The EITC and Intergenerational Mobility

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PRELIMINARY AND INCOMPLETE. PLEASE DO NOT CITE

February 27, 2020

Abstract

The effects of public policy on intergenerational social and economic mobility in the United States are not well understood. We study how the largest federal tax-based policy intended to promote work and increase incomes among the poor—the Earned Income Tax Credit (EITC)—affects the socio-economic standing of children who grew up in households affected by the policy. Using the universe of tax filer records for children linked to their parents and demographic and household information from census demographic data, we exploit exogenous differences by children's ages in the births and "aging out" of siblings to assess the causal effect of EITC generosity on child outcomes—specifically, upward mobility in the child income distribution. Our findings suggest significant and mostly positive effects of more generous EITC refunds on the next generation that vary substantially depending on the age at first exposure to increased EITC and the duration of exposure.

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Disclaimer: Any opinions and conclusions expressed herein are those of the authors and do not necessarily reflect the views of the U.S. Census Bureau. The statistical summaries reported in this paper have been cleared by the Census Bureau's Disclosure Review Board release authorization numbers CBDRB-FY2020-CES005-017 and CBDRB-FY2020-CES010-005.

I. Introduction

The Earned Income Tax Credit (EITC) is the best known and most widely utilized provision of the federal income tax code that targets families of low-income tax filers. As opposed to welfare programs, such as Food Stamps, the EITC is only available to working adults and is administered through the Internal Revenue Service as a refund on filed earned income taxes. The EITC was first adopted in 1975 as a modest transfer to working families. It has expanded substantially and is currently the largest government cash transfer program. In 2018, 22 million working families and individuals received EITC, with an average refund of $3,191 for a family with children. Maximum credit dollars reached $5,828 for a family of four earning around $20,000 in 2019. Refunds for families and individuals without children are much smaller, with an average of $298 in 2018 (Center on Budget and Policy Priorities, 2019).

Researchers credit the EITC with lifting families out of poverty, restoring employment and improving the long-term wellbeing of families and children residing in their parents' households (Evans and Garthwaithe, 2014). Very little is known about the potential effects of EITC on the long-term outcomes of children from affected households, but recent research has examined childhood and early adult outcomes (Dahl and Lochner, 2012; Chetty et al., 2011; Bastian & Michelmore, 2018). Yet a large and growing literature has shown that family financial conditions during childhood, and in particular family income, have strong and persistent effects on children's wellbeing as young adults and beyond (Currie, 2009; Almond et al., 2018, Hoynes et al., 2012, Akee et al., 2013; Akee et al. 2018; Akee et al., forthcoming). Further, research shows that parental use of welfare benefits and government programs affects children's utilization of these programs; if the same is true of intergenerational EITC use, this may result in additional positive effects on labor force attachment and earnings (Dahl et al, 2014).

Other research has found that programs which enable families to "move to opportunity" have lasting impacts on the outcomes of low-income children (Chetty et al. 2016; Chetty and Hendren 2018; Chetty et al. 2019). In light of the fact that the EITC is often used to forestall eviction or improve a family's housing situation (Pilkauskas and Michelmore, 2019), an important and unexplored question in EITC research is how the EITC compares to housing voucher programs in improving children's opportunities and outcomes. By using the same analysis data and similar cohort years as a recent, large-scale study of intergenerational mobility, we are in a position to comparatively assess the impact of EITC dollars.

There are several reasons why the EITC could affect children's long-term outcomes. Prior research has demonstrated that the EITC increased household incomes and reduced the incidence of poverty among at-risk families (Dahl et al, 2009; Hoynes et al, 2015). It also affected labor force participation and attachment, especially for single mothers (Eissa and Liebman, 1996; Bastian, 2017), and reduced levels of maternal stress, potentially leading to gains in health status (Evans and Cartwaithe, 2014). Theoretically, these findings about household wellbeing could have opposite effects on children's long-term labor market outcomes. On the one hand, increased household incomes, parental labor force attachment, and better parental health should have positive effects on children's long-term labor market success. On the other hand, increased labor force participation, especially by single mothers, is often associated with less parental supervision, which could lead to undesirable social behaviors (Dave et al, 2019).

The immediate effects of public policies aimed at reducing poverty are relatively well researched and understood. The long-term and intergenerational effects are not well understood, and may run contrary to initial expectations because of the many different choices involved in deriving maximum individual benefit from the policy for the generation immediately affected by it. In light of the recent surge in interest in the determinants of intergenerational economic and social mobility, it is crucial that we understand better how the most expensive US tax policy intended to promote work has impacted the next generation.

In this study we use individual-level panel data from linked Internal Revenue Service tax data and Census Bureau demographic data to evaluate whether changes in the generosity of the EITC affected the intergenerational transmission of socio-economic status. We make several contributions to the literature. First, to our knowledge, this is the first study to examine how a large federal anti-poverty program in the United States affects *intergenerational* income mobility. Second, because we have access to individual data, we test for important heterogeneities in effects across socio-demographic characteristics of the parents and the children at the time of EITC expansion, such as race, single parenthood, and household size. Third, we examine the critical ages hypothesis and derive differential policy effect estimates depending on the age at first treatment and duration of treatment for children who are some point claimed as dependents in multi-children households. Importantly, because we use variation in the age at which increased EITC generosity affects children, our estimates are not affected by other entitlements and government programs (such as Medicaid expansions), which affected children of all ages at the time of implementation. Relatedly, our individual-level data allow for controls of various geographic levels of unobserved time-invariant confounders and time trends, such as at the levels of the state or the county. Finally, using data on parents' trajectories around the time of policy implementation and beyond allows us to investigate the mechanisms behind our long-term findings about the children, ranging from endogenous family formation to labor force attachment.

We find strong positive correlations between parental income and child income rank for those born in households qualifying for the EITCs. The correlation is stable around 0.27. Consistent with some of the other literature on the effects of positive socio-economic changes to households on children's long-term outcomes, we find a differential impact of expanded EITC refunds depending on the child's age at first treatment. Those treated until age ten experience improvements in their income rank and employment relative to their parents. For example, a child whose parents were below the 45th percentile position in the income rank distribution at the time of the policy change, and who herself was first treated at the age of 14, on average would be 1.6 positions higher in her income rank in her mid-twenties than a child from a family similarly positioned in the parental income rank who was less than four at the time exposure. Children affected until around age ten are also more likely to be employed. We find substantial differences in impact by gender for all outcomes considered. The effects are different for those coming from single mother households, with younger children from such households benefitting somewhat more than the average and those first treated in their late teens impacted negatively.

II. Background

2.1 The Earned Income Tax Credit

The EITC was developed in the 1970s as a way to compensate low-wage workers for regressive payroll taxes. The EITC refunded 10 cents of every earned dollar, up to an earnings maximum level of $5,000, at which point the credit phased out at a rate of 12.5 percent of income. The maximum credit a tax filer could be eligible for was between $400 and $500 between 1975 and 1986 (about $1,200 in 2019 dollars). The tax credit required some positive earnings and the filer had to have a qualified child in the household; there was no childless household EITC during the initial phase of the program.

During the decade of the 1990s, the EITC program experienced dramatic changes in program requirements and benefits. First, the program provided larger refunds for households with two or more children. Second, the phase-in rates of the EITC refund increased from 14 percent per dollar of earned income in 1990 to 34 and 40 percent in 1996 for households with one and two or more children, respectively. Third, the EITC program was extended to childless households in 1994. Finally, the cap on investment income began to apply with these reforms as well.

2.2 Related research

This work is related to several strands of the existing literature. First, we contribute to the work on differences in economic opportunity for children growing up across the U.S. and how exposure to improved opportunity affects the next generation, pioneered by Chetty and Hendren (2018a and b). Our identification strategy most closely resembles the strategy used in those papers. A second strand of emerging related research is dedicated to the intergenerational effects of public policies. Some of this work has focused on the intergenerational effects of fertility policies (e.g. Madestam and Simeonova, 2018; Ananat and Hungerman, 2012); others have investigated large public assistance programs such as Food Stamps (e.g. Hoynes et al., 2014), and the expansion on public health clinics and Title X (Bailey et al., 2019). This work is also related to the large literature on household socio-economic status and children's outcomes, ranging from socio-economic success to long-term health. This literature has demonstrated strong associations between parents' resources and children's success. As the EITC expansion created exogenous positive variation in some families' resources (but not others'), our findings contribute to the small but growing branch of this literature exploiting natural and social experiments to identify the underlying relationship net of selection and omitted variable biases. Lastly, and most directly, this work is related to the many strands of research on the effects of EITC and EITC expansions on the individuals directly affected by the policy (in our case, parents of multiple children) and their dependents.

EITC and Effects on Parent's Outcomes and Own Employment

Eissa and Liebman (1996) investigated the role of the 1986 EITC expansion on mothers' labor force participation and hours worked; they find that there is an almost 3 percentage point increase in labor force participation rates for single mothers with children. Subsequent analysis by Eissa and Hoynes (2004) finds that later expansions of EITC to married couples effectively reduces total family labor supply. Their analysis finds that while males increase their labor force participation, their female spouses tend to more than proportionately reduce their labor force participation rates. On net, this leads to a reduction in total family labor in the market; the authors characterize the expansion as subsidizing married mothers to stay at home. On the other hand, Hotz, Mullin, and Scholz (2006) find that EITC increases labor force participation for single-parent families. Chetty, Friedman, and Saez (2014) find that the EITC provides significant incentives for individuals to increase the number of hours worked so as to maximize their EITC refunds on the initial phase-in portion of benefits. The prevailing analysis for EITC impact shows that the EITC has an effect on hours worked as well as on labor force participation – both on intensive and extensive margins.

Finally, Evans and Garthwaite (2014) find that increased incomes from the 1993 EITC expansion improves mothers' overall health and reduces measures of stress hormones.

The EITC and Children's Outcomes

The most closely related literature is that on EITC and children's educational outcomes – in the period during and right after EITC exposure, and also the college years. Dahl and Lochner (2012) utilize the same variation in EITC as we do – the federal expansion for households with two or more children – and data from the NLSY to investigate the effect of increased household resources on children's test scores. They find that a thousand dollar increase in income improved math and reading test scores by six percent of a standard deviation. This improvement is contemporaneous with EITC receipt by the mothers, and echoes findings on reduced maternal stress by Evans and Carthwaithe (2012), and findings in Akee et al. (2010, 2018) demonstrating that extra income reduces parental stress and improves children's schooling outcomes and emotional and behavioral health.

A contemporaneous paper by Chetty, Friedman, and Rockoff (2011) examines how the EITC affects long-run outcomes through its impact on childhood test scores. Dahl and Lochner rely on the NLSY, which while representative does not contain a large number of individuals. Chetty et al. (2011) combine data from a large urban school district with administrative tax records. Importantly, they also find that a $1,000 increase in tax credits leads to a 6%of a standard deviation increase in childhood test scores. This increase in childhood test scores results in a 0.3 percentage point increase in college going by age 20.

Bastian and Michelmore (2018) consider exposure to EITC throughout childhood and across all children from potentially affected cohorts that are surveyed by the PSID. They sum the total amount of EITC credits that the child could potentially be eligible for during her time in her parents' households, regardless of whether the child's household was ever actually eligible for EITC receipts. Both single children and children from multiple sibship pairs are used, and the identifying variation comes from changes in EITC exposure by birth cohort and state of residence. Thus, the estimated results are interpretable as the average effects of EITC exposure by state and birth cohort across all children. While they use all EITC expansions, and thus are utilizing changes in household refunds starting as early as 1975, relatively few children, and thus a very small fraction of the identifying variation, come from cohorts born after 1990.

III. Data

3.1 Data sources

Our data reflect the same intergenerational relationships as described in Chetty et al. (2020) (hereinafter "CHJP"). The online appendices to that paper provide the details on the sources of variables and their descriptions. In brief, the data comprise information from several Census-held data sets: the decennial 2000 and 2010 short forms; the decennial 2000 long form; the 2005 to 2017 American Community Surveys (ACS); IRS Form 1040 returns from 1994, 1995, and 1998 to 2017; and IRS Form W-2 data from 2005 to 2017).[1] The decennial short forms cover the entire population of the U.S., while the long form and ACSs are stratified random samples covering one-sixths and 2.5 percent of U.S. households.

[1] The original dataset included tax forms and ACS data up to 2015. It has since been expanded by two years.

All these data are linked using a unique person identifier called a Protected Identification Key (PIK) that the Census Bureau assigns using personally identifiable information such as a Social Security Number (SSN), name, address, and date of birth. The algorithm used for record linkage is described in Wagner and Layne (2014). CHJP, both in its text and online appendices provide evidence on the quality of the PIK placement and data match. The population basis for the sample is the 2016 Numident, which is the universe of SSNs issued up to that year.

3.2 Sample and variable definition

Our target sample comprises all children in multi-child households in the 1979--1991 birth cohorts who were born in the U.S. or who came to the U.S. in childhood. Both children and their parents must be authorized immigrants to be included in the sample. We identify all children who were claimed as a child dependent on a Form 1040 at any time between 1994 and 2017 using the child's unique identifier, which is assigned to the Form 1040 data beginning in 1994.[2] The identifier of the person who claims the child is the invariant "parent," but we restrict parents to adults who appear in the 2016 Numident and who were between the ages of 15 and 50 when the child was born.

In assigning siblings, we collect children by the mother's identifier, regardless of whether the mother's filing status changed between sibling births.[3] For example, a child claimed in 1994 by two parents may have a sibling born after 1994 who was claimed only by the mother. In each child's case, the mother's filing status is captured at the time of the index child's claiming. When the mother's identifier is absent, we use the father's identifier. In later iterations of this paper, this choice will be examined. Although we are following CHJP in keeping the claiming parent(s) invariant vis-a-vis the individual child, this choice may be less defensible when the analysis relies on the correct identification of siblings. While the target sample includes cohorts 1979-1991, we capture siblings claimed on parents' 1040s up to the 1999 birth cohort.

We define other variables based on the short form data linked to the Form 1040 and W-2 (later iterations of this paper will examine subsamples of children whom we find in the pooled ACSs,

[2] Before 1994, dependent SSNs were not reported in our 1040 data, although they began to be required on 1040 forms in 1985.
[3] It should be noted that the term "mother" means a female primary or secondary filer who claimed the child as a dependent, who may not necessarily be the child's biological mother.

which will allows us to capture education, household formation, etc.). Our key outcome of interest is the child's rank in the cohort income distribution averaged over ages 25 and 26. For children born in 1991, this value is captured in 2016/2017, our last available years of tax data. This choice of cohort range and the timing of the outcome "sandwiches" our sample between two events: our youngest cohort was 2- to 3-years-old at the time of the EITC policy change we examine and were 17-18 (aging out of eligibility) at the time of the 2009 expansion. A second outcome of interest is whether the child is working at ages 25/. Working is defined as having positive W-2 earnings at ages 25/26. We define child and parent's race based on the most recent race reported for them on a decennial census or an ACS. Gender is also defined using the available demographic data. The filing status of parents, used to identify our sample of single mothers, is derived directly from the Form 1040 on which the child was first claimed. We define single mothers as those who file as "single" or "head of household."

Figure 1 shows available age ranges for the parent/child link for data held at the Census Bureau, where the first year of data used for the child's adult earnings is 2004. Several features of this time frame should be noted. First, all of the children in our sample were still claimable by parents under different EITC regimes, with major EITC changes taking place in 1985 (ages 2 to 7) and over 1993--1996 (ages 10 to 15). We specifically exploit the major policy change that increased generosity for families with more than one child.

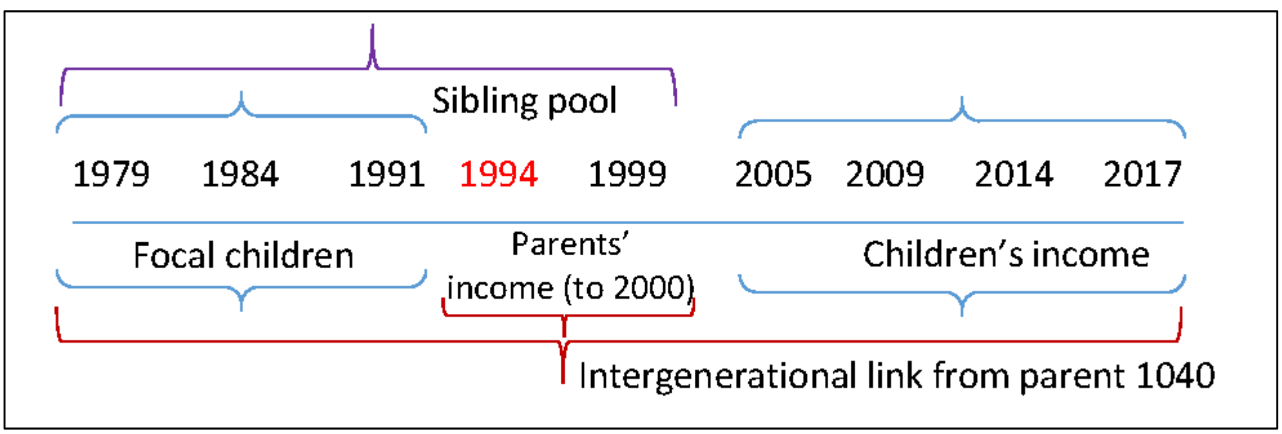


Figure 1. Years used in analysis. Focal children were born between 1979 and 1991. This age range allows us to find 1040 income and W-2 earnings for children between 2005 and 2017, when each cohort was between the ages of 25 and 26. To find siblings, we use a larger span of child cohorts: 1978 to 1999. All intergenerational links rely on the parents' claiming of children on 1994, 1995, and 1998-2017 Form 1040s. We calculate the parent income rank averaged over 1994 to 2000 income reported on Form 1040s.

3.3 Sample description

The table of means presents the means for the main outcomes and explanatory variables for children residing in multi-child households below the forty-fifth percentile of the parental income distribution at the time of EITC generosity changes. We report means for two samples that we consider—children who are first in their sibling order, and all children in multi-children households. In the analyses below, we use both of these samples. We present summary statistics for three cohorts – those born in 1979, who were 14 or 15 at the time of the policy change; one in the middle of the child age distribution at the time of policy change; and one for those born in 1991, who were two to three years old at the time of EITC expansion.

Table 1: Summary statistics of main control and outcome variables by child order in 1994. Included are all children residing in households below the 45 percentile of the parental income rank distribution.

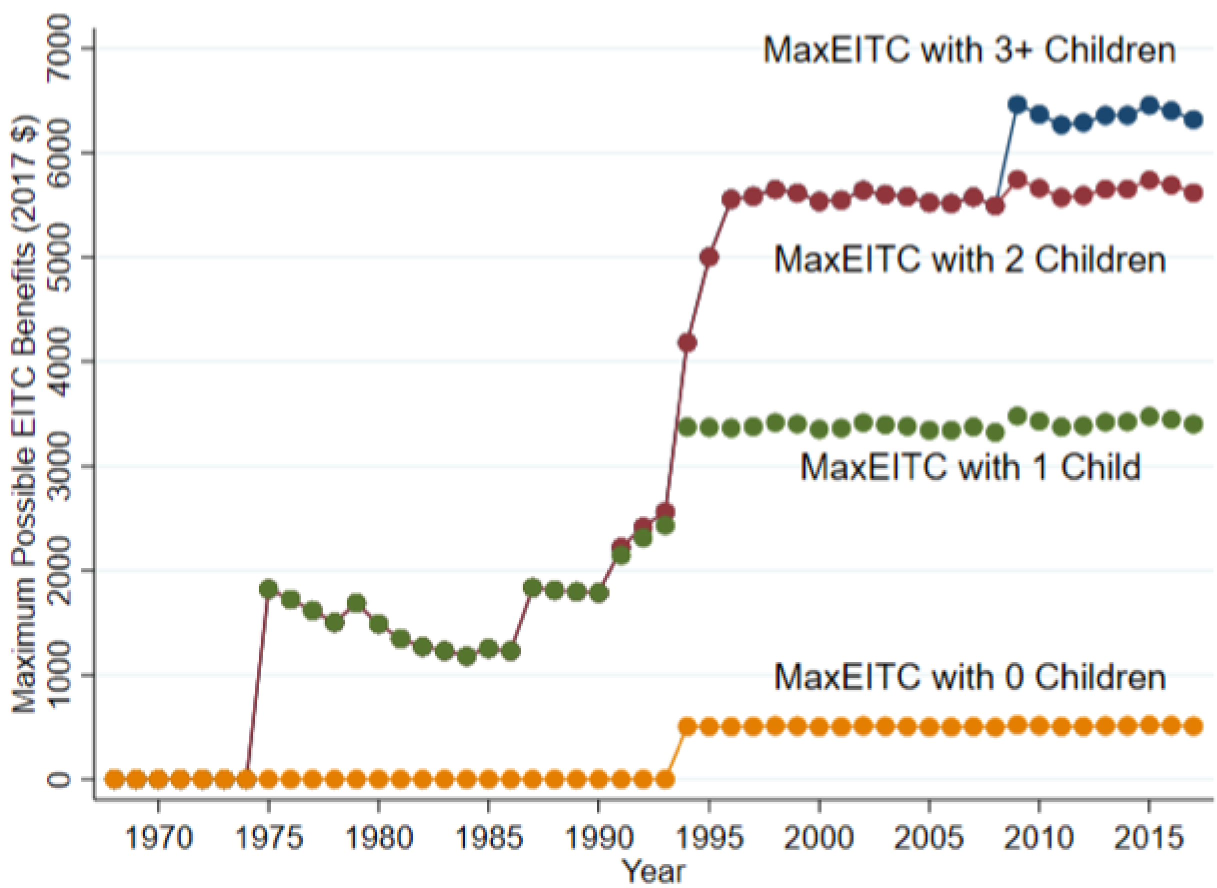
	Birth order	Child birth cohort 1979	1985	1991
Parent rank in the income distribution	2nd +	0.23	0.23	0.23
	1st	0.23	0.22	0.22
Age at first treatment	2nd +	15	9.00	3.00
	1st	15.13	9.18	3.84
Treatment intensity	2nd +	0.98	0.91	0.87
	1st	0.73	0.78	0.83
Number of siblings	2nd +	3.08	3.12	3.08
	1st	2.71	2.63	2.40
Male	2nd +	0.51	0.51	0.51
	1st	0.50	0.51	0.51
Single mother	2nd +	0.34	0.34	0.34
	1st	0.36	0.36	0.35
White	2nd +	0.47	0.47	0.41
	1st	0.48	0.46	0.40
Black	2nd +	0.24	0.21	0.24
	1st	0.23	0.24	0.24
Asian	2nd +	0.04	0.04	0.04
	1st	0.04	0.03	0.03
Hispanic	2nd +	0.22	0.23	0.26
	1st	0.21	0.23	0.27
Child rank	2nd +	0.40	0.43	0.42
	1st	0.42	0.43	0.43
Child works	2nd +	0.77	0.78	0.83
	1st	0.78	0.80	0.84
Child teen birth	2nd +	0.08	0.08	0.06
	1st	0.09	0.08	0.06
N all orders, 1979-1991 cohorts		16,000,000		
N first children, 1979-1991 cohorts		8,000,000		

IV. Empirical setup

The EITC policy change we examine rolled out between tax years 1991 and 1995. Throughout our analysis, we follow the standard procedure of treating EITC policy changes as exogenous in terms of family structure. We apply broad eligibility by family size over the lower half of the parent income distribution, rather than calculating it directly using income or earnings, which may be endogenous. In 1991, the two-child credit schedule was added, although in this year the difference between it and the one-child credit was only $43 at the maximum credit value. This maximum credit difference changed little between 1991 and 1993. Then, between 1993 and 1994, the credit difference expanded from $77 to $490, the largest single increase in percent terms over the policy rollout (a 36 percent change versus

8 to 18 percent in all other years). We thus take 1994 as the policy change year as it is the first year where the difference in credit values by family size was substantial.[4] Figure 2 shows the changes in generosity in the EITC schedule by number of children over time.

Figure 2. Changes in EITC eligibility over time



We face a research challenge when examining how exogenous changes in EITC generosity over time influence children's outcomes.[5] Because all children within a family type experienced the EITC policy change at the same age, there exists no plausible "control" group that shares every characteristic with the treated group except for the policy change. While similar challenges existed for previous researchers who, for example, compared single women with one child to those with two, the differences between only children and their counterparts who have siblings is likely too stark to be overcome. In the absence of a clear control group, we instead focus on the intensity and timing of EITC generosity based on the

[4] The choice of 1993 as the policy change year in Kleven (2019) is likely one of the reasons he finds little effect of the EITC on single women's labor supply.
[5] Bastian and Michelmore (2018) used the total EITC dollars accruing to a child based on changes in EITC policy and household structure.

births of younger siblings and the "aging out" of older siblings. We take two approaches to the question that, taken together, provide compelling evidence on the causal effects of greater EITC generosity on child outcomes.

Our first specification is based on comparisons between children from the same cohorts whose families became eligible, or became ineligible, for the more generous two-child EITCs at different ages. Here we utilize the exogenous variation in the timing of family expansions and contractions across children's ages for those born in the same year. In this specification we account for the aging out of siblings from eligibility. Because of the time frame of our data, we use the age of 18 as the last year of eligibility, even though full-time students may remain eligible until age 24 (we will examine whether truncating the age at 18 makes a difference in later iterations of this paper).

We use the term "treatment" to refer to the higher dollar value of EITC for families with two or more children. Thus a "treated" child lives in a family who receives the higher credit amount as long as it meets other eligibility requirements. As examples, children with older and younger siblings who are all born before 1994 are eligible for the two-child credit for eighteen years and get assigned a treatment intensity of 1. A seventeen-year-old who acquires a younger sibling after 1994 will be eligible for only one year of the two-child credit, and thus would get assigned treatment intensity of 1/18. All of the siblings we capture were claimed on parents' 1040s between 1978 and 1999, but we focus on a subset of these children in the analysis to capture earnings for all children when they are 25/26 years old. Thus, for children born between 1979 and 1991, age at treatment is defined as follows:

1. Youngest and middle children are treated at their age in 1994.
2. Oldest children are treated at the age a sibling is claimed on the parents' 1040 in relation to 1994 (that is, if the sibling is first claimed before 1994, the oldest child's treatment is 1994; if the sibling is first claimed after 1994, the age at treatment for the oldest child is age at the sibling's claiming).

We also define for each child the intensity of the treatment by calculating the fraction of the child's life their families were eligible for the two-child credit. Considering our age and cohort ranges, the intensity of treatment is as follows according to order:

1. Middle children are treated at every age after 1994.
2. Oldest children are treated as the fraction of years out of 18 that they have a younger sibling.

3. Youngest children are treated as the fraction of years out of 18 that their closest older sibling is still an eligible child.

The main estimating equation is:

$$Outcome_i = \alpha + \beta_i par_i + \delta_i tage_i + \rho_i int_i + \varphi_i tageXint_i + \theta_c + \Gamma * X_i + \sigma_s + \varepsilon_i \qquad (1)$$

where the outcome is the child's income rank in her cohort's income distribution averaged between 25 and 26 and par_i is the income rank of the claiming parent averaged within parental birth cohort over 1994 through 2000. We include the age-at-treatment fixed effects interacted with treatment intensity; cohort fixed effects, θ_c; and in X_i, a set of child characteristics including gender, race, sibling order fixed effects, size of household, and a quadratic in the age difference between the child and nearest sibling. We include state fixed effects in σ_s to account for differences in state-level economic conditions and social program generosity.[6] Differences in outcome variables are thus identified through differences across children born in the same cohort, having the same birth order and total sibship size, and residing in the same state, who were subject to expanded EITC eligibility at different ages. The cohort years we examine allow us to calculate our main outcome—child income rank averaged over ages 25 and 26—for all children from the same birth cohort. However, the cohorts chosen, the ages of treatment, and the intensity of treatment are likely correlated with age and order in the family. Oldest children are, by definition, more likely to be in older cohorts *and* treated at older ages, while youngest children by definition will be treated in 1994 and vary in treatment intensity in a way that middle children will not.

Although we control for sibling order in equation 1, we also use a version of this specification that examines age of treatment for first children only. In other words, we examine the impact of age at treatment within a birth order. Treatment intensity in this case will not vary by age of treatment—in this case, every child who gets a sibling at the age of 10 will have the same number of years of treatment, and first children with siblings born before 1994 will all receive treatment in 1994. Thus we estimate equation 1 only on first children while leaving out treatment intensity, its interaction with age of treatment, and the sibling order fixed effects.

[6] State is defined as the state where the child is first claimed on the parent's 1040. A major consideration during this time period are differences between states in AFDC/TANF generosity (time limits, waivers, etc.). We plan to directly control for state differences in a later draft. State-level EITCs are also a concern, although a. they were not terribly generous over the period we study and b. they might be endogenously determined (Hoynes & Patel, 2018; Bastian & Jones, 2019).

A potential threat to identification could arise from endogenous fertility in response to the increased EITC generosity for families with 2 or more children. If some families responded to the policy by acquiring more children, then a specification comparing families of different sizes over time would produce biased estimates affected by selection. This is not a concern for us for two reasons. First, there is no evidence that the EITC affected fertility (e.g. Baugman et al 2009) or marriage formation. Second, our identification is based on differences between children treated at different ages, all of whom acquired a sibling at some point.

V. Results

5.1 Age at treatment times treatment intensity

The treatment intensity variable is constructed as the ratio of the number of years of actual treatment for the index child divided by the total possible treatment duration, which is 18 years. Thus, one year of treatment corresponds to 0.055 units of treatment intensity. Treatment intensity varies from 0.055 to 1, with a mean of around 0.8. We use all children from households with more than one child in the estimation, because treatment duration is affected by older children aging out of EITC eligibility, in addition to younger children being born and thus qualifying the existing older child for expanded EITC generosity.

We find that parental income rank has strong positive association with the child's rank at ages 25/26. The correlation coefficient is the same on average as in the sample of single-mother households. The effects of increasing the duration of exposure to larger EITC refunds are relatively small. Increasing treatment intensity by 10 years (0.55 units for the baseline group) on average improves child income rank by 0.2 percentage points. The effect of treatment intensity is somewhat stronger for children from single-mother households. Interestingly, increasing treatment intensity at young ages does not contribute to improved outcomes, while increasing treatment intensity in the mid- to late-teens mitigates the negative effects of first exposure at an older age, relative to exposure at ages younger than 4. However, the maximum duration of treatment for those first exposed in their late teens is never high enough to offset the negative main effect of treatment at those ages. Similarly, even the maximum duration of exposure at young ages is not sufficient to offset the positive impact of being first exposed to more generous EITC at ages younger than 10. This is true on average, as well as in the subset of children growing up in single-mother-headed households.

5.2 Age at treatment effects for first children

As mentioned previously, due to the cohorts we examine vis-a-vis the change in EITC policy, there is a mechanical relationship in which children who are treated at a younger age are likely to be treated due to an older sibling, while those treated at an older age are likely to be treated due to a younger sibling. The next set of results examines the impact of age at treatment within the first birth order.

The estimates presented in Figures 3-4 represent the average effect of EITC exposure on first children treated at a given age regardless of the duration of that treatment. It is clear that children who are treated at older ages are also treated for a shorter period of time, while those treated at younger ages are treated for a longer period. Moreover, children treated at the same age will experience the same "treatment intensity." These specifications drop treatment intensity and sibling order, but retain the quadratic in sibling age difference (to control for differences in ages between siblings both born before 1994).

The first outcome we examine is, as before, child average rank in the child income distribution at ages 25-26. Children less than age 4 is the omitted category, and we consider children treated up to the age of 18. The solid line plots the coefficient estimates on the age at treatment dummies, and the dashed lines represent the upper and the lower limits of the 95 percent confidence interval around the coefficient.

Figure 3 shows the impact of age at treatment to the two-child EITC based on the age at which a first child acquired a sibling. Appendix Table A1 presents the coefficient estimates and Figure 3 shows the summary plots for all different groups considered in the analysis. A change in the parent's income rank of 1 percentage point results in a change in the child's income rank of 0.27 percentage points. Boys have lower income ranks conditional on all other covariates by 3.5 percentage points relative to girls.

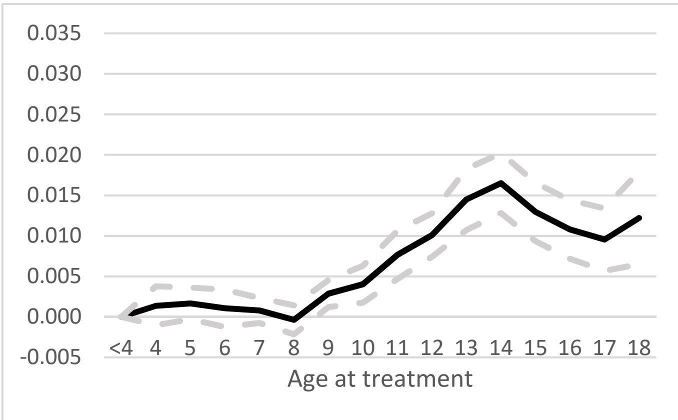
Table 2: EITC and child income rank in their mid-20s: age at treatment time intensity of exposure

	All households		Single mother households	
	Coeff	SE	Coeff	SE
Parent rank	0.263***	0.013	0.264***	0.01
Treatment intensity	0.004***	0.002	0.006***	0.002
Age at treatment				
4	0.008**	0.003	0.005	0.003
xIntensity	-0.006*	0.002	-0.002	0.003
5	0.017***	0.002	0.018***	0.003
xIntensity	-0.012***	0.002	-0.012***	0.003
6	0.017***	0.003	0.023***	0.004
xIntensity	-0.013***	0.003	-0.017***	0.004
7	0.012***	0.002	0.017***	0.003
xIntensity	-0.007*	0.003	-0.008*	0.004
8	0.010***	0.003	0.020***	0.003
xIntensity	-0.007*	0.003	-0.012***	0.003
9	0.011***	0.002	0.021***	0.003
xIntensity	-0.011***	0.003	-0.011**	0.003
10	0.006*	0.003	0.020***	0.004
xIntensity	-0.009**	0.003	-0.016***	0.004
11	0.001	0.002	0.014***	0.003
xIntensity	-0.005	0.003	-0.008*	0.003
12	-0.004	0.002	0.015***	0.003
xIntensity	-0.004	0.003	-0.010**	0.003
13	-0.010***	0.003	0.007*	0.003
xIntensity	0.001	0.003	-0.003	0.003
14	-0.017***	0.003	0.002	0.004
xIntensity	0.007*	0.003	0.004	0.003
15	-0.031***	0.002	-0.015***	0.004
xIntensity	0.014***	0.003	0.014***	0.004
16	-0.052***	0.003	-0.035***	0.004
xIntensity	0.032***	0.003	0.030***	0.003
N	16,000,000			6,000,000

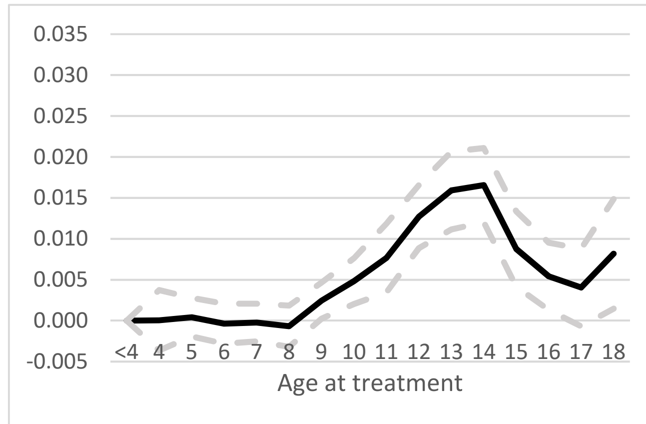
Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, dummies for the child's birth order in the household, and dummies for the total number of children ever residing in the child's household. Standard errors are clustered on the state of household residence level. Census Bureau's Disclosure Avoidance Board release authorization number CBDRB-FY2020-CES005-017.

Figure 3: EITCs expansion and child income rank at ages 25/26

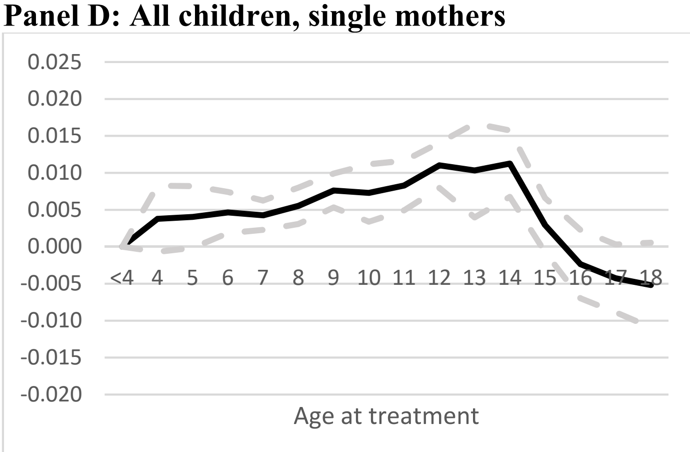
Panel A: All children



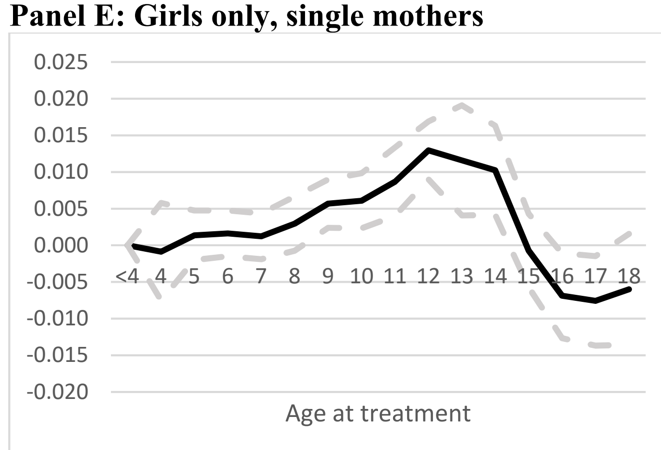
Panel B: Girls only



Panel D: All children, single mothers



Panel E: Girls only, single mothers



Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, and dummies for the total number of children ever residing in the child's household. Standard errors are clustered on the state of household residence level. Census Bureau's Disclosure Avoidance Board release authorization numbers CBDRB-FY2020-CES005-017 and CBDRB-FY2020-CES010-005.

Panels A through C plots age coefficients for the full sample of first children with siblings, while D through F show results for children of single mothers. The results show roughly similar impacts as the previous specification using all children with siblings. There is little to no impact of increased generosity at very young ages, but beginning at age 9, first children's ranks begin to improve. First children who are exposed to the two-child treatment at age 14 are 1.6 percentage points higher in the child income rank within their cohort than children who are exposed before age 4. Separating the analysis by child gender indicates that boys treated at young ages have slightly better outcomes than girls until a dip at age 12, while older teen boys respond more strongly than girls after the age of 14.

For first children of single mothers, positive impacts are seen at earlier ages, with those treated at young school age showing improvements compared with children younger than 4 (a child treated at 6 years old, for example, improves in child rank by about half a percentage point compared with a 3-year-old). Improvements for children of single mothers treated at a young school age is especially dramatic for boys. Girls and boys of single mothers who are treated in their teens show similar patterns as in the full sample.

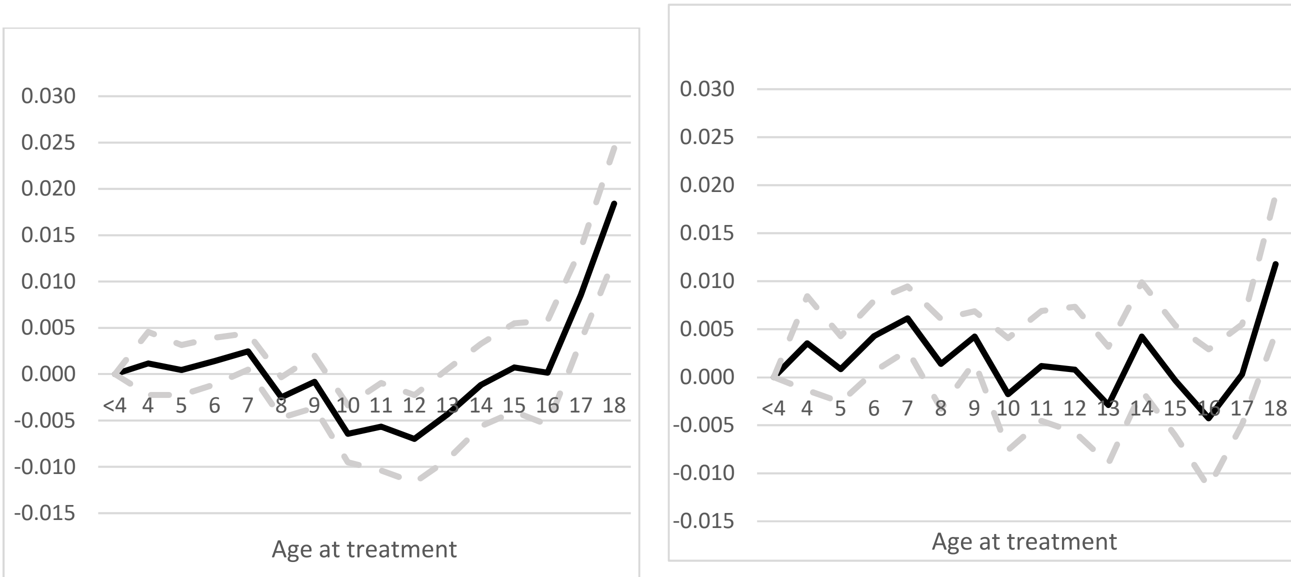
The second outcome considered is the probability of child employment at ages 25-26. We present the results for all households and all children, and then we show estimates for girls only and boys only. We also estimate the model for households with a single mother head. Figure 4 summarizes the results of these estimations.

The age at which children are first treated matters for the effect of EITC expansion on their employment. Considering the sample of all children in Panel A of Figure 4, it is clear that the effects of the expansion are neutral to slightly positive for ages younger than 10, negative for ages 10 through 16, and positive after that. But these averages mask substantial differences in impact across girls and boys. As Panels B and C clearly demonstrate, the effects of EITC expansion on girls' employability are positive throughout the age distribution. The strongest gains accrue to girls who are first treated at ages 13-14, where the size of the effect of EITC exposure is close to 5 percent of the size of the correlation between parental rank and child employability. In other words, a girl who was first treated to extra EITC in her early teenage years, is 1 percentage point more likely to be employed than a girl who was younger than 4 at the time of exposure. The impacts for children from single-mother households show a similar pattern to the full sample.

Table A3 in the Appendix shows the results from Figures 3 in a table format. The coefficient on parental rank stands at 0.21, implying that a 1 percentage point change in the parental rank in the income distribution is associated with a 0.2 percentage points change in the child's probability of employment at around age 25.

Figure 3: Age at EITC expansion and the probability of employment at ages 25/26

Panel A: All children **Panel B: Children of single mothers**



Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, and dummies for the total number of children ever residing in the child's household. Standard errors are clustered on the state of household residence level. Census Bureau's Disclosure Avoidance Board release authorization numbers CBDRB-FY2020-CES005-017.

VI. Discussion

The research presented in this paper is a work in progress. Several other pieces of information will be useful in assessing what we find, including an examination of the impact of the EITC expansion on the labor force participation of mothers in our data, which may go far in explaining some of the differences we see by age and child gender. We also are in a position to examine how the EITC expansion we consider may have changed the rank of single mothers through their labor force participation, which may lead to an overestimate of the gains children make in terms of their own rank.

Other parameters we intend to explore are differences by child race. Further data linkage to available ACS data will allow us to examine other child outcomes, such as education, childbearing, occupation, and homeownership.

Finally, one inspiration for this work is the "movers" literature, which estimates how a family's move from a low-opportunity area to a high-opportunity area influences children's outcomes. An extension of this work is to examine the influence of EITC dollars on family mobility, and whether there is an association between exposure to EITC dollars and moves to better neighborhoods.

Results so far indicate that the timing of EITC generosity, and the number of years over which children were exposed to greater generosity, improved outcomes mainly for older children, although we do find a positive impact for children of single mothers who enter elementary school (especially boys).

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Appendix Tables and Figures

Table A1: EITC expansion and child income rank at ages 25/26 – all households

Effect of age at treatment, all first children, child rank at ages 25/26

	Coeff	SE	Girls Coeff	Girls SE	Boys Coeff	Boys SE
parent rank	0.274	0.012	0.266	0.011	0.281	0.0128
Ages						
4	0.001	0.001	0.000	0.002	0.003	0.001
5	0.002	0.001	0.000	0.001	0.003	0.001
6	0.001	0.001	0.000	0.001	0.002	0.001
7	0.001	0.001	0.000	0.001	0.002	0.001
8	0.000	0.001	-0.001	0.001	0.000	0.001
9	0.003	0.001	0.002	0.001	0.003	0.001
10	0.004	0.001	0.005	0.001	0.004	0.002
11	0.008	0.001	0.008	0.002	0.008	0.002
12	0.010	0.001	0.013	0.002	0.008	0.002
13	0.015	0.002	0.016	0.002	0.014	0.002
14	0.017	0.002	0.017	0.002	0.017	0.002
15	0.013	0.002	0.009	0.002	0.019	0.003
16	0.011	0.002	0.005	0.002	0.023	0.003
17	0.010	0.002	0.004	0.002	0.025	0.003
18	0.012	0.003	0.008	0.003	0.026	0.003
N=	8,000,000		4,000,000		4,000,000	

Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, and dummies for the number of children residing in the child's household. Standard errors are clustered on the state of household residence level Census Bureau's Disclosure Avoidance Board release authorization numbers CBDRB-FY2020-CES005-017 and CBDRB-FY2020-CES010-005.

Table A2: EITC expansion and child income rank at ages 25/26 – single mother heads

Effect of age at treatment, all first children of single mothers, child rank at 25/26

	All		Girls		Boys	
	Coeff	SE	Coeff	SE	Coeff	SE
parent rank	0.277	0.010	0.278	0.009	0.275	0.011
Ages						
4	0.004	0.002	-0.001	0.003	0.009	0.002
5	0.004	0.002	0.001	0.002	0.007	0.003
6	0.005	0.001	0.002	0.002	0.008	0.002
7	0.004	0.001	0.001	0.002	0.008	0.002
8	0.006	0.001	0.003	0.002	0.008	0.002
9	0.008	0.001	0.006	0.002	0.010	0.002
10	0.007	0.002	0.006	0.002	0.009	0.003
11	0.008	0.002	0.009	0.002	0.008	0.002
12	0.011	0.002	0.013	0.002	0.010	0.003
13	0.010	0.003	0.012	0.004	0.010	0.004
14	0.011	0.002	0.010	0.003	0.013	0.003
15	0.003	0.002	-0.001	0.003	0.011	0.003
16	-0.002	0.002	-0.007	0.003	0.013	0.004
17	-0.004	0.002	-0.008	0.003	0.014	0.003
18	-0.005	0.003	-0.006	0.004	0.009	0.004
N	3,000,000					

Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, and dummies for the number of children residing in the child's household. Standard errors are clustered on the state of household residence level. Census Bureau's Disclosure Avoidance Board release authorization numbers CBDRB-FY2020-CES005-017 and CBDRB-FY2020-CES010-005.

Table A3: EITC expansion and the probability of employment at ages 25/26 – all households and single mother heads

Effect of age at treatment, all first children, child works at 25/26

	All children		Children of single mothers	
	Coeff	SE	Coeff	SE
parent rank	0.201	0.006	0.221	0.008
Ages				
4	0.001	0.002	0.004	0.002
5	0.000	0.001	0.001	0.002
6	0.001	0.001	0.004	0.002
7	0.002	0.001	0.006	0.002
8	-0.003	0.001	0.001	0.002
9	-0.001	0.001	0.004	0.001
10	-0.006	0.002	-0.002	0.003
11	-0.006	0.002	0.001	0.003
12	-0.007	0.002	0.001	0.003
13	-0.004	0.002	-0.003	0.003
14	-0.001	0.002	0.004	0.003
15	0.001	0.002	0.000	0.003
16	0.000	0.003	-0.004	0.004
17	0.009	0.003	0.000	0.003
18	0.018	0.003	0.012	0.004
Male	-0.014	0.003	-0.045	0.004
N=		8,000,000	3,000,000	

Notes: all models include state of residence fixed effects, birth cohort fixed effects, controls for difference in ages between the index child and the second sibling, and dummies for the number of children residing in the child's household. Standard errors are clustered on the state of household residence level. Census Bureau's Disclosure Avoidance Board release authorization numbers CBDRB-FY2020-CES005-017.