The Impact of Family Income on Child Achievement

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Abstract

Understanding the consequences of growing up poor for a child's well-being is an important research question, but one that is difficult to answer due to the potential endogeneity of family income. Past estimates of the effect of family income on child development have often been plagued by omitted variable bias and measurement error. In this paper, we use a fixed effect instrumental variables strategy to estimate the causal effect of income on children's math and reading achievement. Our primary source of identification comes from the large, non-linear changes in the Earned Income Tax Credit (EITC) over the last two decades. The largest of these changes increased family income by as much as 20%, or approximately $2,100. Using a panel of over 6,000 children matched to their mothers from National Longitudinal Survey of Youth datasets allows us to address problems associated with unobserved heterogeneity and endogenous transitory income shocks as well as measurement error in income. Our baseline estimates imply that a $1,000 increase in income raises math test scores by 2.1% and reading test scores by 3.6% of a standard deviation. The results are even stronger when looking at children from disadvantaged families who are affected most by the large changes in the EITC, and are robust to a variety of alternative specifications.

1 Introduction

In 2003, 12.9 million children in the U.S. under the age of 18, or more than one in six children, were living in poverty (U.S. Census Bureau, 2004). Given such a high poverty rate, the consequences of growing up poor on child well-being and future success has emerged as an important research topic. Of particular interest is whether income support programs like the Earned Income Tax Credit (EITC) can improve child development. However, the extent to which income maintenance programs, and family income more generally, impact children is not easily estimated.

The major challenge faced by researchers attempting to estimate the causal effect of family income on children's outcomes has been the endogeneity of income. In particular, children growing up in poor families are likely to have adverse home environments or face other challenges which would continue to affect their development even if family income were to increase substantially. These concerns have prevented the literature from reaching a consensus on whether family income has a casual effect on child development (e.g., see Duncan and Brooks-Gunn (1997), Haveman and Wolfe (1995), Mayer (1997)).

Since the mid-1990's, one of the largest federal anti-poverty programs in the U.S. has been the EITC, which provides cash assistance to low-income families and individuals who have earnings from work.[1] Low income families with two or more children can receive a credit of up to 40% of their income in recent years (up to $4,204 in 2003), while families with one child can receive a credit of up to 34%. In 2003, the EITC provided $37.5 billion in income benefits to 20.8 million families and individuals, lifting more children out of poverty than any other government program (Center on Budget and Policy Priorities, 2005). It is natural to ask what effect the EITC and other income maintenance programs have on disadvantaged children. In this paper, we analyze the impact of changes in family income on child cognitive outcomes.

We use a fixed effect instrumental variables (FEIV) strategy to estimate the causal effect of income on children's achievement. Our approach accounts for both permanent unobserved heterogeneity and temporary shocks to children's outcomes which may be correlated with family income. Permanent heterogeneity is dealt with using fixed effects methods, while transitory shocks to the family are addressed using instrumental variables. We estimate the effect of changes in *after-tax/transfer* family income (rather than pre-tax income) on changes in children's outcomes.

[1]See Hotz and Scholz (2003) for a more detailed description of the EITC program and a summary of related research.

As our instrument for changes in after-tax/transfer income, we use predicted changes in after-tax/transfer family income based on predetermined, exogenous family characteristics (such as race and mother's age) and changes in the federal EITC schedule.

To estimate our FEIV model, we use panel data on over 6,000 children matched to their mothers in the Children of the National Longitudinal Survey of Youth (NLSY). These data contain a rich set of income and demographic measures. More importantly, these data have up to five repeated measures of cognitive test scores per child taken every other year, which allows us to account for unobserved child fixed effects.

Our primary source of identification comes from the large changes in the EITC schedule that took place throughout the 1980s and 1990s (see Figures 1 and 2). The largest increase began in 1994, when sizeable expansions of the credit were phased in over a three-year period. From 1993 to 1997, the subsidy rate for low income families with two children rose from 19.5% to 40%, while the maximum allowable credit more than doubled, rising from $1,801 to $3,923 (in year 2000 dollars). Since changes in the federal EITC schedule should not be correlated with idiosyncratic shocks to families, predicted changes in EITC benefits (where the predictions are based on exogenous characteristics of the mother) can serve as an exogenous source of changes in family income.

A simple example helps illustrate our identification strategy. Consider using race as an exogenous predictor of family income. Since blacks have lower family income on average and the EITC expansion in the mid-1990s targeted low income families, black families were more likely to receive an exogenous boost to their family income over that period. If family income has a positive causal effect on children, the increase in EITC income for black families relative to white families should have improved the relative outcomes of black children. Based on this observation, one could construct a simple Wald estimate of the effect of family income on child test scores. That is, one could divide the change in the average difference in test scores between black and white children before and after the EITC increase by the change in the average difference in income. Our estimation strategy is analogous, although we use a vector of exogenous variables to predict income changes and take advantage of changes in the EITC schedule over multiple years to obtain more precise estimates.

We also consider a second novel source of identification, which exploits changes in the national earnings structure over time, to identify exogenous changes in income that vary across families.

For example, consider the dramatic increase in the return to education over the last few decades. This implies that family income has risen more, on average, for more educated families. As a consequence, everything else equal, child test scores from families with more educated parents should have improved relative to those from families with less educated parents. Our estimation strategy also incorporates this insight using multiple years of data and other exogenous parental characteristics to predict income changes.

Using our FEIV approach, we find that current income has a significant effect on a child's math and reading test scores. Our baseline estimates imply that a $1,000 increase in family income would raise math test scores by 2.1% and reading test scores by 3.6% of a standard deviation. The effects are substantially stronger for blacks and hispanics than for whites. When we account for changes in maternal labor supply or consider alternative specifications, our main conclusions are unaffected. All of our findings suggest that supplementing the income of poor parents can significantly increase the scholastic achievement of children.

Our FEIV estimates, while modest, are larger than cross-section OLS or standard fixed effects estimates. One explanation is that income is noisily measured, so that OLS and FE estimates suffer from attenuation bias. It is also possible that income matters more for the most disadvantaged, and that our instrument is largely picking up the effect for these families. Perhaps the most interesting explanation is that expectations about future income play an important role in determining child outcomes. In this case, permanent changes in family income should have larger effects on children than do temporary changes. To the extent that changes in the EITC are expected to last longer than most other shocks to family income, our FEIV estimates should be greater than traditional fixed effect estimates. Additionally, FEIV estimates should be greater during periods when the EITC changed most and for families predicted to receive the largest EITC increases. Our estimates are consistent with all of these predictions.

The remainder of this paper proceeds as follows. In the next section, we provide a brief literature review. Section 3 discusses our strategy for estimating the effect of family income on child outcomes. We then discuss our data and document the large changes in the EITC in Section 4. Section 5 presents the baseline estimates of the effect of income on math and reading test scores, and Section 6 presents robustness checks. We conclude in Section 7.

2 Previous Research

A growing empirical literature questions how poverty affects a child's well-being and whether income support programs can improve children's life chances. However, evidence on the extent to which family income affects child development is mixed. Previous studies differ in data, methods, and findings, as discussed in the recent collection of studies in Brooks-Gunn and Duncan (1997) or the surveys in Haveman and Wolfe (1995) and Mayer (1997).

Researchers have provided several explanations for why family income might affect child development. First, poverty is associated with increased levels of parental stress, depression, and poor health – conditions which might adversely affect parents' ability to nurture their children. For example, in 1998, 27% of kindergartners living in poverty had a parent at risk for depression, compared to 14% for other kindergartners (Child Trends and Center for Child Health Research, 2004). Low income parents also report a higher level of frustration and aggravation with their children, and these children are more likely to have poor verbal development and exhibit higher levels of distractability and hostility in the classroom (Parker et. al, 1999). Extra family income might also matter if parents use the money for child-centered goods like books, for quality daycare or preschool programs, for better dependent health care, or to move to a better neighborhood.[2]

Until recently, empirical studies linking poverty and income to child outcomes have done little to eliminate biases caused by the omission of unobserved family and child characteristics. Most studies employ regressions of an outcome variable (such as scholastic achievement) on some measure of family income and a set of observable family, child, and neighborhood characteristics. While these studies reveal the correlations between income and child outcomes, they do not necessarily estimate a causal relationship as Mayer (1997), Brooks-Gunn and Duncan (1997), and others have pointed out. Children living in poor families may have a worse home environment or other characteristics that the researcher does not observe. These omitted variables may be part of the reason for substandard achievement and may continue to affect children's development even if family income were to rise.

Blau (1999), Duncan, et. al (1998), and Levy and Duncan (1999) use fixed effects estimation strategies to eliminate biases caused by permanent family or child characteristics. All three

[2] Low income parents have fewer children's books in their homes and spend less time reading to their children, markers which are negatively associated with future academic performance. Children in poor families are also less likely to receive adequate health care and nutrition, both of which might affect performance in school. Finally, neighborhood poverty has been associated with underfunded public schools and lower achievement scores among young children (Child Trends and Center for Child Health Research, 2004).

studies use differences in family income levels across siblings to remove fixed family factors when estimating the impacts of income on child outcomes. Using PSID data, both Duncan, et. al (1998) and Levy and Duncan (1999) find that family income at early ages is more important for determining educational attainment whether they control for fixed family effects or not. Using data from the Children of the NLSY, Blau (1999) reaches somewhat different conclusions. When controlling for "grandparent fixed effects" – comparing the children of sisters – he finds larger impacts for "permanent income" than when running standard OLS regressions. On the other hand, he finds smaller and insignificant effects of current family income on ability and behavioral outcomes when he uses fixed effect strategies (regardless of whether he uses comparisons of cousins, siblings, or repeated observations for the same individual). While these papers represent a significant step forward, they do not control for endogenous transitory shocks and they may suffer from severe attenuation bias, since growth rates in income are noisily measured.[3]

Another line of research uses data from welfare and anti-poverty experiments conducted during the 1990s. Except for a recent working paper by Duncan, et. al (2004), these studies focus on program impacts, but do not separate out the effects of family income from other aspects of the programs. Duncan, et. al (2004) combine data from four of these experiments in an attempt to separately estimate the effect of family income versus employment and welfare effects induced by the programs. They find a relatively large effect of family income on school achievement for preschool children but not older children.

The different conclusions reached by recent studies suggest that unobserved heterogeneity may be an important issue. In the following section, we propose a new FEIV strategy which eliminates omitted variable biases due to both permanent and temporary shocks correlated with family income. Our approach also eliminates attenuation bias due to measurement error.

3 Fixed Effect Instrumental Variables Estimation Strategy

3.1 Modeling the Effect of Income on Child Outcomes

A typical model for child i's outcome in period t, y_{it}, is

$$y_{it} = x_i\beta_x + w_{it}\beta_w + \theta I_{it} + \mu_i + \epsilon_{it}, \tag{1}$$

[3]Taking a slightly different approach, Carniero and Heckman (2002) estimate the effects of income at different ages of the child on subsequent college enrollment, controlling for the present discounted value of family income over ages 0-18 of the child (a measure of "permanent income") and math test scores at age twelve. While they estimate significant effects of "permanent income", the estimated effects of income at early childhood ages and at later childhood ages are insignificant.

where family income is represented by I_{it}, permanent family background characteristics which may affect child outcomes are represented by the vector x_i, and temporary or time varying family characteristics are given by the vector w_{it}. A permanent individual fixed effect is captured by μ_i and temporary individual- and age-specific shocks are represented by ϵ_{it}, with $E(\epsilon_{it}) = 0$.

Most previous studies (e.g., see those in Brooks-Gunn and Duncan, 1997), estimate some form of equation (1) using OLS. Because no dataset contains all of the relevant variables that reflect the quality of the child's home environment, failure to account for permanent unobserved heterogeneity μ_i almost certainly yields biased results. Some authors (e.g., Blau, 1999, and Levy and Duncan, 1999) have, therefore, employed fixed effects methods to estimate equation (1). However, when temporary shocks, ϵ_{it}, are correlated with shocks to current family income, I_{it}, both standard OLS and fixed effects estimators will be biased. To the extent that factors affecting parental income in a given year also affect their parenting capacity (e.g., parental depression, sickness in the home, marital stress, or stress associated with moving or a new job), such a bias would seem to be important. In many cases, this bias will be larger for fixed effects strategies than cross-sectional OLS estimation.[4]

Despite the recent emphasis in the literature on fixed effects estimation, it is not clear that this approach produces more accurate estimates than cross-sectional OLS estimation. While fixed effects estimation should eliminate any bias from permanent family or child differences, it may exacerbate bias due to unobserved temporary family shocks. Additionally, fixed effects estimation may magnify any bias due to measurement error in income, since growth rates in income are more noisily measured than levels. Our FEIV strategy addresses both of these potential problems.

To measure full family income, we use the federal tax code to calculate EITC benefits and taxes. Full family income (i.e., post-tax and post-transfer) for child i in period t is given by

$$I_{it} = PI_{it} + \tau_t^{s_{it}}(PI_{it}), \tag{2}$$

where PI_{it} represents reported pre-tax/EITC family income and $\tau_t^{s_{it}}(PI_{it})$ represents net transfers (i.e., EITC less taxes) to the child's family in period t. The function $\tau_t^{s_{it}}(\cdot)$ is given by the federal tax code in year t, where the superscript s_{it} denotes which tax and EITC schedule child i's family faces in year t based on family characteristics like marital status and the number of children

[4] For example, suppose income has a permanent individual-specific component and a stationary autoregressive component, so $I_{it} = \psi_i + \nu_{it}$, $E(\nu_{it}) = E(\nu_{it}\psi_i) = 0\ \forall t$, $E(\nu_{it}\nu_{i,t-j}) = \rho^j\sigma_\nu^2\ \forall j \geq 0$, the variance of ψ_i is σ_ψ^2, and $\rho \in (0,1)$. When $E(\nu_{i,t-1}\epsilon_{it}) = E(\psi_i\epsilon_{it}) = 0$, the ratio of the bias due to correlated transitory shocks from estimating equation (1) in first-differences to that of cross-sectional OLS estimation is $\frac{\sigma_\psi^2+\sigma_\nu^2}{\sigma_\nu^2(1-\rho)} \geq 1$.

qualifying for the EITC. Past studies often ignore the impact of taxes and transfers on family income, even though these amounts can be quite large.[5]

The fixed effect μ_i can be eliminated by transforming all variables into deviations from individual-specific means. Define the deviations operator Δ as

$$\Delta y_{it} = y_{it} - \frac{1}{T_i}\sum_{t=1}^{T_i} y_{it},$$

where T_i is the number of years child i appears in the sample. Applying this deviations operator to each variable appearing in equation (1) yields

$$\Delta y_{it} = \Delta w_{it}\beta_w + \theta \Delta I_{it} + \Delta\epsilon_{it}. \quad (3)$$

This deviations model eliminates individual-specific fixed effects and serves as the starting point for our instrumental variables estimator.

3.2 Constructing the Instrument

Our approach employs an instrumental variable strategy to estimate equation (3), using the fact that total family income is a function of both family characteristics and the tax code. We conceptually separate the set of variables x_i and w_{it} appearing in equation (1) into those that are assumed to determine pre-tax income and those that do not. Denote the subset of exogenous characteristics that affect income as z_{it} (e.g., mother's age, race, education at age 23, and AFQT percentile). Expressing pre-tax (and pre-EITC) family income as a linear function of these exogenous family characteristics and a mean zero error term yields

$$PI_{it} = z_{it}\gamma_t + \eta_{it}. \quad (4)$$

Thus, using equation (2), total family income is simply $I_{it} = z_{it}\gamma_t + \eta_{it} + \tau_t^{s_{it}}(z_{it}\gamma_t + \eta_{it})$. We point out that both γ_t and the function $\tau_t^{s_{it}}(\cdot)$ are allowed to vary over time, and their variation will play an important role in our estimation strategy.

As is typically assumed in fixed effects analyses, we assume $C(\Delta w_{it}, \Delta\epsilon_{it}) = 0$. For the exogenous predictors of pre-tax income appearing in equation (4), we assume strict exogeneity:[6]

$$E(\epsilon_{it}|z_{i1}, ..., z_{iT_i}) = 0 \quad \forall i, t. \quad (5)$$

[5] Mayer (1997), and Duncan, et. al (2004) are notable exceptions.

[6] See Arellano and Honore (2001) for a detailed discussion of this assumption and its use in panel data models. Note that a strict exogeneity assumption involving all w_{it}'s would imply $C(\Delta w_{it}, \Delta\epsilon_{it}) = 0$.

This condition implies that ϵ_{it} is uncorrelated with all past, current, and future values of z_{it}. Moreover, it implies zero covariance between $\Delta\epsilon_{it}$ and any function of past, current, or future values of z_{it}. Although x_i is differenced out in equation (3), some x_i characteristics will be used in our instrumental variable strategy. Therefore, it should be noted that $E(\epsilon_{it}|x_i) = 0$ is assumed to hold for those x_i variables that are included in z_{it}.

A valid instrument must be uncorrelated with $\Delta\epsilon_{it}$. Given assumption (5), any function of z_{it} will meet this criteria. One could, in principle, use z_{it} itself as an instrument for ΔI_{it} as long as $C(\Delta I_{it}, z_{it}|\Delta w_{it}) \neq 0$. For example, if earnings increased more for more educated mothers than less educated mothers due to macroeconomic changes in the labor market, then using maternal education as a z_{it} variable would provide a valid instrument. In a sense, this is based on the same assumptions implicit in a difference-in-differences strategy that compares test score gains among children of low educated mothers with the gains among children of more educated mothers. Of course, the instrument cannot be perfectly collinear with Δw_{it}, or there would be no additional variation induced by the instrument beyond the variables directly determining child outcomes. As such, we cannot use Δz_{it} as an instrument for ΔI_{it} in equation (3) since all time-varying z_{it} variables are a subset of w_{it}. However, more general functions of current and past values of z_{it} can be used as instruments given our assumption in equation (5) as long as those functions are not linear in Δw_{it}.

Our objective, then, is to find an instrument that is highly correlated with changes in family income (or, more precisely, deviations from a family's average income) conditional on Δw_{it} but which is not correlated with temporary family or child shocks, $\Delta\epsilon_{it}$.[7] We use an instrumental variable based on both the exogenous z_{it} variables and exogenous changes in the EITC schedule, taking advantage of the fact that EITC and tax schedules are known functions of pre-tax income.

Construction of our instrumental variable proceeds in three steps. First, we estimate pre-tax/EITC income based on exogenous family characteristics z_{it} (equation 4) using OLS to obtain predicted pre-tax/EITC income: $\widehat{PI}_{it}(z_{it}) = z_{it}\hat{\gamma}_t$. Second, we calculate predicted post-tax/EITC family income. To do this, we calculate the EITC (and other taxes and transfers) based on the appropriate schedule for that year to obtain $\tau_t^{s_{it}}(\widehat{PI}_{it})$.[8] Adding predicted income and the EITC

[7]As we discuss in detail at the end of Section 3.3, our identification strategy relies on the implicit assumption that coefficients in the income equation, γ_t, or the tax schedule $\tau_t^{s_{it}}$ may change over time, while the coefficients in the child outcome equation (equation 1) do not.

[8]In order to apply the correct tax schedule, we make use of current marital status and the number of children in the family. However, to minimize any potential problems with their endogeneity, we do not include these variables in z_{it} when predicting pre-tax/EITC income. To check whether endogeneity of these variables is a problem for our

(plus other taxes and transfers) yields our measure of predicted full family income

$$\hat{I}_{it} = z_{it}\hat{\gamma}_t + \tau_t^{s_{it}}(z_{it}\hat{\gamma}_t),$$

which only depends on strictly exogenous individual characteristics and the tax/EITC schedule for that year. Finally, we apply the deviations operator to $\hat{I}_{it}$ to get

$$\Delta\hat{I}_{it} = \Delta z_{it}\hat{\gamma}_t + \Delta\tau_t^{s_{it}}(z_{it}\hat{\gamma}_t),$$

which serves as our instrumental variable for ΔI_{it} in equation (3).

There are several things worth noting about our instrument. First, it is only a function of the z_{it} variables and the tax code. With strict exogeneity of z_{it} and the assumption that changes in the tax schedule are exogenous with respect to individual family shocks, ϵ_{it}, the instrument is valid (i.e., $C(\Delta\hat{I}_{it}, \Delta\epsilon_{it}|\Delta w_{it}) = 0$). Second, the fact that we use estimates of γ_t to construct $\Delta\hat{I}_{it}$ does not affect the validity of our instrument. As noted earlier, we could have used any function of the z_{it}'s to get a measure of pre-tax/EITC income and still had a valid instrument. Of course, the most natural linear combination would use the true γ_t's, since that would give the best prediction of pre-tax/EITC income. Since we do not know the true γ_t's, we use consistent estimates instead. A third point to note is that we do not need to correct our standard errors in the second stage FEIV regression for the fact that we use estimates of γ_t, since estimation of instruments has no effect on the asymptotic variance of IV estimates (see Newey, 1993). Finally, our approach is not only intuitive, but it is similar in spirit to using the optimal instrument.[9]

estimation strategy, we examined how our results change when we do not use them to assign tax/EITC schedules. For example, consider marital status. We first calculated predicted after-tax income for each child-year using each of the potential (married and head of household) schedules. We also predicted the probability that a mother is married versus unmarried using only the z_{it} characteristics to get predicted probabilities a child's family faces each tax/EITC schedule. Finally, we used these predicted probabilities to compute an expected post-tax/EITC income measure. The results from this exercise were virtually identical and are available on request (the coefficients on current income for both math and reading differ from those appearing in Table 5 by less than 5%). This exercise suggests that endogeneity of marital status and the number of children (insofar as they affect assignment of the appropriate tax schedule) does not bias our results.

[9]If pre-tax income PI_{it} depends on z_{it} as described in equation (4), then the optimal function (up to scale) of the instruments is given by $h(z_{it}) = E(\Delta\epsilon_{it}{}^2|z_{it})^{-1}E(\Delta I_{it}|z_{it})$ (see Newey, 1993). If the error term $\Delta\epsilon_{it}$ is conditionally homoskedastic, the expression (up to scale) simplifies to $h(z_{it}) = \Delta z_{it}\gamma_t + E[\Delta\tau_t^{s_{it}}(z_{it}\gamma_t + \eta_{it})|z_{it}]$. This expression is very similar to our instrument $\Delta\hat{I}_{it}$, although the two are not identical since we use an estimate of γ_t and because $E[\tau_t^{s_{it}}(z_{it}\gamma_t + \eta_{it})] \neq \tau_t^{s_{it}}(z_{it}\gamma_t)$ due to non-linearity of the tax schedule. This non-linearity makes it impractical to use the optimal instrument given by $h(z_{it})$. Although our instrument is not identical to the optimal one (and is, therefore, inefficient), it is still valid.

3.3 Identification

Identification in our FEIV approach requires that our instrument $\Delta\hat{I}_{it}$ be correlated with ΔI_{it} but not perfectly collinear with Δw_{it}. Having established conditions for a valid instrument above, we still must establish that our instrument is not perfectly collinear with Δw_{it}. We now discuss three main sources of variation in $\Delta\hat{I}_{it}$ that can be used to identify the effect of income on children, θ. First, nonlinearity of the tax code can help identify θ, although this source of identification is not particularly interesting, as we discuss below. Instead, we emphasize two other, more important sources of identification. Our second source of identification takes advantage of the highly non-linear changes in the EITC which took place throughout the 1980s and 1990s (see Figures 1 and 2). These changes affected some families more than others and provide an exogenous source of variation in family income over time. As a third source, we exploit changes in the labor market returns to exogenous maternal characteristics (e.g., education) that occurred over this time period. As we discuss below, time invariant covariates provide an important source of identification. These fixed characteristics can be thought of as exclusion restrictions, since they predict changes in post-tax/EITC income but do not appear in the differenced outcome equation (3).

To understand the several sources of identification, it is useful to consider each source individually, with the other sources "turned off". To simplify the discussion, consider two time periods and suppose all individuals face the same tax/EITC schedule, $\tau_t(\cdot)$. In this case, identification requires that

$$\Delta_1\hat{I}_{it} = (z_{it}\hat{\gamma}_t + \tau_t(z_{it}\hat{\gamma}_t)) - (z_{it-1}\hat{\gamma}_{t-1} + \tau_{t-1}(z_{it-1}\hat{\gamma}_{t-1}))$$

cannot be collinear with $\Delta_1 z_{it} = z_{it} - z_{it-1}$ (or, more generally, $\Delta_1 w_{it} = w_{it} - w_{it-1}$).[10]

First, consider identification from nonlinearity in the tax/EITC schedule. To turn off the other sources of identification, suppose (i) there are no changes in the EITC or tax schedules between periods t and $t-1$ (i.e., $\tau_t(\cdot) = \tau_{t-1}(\cdot) = \tau(\cdot)$) and (ii) there is a stable earnings relationship over time (i.e., $\gamma_t = \gamma_{t-1} = \gamma$). Then, identification is achieved via the nonlinearity in the EITC/tax schedule and changes in z_{it} over time. In practice, this identification would come from kinks in the EITC schedule and movements of individuals from one region of the schedule to another over time. With the assumption of a stable earnings relationship over time, this source of identification

[10] With only two periods of data, it is easiest to use first-differences for our instrument and equation (3), which we denote by Δ_1, rather than the more general deviation-from-mean notation.

relies on time varying z_{it}; otherwise, no individual would be predicted to move from one region of the EITC schedule to another.

This first form of identification is not particularly attractive, since it relies heavily on the assumption that the child outcome equation (1) is specified correctly as a linear function of the z_{it} variables. Any misspecification of the relationship between child outcomes and these variables would likely lead to bias. For example, suppose that the child outcome equation was more generally written as

$$y_{it} = x_i\beta_x + g(w_{it}) + \theta I_{it} + \mu_i + \epsilon_{it}. \tag{6}$$

It is easy to see that this model is only identified insofar as $g(w_{it})$ is a known function that differs from $\tau(z_{it})$. In short, without any changes in the $\tau(\cdot)$ function or the γ's over time, one must rely on functional form assumptions for the FEIV strategy to work. Fortunately, our other two sources of identification do not rely on a specific relationship between the z_{it} variables and child outcomes.

A more convincing source of identification comes from the large changes in EITC benefits over time. To focus on this source of identification and eliminate other sources, suppose (i) there is a stable earnings relationship over time (i.e., $\gamma_t = \gamma_{t-1} = \gamma$) and (ii) all z_{it} variables are time invariant (i.e., a subset of x_i), thereby eliminating the previous source of identification. Letting $z_{it} = z_i$, we see that $\Delta_1 \hat{I}_{it} = \tau_t(z_i\hat{\gamma}) - \tau_{t-1}(z_i\hat{\gamma})$, and the only independent source of variation in the instrument comes from variation in $\tau_t(\cdot)$ over time. In general, changes in the EITC or tax schedule can identify θ even in the general model of equation (6).[11] Even though all x_i variables difference out in equation (3), those in z_i directly determine changes in predicted income for the family. With a non-linear change in the tax/EITC schedule, families with different characteristics will experience different predicted changes in their family income. For example, black families (which are poorer on average), should receive a larger boost in income due to expansions of the EITC compared to white families. Children from families with larger increases in predicted EITC payments should exhibit larger improvements in their test scores if there exists a causal relationship between income and test scores.

Figures 1 and 2 show that the changes in the EITC schedule over time have been highly nonlinear. Not only has the maximum benefit amount increased substantially, but the range of family income which qualifies for EITC benefits has also expanded. The maximum credit rose

[11] Even a constant shift up or down of the tax schedule could, in principle, be used to identify θ, but this would require the strong assumption that average child test scores do not change over time.

in real terms (in year 2000 dollars) from \$1,256 in 1990 to \$1,561 in 1991 for families with two or more children. A much larger change in the EITC began in 1994, when sizeable increases in the credit for families with children were phased in over a three-year period. From 1993 to 1997, the subsidy rate for low income families with two children rose from 19.5% to 40% while the maximum allowable credit more than doubled, rising from \$1,801 to \$3,923 (in year 2000 dollars). Since changes in the EITC schedule are not correlated with changes in idiosyncratic shocks to families, these EITC changes should produce a valid instrument.

In contrast to the large changes in the EITC, Figure 3 shows that there have been few changes in the general tax schedule over this same time period.[12] This figure graphs net taxes and transfers (including the EITC) as a function of pre-tax income for every other year from 1987-99 (in year 2000 dollars) for married couples with two children. The graph shows taxes and transfers for those families earning less than \$100,000 in real terms, the income range for most of our NLSY sample. While the large changes in the EITC are evident for incomes below \$30,000, the rest of the tax schedule has been remarkably stable for this income range over the time period used in our analysis. One minor deviation is the year 1999 (the lowest line on the graph), which is slightly lower because of the \$200 per child tax credit introduced that year. The other minor deviation is the year 1987, which had an 11% tax bracket and higher tax rates for high income individuals (individuals earning more than approximately \$90,000 in real terms). The stability of the general tax code suggests that most of the variation in $\tau_t(\cdot)$ over time has come from changes in the EITC schedule for the period of our data.

Our final source of identification comes from changes in the earnings structure over time. To focus on this source of identification, assume (i) there are no changes in the EITC or tax schedules between periods t and $t-1$ (i.e., $\tau_t(\cdot) = \tau_{t-1}(\cdot) = \tau(\cdot)$) and (ii) all z_{it} variables are time invariant (i.e., z_{it}=z_i). In this case, identification is achieved via differences in the financial returns to fixed characteristics over time, since $\Delta_1 \hat{I}_{it} = z_i\hat{\gamma}_t + \tau(z_i\hat{\gamma}_t) - (z_i\hat{\gamma}_{t-1} + \tau(z_i\hat{\gamma}_{t-1}))$. Variation in the earnings structure embodied in γ_t over time can be used as another source of identification, much like changes in the tax or EITC schedule can be used. For example, the fact that the return to education has risen over time should result in a bigger increase in family income for better educated mothers. Relative test scores for children of more educated mothers should, therefore,

[12]The Tax Reform Act of 1986 introduced several changes to the tax code, with large shifts at the high end of the income distribution. To focus our analysis on poor families and on changes in the EITC, we consider the period 1987-2000.

be increasing over time if income positively affects child outcomes. Similar reasoning applies to changes in the coefficients on race or other exogenous variables in the income equation. As in the previous case, time-invariant factors can be used to identify the general model when time-varying factors cannot.[13]

In understanding our identification strategy, it is helpful to think about the general relationship between family income, family background characteristics, and child outcomes. When family income depends on a subset of the background characteristics appearing in equation (1) as we have assumed, identification relies on instruments that alter the relationship between family characteristics and family income but not the relationship between those characteristics and child outcomes. We rely on changes to the EITC schedule and wage structure which alter the relationship between family characteristics and family income over time but do not affect the inter-working of families themselves. Identification from our two preferred sources relies on this assumption. Stated somewhat differently, we allow time to interact with determinants of predicted income, either through changes in the EITC/tax schedule or exogenous changes in the earnings structure. However, we restrict the coefficients in the differenced child outcome equation to remain constant over time. As is typical in the literature, our main results do not allow growth rates in test scores to depend on z_{it} characteristics; however, we explore the importance of this assumption below in Section 6.2.

4 Data

We use data from the Children of the NLSY and the main NLSY sample of mothers. These data are ideal for studying the effects of family income on children for several reasons. First, we can link children to their mothers, and second, we can follow families over time. Third, the NLSY contains repeated measures of various child outcomes and comprehensive measures of family income. Finally, the NLSY oversamples poor and minority families, which provides a larger sample of families eligible for the EITC. We use data drawn from more than 6,000 interviewed children born to over 3,500 interviewed mothers.

The NLSY collects a rich set of variables for both children and mothers repeatedly over time. For children, biannual measures of family background and cognitive and behavioral assessments are available from 1986 to 2000. Detailed longitudinal demographic, educational, and labor

[13] With only two periods of data, time varying z_{it} do not help with this source of identification; however, they can help when more periods of data are used for each person.

market information for the mothers is available annually from 1979 through 1994 and biannually thereafter. Equally important, family income measures are available in all years for the mothers up to 1994 and biannually thereafter.[14] Hence, for children born after 1979, we can compile an income history for almost every year since birth (except for non-responses, of course). While the NLSY contains a broad array of income questions, it does not ask an individual how much they received in EITC payments or paid in taxes.[15] Therefore, we impute a family's federal EITC payment and tax burden using the TAXSIM program maintained by Daniel Feenberg and the NBER.[16] The NLSY data also contain repeated (bi-annual) outcome measures for the children. One of the main benefits of the panel is that we can estimate models which account for fixed effects.

In our analysis, we focus primarily on measures of scholastic achievement in math and reading based on standardized scores on Peabody Individual Achievement Tests (PIAT). The assessments measure ability in mathematics, oral reading ability, and the ability to derive meaning from printed words. From 1986 to 2000, the tests were administered biannually to children five years of age and older.[17] We restrict our main sample to children who take at least one PIAT test within our sample time frame and for whom we can calculate a valid family income measure.

Children are scheduled to take the PIAT tests biannually, so that the maximum number of repeated test score measurements for any child is five.[18] In our empirical analysis, we combine the reading recognition and reading comprehension scores into a single reading measure by taking a simple average. In addition, to make the PIAT test scores more easily interpretable, we create standardized test scores by subtracting off the mean score for the random sample of test takers (i.e., excluding the poor and minority oversamples) and dividing by the sample standard devia-

[14]The survey reports various components of family income, which we add together to generate measures of total pre-tax/transfer family income. See Appendix A for a description of the procedure used to construct total family income and rules used to impute missing income values.

[15]We note that the take-up rate of EITC benefits is high. Both the IRS (2002) and Scholz (1994) estimate that roughly 80 to 87 percent of eligible households receive the credit.

[16]See Feenberg and Coutts (1993) for an introduction to the TAXSIM program. The program can be accessed via the internet at http://www.nber.org/taxsim. We input earned income, marital status, and number of children into TAXSIM, which then calculates EITC payments, and other taxes based on the Federal IRS tax code for each year.

[17]Starting in 1994, the tests were given only to children who had not reached their 15th birthday by the end of the calendar year. Around two percent of children took the PIAT tests after their 15th birthday before this rule was put in place. We include these children in the analysis; the results are very similar if they are excluded.

[18]Children in our sample completed the math and reading recognition tests as scheduled 91% of the time, and the reading recognition test 75% of the time. The number of children taking the PIAT tests in any given year varies from a low of 2,073 (for reading recognition in 2000) to a high of 3,703 (for math in 1993). Many children ages 5-7 do not have valid standardized scores for the reading recognition test, because their scores were out of range based on the national norming sample in 1968. See the NLSY79 User's Guide for details.

tion. Thus, test scores are scaled to have a mean of zero and standard deviation of one in the random sample of test takers; our full sample that includes oversamples of blacks, hispanics, and poor whites typically has a negative mean given that the children in the oversamples are more disadvantaged on average.[19]

Our empirical strategy exploits changes in the EITC over time to create an instrument for changes in family income. Children take the math and reading PIAT tests biannually from 1986 to 2000. We exclude the 1986 survey year (which records lagged income for 1985) to focus our analysis on changes in the EITC, rather than the large changes in the tax code which resulted from the Tax Reform Act of 1986. The large changes in the EITC targeted poor families, while the Tax Reform Act of 1986 introduced much broader changes in the tax structure, especially at the high end of the income distribution. Since we are primarily interested in the effect of income on children from more disadvantaged backgrounds, we prefer to use only the highly non-linear changes in the EITC.[20]

Table 1 provides summary information on family income and EITC eligibility for the years in which our key outcome measures (PIAT math and reading test scores) are available. The third column in the table reveals that median family income (from all sources which can be identified in the NLSY) rose in real terms from $25,874 in 1988 to $50,000 in 2000. The time trend in family income, which outpaced inflation, can partly be attributed to the aging of mothers in this sample. The relevance of changes in the EITC for the children in our sample is also shown in Table 1. Using the income measure in column (3), on average 40% of the children in our sample live in families which qualify for the EITC. This is in large part due to the fact that the NLSY oversamples minorities and poor families, which is ideal for the present study. The number of children in families which benefited from the EITC decreases over time as family incomes rise. However, the average benefit for those who qualify for the EITC increases dramatically over time. Looking at column (6), for the subsample of children in families receiving the EITC with two or more kids, the median benefit nearly triples in real terms, rising from $801 in 1988 to $2,312 in 2000. While EITC benefits amounted to 8 percent of total family income on average for these children in 1988, by 2000 the credit grew to 21 percent of income for families with two or more

[19]As discussed in NLSY79 User's Guide, the initial standardized test scores we begin with are already normalized by age of the child to have a mean of 100 and a standard deviation of 15. Thus, our re-standardized test score distributions are nearly identical within each age group, having close to a mean of zero and standard deviation of one.

[20]Since a limited number of children in the NLSY sample are old enough to take the math and reading tests in the 1986 survey year, excluding the 1986 survey year reduces the sample by less than 7%.

children.

Table 2 describes the sample characteristics of the children, their mothers, and their families. Column 1 provides summary statistics for the entire sample, which includes all children who took at least one PIAT test between 1988 and 2000. There are over seven thousand children in this sample, with each child showing up in three survey years on average. Over half the sample is black or hispanic due to the oversampling of minorities. The average age of mothers is 33 years old, although the youngest mother with child in the sample is 23 years old. When predicting income used to construct our instrumental variable, we use mother's completed education as of age 23 (to avoid any potential endogeneity), while for the outcome regressions, we use current education.[21]

Columns 2 and 3 in Table 2 break down the summary statistics based on whether a child's family is eligible for the EITC. A few striking contrasts stand out. Almost half of the EITC sample is black, compared to only 21% of the non-EITC sample. In addition, only 34% of mothers in the EITC sample are married, compared to 81% in the non-EITC sample. The EITC mothers are also less educated on average and have lower scores on the Armed Forces Qualifying Test (AFQT). Even when the mothers in the EITC sample are married, their husbands have significantly less education, with over one-third of their husbands being high school dropouts. Children in families eligible for the EITC also reside in larger families on average. These differences suggest that some children will be more directly affected by changes in the generosity of the EITC (e.g., black children with unmarried, low educated mothers versus white children with married, highly educated mothers).

5 The Effect of Income on Cognitive Test Scores

In this section, we discuss our estimates of the impact of family income on child math and reading test scores. We first replicate the findings of earlier studies using a much larger sample than previously used. We then examine whether previous OLS and fixed effects estimates are likely to suffer from attenuation bias due to measurement error. We do this using income from lagged survey years (when income is observed but a test score is not) as an instrument for current income. Finally, we explore the effects of income on children using our fixed effects instrumental variable strategy, which accounts for measurement error, permanent unobserved heterogeneity,

[21] 11.5% of mothers increase their education level (as measured by the four categories in Table 2) sometime between the age of 23 and 30.

and temporary unobserved shocks.

5.1 OLS and Fixed Effect Estimates

We begin our empirical analysis by presenting OLS and fixed effects estimates for the effect of family income on child achievement. These estimates use nearly three times the sample size of most earlier studies and are, therefore, substantially more precisely estimated.[22] The top panel of Table 3 reports estimates from regressions of a child's test score on current family income, separately for the PIAT math and reading tests. When only controlling for the age of the child, the estimated effect is large and significant. The estimate implies that a $10,000 increase in family income will increase a student's performance on the PIAT tests by approximately one-tenth of a standard deviation. Including controls for the characteristics of the mother and the child drastically reduces the size of the coefficient. Including information on the spouse and additional controls reduces the coefficient even further, although the estimate remains statistically significant. This set of regressions suggests that current income is correlated with other observed characteristics which also predict whether a child will be successful. The problem with the OLS approach, of course, is that there may be several other unobserved variables which also belong in the regression equation that are correlated with current income.

The second panel of Table 3 uses average income instead of current income as the explanatory variable of interest. Researchers have motivated these types of regressions in several ways. Some argue that an average income measure is more relevant because it measures permanent income (e.g., Blau, 1999). Another benefit is that it reduces the effect of measurement error. As previous research has found, the estimated effects of average income are much larger than the estimated effects of current income. As with the estimates using current income in the top panel, the estimates decline substantially as more background characteristics are included in the regression. Thus, concerns about omitted unobserved characteristics are not fully alleviated.

An alternative estimation approach uses fixed effects, which are shown in the third panel of Table 3. These results are not very sensitive to the inclusion of additional control variables, since most of the covariates used in the upper two panels are time invariant. The fixed effect estimates suggest a much smaller (though statistically significant) effect of income on reading scores than do the cross-sectional OLS estimates. They show no significant effect of income on math scores.

Taken altogether, these patterns are typical of the literature. Estimates tend to be greater

[22]The results in Table 3 correspond closely to those of Blau (1999).

when using measures of income averaged over many years than when using current income alone. While the estimates using average income have some advantages, they do not adequately address concerns about unobserved heterogeneity and may even worsen such problems. It is possible that reductions in measurement error associated with using average income or a higher correlation with unobserved family characteristics may explain why these estimates are typically larger than estimates using only current income. It is also typical to find that fixed effects estimates tend to be smaller than cross-sectional estimates when examining the effects of income on child outcomes. While using fixed effects methods helps mitigate problems with permanent unobserved heterogeneity, it is likely to exacerbate problems associated with measurement error in income. Additionally, neither approach addresses temporary shocks to the family which may directly affect both parental earnings and child development.

5.2 Attenuation Bias Due to Measurement Error in OLS and FE Estimates

Before turning to our fixed effects instrumental variables results, we first examine whether measurement error is likely to be a problem for the OLS and FE estimates appearing in Table 3. As is well-known, income is noisily measured in most surveys, and the NLSY is no exception. If the measurement error is classical, this would bias the OLS estimates towards zero. The problem becomes more severe in a fixed effects regression, since the positive correlation in income over time implies that changes in income will be even more noisily measured than income levels.

We can take advantage of the panel nature of the NLSY data to eliminate the effects of measurement error. As previously mentioned, PIAT tests are administered to children every other year. However, up until 1994, family income measures are collected every year. We can use income from lagged survey years (when income is observed but a test score is not) as an instrument for income in the year a PIAT test is taken. Since income is correlated over time, lagged income should be strongly correlated with current income. If measurement error is the only problem, using lagged income as an instrument should correct any attenuation bias. While this approach only corrects for measurement error and not endogeneity, it provides some insight regarding the magnitude of bias due to mismeasured income in previous studies and the estimates of Table 3. In the following section, we correct for endogeneity as well as measurement error using our FEIV approach described in Section 3.

Table 4 uses the same sample and covariates as in Table 3, with the exception that the sample period only spans 1988 to 1994 (i.e., the years when lagged income is available).[23] The top panel reveals that measurement error is a problem even in levels. The instrumental variable (IV) estimates are larger for both the math and reading estimates; for example, in columns (3) and (6) which control for a variety of observed covariates, the IV estimates are two to three times larger than the OLS estimates. For math, the estimates jump from approximately .020 to .047 while for reading the estimates rise from .016 to .047. As expected, lagged income is strongly positively correlated with current income, which is reflected in the large t-statistics from the first stage.

The bottom panel in Table 4 estimates a fixed effects model, using lagged deviations from means as instruments for deviations from means for the current survey year income measure. Note that the lagged income variables are taken from entirely different survey years compared to the current income variables. Hence, there is no overlap and using the lagged income variables as instruments should eliminate any bias due to measurement error. In contrast to Table 3, the FE estimates which instrument using lagged income are quite similar to the OLS estimates (at least once a rich set of controls are included), calling into question the conclusions from previous studies that use fixed effects regressions to claim that income has little or no effect on child development. Accounting for measurement error using lagged income as an instrument for current income, both cross-sectional and fixed effects strategies suggest similar positive effects of income on math and reading outcomes.

5.3 Baseline Fixed Effect Instrumental Variables (FEIV) Estimates

To overcome the potential criticisms and drawbacks of cross-sectional OLS and FE estimation, we now turn to our FEIV approach, using predicted changes in EITC (and other) income as instruments. Our approach proceeds in three steps. First, we predict income based on variables that are predetermined and exogenous to changes in the EITC. Second, we use this income prediction to calculate predicted EITC and tax payments to generate a measure of predicted after-tax income. In the third step, we use this predicted after-tax income as our instrument in a fixed-effects regression (equation 3).

In the baseline specification, we allow the coefficients in first step OLS regressions to vary year-by-year. In these regressions, we only include covariates that are most credibly exogenous: mother's age and age-squared, race, education at age 23, AFQT score, and dummy variables for

[23] For comparison purposes, we note that the estimates appearing in Table 3 do not change much if the sample is restricted to the time period 1988 to 1994.

whether the mother is foreign born, lived in a rural area at age 14, and lived with both parents at age 14. Except for the quadratic in mother's age, all of these variables are fixed and therefore drop out of the second-stage outcome equation. In addition, none of these variables can change in response to changes in the EITC. The R-squared values for these income prediction regressions range from 0.24 to 0.27 depending on the year. We then calculate predicted EITC payments and predicted taxes using the TAXSIM program. This program applies the Federal IRS tax and EITC schedule to our predicted income variable, resulting in a measure of predicted after-tax income.

In the baseline specifications, predicted after-tax income is strongly correlated with actual after-tax income.[24] Since we will be estimating FEIV regressions, however, what is more relevant is the correlation after accounting for fixed effects. The t-statistics on predicted income in these "first stage" FE regressions (which include all of the covariates appearing in the "second stage" outcome equation) are highly significant. The coefficient on predicted after tax income is 0.67 (se=.07) for the math sample and 0.65 (.08) for the reading sample.[25]

The baseline results from the second stage FEIV estimation procedure are shown in Table 5. The age of the child, the mother, and the spouse are all important determinants of the change in a child's test score. A majority of the other variables are not significant. There seems to be some gain to the child if the mother or father returns to school, and potentially some impact of changes in household composition.[26]

The key finding in Table 5 is that current income is a significant determinant of changes in a child's test performance over time. The estimates for the math test indicate that an additional thousand dollars will increase a student's score by 2.1% of a standard deviation. The estimates for the PIAT reading test are even stronger, suggesting that an additional thousand dollars will raise a child's performance by 3.6% of a standard deviation.[27] These estimated effects are larger than the corresponding cross-sectional OLS and FE estimates of Table 4 that attempt to correct

[24]When referring to "actual after-tax income", we mean after-tax income as calculated by applying the tax code to a family's income. We do not have reported measures of EITC or tax payments. We call this imputed after-tax income "actual after-tax income" to avoid confusion with the imputed "predicted after-tax income".

[25]Using similar first stage regressions of actual after-tax income on the components of predicted after-tax income, the standard errors on the individual components are large, even though the coefficients are jointly significant. For example, the estimate on the EITC component variable is 1.08 (se=.62) and the estimate on predicted taxes is .69 (.68). The joint F-test for all components is highly significant.

[26]Recall that to predict income, we use education at age 23, while we allow current education to enter in the outcome regression. If we use current education to predict income, the results are similar, with somewhat smaller standard errors.

[27]A similar analysis for behavioral problems shows no significant effects, consistent with previous studies (e.g., Blau, 1999).

for measurement error. We discuss a few possible reasons for this at the end of Section 6.

While the estimated impacts of Table 5 are modest, they are also encouraging. They imply that the maximum EITC credit of approximately $4,000 increases the math scores of affected children by one-twelfth of a standard deviation and reading scores by nearly one-sixth of a standard deviation. By comparison, the Tennessee STAR experiment, which spent approximately $7,500 per pupil to reduce class size in elementary school, raised future performance on standardized tests by approximately one-fifth of a standard deviation (Krueger and Whitmore, 2001).

A number of recent studies (e.g., Mulligan, 1999, Murnane, et al., 2000, and Lazear, 2003) estimate the effects of achievement test scores on subsequent earnings. All find similar results: a one standard deviation increase in test scores raises future income by about 12%, holding final schooling levels constant. Taking into account the fact that improvements in test scores also increase schooling attainment, Murnane, et al. (2000) estimate that the full effect of a one standard deviation increase in math test scores is to increase future earnings by 15-20%. Combined with our estimates, every $10,000 increase in family income should raise the subsequent earnings of children by 3-4% (0.21 standard deviation increase in math scores × 15-20% increase in income per standard deviation change).[28] The maximum EITC credit of about $4,000 should, therefore, raise the future incomes of children by about 1-2%.

In Table 5, and throughout the paper, we adjust the standard errors to account for arbitrary heteroskedasticity and correlation over time in a child's error term (ϵ_{it} in equation (1)). Cluster robust standard errors are often not reported for fixed effect estimates, since researchers implicitly assume an i.i.d. process for the error term remaining after the fixed effect component has been removed. That is, researchers implicitly assume that the only correlation over time in a child's error term is the fixed effect component μ_i and that ϵ_{it} is homoskedastic. However, we recognize that temporary shocks to children's outcomes might be correlated over time or have child-specific variances. Indeed, the cluster robust standard errors reported in the tables are generally about 50% larger compared to unadjusted standard errors.

[28] Hanushek and Kimko (2000) show the importance of test scores for economic growth. Their estimates suggest that a one standard deviation change in a nation's test scores is related to a one percent change in growth rates of GDP per capita.

6 Additional FEIV Estimates

In this section, we examine in more detail the relationship between income and children's scholastic achievement using our identification strategy. We provide additional estimates to explore which type of family our instrument is affecting most and whether our main findings are robust to alternative specifications.

6.1 Estimates for Subsets of the Data

It is worth exploring whether income plays an important role in determining the outcomes of children from families most affected by the EITC and its expansion, and how the impacts of income on children differ across families. Figure 4 plots the average EITC payment over time for various family characteristics. The biggest change in the EITC occurred in 1994, but since income reported in a survey year refers to income from the previous year, the increase does not show up until the following survey period. The large changes in the EITC occurring between the 1994 and 1996 survey years most affected disadvantaged families and families with two or more children – families who were already receiving a sizeable credit. For example, children with unmarried mothers experienced an $800 increase in family income on average due to the rise in the EITC. Children with married mothers saw less than a $200 increase. Differential impacts are also apparent by race, maternal education (as of age 23), and number of children in the family. A large part of the difference by number of children can be traced to the fact that the benefit increased far less for single child families (see Table 1).

In Table 6, we provide separate FEIV estimates for the groups appearing in Figure 4. For both the math and the reading outcomes, the estimated effects are generally significant and large for those groups most affected by the EITC expansion of the mid-1990s. The coefficient estimates for the black and hispanic sample are over 2 and 3 times larger for the math and reading outcomes, respectively, compared to the white sample. The difference in the estimates is significant at the 5% confidence level for the reading outcome. There is a large difference in the estimated math coefficients (but only a small difference for reading) when comparing low-educated versus high-educated mothers. For the unmarried sample, both the math and reading estimates are higher, although the standard errors are large for the unmarried sample. The same is true for the comparison between families with one child versus two or more children, although the sample of one child families is so small that the accompanying standard error is large. The fact that a majority of mothers in our sample have two or more children helps with identification from changes in the EITC, since the biggest increases are concentrated among these families. As we discuss below in Section 6.5, these patterns – larger estimated effects for groups most affected by the EITC change – may help explain why our FEIV estimates are larger than traditional fixed effects or cross-sectional OLS estimates.

We also explore whether our estimates differ across time periods characterized by expansion or stability of the EITC. In Table 7, we report FEIV estimates for three periods: 1) years before the large increase in benefits, 2) years straddling the increase in benefits, and 3) years after the large increase in benefits. Each period contains three years of data covering a six-year time interval. In the "pre-EITC increase" and "post-EITC increase" periods, real changes in EITC benefits were relatively minor. During the "straddle" period, the maximum EITC benefit more than doubled (see Figure 2). The estimates suggest a sizeable effect over the straddle period, with no significant effect during the pre- and post-periods. Estimates for the straddle period are approximately twice as large as the baseline estimates in Table 5. In Section 6.5, we offer two potential explanations for these dramatic differences.[29]

6.2 Checking the Source of Identification and Specification Robustness

As a robustness check, in the top panel of Table 8 we shut down the time varying coefficients for predicted income, forcing identification to come through changes in the EITC over time. For this robustness check, we run a single common income regression for all years when predicting family income as in equation (4). The income specification is identical to the year-by-year regressions used in the baseline case, except it does not allow the coefficients on education, race, etc. to vary over time (although we do include year dummies in the regression).

The top panel of Table 8 shows that the FEIV estimated effects of current income are larger and significant at the 10 percent level with this specification. Not surprisingly, the standard errors

[29] If our baseline estimates are identified primarily from changes in the EITC, one might expect more precise estimates for the sample period straddling the EITC expansion than for the pre- or post-periods. Yet, the standard errors are smallest in the post-period. This is because the standard errors are largely driven by the ability to predict pre-tax income changes well rather than changes in the EITC received by a family. Thus, the small standard errors in the post-period reflect more precisely estimated pre-tax income equations (primarily because parents in the sample are older and more economically stable) and not the fact that the EITC changes are unimportant. More to the point, estimates for the pre- and post-periods break down entirely (i.e., first and second stage estimates are very imprecise) if we restrict the γ coefficients in equation (4) to be the same over time as we do for the full sample in Table 8. In contrast, restricted estimates over the straddle period are quite similar to those in Table 7 (with somewhat larger standard errors), suggesting that the EITC expansion is an important source of identification over this period.

for the coefficient of interest rise three-fold for the math sample and seven-fold for the reading sample. The t-statistics for both the math and reading first stage regression fall substantially when restricting the income coefficients to be identical across years. Using a single regression to predict income in all years does a poor job, since the effects of covariates which determine income are not stable over time. For example, the return to education is rising over time in our sample, and black families are losing ground relative to white families. While we are able to estimate the effects of income solely from variation in the EITC (and tax schedule) over time, using variation in the structure of earnings (with respect to education, race, and other exogenous characteristics) substantially improves the precision of our estimates.

To test whether our estimates rely heavily on income entering linearly in the outcome equation, we explore an alternative functional form in the bottom panel of Table 8. In these regressions, we let the log of income explain a child's performance on the math and reading achievement tests (using the log of predicted income as an instrument). The t-statistic from the first stage remains highly significant in this specification. Both the math and reading estimates are significant at the 5% level, suggesting that our findings are robust to other functional forms.

Given our primary sources of identification come from changes in the EITC and in γ_t coefficients over time, we cannot allow for general changes in the effects of z_i characteristics on child outcomes over time (or, equivalently, by child's age).[30] We are forced to restrict the relationship between the exogenous variables used to predict income, z_i, and child test scores at different ages. Our main results assume z_i has the same effect on children at all ages, thereby ruling out differential achievement growth rates by race, parental education, and other fixed child or family characteristics. While this assumption is common in the literature examining the relationship between family income and child outcomes, it is natural to question its significance. To the extent that changes in the relationship between z_i characteristics and after-tax/EITC income do not follow a smooth time trend throughout our sample period, it is possible to allow for differential growth rates in test scores by z_i (i.e. to introduce interactions between z_i characteristics and child's age in equation 1).[31] As discussed earlier, changes in the EITC were more dramatic in the mid-1990s than during other periods, so it may be possible to allow interactions of child's age

[30] Because all of our variables represent deviations from individual-specific means, there is no distinction in our FEIV estimation between child's age, mother's age, and time. Consequently, interactions of z_i with child's age are equivalent to interactions of time and z_i.

[31] Mathematically, including interactions of time invariant z_i characteristics with child's age requires that $z_i(\gamma_t - \gamma_{t'}) + \tau_t(z_i\gamma_t) - \tau_{t'}(z_i\gamma_{t'}) \neq \lambda z_i$ for all t, t'; otherwise, predicted post-tax income is perfectly collinear with the interaction between child's age and z_i once deviations from individual-specific means are taken.

with race and mother's education or AFQT and still have identification.

We find that introducing an interaction of mother's education with child's age has virtually no effect on our estimates. Introducing race and ethnicity interactions with child's age reduces the estimated effect of income on math scores by about one-fifth and reading scores by about two-fifths, while introducing interactions of AFQT terciles with child's age reduces the estimated effects of income by around fifty percent. The effects of income remain statistically significant at the 10% level in each case.[32] While allowing for differential growth rates in test scores by race and mother's AFQT appears to reduce the estimated effects of income on children, it does not change our main conclusion that income has important effects on children's math and reading scores.

6.3 Accounting for Labor Supply Responses

It is natural to question whether the large changes in the EITC generated any labor supply responses among mothers which may have affected children. Most empirical studies have found very small negative effects of the EITC expansions on hours worked by women who were already working. There appears to be a positive effect on labor market participation among single mothers, but minor negative effects on married mothers with working husbands.[33] To the extent that the EITC expansions encouraged single mothers to work, this is likely to negatively bias our estimates if parental time with the child is a positive input into child production. Since the findings reported in Table 6 suggest that the effects of income are larger among single parent families, it is unlikely that this type of bias is empirically very important.

In Table 9, we add a labor force participation variable to the math and reading regression equations. Columns (1) and (3) treat participation as exogenous, but continue to instrument for current income as before. These columns indicate that children with a working mother do somewhat worse, although the negative estimate is significant for the reading outcome only. More importantly, the coefficients on current income are virtually unchanged from the baseline results reported in Table 5.

The endogeneity of which mothers choose to work is an obvious concern for this specification.

[32]Including interactions of mother's education with child age, we estimate income effects on math equal to 0.203 (0.068) and on reading equal to 0.362 (0.071). Including race interactions, we estimate income effects on math equal to 0.163 (0.060) and on reading equal to 0.206 (0.062), Including AFQT interactions, we estimate income effects on math equal to 0.110 (0.062) and on reading equal to 0.169 (0.063).

[33]See Dickert, et. al (1995), Eissa and Hoynes (2004), Eissa and Liebman (1996), Hotz, et. al (2000), Meyer (2002), and Meyer and Rosenbaum (2001).

Therefore, in columns (2) and (4) we treat participation as endogenous and attempt to instrument for whether a mother works. We use the changing parameters of the EITC schedules in each year as additional instrumental variables. In particular, the phase-in rate, the maximum credit, and point at which the credit phase-out begins are likely to be important determinants of participation. They are also likely to be exogenous to any unobserved shocks appearing in the math and reading outcome equations. Using these additional measures as instruments for labor market participation, we re-estimate our model. The estimated coefficients on participation are noisy and statistically insignificant. However, the estimated effects of family income on children are quite similar to those reported in Table 5. Accounting for mother's labor market participation does not appear to affect our main conclusions.[34]

6.4 Time and Age Varying FEIV Estimates

Other researchers have found that income matters more when a child is young than when a child is older. To test this possibility, in Table 10, we allow separate coefficients for income based on whether the child is older than 9, or between the ages of 5 and 9. Somewhat surprisingly, the coefficient estimates for both math and reading are virtually identical for both age groups. One partial reconciliation with the previous literature is that our data do not uncover the effect of income on the very young, since children in our data first take cognitive tests when they are five. Previous studies have found the strongest effects of income during the child's pre-school years. (See, e.g., Duncan, et. al (1998) and Duncan, et. al (2004), which both find stronger effects for children under the age of five.)

Extending our analysis in a different way, we briefly examine whether or not past income affects current child outcomes. That is, do changes in income have lasting effects on children, as human capital theory would suggest? Consider a simple specification based on the idea that income at all past ages may affect current outcomes:

$$y_{it} = x_i\beta_x + w_{it}\beta_w + \sum_{j=0}^{t-1} \alpha_{jt} I_{ij} + \theta I_{it} + \epsilon_{it}. \tag{7}$$

This extends the model of equation (1) by including all past measures of income, where α_{jt} represents the effects of family income earned when the child is age j on the child outcome

[34] In Table 9, participation is defined as a mother working any number of hours for pay. When participation is defined as working more than 250 hours a year for pay, results are very similar. Using hours worked by the mother instead of a dummy variable for a working mother also yields similar findings.

measure at age t. Allowing for linear depreciation of income effects, a reasonable assumption on the α_{jt} terms is $\alpha_{jt} = \alpha_0 + \alpha_1(t-j)$, where α_1 measures the rate at which income effects depreciate over time. A positive α_0 and negative α_1 suggests that income has lasting effects on children but that those effects decline over time. This specification can be estimated by introducing two additional measures of past income to our main specification (equation 1): (i) the sum of all past income and (ii) the sum over all past periods of income in period j times $(t-j)$, or $\sum_{j=0}^{t-1}(t-j)I_{ij}$. We instrument for these additional past income variables using their corresponding predicted after-tax counterparts. The assumptions needed for this regression to be interpreted as causal are stronger, since we now instrument for past income measures but do not allow past values of time-varying covariates to appear in the outcome equation.[35]

Table 11 reports the results for this more general specification. While some of the income coefficients are not individually significant, an F-test reveals that they are jointly significant. For math achievement, it appears that past income matters but depreciates slowly over time. For reading achievement, past income actually appears to appreciate. One explanation for this pattern is that for reading, early investments are important for future reading success and outweigh any effects of depreciation.[36] It would be interesting to estimate a much more general model which allows for income to have different temporary and permanent effects at different ages. However, with our current data, the FEIV approach is not feasible for more general models.

6.5 Discussion of FEIV Estimates

Our FEIV results indicate small, but encouraging, effects of family income on children's scholastic achievement. Although our estimates are modest in an absolute sense, they are large relative to most estimates in the literature and relative to the OLS and FE estimates reported in Table 3.[37] As a comparison of Tables 3 and 4 shows, measurement error may explain some of the difference between our FEIV estimates and those of most previous studies using OLS or fixed effects estimation. Yet, there is still a sizeable difference between the estimates which only correct for measurement error (around 0.05 for both math and reading) and our baseline FEIV estimates in Table 5 (0.21 for math and 0.36 for reading).

[35] Since few covariates vary over time, this is not as strong of an assumption as it might first appear. It essentially requires that changes in past marital status and the number of children in the household do not affect a child's current test scores.

[36] Research has documented that early language and literacy development is related to future academic success and that reading deficits persist and even widen throughout a child's school years (Otaiba and Fuchs, 2002).

[37] Duncan, et. al (2004) is a notable exception, which also finds a sizeable effect.

Table 6 suggests that some of this discrepancy may be due to the fact that income matters more for the most disadvantaged and that our FEIV estimate largely reflects the effect for disadvantaged families that are predicted to benefit from EITC increases. When we condition on disadvantaged and advantaged samples and repeat the exercise of correcting only for measurement error (i.e., instrumenting with lagged income as in Table 4), we find some evidence that supports this explanation. For example, for the conditional sample of unmarried, black, less-educated (high school or below) mothers, the estimates are .11 (se=.04) for math and .09 (se=.04) for reading. In contrast, for the conditional sample of married, white, high-educated mothers, the estimates are -.01 (se=.02) for math and .001 (se=.02) for reading.[38]

An interesting third explanation recognizes that an expansion of the EITC will raise the incomes of some families for many years. To the extent that families are forward-looking and react to this long-term rise in income more than they would a transitory increase in income, we might expect larger estimated effects using the EITC expansion as an instrument than we would from a transitory exogenous shock to income. Appendix B develops this idea more formally, letting shocks to both γ_t and the tax structure be either transitory or permanent. The results show that the FEIV estimator converges to a weighted average of the effect of a one-period current income change and the effect of a permanent increase in income, where the weights depend on the fraction of predicted income changes ($\Delta\hat{I}_t$) due to permanent vs. transitory shocks. If changes in predicted income are largely temporary, our FEIV estimator will tend to estimate the effect of a single-period income change. On the other hand, if the changes are largely permanent, our estimator will be much closer to the effect of a permanent income change. More generally, one can read the estimated effects in Table 5 as an over-estimate of the impact of increasing income by $10,000 for one year and an under-estimate of the impact of increasing income by $10,000 every year from now into the forseeable future.

Comparing the FEIV estimator with a standard fixed effects estimator when individuals are forward-looking, Appendix B shows that the FEIV estimator should be larger whenever income shocks are not correlated with the error in the child outcome equation (i.e., no endogeneity bias). In the absence of endogeneity bias, the standard fixed effects estimator also converges to a weighted average of the impact of a single-period income change and a permanent income change; however, it places much more weight on the smaller effect of a single-period income change. This

[38]These conditional estimates are analogous to the top panel cross-sectional IV estimates of columns (3) and (6) in Table 4.

offers an interesting explanation for the pattern of estimates in Tables 3, 4, and 5.

Estimates in Tables 6 and 7 also lend credibility to the hypothesis of forward-looking families. Assuming families view the EITC changes as permanent, the greater the fraction of predictable income changes due to changes in the EITC, the larger should be the FEIV estimate. Table 6 reveals that the FEIV estimates are largest for the family types that experienced the largest change in their EITC benefits, while Table 7 shows substantially larger estimates for the period when the EITC expanded. Both of these patterns are consistent with forward-looking families and the fact that EITC changes are long-lasting.

7 Conclusion

Understanding the consequences of growing up poor for a child's well-being is an important research question, but one that is difficult to answer due to the potential endogeneity of family income. The question is particularly interesting to policymakers, since part of the explicit rationale for income support programs (such as the EITC) is to improve the lot of children. Past estimates of the effect of family income on child development have often been plagued by omitted variable bias. That is, children growing up in poor families are likely to have home environments or face other challenges which would continue to affect development even if family income rose substantially.

In this paper, we use a fixed effect instrumental variables (FEIV) strategy to estimate the causal effect of income on children's math and reading achievement. Using a panel of over 6,000 children matched to their mothers allows us to address problems associated with both unobserved heterogeneity and endogenous transitory income shocks. Our FEIV strategy relies on two sources of identification from exogenous changes in family income. The first source derives from the large, non-linear changes in the EITC over the last two decades. The largest of these EITC changes doubled benefit amounts for some families, accounting for as much as $2,100 in extra income. On average, the EITC expansion raised income by nearly 10% for EITC eligible families with two or more children. We also propose a somewhat novel approach to identification that exploits well-known exogenous changes in the earnings structure over time, such as the rising return to education. The fact that these changes affected some families more than others is used to estimate the impacts of exogenous changes in family income on child test scores.

Our results indicate that current income has significant effects on a child's math and reading test scores. Our estimates imply that a $1,000 increase in income raises math test scores by 2.1% and reading test scores by 3.6% of a standard deviation. The results are even stronger when looking at children in families most likely to be affected by the large changes in the EITC, and are robust to a variety of specifications, including the inclusion of maternal labor supply. We also find some evidence of interesting dynamic relationships between past income and current outcomes, although we are limited in the dynamics we can incorporate. Finally, we uncover evidence consistent with the hypothesis that families are forward-looking and that expectations about future income affect child outcomes.

We speculate that our estimates are larger than those of most previous studies due to a combination of three reasons: (i) the elimination of attenuation bias due to measurement error, (ii) a larger impact for the disadvantaged children affected by our instrument, and (iii) the income changes associated with the EITC are long-lasting and families are forward-looking. It would be interesting to see future research on the relationship between child outcomes and family income incorporate additional dynamics to explore more deeply some of these issues.

For children growing up in poor families, extra income does appear to have a positive causal effect. While our estimated effects are modest, they are also encouraging. They imply that the maximum EITC credit of approximately $4,000 increases the math scores of affected children by one-twelfth of a standard deviation and reading scores by nearly one-sixth of a standard deviation. Based on previous estimates of the effects of test scores on subsequent earnings, our results suggest that the EITC raises the future earnings of affected children by as much as 1-2%.

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Appendix A: Description of NLSY Children Data Creation

Child Characteristics

Most child characteristics are taken directly from the Children of the NLSY survey responses. In addition, we create normalized measures of PIAT math and reading using the standardized scores. These scores are initially normed by the NLSY based on a random sample of 1968 children to have a constant mean (100) and standard deviation (15) for each age. For interpretation purposes, we re-normalize math, reading recognition, and reading cognition scores by subtracting the sample mean from the NLSY random sample and then dividing by the sample standard deviation. This produces individual test scores with a mean of zero and standard deviation of one for the random sample of respondents. To create a combined reading measure, we sum the normalized reading scores and re-normalize so that this measure is mean zero and has a standard deviation of one.

Parental Characteristics

Most parental characteristics are taken directly from the NLSY survey responses. Additionally, we create an age adjusted, normalized AFQT measure using the percentile scores reported based on the 1979 calculation. We first create a normalized value by subtracting off the mean from the random sample and dividing by the sample standard deviation. Then, we regress these normalized scores on age dummies and use the residuals from this regression as our adjusted AFQT measure. We also fill in missing values for education and marital status using observed values in surrounding years.

Family Income

We calculate total family income combining all available measures of income in the NLSY, deflating them using the annual CPI-U so that they are in year 2000 dollars. Because many of the income measures are missing in one or more years, imputations are necessary. First, we describe the available measures of family income that come from a battery of income questions that differ slightly from year to year; then, we discuss imputation of missing values.

We include all reported income of the respondent (i.e., the child's mother) and her spouse (or partner), including income from wages, salary and tips, business or farm income, and income from military service, unemployment income, educational benefits, veteran benefits, worker compensation or disability payments, income from savings, rental income, social security income, welfare/AFDC, alimony, and child support. We also include the total amount of money received by the respondent (or wife/husband) from persons living outside the household and from other related adults that live in the household. Summing income from all of these sources produces our measure of total family income.

We adopt two imputation procedures after setting missing values with a valid skip to zero and values greater than $200,000 to missing. The first method uses family-level regressions of total family income on the mother's age and age-squared, using all positive income measures. From these estimates, income is imputed for all missing years if at least 8 non-missing income measures are available. Negative imputed values are set to missing. The second method imputes missing values for components of income and then sums those components for an imputed measure of total family income. The components include (1) respondent's earned income plus income from unemployment compensation, education benefits, and income from persons living outside the house; (2) spouse's earned income plus income from unemployment compensation and education benefits; (3) partner's income from any source; (4) income from other sources (e.g., savings, social security, rental income, veteran's benefits, worker compensation or disability payments); (5) total income from other person's living in the household; (6) income from alimony and child support; and (7) mother's welfare income. Missing values for components (1) to (3) are imputed from family-specific regressions on mother's age using all positive income measures (at least 8 positive observations are required for imputing component 1 and at least five are required for imputing components 2 and 3). Missing values for components (4)-(6) are imputed from family-specific means of all positive values if at least three non-missing observations are available; if all values are missing or zero, then missing values are set to zero. Missing values for component (7) are imputed from a regression on the number of children in the household, mother's education, reported income of the mother and spouse (i.e., the sum of components 1 and 2), and reported income-squared. If the imputed value is negative or reported income of the mother and spouse is greater than $40,000, the value is set to zero. After a few other minor adjustments, these components are summed to create our second imputed measure of total family income. Any imputed total income values equal to zero or greater than $200,000 are set to missing and not used in our analysis.

Our final measure for total family income replaces missing values with their imputed values from the second procedure if available; if no imputation from that procedure can be made, the imputed value from the first procedure is used. We use the second imputation as the primary method, since the respondent's earned income and spousal earned income are often reported when other income components that typically make up only a small fraction of total family income are missing. The second imputation procedure uses the actual observed components and only imputes values for those components that are missing. So, if an income measure for a minor component of total income is missing, the second procedure only imputes that component and uses the actual value reported for other more important components in generating our measure of total family income.

We note that varying the imputation method has little effect on estimates of the effect of family income on our child outcome measures. The correlation between all imputed total income measures using method 1 with all imputed total income measures using method 2 is 0.79. The correlation for all total income measures (including those that are non-missing) using the two different methods is 0.95. More detailed notes on the imputation procedure are available from the authors upon request.

Appendix B: Permanent Income Model and Estimation

In this appendix, we discuss FEIV and fixed effects estimation approaches when child outcomes depend on 'permanent income', defined here as the expected present value of current and future income, rather than only current income. Under reasonable assumptions, we show that our FEIV estimator converges to a weighted average of the effect of changing income in only the current period and the effect of changing income in the current period plus all future periods. In the absence of endogeneity bias, the standard fixed effects estimator also converges to a weighted average of the two effects. The weights depend on the fraction of variation in income changes that can be explained by transitory vs. permanent shocks. The fixed effects estimator places more weight on the effect of a single-period income change than the FEIV estimator and is, therefore, smaller (in the absence of endogeneity bias).

For expositional purposes, we simplify the problem here and only consider permanent characteristics, z_i, that may affect earnings. We also limit discussion to the case when the relationship between after-tax income and z_i characteristics is linear.[39] Specifically, assume family income can be written as

$$I_{it} = \psi_i + z_i\gamma_t + v_{it},$$

[39] Of course, z_i could include polynomials, indicator functions, or splines in any underlying characteristic, so it does allow for fairly general relationships.

where ψ_i is an individual fixed effect and v_{it} is an iid transitory mean zero income shock.

Here, the coefficient vector γ_t embodies the effects of characteristics on pre-tax income along with any effects of the tax code on post-tax income. This coefficient vector may vary over time due to changes in either the earnings structure or the tax code. To focus on the role of transitory vs. permanent changes in the earnings structure and tax system, consider the case $\gamma_t = \gamma + \eta_t + \omega_t$, where η_t are transitory shocks and ω_t are permanent shocks that follow a random walk. That is, assume

$$E(\eta_{t+1}|\eta_t) = E(\eta_t) = 0,$$

and

$$E(\omega_{t+1}|\omega_t) = \omega_t, \qquad E(\omega_t) = 0.$$

We further assume that each of the time-specific shocks are independent of each other so

$$v_{it} \perp\!\!\!\perp (\eta_{t'}, \omega_{t'}) \quad and \quad \eta_t \perp\!\!\!\perp \omega_{t'}, \quad \forall t, t'.$$

Given these assumptions and a discount rate $\beta \in (0, 1)$, it is possible to compute the expected discounted present value of future family income conditional on current information, Ω_{it}, which we denote by $\bar{I}_{it} = E\left(\sum_{j=0}^{\infty} \beta^j I_{i,t+j}|\Omega_{it}\right)$. (For simplicity, we assume an infinite horizon.) Assuming that an individual's information set includes z_i, ψ_i, and γ, along with all current values of the shocks affecting income $(\eta_t, \omega_t, v_{it})$,

$$\begin{aligned}
\bar{I}_{it} &= E\left(\sum_{j=0}^{\infty} \beta^j I_{i,t+j}|z_i, \psi_i, \eta_t, \omega_t, v_{it}\right) \\
&= (\psi_i + z_i[\gamma + \eta_t + \omega_t] + v_{it}) + \sum_{j=1}^{\infty} \beta^j (\psi_i + z_i[\gamma + E(\eta_{t+j} + \omega_{t+j}|\eta_t, \omega_t)]) \\
&= (\psi_i + z_i[\gamma + \eta_t + \omega_t] + v_{it}) + \sum_{j=1}^{\infty} \beta^j (\psi_i + z_i[\gamma + \omega_t]) \\
&= B(\psi_i + z_i[\gamma + \omega_t]) + z_i\eta_t + v_{it}
\end{aligned}$$

where the constant $B = \frac{1}{1-\beta} > 1$.

Suppose child outcomes at date t depend on the expected present value of lifetime income as of that date such that

$$y_{it} = x_i\beta + \theta\bar{I}_{it} + \mu_i + \epsilon_{it}, \tag{8}$$

where we will assume that $E(\epsilon_{it}|z_i) = 0$ and $\epsilon_{it} \perp\!\!\!\perp (\psi_i, \eta_{t'}, \omega_{t'})$ for all t, t'. (As above, we simplify the exposition by abstracting from time-varying individual or family characteristics.)

As we will consider fixed effects models, it is useful to consider each of the relevant variables in deviations from their individual means:

$$\begin{aligned}
\Delta I_{it} &= z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, \\
\Delta \bar{I}_{it} &= B z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, \\
\Delta y_{it} &= \theta \Delta \bar{I}_{it} + \Delta\epsilon_{it}.
\end{aligned}$$

Our FEIV estimation method uses changes in predicted income

$$\Delta \hat{I}_{it} = z_i \Delta \hat{\gamma}_t$$

as an instrumental variable for ΔI_{it} in a regression of changes in outcomes on changes in current income ($\Delta y_{it} = \tilde{\theta} \Delta I_{it} + \Delta \tilde{\epsilon}_{it}$). With consistent estimates of γ_t (for all observed periods) and the assumptions above, this estimator, $\hat{\theta}^{FEIV}$, converges in probability to

$$\begin{aligned}
plim\ \hat{\theta}^{FEIV} &= \frac{Cov(\Delta y_{it}, \Delta \hat{I}_{it})}{Cov(\Delta I_{it}, \Delta \hat{I}_{it})} \\
&= \theta \left[\frac{Cov(\Delta \bar{I}_{it}, \Delta \hat{I}_{it})}{Cov(\Delta I_{it}, \Delta \hat{I}_{it})} \right] \\
&= \theta \left[\frac{Cov(B z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, z_i \Delta\hat{\gamma}_t)}{Cov(z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, z_i \Delta\hat{\gamma}_t)} \right] \\
&= \theta \left[\frac{Cov(B z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, z_i \Delta\omega_t + z_i \Delta\eta_t)}{Cov(z_i \Delta\omega_t + z_i \Delta\eta_t + \Delta v_{it}, z_i \Delta\omega_t + z_i \Delta\eta_t)} \right] \\
&= \theta \left[\frac{B Var(z_i \Delta\omega_t) + Var(z_i \Delta\eta_t)}{Var(z_i \Delta\omega_t) + Var(z_i \Delta\eta_t)} \right] \\
&= (1 - \Pi^{FEIV})\theta + \Pi^{FEIV} \left(\frac{\theta}{1-\beta} \right), \qquad (9)
\end{aligned}$$

where

$$\Pi^{FEIV} = \frac{Var(z_i \Delta\omega_t)}{V(z_i \Delta\omega_t) + Var(z_i \Delta\eta_t)}.$$

Equation (9) is quite intuitive once one recognizes that θ represents the effect of increasing current income by a dollar, while $\frac{\theta}{1-\beta}$ reflects the impact of permanently increasing income by a dollar every year from the current period into the infinite future. Thus, when 'permanent' income determines child outcomes, our FEIV estimator produces a weighted average of the effect of a single-period income change and a permanent income change, where the weight on the latter effect, Π^{FEIV}, equals the fraction of the variance in predicted income changes explained by changes in permanent income coefficient shocks. If shocks determining γ_t are largely temporary, our FEIV estimator will tend to estimate the effect of a single-period income change. On the other hand, if the shocks are largely permanent, our estimator will be much closer to the effect of a permanent income change. As $\frac{1}{1-\beta} > 1$, our FEIV estimator will tend to be biased upward for the single-period effect and downward for the effect of a permanent income change.

Now, consider the standard fixed effects estimator. This estimator converges to

$$\begin{aligned} plim\ \hat{\theta}^{FE} &= \frac{Cov(\Delta y_{it}, \Delta I_{it})}{Var(\Delta I_{it})} \\ &= (1-\Pi^{FE})\theta + \Pi^{FE}\left(\frac{\theta}{1-\beta}\right) + \frac{Cov(\Delta\epsilon_{it}, \Delta v_{it})}{Var(z_i\Delta\omega_t) + Var(z_i\Delta\eta_t) + Var(\Delta v_{it})}, \end{aligned}$$

where Π^{FE} represents the fraction of the total variance in income changes (predictable and unpredictable) due to permanent coefficient shocks:

$$\Pi^{FE} = \frac{Var(z_i\Delta\omega_t)}{Var(z_i\Delta\omega_t) + Var(z_i\Delta\eta_t) + Var(\Delta v_{it})}.$$

Comparing Π^{FE} with Π^{FEIV}, observe that

$$\Pi^{FE} = \left[\frac{Var(z_i\Delta\omega_t) + Var(z_i\Delta\eta_t)}{Var(z_i\Delta\omega_t) + Var(z_i\Delta\eta_t) + Var(\Delta v_{it})}\right]\Pi^{FEIV} \le \Pi^{FEIV}.$$

Thus, in the absence of endogeneity bias (i.e., if $Cov(\Delta\epsilon_{it}, \Delta v_{it}) = 0$), the fixed effects estimator will be smaller and closer to the effect of a single-period income change, θ, than the FEIV estimator. Intuitively, this is because the fixed effects estimator incorporates more idiosyncratic fluctuations in income embodied in Δv_{it}. If most of the variation in income changes is difficult to predict when projecting those changes onto z_i (i.e., the R^2 in a regression of income changes on z_i characteristics is very low), Π^{FE} will tend to be much smaller than Π^{FEIV} and the two estimators may differ considerably.

Table 1. Family Income, EITC Eligibility, and EITC Income over Time.

				For Children in Families Qualifying for the EITC			
						Average EITC Payment as a Fraction of Pre-Tax Family Income	
Year (1)	**Number of Children** (2)	**Median Lagged Family Income (in year 2000 $)** (3)	**Fraction of Children in EITC Eligible Families** (4)	**Median Lagged Family Income (in year 2000 $)** (5)	**Median EITC Payment (in year 2000 $)** (6)	**1 Child Families** (7)	**2+ Child Families** (8)
1988	2,955	25,874	.46	12,978	801	.08	.08
1990	3,134	30,193	.44	14,286	981	.08	.08
1992	3,727	31,314	.42	14,718	1,193	.09	.09
1994	3,705	34,044	.39	14,649	1,344	.10	.11
1996	3,493	39,362	.37	16,401	2,163	.14	.18
1998	3,196	41,520	.36	16,150	2,294	.15	.21
2000	2,336	50,000	.30	15,970	2,312	.14	.21
All Years	22,546	35,078	.40	14,770	1,223	.10	.13

Notes: Data are from the Children of the NLSY linked to their mothers in the main NLSY79. The unit of observation is a child. For each year, the sample includes those children taking a math or reading PIAT test that year. Year in column (1) refers to the NLSY survey year; income and EITC payment variables refer to the previous year's income. Income in this table is pre-tax and pre-transfer.

Table 2. Sample Characteristics for Children, Their Mothers, and Their Families.

	Entire Sample (1)	**Eligible for EITC** (2)	**Not Eligible for EITC** (3)	**Difference (2)-(3)** (4)
Median Family Income (in year 2000 $)	35,078	14,770	51,483	36,713
Mother Variables				
Age	33.1	32.4	33.5	-1.1
Race				
Black	.32	.49	.21	.28
Hispanic	.21	.22	.20	.02
White (not Hisp.)	.47	.29	.59	-.30
Education (at age 23)				
H.S. Dropout	.23	.39	.13	.26
High School	.49	.47	.50	.03
Some College	.20	.14	.25	-.11
College Graduate	.08	.01	.12	-.11
AFQT Score (normalized & age adjusted)	-.46	-.91	-.16	-.75
AFQT Score Missing	.03	.03	.03	.00
Foreign Born	.07	.07	.07	.00
Lived in Rural Area (at age 14)	.21	.20	.21	-.01
Lived with Both Parents (at age 14)	.64	.53	.71	-.18
Married	.63	.34	.81	-.47
Number of Children				
One	.11	.10	.12	-.02
Two	.39	.32	.44	-.12
Three	.30	.30	.30	.00
Four or more	.20	.27	.15	.12
Child Variables				
Age	9.5	9.7	9.3	.4
Male	.50	.50	.50	.00
Spouse Variables (for Married Mothers)				
Age	36.2	35.8	36.2	-.4
Education				
H.S. Dropout	.16	.36	.12	.24
High School	.45	.48	.44	.04
Some College	.21	.12	.22	-.10
College Graduate	.18	.04	.21	-.17
Additional Variables				
Mother's Mother Present in HH	.06	.08	.05	.03
Mother's Father Present in HH	.03	.03	.03	.00
Number of Adult Family Members in HH	1.8	1.6	2.0	-.04
Years of Schooling Completed				
By Mother's Mother	9.6	8.6	10.3	-1.7
By Mother's Father	8.5	6.7	9.6	-2.9
Observations	22,546	8,930	13,616	
Number of Children	7,374	3,839	5,317	

Notes: Data are from the Children of the NLSY linked to their mothers in the main NLSY79. The sample includes children taking a math or reading PIAT test in the 1988 survey year or later. The unit of observation is a child-year, where a child and his or her parents can appear repeatedly in the sample. The Additional Variables have some missing values (around 1 percent), so the sample sizes for these variables are smaller.

Table 3. OLS and Fixed Effect Estimates of the Effect of Income on a Child's Test Scores.

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
			OLS Estimates			
Current Income	.1028**	.0318**	.0196**	.0955**	.0301**	.0162**
	(.0035)	(.0035)	(.0036)	(.0036)	(.0037)	(.0038)
Control Variables						
Age & Age^2 of Child	X	X	X	X	X	X
Mother & Child Controls		X	X		X	X
Spouse & Additional Controls			X			X
R-squared	.090	.214	.223	.142	.267	.278
Observations	22,476	22,476	22,476	18,568	18,568	18,568
Number of Children	7,371	7,371	7,371	6,757	6,757	6,757
			OLS Estimates with Average Income			
Average Income	.3349**	.1110**	.0808**	.3170**	.1126**	.0712**
	(.0120)	(.0122)	(.0129)	(.0117)	(.0124)	(.0132)
Control Variables						
Age & Age^2 of Child	X	X	X	X	X	X
Mother & Child Controls		X	X		X	X
Spouse & Additional Controls			X			X
R-squared	.115	.216	.224	.168	.270	.279
Observations	22,476	22,476	22,476	18,568	18,568	18,568
Number of Children	7,371	7,371	7,371	6,757	6,757	6,757
			Fixed Effect Estimates			
Current Income	.0024	.0022	.0008	.0091**	.0085**	.0085**
	(.0041)	(.0042)	(.0043)	(.0041)	(.0041)	(.0043)
Control Variables						
Age & Age^2 of Child	X	X	X	X	X	X
Mother & Child Controls		X	X		X	X
Spouse & Additional Controls			X			X
R-squared	.012	.014	.016	.085	.088	.095
Observations	21,407	21,407	21,407	17,391	17,391	17,391
Number of Children	6,302	6,302	6,302	5,580	5,580	5,580

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Data are from the Children of the NLSY linked to their mothers in the main NLSY79. The sample includes children taking a math or reading PIAT test in the 1988 survey year or later. The unit of observation is a child-year, where a child and his or her parents can appear repeatedly in the sample. Mother, Child, Spouse, and Additional variables are as defined in Table 2, with the addition of quadratic terms in age of the mother, child, and spouse. Year dummies are also added in columns (3) and (6). Dummies indicating whether Additional variables are missing are also included in the regressions (around 1 percent are missing). There is also a dummy for a missing AFQT score (around 3 percent are missing); the average of the AFQT variable is substituted for individuals with missing AFQT scores.
**Significant at the 5% level, *significant at the 10% level

Table 4. Cross-Sectional and Fixed Effect Estimates of the Effect of Income on a Child's Test Scores, Instrumenting Current Income with Lagged Income.

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
			Cross-Sectional IV Estimates			
Current Income	.1419**	.0581**	.0470**	.1385**	.0614**	.0469**
	(.0067)	(.0071)	(.0083)	(.0068)	(.0073)	(.0084)
Control Variables						
Age & Age^2 of Child	X	X	X	X	X	X
Mother & Child Controls		X	X		X	X
Spouse & Additional Controls			X			X
t-statistic from 1st Stage	53.26	41.26	33.68	47.29	36.91	30.18
Observations	13,464	13,464	13,464	10,918	10,918	10,918
Number of Children	5,661	5,661	5,661	4,896	4,896	4,896
			Fixed Effect IV Estimates			
Current Income	.0497	.0519*	.0641*	.0432	.0448	.0458
	(.0305)	(.0310)	(.0383)	(.0272)	(.0275)	(.0338)
Control Variables						
Age & Age^2 of Child	X	X	X	X	X	X
Mother & Child Controls		X	X		X	X
Spouse & Additional Controls			X			X
t-statistic from 1st Stage	9.27	9.21	7.83	7.82	7.75	6.63
Observations	13,047	13,047	13,047	10,509	10,509	10,509
Number of Children	5,244	5,244	5,244	4,487	4,487	4,487

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. The sample and included covariates are the same as in Table 3, with the exception that the sample covers the shorter period 1988 to 1994 (since income is only measured biannually after 1994). The PIAT math and reading tests along with current income come from the 1988, 1990, 1992, and 1994 NLSY surveys, while lagged income is taken from the 1987, 1989, 1991, and 1993 NLSY surveys.

**Significant at the 5% level, *significant at the 10% level

Table 5. Baseline Fixed Effects Instrumental Variables Estimates.

	Math (1)	**Reading** (2)
Current Income	.2050**	.3606**
	(.0596)	(.0766)
Mother Variables		
Age	-.0990*	-.1824**
	(.0546)	(.0679)
Age^2	.0012*	.0023**
	(.0007)	(.0009)
Current Education (dropout omitted)		
High School	.0619	.0927
	(.0678)	(.0913)
Some College	.2301**	.2048*
	(.0900)	(.1192)
College Graduate	.1180	.1361
	(.1411)	(.1808)
Married	.1014*	.0387
	(.0552)	(.0626)
Number of Children (one omitted)		
Two	.0134	.0290
	(.0481)	(.0825)
Three	.0131	.0348
	(.0582)	(.0614)
Four or more	-.0316	.0625
	(.0737)	(.0508)
Child Variables		
Age	.1489**	-.1395**
	(.0329)	(.0423)
Age^2	-.0073**	.0039**
	(.0009)	(.0012)
Spouse Variables		
Age	-.0154**	-.0249**
	(.0049)	(.0062)
Age^2	.0002	.0002**
	(.0001)	(.0001)
Education (dropout omitted)		
High School	-.0150	.1389**
	(.0596)	(.0704)
Some College	-.0608	.0437
	(.0727)	(.0881)
College Graduate	-.0472	.0625
	(.1053)	(.1236)
Additional Variables		
Mother's Mother Present in HH	.0090	-.0190
	(.0528)	(.0641)
Mother's Father Present in HH	-.1070	-.0963
	(.0712)	(.0889)
Number of Adult Family Members in HH	-.0397*	-.0850**
	(.0216)	(.0268)
t-statistic from 1^{st} Stage	9.31	7.81
Observations	21,407	17,391
Number of Children	6,302	5,580

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions also include year dummies and three separate dummies indicating whether each of the "Additional Variables" is missing.
**Significant at the 5% level, *significant at the 10% level

Table 6. Fixed Effects Instrumental Variable Estimates for Various Subgroups.

	Math		**Reading**	
	(1)	(2)	(3)	(4)
	Black or Hispanic	White (not Hisp.)	Black or Hispanic	White (not Hisp.)
Current Income	.2996**	.1395*	.4772**	.1324*
	(.1152)	(.0742)	(.1564)	(.0725)
t-statistic from 1st Stage	5.45	6.89	4.74	5.56
Observations	11,512	9,895	9,420	7,971
Number of Children	3,317	2,985	2,992	2,588
	High School or Less	Some College +	High School or Less	Some College +
Current Income	.2548**	.0479	.3839**	.3148**
	(.0782)	(.1017)	(.0940)	(.1522)
t-statistic from 1st Stage	7.69	4.47	6.49	3.67
Observations	15,538	5,869	12,752	4,639
Number of Children	4,617	1,685	4,139	1,441
	Not Married	Married	Not Married	Married
Current Income	.2730	.1725**	.6077**	.2385**
	(.1666)	(.0737)	(.2270)	(.0888)
t-statistic from 1st Stage	5.00	7.16	4.42	5.55
Observations	8,101	13,306	6,679	10,712
Number of Children	2,998	4,451	2,637	3,832
	2+ Children	One Child	2+ Children	One Child
Current Income	.1998**	.0297	.3401**	..2384
	(.0594)	(.1947)	(.0725)	(.4533)
t-statistic from 1st Stage	9.36	1.61	7.99	1.27
Observations	19,047	2,360	15,539	1,852
Number of Children	5,762	985	5,111	782

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5.
**Significant at the 5% level, *significant at the 10% level

Table 7. Fixed Effects Instrumental Variable Estimates Before, Straddling, and After the Large Increase in the EITC.

	Math			Reading		
	(1)	(2)	(3)	(4)	(5)	(6)
	Pre EITC Increase (88, 90, 92)	Straddle Increase (92, 94, 96)	Post EITC Increase (96, 98, 00)	Pre EITC Increase (88, 90, 92)	Straddle Increase (92, 94, 96)	Post EITC Increase (96, 98, 00)
Current Income	-.1416	.4150**	.0015	.1109	.5774**	.1019
	(.2947)	(.1632)	(.0827)	(.2650)	(.2165)	(.0736)
t-statistic from 1st Stage	2.69	5.15	7.19	2.54	4.36	6.33
Observations	9,407	10,752	8,360	7,448	8,871	6,882
Number of Children	4,424	5,272	4,024	3,660	4,527	3,472

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5.
**Significant at the 5% level, *significant at the 10% level

Table 8. Specification Checks for the Fixed Effects Instrumental Variables Estimates.

	Math (1)	**Reading** (2)
	Constant Coefficients across Years to Predict Family Income	
Current Income	.3426*	.9383*
	(.1704)	(.5423)
t-statistic from 1st Stage	3.23	2.18
Observations	21,407	17,391
Number of Children	6,302	5,580
	Natural Logarithm of Income	
Log of Income	.5872**	.6744**
	(.2762)	(.2573)
t-statistic from 1st Stage	7.04	6.60
Observations	21,407	17,391
Number of Children	6,302	5,580

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5.
**Significant at the 5% level, *significant at the 10% level

Table 9. Fixed Effects Instrumental Variable Estimates Accounting for Labor Force Participation.

	Math		Reading	
	Participation Treated as Exogenous	Instrumenting for Working Mother	Participation Treated as Exogenous	Instrumenting for Working Mother
	(1)	(2)	(3)	(4)
Current Income	.2037**	.2169**	.3553**	.3198**
	(.0589)	(.0618)	(.0750)	(.0730)
Working Mother	-.0342	.2998	-.1069**	-.6819
	(.0277)	(.6663)	(.0324)	(.6250)
F-statistic from 1st Stage				
Current Income Equation	66.54	23.11	46.91	16.86
[p-value]	[.0000]	[.0000]	[.0000]	[.0000]
Working Mother Equation		5.97		5.87
[p-value]		[.0001]		[.0001]
Observations	21,407	20,996	17,391	17,074
Number of Children	6,302	5,891	5,580	5,273

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5. Working Mother is a dummy variable equal to one if the mother works for pay. All specifications instrument for current income as in the baseline model in Table 5. In columns (2) and (4), the phase-in rate, the maximum credit, and the point at which the credit phase out begins are included as additional instruments.
**Significant at the 5% level, *significant at the 10% level

Table 10. Fixed Effects Instrumental Variable Estimates Allowing the Effect of Income to Vary by Age.

	Math (1)	**Reading** (2)
Current Income when Young (5 ≤ Age ≤ 9)	.1768**	.3078**
	(.0609)	(.0762)
Current Income when Old (Age > 9)	.1865**	.3261**
	(.0595)	(.0743)
Observations	21,407	17,391
Number of Children	6,302	5,580

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5.

**Significant at the 5% level, *significant at the 10% level

Table 11. Fixed Effects Instrumental Variable Estimates with Past Income and Linear Depreciation.

	Math (1)	Reading (2)
Current Income	.0721	.0851
	(.0726)	(.0600)
Past Income (α_0)	.0183**	.0103
	(.0092)	(.0074)
Linear Depreciation Term (α_1)	-.0011	.0011
	(.0010)	(.0007)
Joint F-test for Three Income Terms	9.95	59.50
[p-value]	[.0000]	[.0000]
Observations	21,405	17,391
Number of Children	6,301	5,580

Notes: Cluster robust standard errors in parentheses, adjusted for arbitrary within-child correlation and heteroskedasticity. Income is measured in 10,000 of year 2000 dollars. Regressions include the same covariates as in Table 5.

**Significant at the 5% level, *significant at the 10% level

Figure 1a: Federal EITC Schedules in Selected Years for Families with One Child (2000 Dollars)

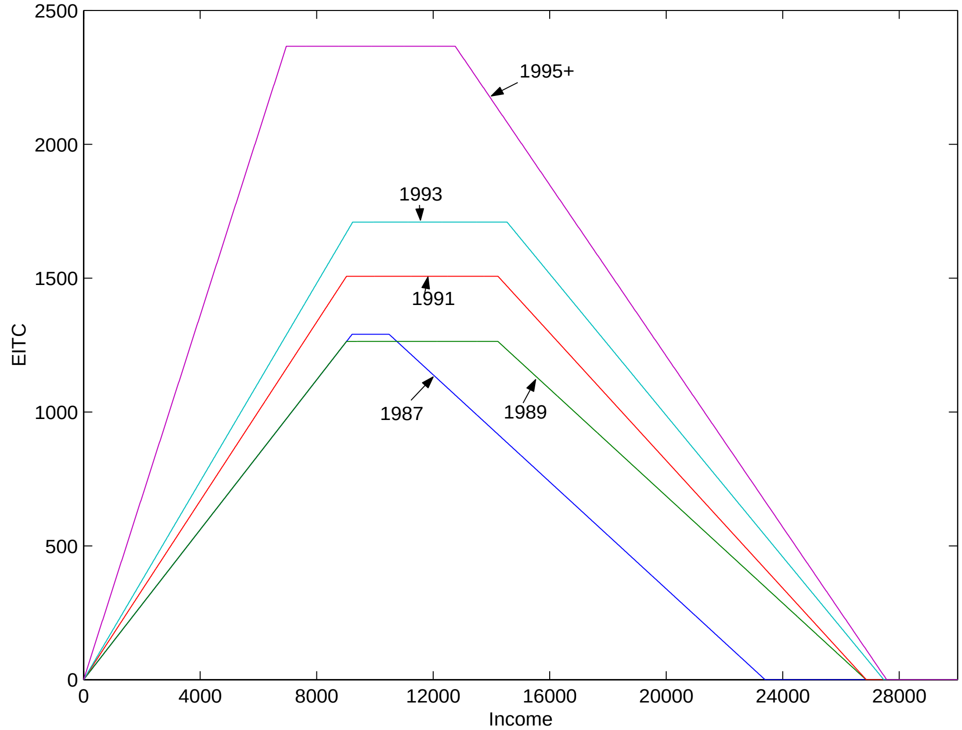


Figure 1b: Changes in Federal EITC Relative to 1987 for Families with One Child (2000 Dollars)

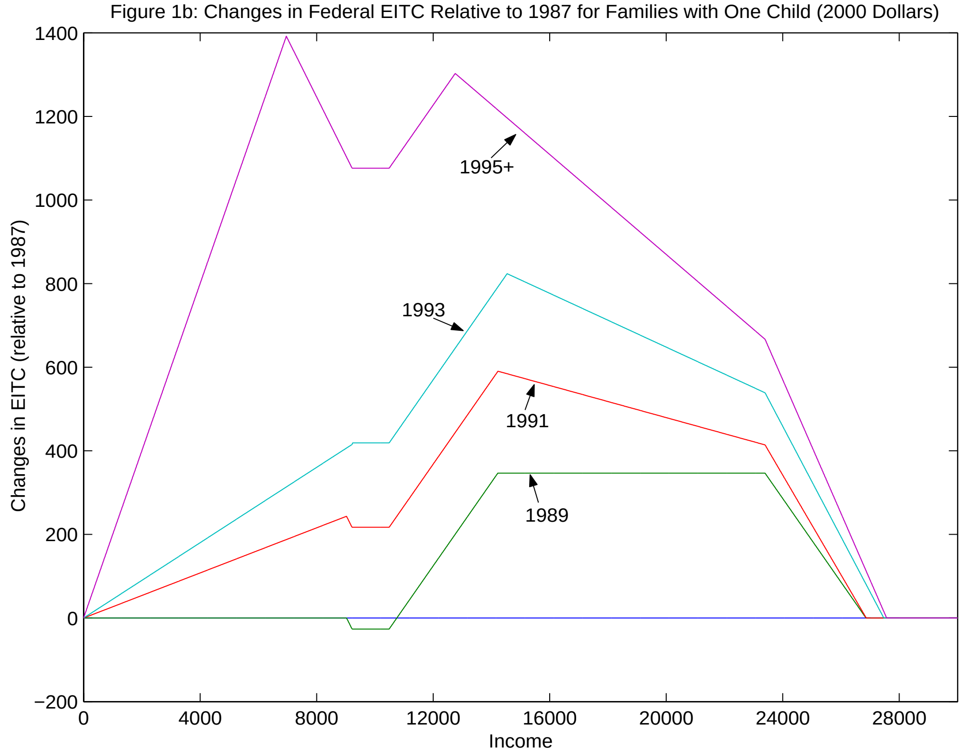


Figure 2a: Federal EITC Schedules in Selected Years for Families with Two or More Children (2000 Dollars)

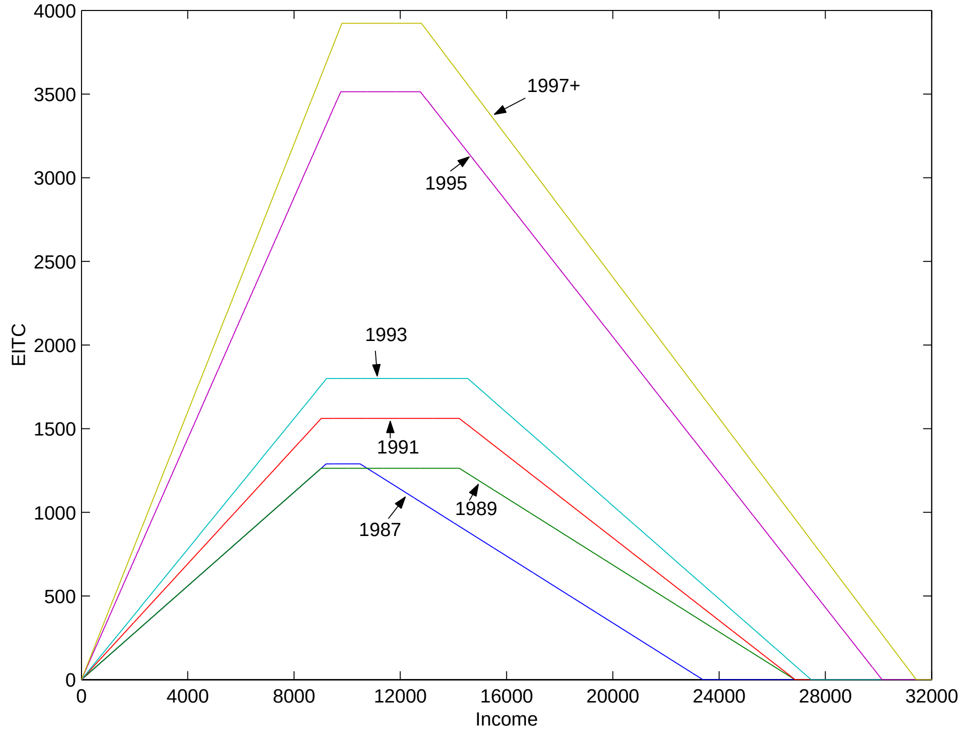


Figure 2b: Changes in Federal EITC Relative to 1987 for Families with Two or More Children (2000 Dollars)

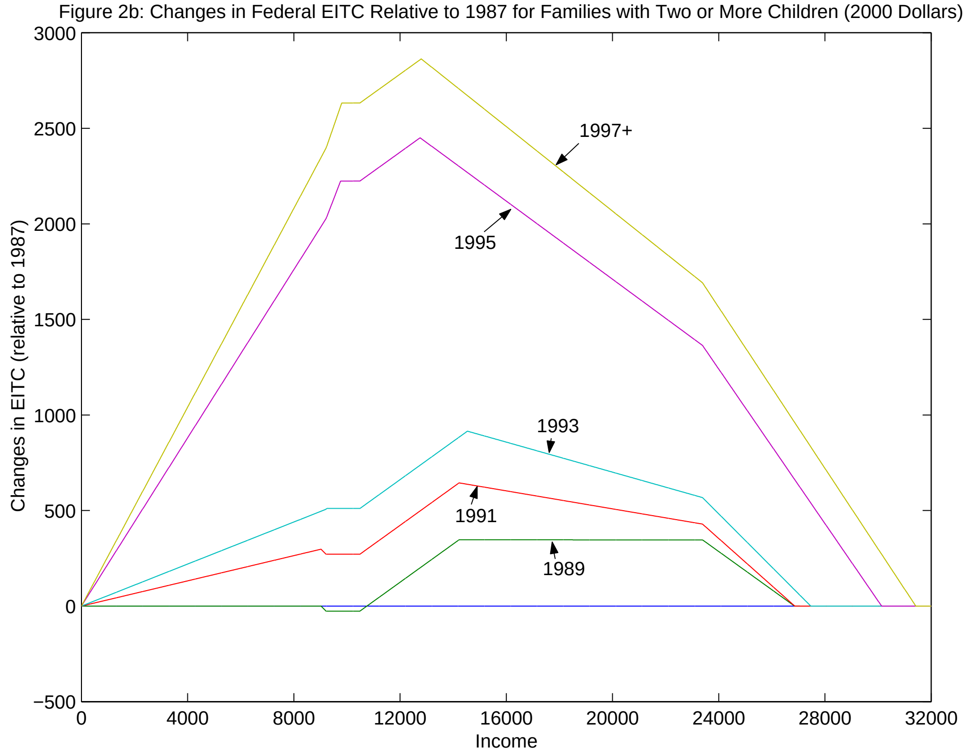
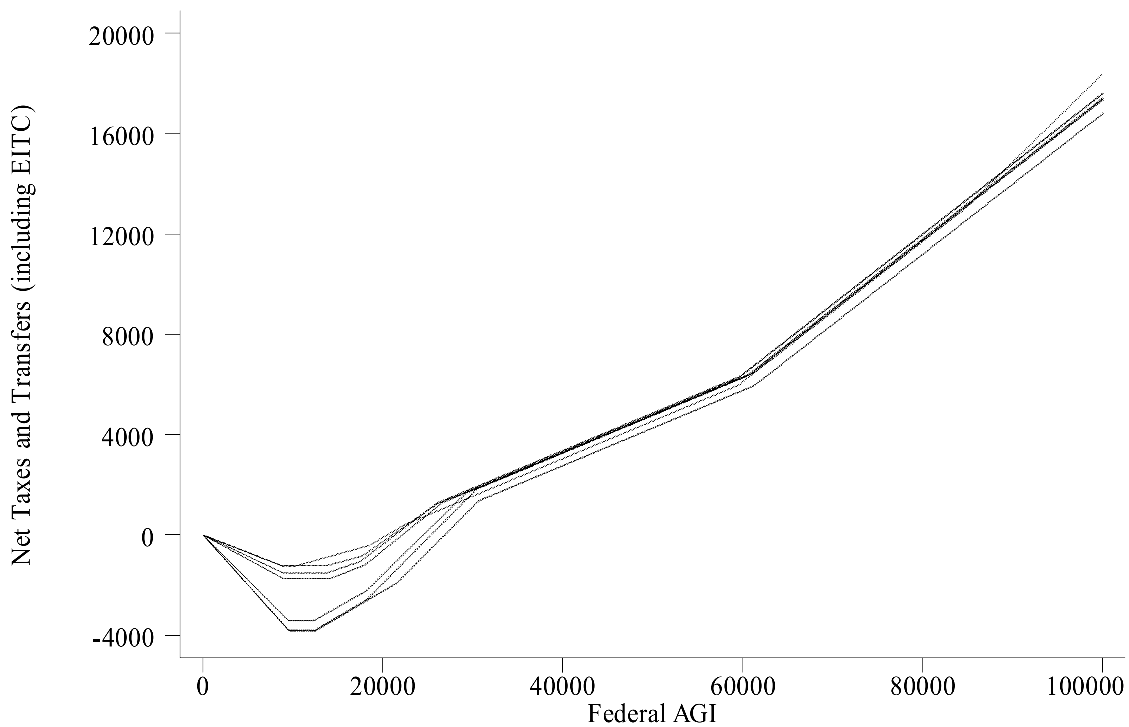


Figure 3. Changes in the Federal Tax Code over Time (for Married Families with 2 Children).



Notes: The lines plot the tax schedules as a function of Federal Adjusted Gross Income for the tax years used in our analysis, namely, every two years from 1987 to 1999. The bottom line is the year 1999, which is somewhat lower due to the $200 tax credit per child which began that year.

Figure 4. Average EITC Payment over Time by Maternal Characteristics, Based on Lagged Income.