The Earned Income Tax Credit's Intergenerational Impact on Education

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Abstract

I use decades of data from the PSID and variation in a measure of parental exposure to the Earned Income Tax Credit to estimate the impact of the EITC on the education of children whose parents were exposed. Parental EITC exposure is also used as an instrument for a parent's childhood family income. When looking at exposure in different age ranges, effects appear ambiguous unless the sample is restricted by income. Parental EITC exposure from ages 0-5 of parents appears to have a negative impact on a child's standardized reading test scores, while from ages 6-12 the effect appears positive. There is some evidence that the impact on math scores is positive from ages 6-12. When looking at other subsamples, I find that the negative impact of exposure from ages 0-5 on reading scores is more pronounced for Black families. The instrumental variables specification is weakly identified due to the instruments' poor predictive strength for incomes from 13-18.

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Contents

I. Introduction

In 1967, 28.4% of children in the United States lived in poverty, a percentage that declined to 15.6% by 2016. The bulk of this decline has been due to an expanded social safety net; before taxes and transfers, 25.1% of children lived in households whose incomes were below the poverty line in 2016 (Shapiro and Trisi 2017). The dramatic difference between the pre- and post-transfers child poverty level was achieved through several federal poverty reduction programs, such as Medicaid, the Supplemental Nutrition Assistance Program, and the Earned Income Tax Credit. Spending on programs aimed at reducing child poverty totaled roughly $200 billion in 2016, justified on mainly humanitarian grounds (Hoynes and Schanzenbach 2018). These programs received much attention following the COVID-19 pandemic, with some policymakers and experts calling for the recent expansion of the Child and Earned Income Tax Credits, introduced temporarily as support through the pandemic, to be made a permanent feature of the social safety net (Marr et al. 2021).

Despite the substantial reductions in poverty, these programs have been sources of controversy and debate for decades. One prominent issue is a longstanding perceived conflict between cash aid and incentives to work and invest in human capital. Even the administration of Lyndon Johnson, which began the War on Poverty, held this view, arguing in 1964 that "it will be far better... to equip and permit the poor of the Nation to produce and earn" (Nichols and Rothstein 2015). Another large portion of public spending targeted at children, the public provision of education, is designed alternatively as a long-run human capital investment and thus avoids many criticisms levelled at direct poverty alleviation. Recent studies, however, have increasingly shown compelling evidence that social safety programs targeting children have long-run benefits in areas like health, education, and employment, comparable to a "human capital investment" (Hoynes and Schanzenbach 2018).

The evidence motivates investigating whether the long-run investments of child poverty reduction programs will improve outcomes for the next generation of children as well. My paper seeks to add this literature by asking to what extent the Earned Income Tax Credit impacts the educational outcomes of descendants of people whose childhood households benefited from the credit. To do so, I build upon Bastian and Michelmore (2018), which uses a measure of exposure to the EITC to estimate its long-run effect on educational and employment outcomes, by implementing a similar strategy to estimate the effect of the EITC on descendants of individuals in their data.

I use data from the Panel Study of Income Dynamics to measure child outcomes and current household characteristics, and data from Bastian and Michelmore (2018) to measure the characteristics of a parent and their childhood household. The latter data include parental EITC exposure, defined as the maximum credit the parent's childhood family could receive, given only the state, family size, and tax year. I use parental EITC exposure summed over different age ranges both in reduced-form regressions and as an instrument for a parent's childhood family income to estimate the intergenerational effects of the EITC and income shocks on a child's Woodcock-Johnson test scores, enrollment in a gifted program, disciplinary outcomes, and self-assessed reading and math ability.

Results suggest the intergenerational effect of the EITC depends on the period of the parent's childhood during which they were exposed. I find evidence that parental EITC exposure during ages 0-5 has a negative effect on a child's reading scores, particularly among children of Black parents. This unexpected negative effect is not significant in the full-sample, full-control specification but is significant in and consistent across many subsamples and alternative specifications. Conversely, I find some evidence, albeit weaker, of a positive effect of parental exposure during ages 6-12 on a child's math and reading scores when the sample is restricted to include only children of parents raised in less affluent households. This result is also found in specifications using the logarithm of exposure, but the coefficient is less precise and its significance depends on the upper bound placed on a parent's childhood family income. Results from the instrumental variables specification are weakly identified and wholly inconclusive due to the instruments' poor predictive strength for parental family income from 13-18. The varied results of this paper motivate future research of the intergenerational effects of poverty alleviation, particularly as programs age and more data are collected.

Section II describes the history of the EITC and prior research on the EITC (or similar programs) particularly in relation to educational outcomes. Section III describes the data and sample, while Section IV details my methodology. Results are presented in Section V and checks on the robustness of these results are presented in Section VI. Section VII concludes.

II. Background & Previous Literature

The Earned Income Tax Credit was introduced in 1975. Since then, it has grown to be one of the largest programs in the US social safety net, with $63 billion dollars in federal expenditures going to 26.7 million recipients in 2013 (Nichols and Rothstein 2015). These benefits are unique among the US safety net in that they are distributed by the Internal Revenue Service. Several states have expanded the EITC through their own budgets as well. Rhode Island became the first state to introduce its own EITC in 1986 (Bastian and Michelmore 2018a). By 2000, Rhode Island had been joined by 13 other states and the District of Columbia, and the total number of states (plus DC) with their own EITC grew to 25 by 2015 (Maxfield 2015; Nichols and Rothstein 2015).

The EITC has enjoyed bipartisan support throughout its existence, and many of its provisions grew out of legislative compromise. Amid debate surrounding the welfare state sparked by the War on Poverty, lawmakers originally proposed the program as an alternative to Nixon's Family Assistance Plan (FAP), a means-tested negative income tax. The EITC was originally only available to working families with children. The EITC's phase-in rate, designed to increase labor supply and prevent the unemployed from receiving it, was designed partially as a response to concerns about the FAP and other welfare programs disincentivizing work (Nichols and Rothstein 2015). The resulting overall federal benefits schedule, that is, credit size as a function of family income, is divided into three regions: the phase-in, in which benefits increase as a percentage of income; the maximum credit or "plateau," in which the function is constant; and the phase-out, in which the benefits decrease as a percentage of each new dollar earned (Manoli and Turner 2018).

The parameters of these three regions have changed dramatically throughout the EITC's history. At its introduction, it had a 10% phase-in rate up to a $400 credit for families with $4,000 in income, at which point there began a 10% phase-out rate with benefits reaching $0 at incomes of $8,000 (Nichols and Rothstein 2015). Benefits could be taken periodically with each paycheck or as an annual lump-sum; the overwhelming majority of families chose the latter (Maxfield 2015). In 1978 the benefits schedule acquired its characteristic trapezoidal shape when the maximum credit was increased to \$500 and was available for incomes between \$5,000 and \$6,000. The phase-out rate was increased to 12.5%, with benefits reaching \$0 at incomes of \$10,000. Since the benefits were not indexed to inflation, the value of the EITC declined steadily by 18.2% until 1986. That year, the maximum credit was increased to its real value in 1975, and the maximum credit was available for incomes up to (nominally) \$9,480. Some benefits were available to working households with children making up to \$18,756, or \$36,579 in 2013 dollars (Nichols and Rothstein 2015).

The most significant changes to the EITC came in the 1990s and would have affected only the very youngest parents in this paper's sample. In 1990, families with two or more children were given a more generous schedule than families with just one child. When changes again occurred in 1993 and 1996, many of the gains were concentrated in these larger families. By 1996, the maximum credit (in 2013 dollars) had grown to \$5,197 for families with two or more eligible children (with a 40% phase-in) and \$3,192 for families with one eligible child (34% phase-in). Low-income working households without children also became eligible for the first time, albeit for a much smaller credit (Nichols and Rothstein 2015).

Short-Run Impacts.

Since its introduction in 1975, a wealth of literature has been written on the short-run impacts of the EITC on children. Dahl and Lochner (2012) use changes to the EITC to estimate the effect of a family's current, past year of, and past two years of income on a child's test scores. The authors, using the assumption that changes in the EITC's eligibility structure are independent of individual family circumstances, instrument for changes in total income using predicted changes in EITC income as a function of lagged pretax income and changes in the EITC schedule. This instrumental variable strategy suggests that a $1,000 increase in income raises combined math and reading test scores by 6 percent of a standard deviation, a larger estimate than previous literature and simpler strategies suggested. The authors predict a larger effect for children in more disadvantaged families.

These results suggest that contemporaneous family income is an important factor in determining children's cognitive outcomes. This result is important to consider when discussing intergenerational effect. Suppose these scores can predict higher incomes once the children reach adulthood and enter the labor market, as literature discussed later suggests. Then, the EITC may create a self-reinforcing virtuous cycle of higher incomes and educational or cognitive ability.

Short-run impacts are especially important to consider at pivotal moments that are likely to lead to long-lasting effects, such as a child's prenatal development. In a working paper, Baker (2008) looks at the EITC's effect on birth weight, a common proxy for a newborn's general health. Baker exploits the EITC's 1993 expansion using a difference-in-differences identification strategy. That expansion greatly increased credits to families with two or more children compared to families without children. Baker uses mothers giving birth to their first child as the control group and mothers giving birth to their third or subsequent child as the treatment group. This strategy is refined into a triple difference strategy by using whether or not a mother has a high school degree as a proxy for EITC-eligibility. The estimate of the expansion's impact in the former strategy is an average treatment effect of 7.3 grams, while it is 13.8 grams for the latter strategy. These estimates provide evidence that an increase in income through the EITC can have a small positive effect on prenatal development and infant health.

One of the most significant choices in a person's life is whether to enroll in post-secondary education. Manoli and Turner (2018) examine the impact of "cash-on-hand" on college enrollment decisions by students from low-income families. The authors use a regression kink design to exploit kinks in the EITC benefit schedule. At the first kink point, the authors find an additional $1,000 of after-tax income received in the spring of a student's senior year of high school increases college enrollment by 1.3 percentage points. The authors find much lower estimates for junior year, though this estimate is too imprecise to conclude it is smaller. This suggests liquidity constraints may influence a student's decision to enroll in college. A cash transfer near high school graduation may therefore induce a student to enroll, potentially leading to long-run benefits.

Long-Run Impacts

Empirical evidence suggests the short-run benefits from anti-poverty policies last, or even increase in the long-run. First, consider whether early childhood investments can persist into adolescence. Dynarski et al. (2011) examine the impact of Project STAR, which randomly assigned smaller primary school classes, on college enrollment and completion. Exploiting randomization, the authors find that assignment to a smaller class increases the probability of college enrollment before age 30 by 3 percentage points. While Project STAR is a change to the structure of education, rather than a cash transfer, this paper provides evidence that improvements to primary education can lead to improvements in post-secondary outcomes. The authors also discuss that STAR's impact on contemporaneous test scores is a strong predictor of STAR's impact on college attendance. This result justifies continuing to treat childhood test scores as an informative outcome.

Chetty et al. (2011) continue investigating the link between test scores and long-run outcomes, but now directly in the context of the EITC with the Child Tax Credit. The authors exploit the "highly non-linear" schedules of the tax credits in contrast to other determinants of a child's academic achievement that change smoothly over the income distribution. The authors' estimate with their primary specification that a $1,000 increase in income increases test scores by 8 percent of a standard deviation, or 9.3 percent for math scores and 6.2 percent for reading scores. Next, the authors look to the link between scores and earnings. Here, the authors use a measure of teacher quality as an instrument for changes in students' test scores. The authors estimate that a 1 SD increase in test scores increases lifetime earnings by 9 percentage points. Using a conservative estimate for the EITC's impact on test scores, the authors translate this to a $1,000 tax credit raising lifetime earnings by .54 percentage points. Therefore, the EITC's impact on test scores can be predictive of a substantial increase in lifetime earnings.

Maxfield (2015) continues to examine the short- and long-run impacts of the EITC on children's educational outcomes. For identification, Maxfield uses variation in the maximum EITC credit possible for a family with a given number of children, in a given state and year. The author compares this to a difference-in-differences specification taking children in families with two or more children residing in states with their own EITCs as the treatment. The author finds similar effects on contemporaneous test scores, estimating that a \$1,000 increase in maximum potential EITC credit increases math scores by 0.072 SD and reading by 0.039 SD. In longer-run outcomes for children, Maxfield finds that a \$1,000 EITC expansion increases the probability of high school diploma or GED receipt by 2.1 percentage points and completing at least one year of college by 1.4 percentage points. Maxfield therefore builds upon the wealth of evidence suggesting lasting educational gains from the EITC.

Bastian and Michelmore (2018) further build upon this evidence. Like Chetty et al. (2011), the authors also examine labor market outcomes. Potentially translating educational gains into increased earnings is a important potential mechanism through which the EITC may have an intergenerational effect. The authors use a similar identification strategy to Maxfield (2015), creating a measure of exogenous EITC exposure using the maximum possible credit a child's household could receive given state and number of children, between the child's birth and the tax year they turn 19. The authors estimate that a \$1,000 increase in EITC exposure between ages 13 and 18 leads to a \$564 increase in annual earnings during adulthood. This paper therefore provides evidence that the EITC is successful at improving adult outcomes for children in families that received the EITC. This paper uses a similar methodology and dataset.

Intergenerational Transfer of Positive Outcomes

Using the evidence available, one could construct a narrative of how the EITC's impact may last for generations. If the credit indeed improves a child's test scores and educational attainment, and if that education improves labor market outcomes, then the EITC would also raise family income for the children of those who benefited directly. This increase would likely yield similar educational benefits. However, to my knowledge, no research has studied this narrative empirically.

Some research, however, has investigated the intergenerational impact of other anti-poverty programs that target children and their education. Barr and Gibbs (2017) examine Head Start, which, at its implementation in the 1960s, was an early childhood program promoting education, health, and community development. The authors leverage the differential exposure to Head Start among mothers born between 1960 and 1964, which occurred due to a staggered rollout and geographic variation in grant-writing assistance. Using the NLSY to link these mothers to their children, they find evidence for second-generation reductions in teen pregnancy and crime, as well as increases in educational attainment. This suggests that programs which benefit childhood education can indeed lead to positive intergenerational outcomes, potentially beginning a path out from the cycle of poverty. Investigation of the EITC in particular is warranted.

III. Data

III.1. Construction of Data

Data come from the Panel Study of Income Dynamics (PSID), its Child Development Supplement (CDS), and Bastian and Michelmore (2018). The PSID is a nationally representative survey which has been following its original cohort of households and their descendants since 1968 (PSID 2021). Interviews were conducted annually until 1997, and have since been conducted biennially. Each household newly added to the PSID is given a unique identifier, called the 1968 Family Interview Number (regardless of the year), which is carried on by all its members' descendants and those who join the original household or a descendant's household. Among each new household and its descendants each member is a given a unique Person Number, so that individuals in the PSID are uniquely identified by the two identifiers in combination (henceforth, "unique IDs").

The PSID also provides the unique IDs of an individual's parents. These unique IDs are used to link children in the CDS to their parent whose childhood is observed in Bastian and Michelmore's dataset, which was created from the PSID and the authors' own calculations. Each child in the sample only has one parent observed in Bastian and Michelmore's data, which is therefore merged into data directly from the CDS and PSID on the observed parent's unique ID.

Educational Outcomes

Educational outcomes come from the 1997, 2002, 2007, and 2014 waves of the CDS. The primary outcomes of interest are the standardized scores of children for the broad reading and applied problems (mathematics) Woodcock-Johnson subtests. These scores were collected in all waves of the CDS, and are nationally standardized by age with a mean of 100 and a standard deviation of 15 (Jaffe 2009). As an alternative to these scores, I also use a measure of the child's self-assessed math and reading ability. This measure takes a child's average response to a series of questions asking them to rate their interest or success in math and reading on a scale of 1–7 or 1–5. Additional outcomes include indicator variables for whether or not a child has ever been enrolled in a gifted program or received a disciplinary infraction resulting in suspension or expulsion.

Demographic Controls

Demographic information on the children come directly from the PSID, while that on their observed parent comes from from Bastian and Michelmore (2018). Variables directly from the PSID include the CDS wave during which the observations were taken, the number of children in a child's family unit, as well as a child's age, gender, and state of residence during that year. During the 2002, 2007, and 2014 waves of the CDS, age was measured in months, which I divide by twelve to create an age variable compatible with the 1997 wave. From Bastian and Michelmore (2018), I use indicators for the observed parent's race, gender, state of residence, number of siblings, and birth year. I also adapt indicators for the grandparents' degree attainment, college attendance, and marital status. All observations from Bastian and Michelmore are taken from the year a parent turned 18, if available, or age 19 to account for the biennial nature of the later PSID waves.

Economic Data

Data on parental childhood EITC exposure were created by Bastian and Michelmore (2018) using data from the PSID containing the parent's state of residence, the year of observation, and number of siblings. The data also include the childhood family income of the observed parent, including EITC benefits which were imputed by Bastian and Michelmore (2018) using family income, marital status, and the aforementioned determinants of the tax credit for which a household was eligible. Exposure and income are measured in each year, then summed over the age ranges of 0 to 18, 0 to 5, 6 to 12, and 13 to 18. Also adapted from Bastian and Michelmore are data on the GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average college tuition of the observed parent's state of residence during the year they turned 18 (or 19). I obtain data on family income during the CDS wave years directly from the PSID and calculate its value in terms of 2013 dollars using the

Consumer Price Index.

III.2. Sample

The total sample is all children with both observed Woodcock-Johnson test scores for whom we can observe all three age ranges of parental EITC exposure, excluding those who scored below 50 or above 150 on either test and those from 13 states from which samples were too small to adequately estimate a fixed effect. Excluded states include Alaska, Delaware, Hawaii, Idaho, Maine, Montana, Nevada, New Hampshire, New Mexico, North Dakota, Rhode Island, Vermont, and Wyoming. This sample includes 1,590 unique children and, taking each year with an observed test score as a separate observation, 2,477 observations. Summary statistics are presented in Table I. Averages and standard deviations are shown for each presented variable, both in the total sample and the unique sample. Sample weights come from the CDS. Weights used are child-level weights for outcomes from the CDS-I, -II, or -III, and child in-home weights for outcomes from the CDS-2014. These weights are used per the recommendation of the CDS-III and CDS-2014 user guide (CDS-III 2012; Fomby and Sastry 2017). All dollar values are adjusted for inflation and reported as thousands of 2013 dollars.

In the full sample, the weighted average total parental EITC exposure is $20,960, with a standard deviation of $10,820. This is very similar to the weighted average sample of unique observations, $22,600. The distribution of total parental EITC exposure is presented in figure 1. Three spikes in density between $10,000-$12,000, $14,000-$16,000, and $18,000-$20,000 are caused by older cohorts of parents experiencing less variation by state and family size. Table I suggests that parents generally experience more EITC

Table I. Summary Statistics

Sample	Total Sample			Unique Observations		
Variable	Mean	S.D.	N	Mean	S.D.	N
Parental EITC Exposure (0-5)	2.77	3.66	2477	3.20	3.89	1590
Parental EITC Exposure (6-12)	8.24	4.68	2477	8.87	5.15	1590
Parental EITC Exposure (13-18)	9.97	4.36	2477	10.55	4.90	1590
Total Parental EITC Exposure	20.96	10.82	2477	22.60	11.94	1590
Parental Family Income (0-5)	199.70	145.90	2477	200.53	145.07	1590
Parental Family Income (6-12)	358.51	258.33	2477	348.73	251.81	1590
Parental Family Income (13-18)	308.99	252.70	2477	302.65	245.00	1590
Age (Child)	11.74	3.40	2477	13.07	3.11	1590
Parent Birth Year	1969.97	6.06	2477	1970.87	6.60	1590
Female (Child)	0.48	0.50	2477	0.48	0.50	1590
Female (Parent)	0.72	0.45	2477	0.72	0.45	1590
Black	0.22	0.41	2477	0.21	0.41	1590
Std. Reading Score	103.69	15.57	2477	102.89	15.44	1590
Std. Math Score	104.39	15.54	2477	104.59	15.12	1590
Enrolled in Gifted Program	0.28	0.45	2183	0.34	0.47	1313
Suspended or Expelled	0.13	0.34	2434	0.14	0.35	1564
Self-Assessed Ability (Math)	4.61	1.09	1999	4.43	1.10	1430
Self-Assessed Ability (Reading)	4.85	1.14	2000	4.70	1.15	1431

Source. Panel Study of Income Dynamics (PSID); Bastian and Michelmore (2018).
Note. All measures of parental EITC exposure and parental family income are given in thousands of 2013 dollars. Female, Black, Enrolled in Gifted Program, and Suspended or Expelled are measured in percentages.

exposure later in their childhood, with weighted averages of parental exposure being \$2,770, \$8,240, and \$9,970 over the 0-5, 6-12, and 13-18 age ranges, respectively. This is caused by the EITC being progressively expanded over time, both federally and at the state level, since its introduction. Note that the 6-12 age range is over 7 years rather than 6, so the smaller gap between the latter averages is partially caused by the extra year.

The mean birth year of observed parents in the total sample is 1970, which is 5 years prior to the enactment of the EITC in 1975. Because of this, 1297 observations, or 52% of the total sample, have an observed parental EITC exposure of \$0 in the 0-5 age range. Note also that, while the total sample gives somewhat more weight to older children, (i.e. those who could be in the CDS sample in more waves) the mean birth year of observed parents with only unique observations only differs from that in the total sample by .9 years. The difference in the children's average age between the total and unique-only sample, 11.74 versus 13.07, is due to the unique sample keeping only the child's latest observation. While in both samples, the gender distribution is fairly even (48% female) for the children included in the sample, it is also much more common for the observed parent to be a mother (72%) than father. This suggests that, among people who were children included in early waves of the PSID or who were born into a PSID household, women are much more likely to either have or reside with children in adulthood. Also reflected in demographic averages is that the PSID over-samples Black respondents, which is conducive to subsample analyses. Unweighted, the observed parent of 49.6% of the sample is Black, a proportion which falls to 22% when weighted. While the current makeup of PSID respondents is more diverse, the earlier waves of the PSID which these data rely on for intergenerational trends do not have large enough samples of ethnic and racial identities other than Black or White to make meaningful statements about the impact of the EITC on such populations.

Both the reading and math standardized Woodcock-Johnson scores have averages and standard deviations slightly above the nationally standardized 100 point mean and 15 point standard deviation. Math scores have a 103.7 point mean and 15.6 point standard deviation, while the reading scores have 104.4 and 15.5. Regarding the binary outcomes, the summary statistics shows that in the weighted total sample, by the time of the interview, 28% of children had ever been in a gifted program and 13% had ever been

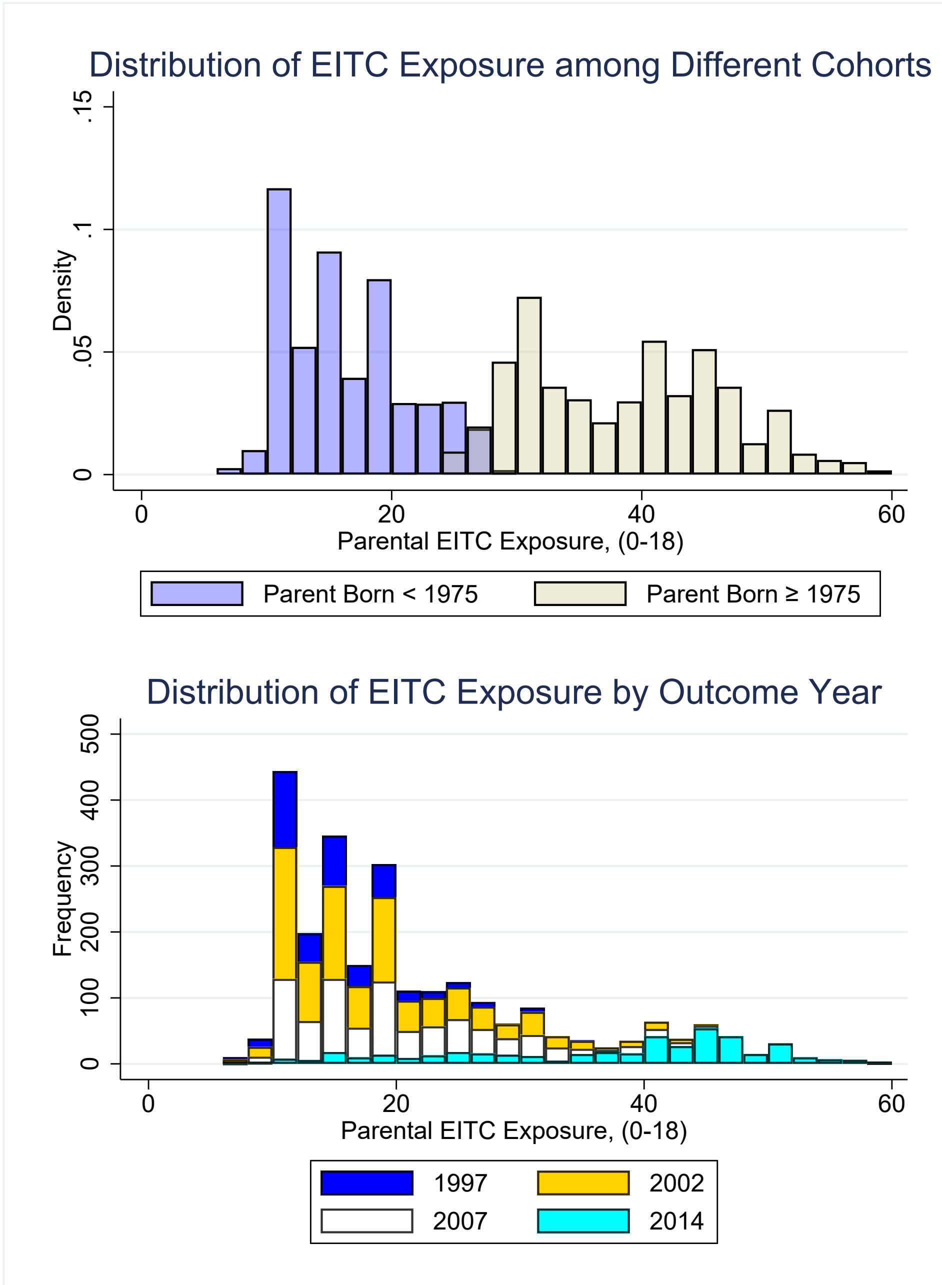

Fig. 1. Distribution of parental EITC exposure by parent age and outcome year. All measures of parental EITC exposure are given in thousands of 2013 dollars. Source: PSID, Bastian and Michelmore (2018).

suspended or expelled. These percentages increase to 34% and 14% percent in the unique-only sample, possibly due to the fact that the unique-only sample is older for reasons described above. The means (4.61 and 4.85) and standard deviations (1.09 and 1.14) of the math and reading self-assessed ability variables (respectively) are difficult to interpret on their own, and will be used primarily to measure relative effects.

IV. Empirical Method

IV.1. Reduced-Form Model

I adapt a measure of EITC exposure from Bastian and Michelmore (2018) to a multigenerational setting. If a child in the CDS sample has a parent whose childhood household was observed, then I define parental EITC exposure as the maximum potential federal and state credit the parent's childhood family could receive, given their state of residence, family size, and tax year, independent of own family income or parental marital status. This value is then summed from the parent's birth to the year they turn 18 or the last year they reside in the grandparents' household, whichever is first.

The aim of using parental EITC exposure is to capture exogenous variation in the EITC benefits a parent's household received in childhood. Actual variation in EITC benefits is determined by variation in income, so children of parents whose childhood household received higher benefits are likely to be children of parents who faced economic hardship growing up. This is particularly problematic as one of the principal ways in which a parent's childhood EITC receipt may impact their child's educational outcomes is through changes in the parent's childhood consumption levels. Using parental EITC exposure alleviates this issue by relying on plausibly exogenous policy changes as the main source of variation. Estimating the reduced-form impact of parental EITC exposure on a child's educational outcomes therefore can provide an estimate of the intergenerational impact of changes in the EITC.

I model the reduced-form impact of EITC exposure on a range of educational outcomes of one's children:

$$Y_{it} = \alpha + \beta \text{EXP}_{i,(0-18)} + \gamma_1 \mathbf{X}_i + \gamma_2 \mathbf{V}_{s,p} + \gamma_3 \mathbf{Z}_s + \gamma_4 \mathbf{W}_p + \gamma_5 \mathbf{U}_t + \epsilon_{it} \quad (1)$$

where i indexes children of exposed parents, t indicates the year the educational outcome was observed, s indexes the parents' states of residence at 18, and p indexes parents' birth years. The educational outcome of interest is represented by Y_{it}. The coefficient of interest is β, which represents the impact of an additional \$1,000 of parental EITC exposure on subsequent educational outcomes. Taking multiple observations from the same child, or even the same parent, would clearly violate an assumption that outcomes are independently and identically distributed. To account for this, standard errors will be clustered by the parent whose exposure is observed.

The term $\mathbf{X}_i$ represents a vector of personal characteristics, such as age, race, parent and child gender, the observed parent's number of siblings, and the observed grandparents' educational attainment. The term $\mathbf{V}_{s,p}$ represents state-by-parent-birth-year variables measuring the economic prosperity and government generosity of the state the observed parent resided in during the year they turned 18. These variables include quadratic trends in each state to account for other unobserved changes in a state over time. The terms $\mathbf{Z}_s$, $\mathbf{W}_p$ and $\mathbf{U}_t$ represent fixed effects for the state in which the observed parent resided in at 18, a parent's birth year, and the year the educational outcome was observed, respectively.

The age at which the parent is exposed to the EITC is also of interest. Children of different ages may be differentially impacted by exogenous changes in EITC generosity. One possible hypothesis is that changes to a young child's levels of consumption impacts their education and development in ways that will affect their future children's academic preparedness. Another possibility is that the credit restraints of a high school student's family impacts their job market and higher education decisions, which will affect their future children's education through changes in family income. Therefore, following Bastian and Michelmore (2018), I also measure exposure over 3 age intervals: 0-5, 6-12, and 13-18. Therefore, I alternatively model the reduced-form impact of EITC exposure during these age intervals on a range of educational outcomes of one's children:

$$\begin{aligned} Y_{it} = &\alpha + \beta_1 \text{EXP}_{i,(0-5)} + \beta_2 \text{EXP}_{i,(6-12)} + \beta_3 \text{EXP}_{i,(13-18)} \\ &+ \gamma_1 \mathbf{X}_i + \gamma_2 \mathbf{V}_{s,p} + \gamma_3 \mathbf{Z}_s + \gamma_4 \mathbf{W}_p + \gamma_5 \mathbf{U}_t + \epsilon_{it} \end{aligned} \tag{2}$$

where the coefficients of interest are now $\beta_1, \beta_2, \beta_3$, representing the impact of an additional \$1,000 of parental EITC exposure when the parent is 0-5, 6-12, and 13-18, respectively, on the child's subsequent educational outcomes.

IV.2. Instrumental Variables Model

I next implement an instrumental variables strategy to estimate the impact of an exogenous increase in a parent's childhood family income using parental exposure as an instrument. A necessary assumption for exposure to be a good instrument is that parental

EITC exposure impacts the child's educational outcome only through exposure's impact on the observed parent's childhood household income. This impact may either be through direct benefits or incentivizing work. The latter possibility raises the threat to this assumption posed by the EITC's distortion of labor market decisions. Parental EITC exposure may induce a grandparent to work, which could result in less time spent raising the parent. This becomes a threat if this decrease causes the parent to be less equipped to support their own children academically. Because it still reflects the effects of the EITC, this would still yield useful estimates but complicates interpretation. Other relationships between the determinants of exposure (state generosity, family size, a parent's birth year) and the outcomes of interest remain of greater concern.

To investigate, and again following Bastian and Michelmore (2018), I measure exposure and received credit over the same three age intervals used in the reduced-form specification. Therefore I model three different first-stage equations for each respective income interval:

$$\begin{aligned} \text{INC}_{i,a} = \alpha_a + \beta_{1,a}\text{EXP}_{i,(0-5)} + \beta_{2,a}\text{EXP}_{i,(6-12)} + \beta_{3,a}\text{EXP}_{i,(13-18)} \\ + \gamma_{1,a}\mathbf{X}_i + \gamma_{2,a}\mathbf{V}_{s,p} + \gamma_{3,a}\mathbf{Z}_s + \gamma_{4,a}\mathbf{W}_p + \gamma_{5,a}\mathbf{U}_t + \epsilon_{i,a} \end{aligned} \tag{3}$$

where $\text{INC}_{i,a}$ represents a parent's childhood family income summed over the age ranges a (0-5, 6-12, and 13-18) and is modeled as a function of parental EITC exposure at each of those age intervals. This yields nine coefficients of interest $\beta_{n,a}$ when evaluating whether this equation is a strong first stage. It is important to note that, because EITC-induced changes in labor market decisions may result in wage increases that accrue over time due to increased experience, exposure in earlier age ranges may affect income in the later age ranges.

I then use the three different estimated $\hat{\text{INC}}_{i,a}$ functions to model the impact of a \$1,000 increase in a parent's childhood family income over each age range on their child's educational outcomes. This yields the following single second stage equation:

$$\begin{aligned} Y_{it} = \alpha + \beta_1 \hat{\text{INC}}_{i,(0-5)} + \beta_2 \hat{\text{INC}}_{i,(6-12)} + \beta_3 \hat{\text{INC}}_{i,(13-18)} \\ + \gamma_1 \mathbf{X}_i + \gamma_2 \mathbf{V}_{s,p} + \gamma_3 \mathbf{Z}_s + \gamma_4 \mathbf{W}_p + \gamma_5 \mathbf{U}_t + \epsilon_{it} \end{aligned} \tag{4}$$

which, as in the reduced-form model using these age ranges, gives three coefficients of interest $\beta_1, \beta_2, \beta_3$.

V. Results

The results presented in Tables II, III, and IV come from four different specifications which each gain an additional set of controls from the previous. The basic set of controls include fixed effects for the observed parent's number of siblings, state of residence at 18, and birth year, as well as the child's age, gender, and the year of the observed outcome. Included in the next set of controls are the observed parent's race and gender and observed grandparents' education and marital status, referred to as the demographic controls. Then I include economic controls for the GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average college tuition of a parent's state of residence at 18 or 19. The final addition to the equation are state-time quadratic trends. Results are weighted by child-level (CDS-I, -II, -III) and child in-home weights (CDS-2014) from the CDS, and standard errors are clustered by observed parent.

V.1. Reduced-Form Results

Results estimating the impact of a \$1,000 increase in a parent's total EITC exposure are presented in Table IIa. The coefficients suggest an ambiguous effect of EITC exposure on all outcomes. Standard errors are large, and the sign of the coefficient is inconsistent across different specifications for Woodcock-Johnson reading scores, participation in a gifted program, and suspension. The full specification does suggest an additional \$1,000 of exposure increases self-assessed reading ability by 0.0357, or 3.1% of a standard deviation. However, this estimate is only significant at the 10% level, and is not significant at other specifications.

One possible explanation for the lack of an effect, besides an actual lack of a causal effect, is that the inclusion of parents who were exposed to but whose childhood household did not qualify for the EITC attenuates the coefficients toward zero. To investigate this possibility, I restrict the sample by placing an upper bound on the income of the observed parent's household at 18, which I successively increase by \$10,000, and run the full-control specification on each Woodcock-Johnson score for each upper bound. If parental EITC exposure accurately reflects the received EITC low-income families, then at the lowest upper bounds the coefficients will represent the impact of a parent's childhood household's eligibility for an additional \$1000 from the EITC given that the household is low-income. As the upper bound is increased, estimates should become more precise but attenuate toward zero. One possible threat to this is that, as will be shown in Table III, parental EITC exposure is positively predictive of parental childhood family income.

Table IIa. Effect of Parental EITC Exposure on Educational Outcomes (Reduced-Form)

Variable	**Dependent Variable**					
	WJ Reading	WJ Math	Gifted	Suspend	Self. Math.	Self. Read.
	A. State, Year, Par. Cohort, Sibling FE & Child Age					
Par. EITC exp., 0–18	-0.0031	0.3609	0.0046	-0.0010	0.0243	0.0092
	(0.3609)	(0.3334)	(0.0077)	(0.0063)	(0.0153)	(0.0178)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2034	0.2182	0.1836	0.1918	0.3754	0.3336
	B. Fixed Effects and Demographic Controls					
Par. EITC exp., 0–18	-0.0620	0.3542	0.0041	-0.0024	0.0216	0.0081
	(0.3283)	(0.3136)	(0.0074)	(0.0054)	(0.0159)	(0.0158)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2925	0.3050	0.2197	0.2488	0.3885	0.3549
	C. FE, Demographic and Economic Controls					
Par. EITC exp., 0–18	-0.0547	0.2844	-0.0040	0.0005	0.0270	0.0078
	(0.3089)	(0.2765)	(0.0079)	(0.0054)	(0.0191)	(0.0175)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2978	0.3075	0.2288	0.2530	0.3906	0.3608
	D. FE, Demographic and Economic Controls, State-Trends					
Par. EITC exp., 0–18	0.2179	0.2379	0.0027	-0.0046	0.0248	0.0357*
	(0.3514)	(0.2759)	(0.0095)	(0.0057)	(0.0219)	(0.0215)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.3501	0.3643	0.2902	0.3086	0.4252	0.4107

Note. Exposure is measured in thousands of 2013 dollars. Results reflect estimations of equation (2). Regressions in (A.) include fixed effects for parent siblings, state, and birth year, and child age, gender, and outcome year. In (B.), included are parent gender, race, and grandparent education and marital status. (C.) adds controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average tuition. (D.) adds quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

Selection into a higher income decile by age 18 is therefore endogenous, and therefore exposure in earlier periods of the parent's childhood may be more predictive of actual benefits than their income decile suggests. Coefficients on total exposure and their 95% confidence intervals are presented in Figure 2. Results remain similarly inconclusive, with no income group experiencing an impact significantly different from zero.

Another possibility is that the sign of the impact of EITC exposure depends on the

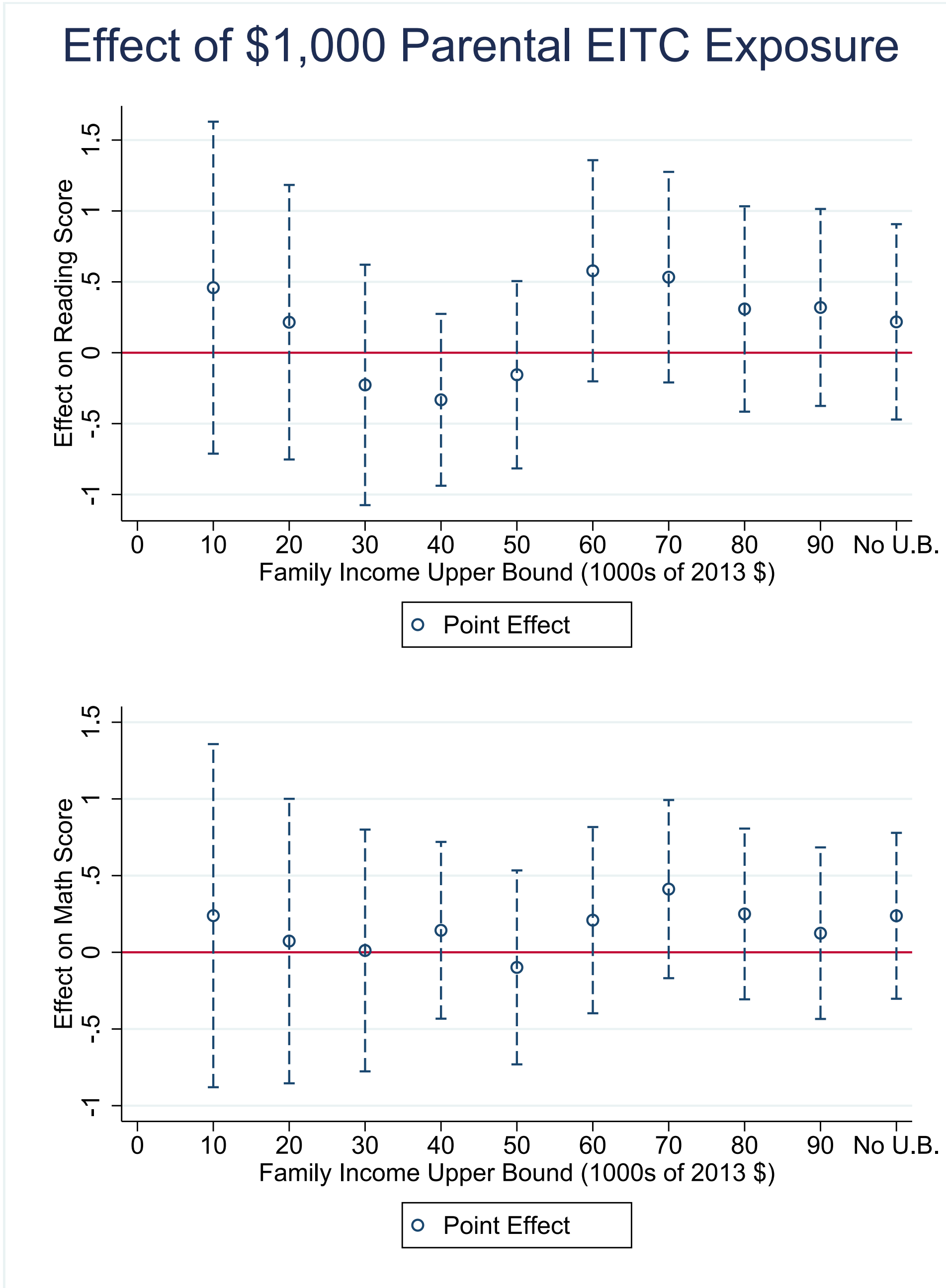

Fig. 2. Effect of additional $1000 of total parental EITC exposure by parental family income at 18, with 95% confidence intervals shown. Results reflect equation (1). Source: PSID, Bastian and Michelmore (2018).

ages during which the parent was exposed, causing overall exposure to have a null effect. As described in Section 4, I run the previously used specifications using parental EITC exposure measured over the 0-5, 6-12, and 13-18 age ranges in place of total parental exposure. Results are presented in Table IIb.

Standard errors are again too large to make conclusive claims about the direction of parental EITC exposure's impact on most of the considered outcomes. There is some evidence that parental exposure in the 0-5 age range has a positive impact on Woodcock-Johnson math scores, as there is a positive coefficient significant at the 5%-10% level in the first three specifications. While these coefficients suggest that a $1,000 increase in parental exposure during ages 0-5 leads to a 1.26-1.08 point increase (8.4%-7.2% of a standard deviation), this effect is diminished with the addition of state quadratic trends. This suggests it is caused in part due to changes in certain states over time. The 4th specification does include two significant estimates at the 10% level. These estimates suggest that a $1,000 increase in parental exposure during the 6-12 age range leads to a 6.9% standard deviation increase in self-assessed math ability. During the 13-18 range, the same increase in exposure leads to a 4.5% standard deviation increase in self-assessed reading ability. That these coefficients still have relatively high p-values and are not significant in other specifications calls into question their strength as evidence.

As considered with total parental EITC exposure, the inclusion of children whose observed parent grew up in a high-income family may obfuscate the impact of exposure. I therefore apply the same upper bounds as in Figure 3, and present coefficients on exposure in each age range and their 95% confidence intervals for each Woodcock-Johnson standardized score in Figures 4 and 5. Coefficients on exposure during ages 0-5 and 13-18

Table IIb. Effect of Parental EITC Exposure on Educational Outcomes (Reduced-Form)

Variable	**Dependent Variable**					
	WJ Reading	WJ Math	Gifted	Suspend	Self. Math.	Self. Read.
	A. State, Year, Par. Cohort, Sibling FE & Child Age					
Par. EITC exp., 0–5	-0.9087	1.2563**	-0.0070	-0.0180	0.0282	-0.0298
	(0.5776)	(0.6050)	(0.0256)	(0.0159)	(0.0392)	(0.0438)
Par. EITC exp., 6–12	-0.1560	-0.4250	0.0130	-0.0068	0.0071	-0.0357
	(0.4027)	(0.3896)	(0.0164)	(0.0126)	(0.0323)	(0.0273)
Par. EITC exp., 13–18	0.5166	0.2136	0.0053	0.0113	0.0269	0.0473
	(0.5443)	(0.4910)	(0.0130)	(0.0104)	(0.0271)	(0.0335)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2053	0.2196	0.1837	0.1937	0.3753	0.3353
	B. Fixed Effects and Demographic Controls					
Par. EITC exp., 0–5	-1.0687*	1.0872*	0.0006	-0.0124	0.0365	-0.0172
	(0.6051)	(0.5996)	(0.0266)	(0.0149)	(0.0385)	(0.0433)
Par. EITC exp., 6–12	-0.0152	-0.2181	0.0157	-0.0068	0.0133	-0.0283
	(0.3785)	(0.4196)	(0.0166)	(0.0121)	(0.0307)	(0.0276)
Par. EITC exp., 13–18	0.3842	0.1508	0.0001	0.0053	0.0147	0.0362
	(0.5367)	(0.4809)	(0.0124)	(0.0093)	(0.0275)	(0.0307)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2944	0.3058	0.2199	0.2495	0.3883	0.3558
	C. FE, Demographic and Economic Controls					
Par. EITC exp., 0–5	-0.9100	1.0773*	-0.0286	-0.0075	0.0544	-0.0052
	(0.5591)	(0.5546)	(0.0261)	(0.0160)	(0.0429)	(0.0468)
Par. EITC exp., 6–12	-0.0193	-0.1792	0.0243	-0.0070	0.0121	-0.0317
	(0.3697)	(0.4257)	(0.0160)	(0.0121)	(0.0310)	(0.0279)
Par. EITC exp., 13–18	0.2790	0.0174	-0.0074	0.0087	0.0149	0.0307
	(0.5166)	(0.4566)	(0.0123)	(0.0085)	(0.0291)	(0.0296)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.2990	0.3084	0.2298	0.2538	0.3905	0.3615
	D. FE, Demographic and Economic Controls, State-Trends					
Par. EITC exp., 0–5	-1.1390	0.7995	-0.0476	-0.0221	0.0900	0.0283
	(0.7387)	(0.6987)	(0.0289)	(0.0158)	(0.0629)	(0.0664)
Par. EITC exp., 6–12	-0.2585	-0.3828	0.0095	-0.0232	0.0759*	-0.0212
	(0.5364)	(0.4879)	(0.0216)	(0.0157)	(0.0418)	(0.0408)
Par. EITC exp., 13–18	0.9318	0.3133	0.0118	0.0108	-0.0344	0.0672*
	(0.5873)	(0.4729)	(0.0138)	(0.0079)	(0.0357)	(0.0375)
Observations	2,477	2,477	2,183	2,434	1,999	2,000
R^2	0.3525	0.3648	0.2911	0.3108	0.4268	0.4116

Note. Exposure is measured in thousands of 2013 dollars. Results reflect estimations of equation (2). Regressions in (A.) include fixed effects for parent siblings, state, and birth year, and child age, gender, and outcome year. In (B.), included are parent gender, race, and grandparent education and marital status. (C.) adds controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average tuition. (D.) adds quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

do not estimate any significant effect on math and reading scores, respectively.

For regressions on reading scores, using the lowest set of income upper bounds yields coefficients on parental EITC exposure that are significant and negative during ages 0-5 but significant and positive during ages 6-12. Due to the sample restriction, the drop in precision of these estimates for the 0-5 range is low enough that significance is very close to 5% for the $20,000 and $40,000 upper bounds. Coefficients for the 6-12 range and their confidence intervals follow more closely the pattern that would be expected if increases in the EITC indeed had a significant intergenerational impact. Coefficients begin positive, relatively large in magnitude, and significant despite the imprecision caused by the smaller sample. As the upper bound on the observed parent's family income at 18 is increased, coefficients become more precise and clearly trend toward zero. However, coefficients stop being significant starting with the $40,000 upper bound, a lower bound than for ages 0-5. One possible explanation for this difference is growth in family income over time. Patterns of coefficients for ages 13-18 do not suggest an effect, except that the coefficient is significant and positive for the $80,000 upper bound. This is likely spurious, as the bound is well above the maximum income which could receive EITC benefits.

For regressions on mathematics scores, patterns are less clear. For exposure in the 0-5 age range, coefficients are not significant except for those from the $70,000 and $80,000 upper bounds on family income. This is still likely to be spurious, but it is possible that families receiving an income of such magnitude had, 13-18 years prior, been eligible for the earned income tax credit. The pattern of coefficients for ages 6-12 is similar to those from regressions on reading scores, but a decrease in magnitude of the coefficients when using a $10,000 and $20,000 upper bound causes a drop in significance. Raising the upper

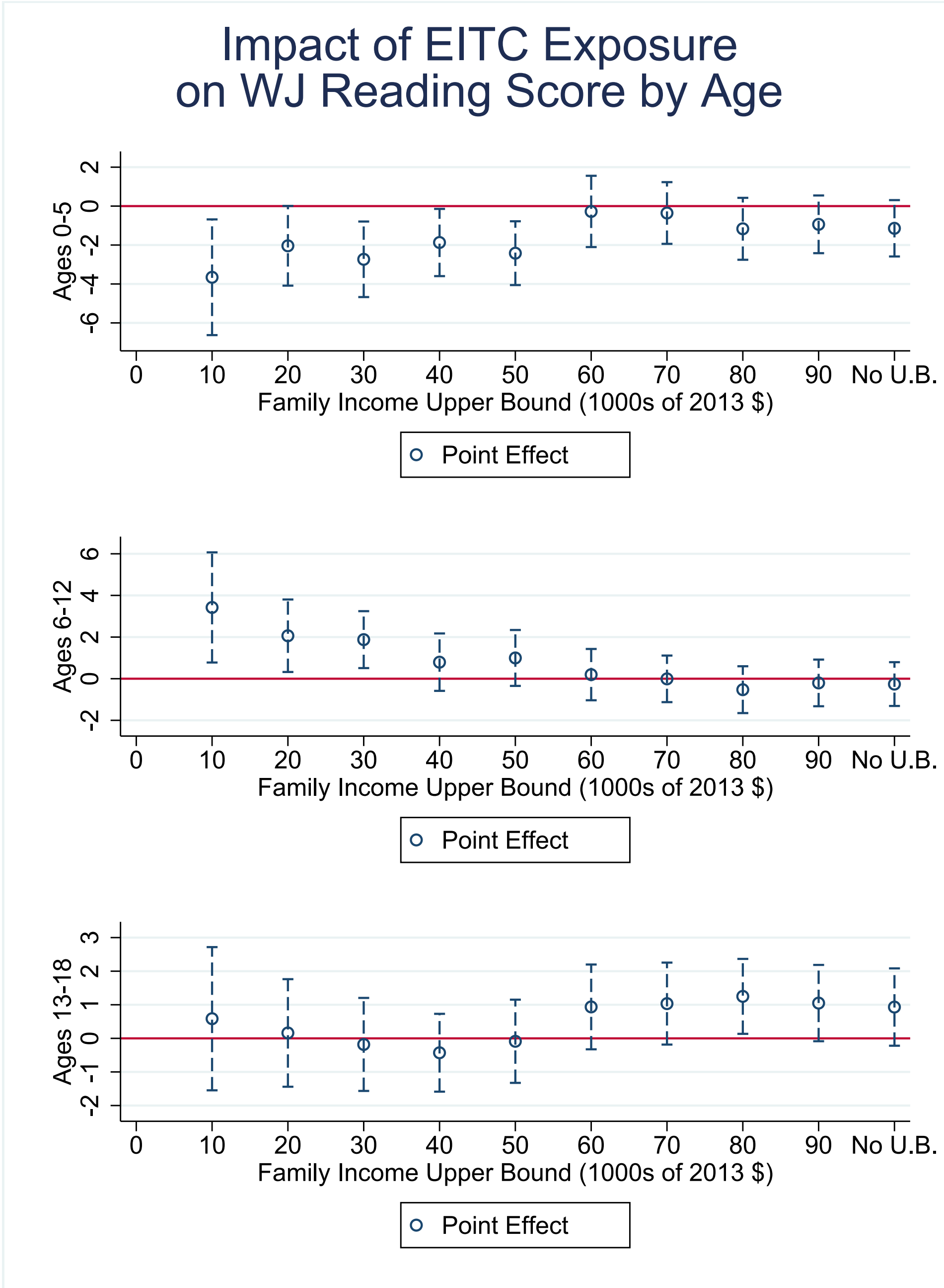

Fig. 3. Effect of additional \$1000 of parental EITC exposure in a particular age range by parental family income at 18, with 95% confidence intervals shown. Results reflect equation (2). Source: PSID, Bastian and Michelmore (2018).

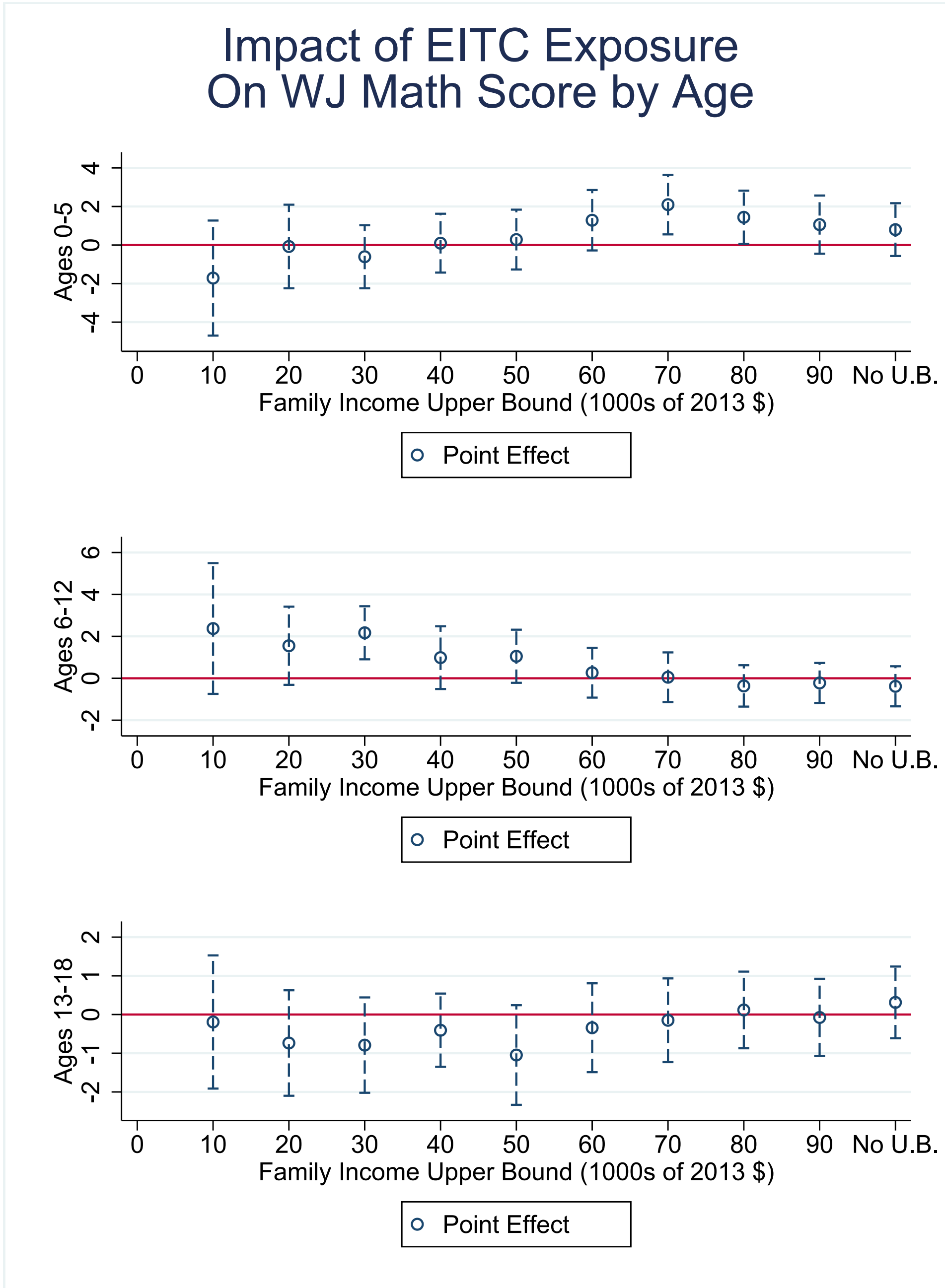

Fig. 4. Effect of additional $1000 of parental EITC exposure in a particular age range by parental family income at 18, with 95% confidence intervals shown. Results reflect equation (2). Source: PSID, Bastian and Michelmore (2018).

bound to \$30,000 yields a much more precise, positive, and significant coefficient that is close in magnitude to those for the first two bounds. Subsequent increases to the upper bound yield estimates that follow a pattern of decreasing magnitude and significance (despite greater precision) which closely resembles estimates from regressions on reading scores for the 6-12 age range. Coefficients on parental exposure during ages 13-18 do not suggest an effect.

V.2. IV Results

Table III presents estimates from the first stage regressions of a parent's childhood family income on parental EITC exposure. To prevent outliers skewing the results, children of parents whose family income (in real terms using 2013 dollars) summed over ages 13-18 was more than \$1,000,000 dollars (or \$166,667 annually) are excluded. Each age range of income is regressed on all age ranges of exposure, and therefore nine coefficients of interest are presented under each specification. The coefficients on exposure in the age ranges of 0-5 and 6-12 are large, positive, and significant for their respective age ranges, which would be expected if parental exposure is indeed predictive of parental childhood family income. Coefficients on any non-corresponding age range of exposure are not significant.

The estimates raise concern for the strength of the first stage. The last two rows display statistics testing for underidentification and weak identification, respectively. The statistics indicate that we cannot reject either null hypothesis, suggesting the instruments are weak. The primary cause appears to be to the inability of exposure to predict income in the 13-18 age range. In particular, for that range of income, the coefficient on exposure

Table III. First-Stage Est. (Effect of Par. EITC Exp. on Par. Fam. Inc., 13–18)

	(A)	(B)	(C)	(D)
	Par. Fam. Income (0-5)			
Par. EITC exp., 0–5	23.3055***	19.8693***	20.5348***	15.3991***
	(4.7122)	(5.4072)	(6.1979)	(5.6801)
Par. EITC exp., 6–12	-1.8158	-0.7973	-0.3984	0.5738
	(4.5528)	(4.6331)	(4.7852)	(5.1389)
Par. EITC exp., 13–18	1.3329	2.6220	5.2987	3.5865
	(3.8121)	(4.2045)	(4.0737)	(4.1909)
Observations	2,449	2,449	2,449	2,449
R^2	0.4569	0.5656	0.5815	0.6379
	Par. Fam. Income (6-12)			
Par. EITC exp., 0–5	-5.1091	-10.7316	-5.7329	-5.1510
	(8.2993)	(8.7382)	(9.8088)	(10.3116)
Par. EITC exp., 6–12	19.3417***	22.4565***	22.2857***	22.0854***
	(6.6470)	(7.6207)	(7.8772)	(7.9933)
Par. EITC exp., 13–18	-7.0957	-3.7300	4.7466	2.9340
	(7.1778)	(7.3670)	(7.1667)	(7.4583)
Observations	2,449	2,449	2,449	2,449
R^2	0.3264	0.4824	0.5128	0.5739
	Par. Fam. Income 13-18			
Par. EITC exp., 0–5	-4.7198	-9.6947	-7.4247	-3.2088
	(8.8055)	(9.8585)	(11.9121)	(11.3900)
Par. EITC exp., 6–12	2.4862	5.3275	5.0545	-0.9163
	(6.2266)	(7.3258)	(8.0872)	(7.5390)
Par. EITC exp., 13–18	1.6751	5.6765	11.0964*	9.5936
	(5.9934)	(6.5373)	(6.7100)	(6.7898)
Observations	2,449	2,449	2,449	2,449
R^2	0.2830	0.4173	0.4329	0.5057
Kleibergen-Paap rk LM statistic	0.164	1.034	3.254	1.942
Kleibergen-Paap rk Wald F-statistic	0.0533	0.335	1.027	0.614

Note. Exposure and income are measured in thousands of 2013 dollars. Results reflect estimations of equation (3). Regressions in (A.) include fixed effects for parent siblings, state, and birth year, and child age, gender, and outcome year. In (B.), included are parent gender, race, and grandparent education and marital status. (C.) adds controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average tuition. (D.) adds quadratic state trends. Source: PSID, Bastian and Michelmore (2018). ${}^{*}p < .1,{}^{**} p < .05,{}^{***} p < .01$.

during ages 13-18 is only significant in column **(C)** and even then is only significant at the 10% level. (The coefficient is however of a comparable magnitude to that found in Bastian and Michelmore (2018).) The specification corresponding to column **(C)** is also that with the largest pair of statistics testing identification, though it is still not possible to reject underidentification or weak identification. The lower predictability of income in the 13-18 age range is not unexpected. Not only is exposure in this age range much higher, but income growth over time may also have made many fewer families in the sample eligible for the EITC. Labor market decisions may also be more inelastic relative to the EITC in this period as households settle into careers.

There are further differences between the results of the first-stage under this paper's sample and that in Bastian and Michelmore. The estimates predict that an increase of $1000 in parental EITC exposure during the 6-12 age range leads to a $22,085 increase in total income over the same age range, or an increase of $3,155 annually. This is less plausible and much larger than the estimates found by Bastian and Michelmore (2018). It is not clear what causes this difference, but if taken as correct, it would suggest that (compared to Bastian and Michelmore's sample) an earlier-born sample which exclusively includes future parents is more likely to have grown up with parents whose labor market decisions are very responsive to changes in the EITC when their child is 6-12. This interpretation is questionable also due to the inclusion of several people who would not have been eligible in the sample.

Results from the second-stage equations are presented in Table IV. Coefficients represent the estimated impact of a $1,000 increase in parental family income summed over each age range. As would be expected because of the weak first stage, the magnitudes of most coefficients are too small and standard errors too large to make any conclusive statements about the impact of parental childhood family income on a child's educational outcome. The one coefficient of significance (at the 10% level) predicts that an increase in a parent's family income over ages 0-5 will increase their future child's math scores by

Table IV. Effect of Parental EITC Exposure on Educational Outcomes (IV)

Variable	WJ Reading	WJ Math	Gifted	Suspend	Self. Math.	Self. Read.
	Dependent Variable					
	A. State, Year, Par. Cohort, Sibling FE & Child Age					
Par. Family Inc., 0–5	-0.0040	0.0556	-0.0082	-0.0003	0.0019	-0.0010
	(0.0908)	(0.0456)	(0.2243)	(0.0016)	(0.0022)	(0.0026)
Par. Family Inc., 6–12	-0.0316	-0.0176	0.0083	-0.0008	-0.0019	-0.0038
	(0.0563)	(0.0227)	(0.2211)	(0.0010)	(0.0026)	(0.0024)
Par. Family Inc., 13–18	0.1983	0.0186	-0.0361	0.0033	0.0042	0.0057
	(0.4364)	(0.1945)	(0.9812)	(0.0077)	(0.0069)	(0.0068)
	B. Fixed Effects and Demographic Controls					
Par. Family Inc., 0–5	-0.0264	0.0524	0.0005	-0.0005	0.0025	-0.0006
	(0.0456)	(0.0392)	(0.0010)	(0.0008)	(0.0024)	(0.0025)
Par. Family Inc., 6–12	-0.0168	-0.0064	0.0002	-0.0005	-0.0006	-0.0027
	(0.0256)	(0.0198)	(0.0008)	(0.0005)	(0.0018)	(0.0017)
Par. Family Inc., 13–18	0.0720	-0.0010	0.0004	0.0009	0.0018	0.0038
	(0.0768)	(0.0643)	(0.0029)	(0.0015)	(0.0043)	(0.0042)
	C. FE, Demographic and Economic Controls					
Par. Family Inc., 0–5	-0.0309	0.0451*	-0.0008	-0.0001	0.0025	0.0000
	(0.0290)	(0.0272)	(0.0013)	(0.0007)	(0.0019)	(0.0023)
Par. Family Inc., 6–12	-0.0100	-0.0019	0.0010	-0.0006	-0.0002	-0.0026
	(0.0234)	(0.0218)	(0.0006)	(0.0006)	(0.0016)	(0.0016)
Par. Family Inc., 13–18	0.0442	-0.0200	-0.0009	0.0011	0.0005	0.0034
	(0.0445)	(0.0420)	(0.0020)	(0.0009)	(0.0029)	(0.0031)
	D. FE, Demographic and Economic Controls, State-Trends					
Par. Family Inc., 0–5	-0.0484	0.0520	-0.0033	-0.0013	0.0107	0.0010
	(0.0715)	(0.0431)	(0.0032)	(0.0013)	(0.0112)	(0.0096)
Par. Family Inc., 6–12	-0.0065	-0.0196	0.0010	-0.0009	0.0014	-0.0016
	(0.0388)	(0.0196)	(0.0010)	(0.0008)	(0.0041)	(0.0031)
Par. Family Inc., 13–18	0.1142	0.0148	0.0008	0.0019	-0.0073	0.0065
	(0.0846)	(0.0546)	(0.0012)	(0.0014)	(0.0087)	(0.0077)
Observations	2,449	2,449	2,155	2,406	1,980	1,981

Note. Exposure and income are measured in thousands of 2013 dollars. Results reflect estimations of equation (4). Regressions in (A.) include fixed effects for parent siblings, state, and birth year, and child age, gender, and outcome year. In (B.), included are parent gender, race, and grandparent education and marital status. (C.) adds controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, and average tuition. (D.) adds quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

.0451, which is only .3% of a standard deviation. This significance occurs under specification **C.**, which is expected as the specification with the strongest first-stage. Also recall that a significant positive coefficient on parental exposure during ages 0-5 was found for math scores with the first 3 specifications of the reduced-form model. The signs of coefficients on exposure or income in a specific age range and outcomes are consistent between the reduced-form and instrumental variables model, excepting a few small and imprecise estimates.

V.3. Subsamples

How the EITC impacts different groups within the population is crucial to understanding its long-run distributional effects. Table V presents results from three different subgroups using the reduced-form regression specification with the full set of controls. The three subgroups are children whose observed parent is Black, whose observed parent is female, and whose observed parent is male. These subgroups were chosen to investigate how a parent's race or gender affects the ways in which their material well-being in childhood influences their child's outcomes. Subgroups are used instead of interaction terms to allow all parameters of the model to vary by race and gender. To illustrate this justification, consider that the effect of living in a particular state in a particular year, as represented by fixed effects and quadratic trends, is likely to vary by race and gender due to different institutional legacies (e.g., slavery and Jim Crow in the South) and social movements (e.g., feminism) across regions and decades.

Now consider first the subsample of children whose observed parent is Black. Estimates suggest that an additional $1,000 of a Black parent's EITC exposure during ages 0-5 leads to a decrease of 2.65 points (17.7% of a standard deviation) in the Woodcock-Johnson reading scores of their children. This result is significant at the 1% level. This

Table V. Effect of Parental EITC Exposure on Subgroup Outcomes

Variable	Dependent Variable					
	WJ Reading	WJ Math	Gifted	Suspend	Self. Math.	Self. Read.
	A. Subsample of Black Observed Parents					
Par. EITC exp., 0–5	-2.6466***	0.2071	-0.0278	-0.0197	-0.0001	-0.0468
	(0.5862)	(0.5487)	(0.0364)	(0.0172)	(0.0589)	(0.0501)
Par. EITC exp., 6–12	0.3922	-0.3273	-0.0041	-0.0110	-0.0036	-0.0294
	(0.4884)	(0.5503)	(0.0204)	(0.0246)	(0.0283)	(0.0240)
Par. EITC exp., 13–18	0.2791	0.4529	0.0271	-0.0151	0.0277	0.0846*
	(0.8028)	(0.6896)	(0.0296)	(0.0205)	(0.0448)	(0.0513)
Observations	1,228	1,228	1,053	1,210	1,005	1,004
R^2	0.3686	0.2679	0.3102	0.3400	0.3819	0.3875
	B. Subsample of Female Observed Parents					
Par. EITC exp., 0–5	0.0279	1.5790*	0.0174	0.0056	0.0928	0.0580
	(0.8089)	(0.8608)	(0.0410)	(0.0207)	(0.0599)	(0.0486)
Par. EITC exp., 6–12	-0.1480	0.7301	0.0045	-0.0185	0.0848**	-0.0556
	(0.6250)	(0.5990)	(0.0249)	(0.0217)	(0.0395)	(0.0390)
Par. EITC exp., 13–18	0.4586	-0.2174	-0.0110	0.0098	-0.0304	0.0736*
	(0.7092)	(0.5402)	(0.0143)	(0.0101)	(0.0360)	(0.0415)
Observations	1,830	1,830	1,600	1,795	1,486	1,487
R^2	0.3926	0.3760	0.3253	0.3308	0.3594	0.3755
	C. Subsample of Male Observed Parents					
Par. EITC exp., 0–5	-0.9133	-0.4394	-0.1194**	-0.0642***	0.1262	0.2207
	(1.5399)	(1.1191)	(0.0490)	(0.0235)	(0.1880)	(0.1847)
Par. EITC exp., 6–12	1.3892	-3.7491	0.0057	-0.0702*	-0.8601**	0.2094
	(2.6217)	(2.4537)	(0.0952)	(0.0374)	(0.3775)	(0.4274)
Par. EITC exp., 13–18	2.0602	0.2715	0.0302	0.0254	0.1410	0.1286
	(1.9313)	(2.0301)	(0.0434)	(0.0254)	(0.1409)	(0.1353)
Observations	645	645	582	637	514	514
R^2	0.5206	0.4701	0.4160	0.3847	0.5408	0.5729

Note. Exposure is measured in thousands of 2013 dollars. Results reflect estimations of equation (2). Regressions include fixed effects for parent siblings, state, and birth year; fixed effects for child age, gender, and outcome year; indicators parent gender, race, and grandparent education and marital status; controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, tuition, and quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

provides evidence that the negative impact of that range of exposure previously shown in Figure 3 is especially prominent for the children of Black parents. Given the inequality in test score outcomes already present between Black and white children, this result is especially concerning. Though the result is of low significance (10%), estimates also suggest that a \$1,000 increase in a Black parent's exposure during ages 13-18 leads to a 7.4% standard deviation increase (.0846 score) in their children's self-assessed reading ability. Compared to the results with scores, this again suggests the age at which a parent was exposed is very important to the long-run intergenerational effects.

The next subsample is the 72% of children in the sample whose observed parent is a mother. Despite the large overlap, the results for this subsample do have some noticeable differences from those for the total sample under the full specification. The magnitude of the coefficient on exposure in the 0-5 range for Woodcock-Johnson math scores has increased to 1.579 points (10.5% of a standard deviation) and become significant at the 10% level. Recall that this coefficient was also significant (but of a lower magnitude) under the first three reduced-form specifications. Considering self-assessed ability, I find significant coefficients on the same ranges of exposure as in the full sample. For math scores, the coefficient on exposure during ages 6-12 has increased slightly (0.0848 vs. 0.0759, or 7.8% vs. 6.9% of a standard deviation) and become more precise (5% vs. 10% significance). The coefficients on exposure in the 13-18 age range for reading has similarly increased in magnitude (0.0736 vs. 0.0672, or 6.6% vs. 4.5% of a standard deviation).

The last subsample is that of children whose observed parent is male. Due to the smaller sample size, standard errors are generally much larger than for the previous two samples, but a few results attain significance. One difference of particular note is that the coefficient on exposure in the 6-12 for self-assessed math ability, compared to the subsample whose mothers were observed, is opposite-signed (negative) and about ten times as large in magnitude. This estimate is striking and difficult to believe, as it implies that a \$1,000 increase in a father's exposure leads to a decline in self-assessed math ability that is over three-quarters of a standard deviation. Another result of note is that, for the probability of having been expelled or suspended, this subsample yields the first significant coefficients on exposure in any age range. A $1,000 increase in parental exposure in the 0-5 and 6-12 ranges leads, respectively, to a 6.42 and 7.02 percentage point decrease in the likelihood a child will be suspended or expelled. The estimates are significant at the 5% and 10% levels, respectively. These results suggest that social expectations surrounding masculinity and discipline may play a role. Conversely, estimates suggest that an additional $1,000 in exposure during ages 0-5 leads to a 11.94 percentage point decrease in the likelihood a father's child enters a gifted program.

V.4. Mechanisms

Understanding the mechanisms by which the EITC impacts the educational outcomes of children whose parent grew up in households receiving the credit is a complex undertaking. These mechanisms must be considered over very long ranges of time, during which a multitude of changes in the parent's life will have occurred. Effects may relate to changes in the parent's material wellbeing, behavior, or personal education that in some way affect their parenting or the environment in which their child is raised.

One way to begin developing this understanding is drawing inferences from how the different age ranges of exposure impact an outcome. I focus on interpreting the evidence that parental EITC exposure during ages 0-5 decreases Woodcock-Johnson reading scores, while exposure during ages 6-12 increases them. The two direct effects of the EITC to consider are increases in income and increases in labor supply. Literature on the labor-supply effects of the EITC finds consensus that the employment rates of single mothers are sizeably increased by the EITC (Nichols and Rothstein 2015). Only 26% of the total sample are the children of parents whose own parents were ever married, so the long-run effects of labor-supply increases may be particularly prevalent in the results.

During ages 0-5, especially among low-income families who may not be able to afford childcare, children may be more likely to learn basic skills like reading from their parents. Increases to labor supply during this age may decrease the time parents have available to teach their children these skills, which could have adverse effects on their development which are not offset by higher income if childcare remains unaffordable. When the affected child reaches adulthood and has children of their own, they may either be less equipped to teach their children how to read or value doing so less, resulting in the lower test scores for their child. Bastian and Michelmore (2018), with their sample, find a negative but relatively small and insignificant effect of contemporaneous EITC exposure on the time parents spend with children. Conversely, during ages 6-12, the child would spend more time at school, where higher levels of consumption because of increased income would benefit their development. This would better equip them to pass on reading skills to their future children, causing higher scores. For further discussion of the limited literature on the interactions between the EITC and at-home or paid child-care, see Nichols and Rothstein (2016).

I also run 12 regressions using the full reduced-form specification to estimate the impact of 4 different mechanisms empirically. I investigate the role of the observed parent moving after age 18 (as a dummy variable), the number of children in the child's family unit, family income, (measured in 2013 dollars) and the age of the observed parent at the child's birth. I first take each of these mechanisms as outcomes to determine exposure's impact on them. I then include them as additional regressors when taking the Woodcock-Johnson scores as outcomes to determine the extent to which exposure's impact on the test scores is explained by these four mechanisms. I exclude families making more than \$200,000 annually when considering income, and children whose observed parent was 17 or younger at their birth when considering the parent's age. The child's age is excluded as a control when predicting a parent's age at their birth.

Results are presented in Table VI. The top section indicates that exposure does not have much impact on the four investigated mechanisms. The only coefficient of significant (10% level) precision suggests that an additional \$1,000 of parental exposure during ages 6-12 increases the average age the observed parent will be at the birth of their child by .1316 years. Coefficients on exposure during ages 13-18 are all negative, but their standard errors are larger in magnitude. Coefficients on exposure during ages 0-5 are negative except for the effect on the likelihood of a parent moving after age 18, which is also one of three coefficients larger in magnitude than its standard error. That coefficient suggests a \$1,000 increase in exposure during ages 0-5 leads to a 3.47 percentage point increase in the likelihood a parent moves. The third coefficient larger in magnitude than its standard error is also on exposure from 0-5, predicting that a \$1,000 increase in exposure leads to an average reduction in the number of children in the family unit by .1023.

The second section includes four regressions using Woodcock-Johnson reading scores as the dependent variable, each including a different mechanism. The result suggests a strong and positive statistical relationship between a child's reading scores and the observed parent having moved, as well as between a child's scores and the observed

Table VI. Effect on and Effect of Intermediate Outcomes.

Variable	Dependent Variable			
	Moved	Family Size	Income	Parent Age
Par. EITC exp., 0–5	0.0347	-0.1023	-1.5016	-0.0584
	(0.0211)	(0.0652)	(1.9027)	(0.1062)
Par. EITC exp., 6–12	0.0017	0.0251	1.0162	0.1316*
	(0.0149)	(0.0526)	(1.5438)	(0.0751)
Par. EITC exp., 13–18	-0.0120	-0.0261	-0.8593	-0.0087
	(0.0163)	(0.0487)	(1.3970)	(0.0721)
Observations	2,477	2,477	2,408	2,232
R^2	0.3584	0.4021	0.5174	0.7293
	Woodcock-Johnson Reading			
	Moved	Family Size	Income	Parent Age
Mechanism	4.2520***	-0.6717*	0.0205	3.6902***
	(1.4545)	(0.4034)	(0.0130)	(1.4146)
Par. EITC exp., 0–5	-1.2865*	-1.2076	-1.1665	-2.6812***
	(0.7253)	(0.7401)	(0.7457)	(0.8217)
Par. EITC exp., 6–12	-0.2656	-0.2417	-0.0518	-0.3402
	(0.5311)	(0.5371)	(0.5102)	(0.5487)
Par. EITC exp., 13–18	0.9830*	0.9143	0.8685	0.3411
	(0.5808)	(0.5865)	(0.6014)	(0.6047)
Observations	2,477	2,477	2,408	2,232
R^2	0.3597	0.3542	0.3624	0.3449
	Woodcock-Johnson Math			
	Moved	Family Size	Income	Parent Age
Mechanism	0.9857	-0.1571	0.0310**	0.2567
	(1.2822)	(0.3573)	(0.0127)	(1.4164)
Par. EITC exp., 0–5	0.7653	0.7834	0.9672	-0.3880
	(0.7046)	(0.7017)	(0.7250)	(0.7476)
Par. EITC exp., 6–12	-0.3845	-0.3789	-0.3293	-0.3566
	(0.4887)	(0.4882)	(0.4934)	(0.5006)
Par. EITC exp., 13–18	0.3251	0.3092	0.1469	-0.2294
	(0.4755)	(0.4738)	(0.4791)	(0.5870)
Observations	2,477	2,477	2,408	2,232
R^2	0.3652	0.3649	0.3612	0.3624

Note. Exposure and income are measured in thousands of 2013 dollars. Results reflect estimations of equation (2). Regressions include fixed effects for parent siblings, state, and birth year; fixed effects for child age, gender, and outcome year; indicators parent gender, race, and grandparent education and marital status; controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, tuition, and quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**} p < .05,^{***} p < .01$.

parent's age at their birth. Including the moved dummy does not however cause large changes in the coefficients compared to results in Table IIb, suggesting that incentivizing or removing constraints to moving plays only a very small role in how parental EITC exposure affects a child's reading ability. Conversely including the observed parent's age at the child's birth causes the coefficient on parental exposure during ages 0-5 to more than double and during ages 13-18 to decline by nearly two-thirds. Since this is a large change compared to exposure's small effect on this age, results suggest that delays in fertility may be predictive of other mechanisms through which the EITC impacts reading ability.

The third and final section replaces Woodcock-Johnson reading scores with math scores. Including the moved dummy or the number of children in the family unit does not cause a considerable change in the coefficients, nor does either mechanism have a significant impact. Income, however, does have a significant impact on math scores, and its inclusion causes small but not statistically significant changes in exposure's coefficients. Interestingly, despite the negative association found between exposure during ages 13-18 and family income, including income reduces the magnitude of the coefficient on exposure from ages 13-18 by more than half. Including the observed parent's age at a child's birth causes large changes to exposure during ages 0-5 and 13-18, changing the sign to negative on both coefficients. Despite the stronger association between the age mechanism and exposure during 6-12, the coefficient on that age range changes only slightly. This suggests that the earlier specification may have underestimated the relationship between exposure and the observed parent's age at a child's birth.

VI. Robustness Checks

Effects may also be misidentified. One significant point of concern is that the construction of the sample may impact the results through selection bias given that I am studying the impact on a later generation. Consider for simplicity a binary model of EITC exposure. If categorizing people by their entrance into the sample, that is, their decision to have children, there may be four categories. Some people would decide to have kids or not to have kids regardless of EITC exposure. Some people's decisions may change depending on whether or not they were exposed to the EITC. If one were to randomly assign a representative sample of people to be treated with EITC exposure or not, then the treatment and control groups among the subsample of eventual parents would not be adequately similar. That is, assignment becomes non-random because the choice to have a child is endogenous.

If the group who decides not to have children because of the EITC would have had children with better educational outcomes, then this potential selection bias would have decreased coefficients on exposure. The inclusion of family size as an investigated mechanism in the preceding section acts as a check for this. However, due to the sample selection issues described above I can only estimate the impact of exposure on the intensive but not extensive margin of having children. The results indicated a negligible effect except for a small (though imprecisely estimated) negative impact from exposure during ages 0-5.

As an additional check on the robustness of these results I run the reduced-form specifications with different samples and the natural logarithm of parental EITC exposure (in thousands) plus one in place of parental EITC exposure. The only outcomes used are a child's Woodcock-Johnson test scores. Three samples are used with the natural logarithm specification: the total sample and the total sample with upper bounds at \$50,000 and \$30,000 (2013 dollars) placed on the income of the observed parent's household at 18. Two samples are used with the previous specification: the total sample with score outliers and observations from small states included and the sample of unique observations only (with the same restrictions as the total sample applied).

Table VII. Robustness to Natural Logarithm and Other Samples.

Variable	**Dependent Variable**					
	WJ Read	WJ Math	WJ Read	WJ Math	WJ Read	WJ Math
Income Upper Bound	None		\$50,000		\$30,000	
ln(EITC exp.+1) 0–5	-2.5261	3.4065	-11.3598*	5.0455	-13.7035*	-0.2117
	(5.1043)	(5.6625)	(6.2197)	(5.7587)	(7.9858)	(6.2706)
ln(EITC exp.+1) 6–12	-0.4011	0.8445	6.0291	6.0644*	5.3766*	5.1415
	(4.2201)	(2.9416)	(4.2601)	(3.2361)	(2.7347)	(3.3372)
ln(EITC exp.+1) 13–18	3.0826	-3.1817	-5.2699	-18.8051**	-5.4816	-12.5399*
	(7.1411)	(6.4924)	(8.0125)	(8.7610)	(8.1548)	(7.5855)
Observations	2,477	2,477	1,597	1,597	1,183	1,183
R^2	0.3501	0.3644	0.4185	0.3925	0.5001	0.4696
Sample	All states & scores		Unique only			
Par. EITC exp., 0–5	-0.8870	0.2553	-1.2815*	0.7446		
	(0.7951)	(0.3830)	(0.7475)	(0.7118)		
Par. EITC exp., 6–12	-0.2843	-0.2219	-0.3451	-0.4871		
	(0.5357)	(0.5353)	(0.5437)	(0.4768)		
Par. EITC exp., 13–18	1.2503**	0.0461	1.2224**	0.4107		
	(0.6154)	(0.4066)	(0.6038)	(0.4747)		
Observations	2,538	3,091	1,590	1,590		
R^2	0.3793	0.3456	0.3808	0.4026		

Note. Exposure and income are measured in thousands of 2013 dollars. Results reflect estimations of equation (2), with the natural logarithm of exposure (in thousands) plus one in place of exposure for the first section. Regressions include fixed effects for parent siblings, state, and birth year; fixed effects for child age, gender, and outcome year; indicators parent gender, race, and grandparent education and marital status; controls for GDP per capita, unemployment, top marginal income tax rate, minimum wage, maximum welfare benefits, tuition, and quadratic state trends. Source: PSID, Bastian and Michelmore (2018). $^{*}p < .1,^{**}p < .05,^{***}p < .01$.

Results are presented in Table VII. When considering the logarithm of exposure, there are two main differences between the results using the full sample. For math scores, the coefficients on exposure during ages 6-12 and 13-18 have switched from being negative and positive (respectively) in the original specification to being positive and negative (respectively). These coefficients are not precise or significant under either specification, but the coefficients under the logarithm better correspond to original estimates (Fig. 4) when limiting the sample by the observed parent's childhood family income. In Figure 4, the coefficient was largest when applying an upper bound of \$50,000, under which the coefficient on the logarithm of exposure is large, negative, and significant. At \$1,000 of exposure during ages 13-18, this estimate predicts a decline of $\ln(2) * (18.8) = 13.03$ points, while at the average of \$9,970 of exposure during ages 13-18 this estimate predicts a large decline of $\ln(10.97) * (18.8) = 45.03$ points. As this would constitute a decline of 3 standard deviations, it seems unlikely to accurately reflect the impact of exposure. That this magnitude decreases when the upper bound is lowered to \$30,000 suggests that the effect could be driven by parents with high exposure but low actual credit received, though one cannot conclude that the two coefficients are significantly different.

A few notable results appear when considering the sample with outliers and small state observations included, the most prominent being an estimate, significant at the 5% level, that suggests a \$1,000 increase in exposure during ages 13-18 increases reading scores by 1.25 points. This is very close to the findings of the previous sample with an income upper bound of \$80,000 (Fig. 3) but contradicts the findings for lower income bounds. Also note that the all-states-and-scores sample has far fewer observations of the Woodcock-Johnson reading scores, as fewer children were given the test. For this reason, results for the reading scores more closely resemble those in Table IIb than math.

Including dropped observations, for math scores, causes a 68% and 85% reduction in the magnitude of the coefficients on exposure during ages 0-5 and 13-18, respectively.

Results for the unique-observations sample closely resemble those in Table IIa for the corresponding specification. Two estimates increase somewhat in precision, those being the coefficient on exposure during ages 0-5 and 13-18. Their magnitudes have increased to a decrease of 1.28 points (10% significance) and an increase of 1.22 points (5% significance), respectively. This strengthens the evidence for exposure during ages 0-5 having a negative impact on reading scores. The positive coefficient on exposure during ages 13-18 once again suggests there may be an unobserved variable driving positive relationships between exposure in this range and reading scores for high incomes.

VII. Conclusion

The results provide evidence that the Earned Income Tax Credit's intergenerational impact on educational outcomes varies by the particular age a parent was when their childhood household received the credit. The strongest evidence is for a negative impact of parental EITC exposure from ages 0-5 on children's Woodcock-Johnson reading scores. This result appears with varying significance in specifications without economic or state-trend controls or specifications using the logarithm of exposure, and in the full specification when the sample is limited to parents from low-income households, Black parents, and only the latest observation of each child. The strongest effect (without limiting income) appears to fall on children of Black parents with each additional $1,000 of exposure decreasing 17.7% of a standard deviation. An unexpected negative result that is more severe among children of parents raised in disadvantaged households raises concerns for the long-run distributional effects of the EITC, suggesting that its design may have unintended consequences for future generations.

Through which channel this negative effect occurs is not clear. The inclusion of possible mechanisms (the parent moving after 18, number of children in the household, family income, and parent age at child's birth) in the reduced-form regression does not attenuate the coefficient on exposure during ages 0-5, and in the case of parent age even makes the negative effect appear much larger. Another possibility is that the work incentives of the EITC lead to worse reading outcomes for the parent by decreasing a parent's time spent on at-home childcare, and these outcomes have a spillover effect as the parent raises the next generation. Literature on this possibility finds mixed results, and data on the time a parent spends with children are hard to collect. Deeper investigation into the validity of this finding and the mechanisms behind it is left to future research.

Results provide some indication of positive outcomes, though this evidence is less consistent. Applying an upper bound to a parent's childhood family income suggests that a \$1,000 increase in parental EITC exposure during ages 6-12 leads to a roughly 2 point, or 13.3% of a standard deviation, increase in both of a child's Woodcock-Johnson scores. This result is also found when substituting exposure for its logarithm, though it is less significant and depends on the income upper bound applied. Regressions using the subsample of observed fathers also indicate that their exposure, particularly during ages 0-5, may decrease the likelihood their child is suspended or expelled. Therefore, while the strongest evidence is for a particular negative effect, there are reasons to believe that the long-run benefits for children of the EITC are also improving some outcomes for their descendants.

The main weakness of this paper is the limited availability of data, particularly data with high and exploitable variation in the size of the EITC. While 47 years have passed since the creation of the EITC, the majority of the observed parents in this paper's sample were born years before then. Only the youngest parents of this sample would have been affected by the large expansions of the EITC during the 1990s. However, the prominence of the EITC and related programs like the Child Tax Credit, the mixed results found in this paper, and persistent gaps in the literature underscore the necessity of understanding the intergenerational impact of these programs to alleviating poverty in the long-run. As more data are released with time, future research must continue to update and broaden that understanding to ensure the social safety net continues to create social mobility for children in economically disadvantaged families and their descendants.

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