	0.0500	0.0220**	0.0140*	0.0366
	(0.0293)	(0.0444)	(0.0103)	(0.0080)	(0.0294)
Girls	0.0474	0.0235	0.0182*	0.0138*	0.0220
	(0.0307)	(0.0413)	(0.0100)	(0.0081)	(0.0323)
Minority	0.0894***	0.0210	0.0232**	0.0138*	0.0302
	(0.0281)	(0.0444)	(0.0100)	(0.0079)	(0.0299)
White	0.0593**	0.0536	0.0183*	0.0140*	0.0286
	(0.0265)	(0.0431)	(0.0099)	(0.0080)	(0.0308)

Notes: Robust standard errors (clustered at the state level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regression for various subgroups with full set of controls from Table 6.

TABLE 8
OLS Results with Family Fixed Effects on Maternal LS and Family Income, 1988-2000

Mother In LF	*Hours Worked*	*EITC Payment*	*After-Tax Income*
0.0640***	93.25*	0.328***	1.446
(0.0175)	(55.56)	(0.108)	(1.182)
Tax Liability	*AFDC/TANF*	*Food Stamps*	*Total Net Income*
-0.598	-0.525*	-0.135**	0.888
(0.388)	(0.276)	(0.0646)	(1.110)

Notes: Robust standard errors (clustered at the state level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. OLS regression for various outcomes with full set of controls from Table 6.

TABLE 9
Robustness to Alternative Specifications and Falsification Tests, 1988-2000

	Contemporaneous		Long-Run		
VARIABLES	Math	Reading	High School Diploma or GED	Completed 1 or More Yrs. College	Highest Grade Completed
Original	0.0717**	0.0388	0.0207**	0.0139*	0.0295
	(0.0274)	(0.0426)	(0.0099)	(0.0078)	(0.0301)
Log MaxEITC	0.2650*	0.2620	0.0522	0.0422	0.1140
	(0.1360)	(0.1860)	(0.0407)	(0.0297)	(0.1250)
Weighted	0.0315	0.0177	0.0196**	0.0052	0.0475
	(0.0290)	(0.0512)	(0.0089)	(0.0066)	(0.0306)
Only Federal MaxEITC	0.1140***	0.0163	0.0214	0.0118	0.0154
	(0.0401)	(0.0652)	(0.0143)	(0.0130)	(0.0551)
Non-EITC eligible	0.0062	0.0740	0.0041	-0.0120*	-0.0172
	(0.0465)	(0.0623)	(0.0037)	(0.0070)	(0.0170)
2 vs. 3+ Children	0.0225	0.0040	-0.0007	-0.0009	-0.0009
	(0.0212)	(0.0226)	(0.0047)	(0.0042)	(0.0119)
2 vs. 3+ Children Only Federal EITC	0.0080	-0.0090	-0.0043	-0.0025	-0.0140
	(0.0217)	(0.0222)	(0.0049)	(0.0044)	(0.0134)

Notes: Robust standard errors (clustered at the state level) in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Each cell represents a separate OLS regression for various outcomes with full set of controls from Table 6.