Does the Earned Income Tax Credit Increase Children's Weight? The Impact of Policy-Driven Income on Childhood Obesity*

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Abstract

I exploit substantial increases in the Earned Income Tax Credit (EITC) to study how a policy-driven change in family income affects childhood obesity. In contrast to a widely accepted belief, I find that income has an adverse effect on children's weight. My difference-in-difference estimates indicate that the probability of being obese increased more among children whose families experienced a greater income shock. Moreover, instrumental variable estimates demonstrate that a $1,000 increase in policy-driven income leads to a 0.5 percentage point increase in obesity rates. The finding suggests that a non-health social safety net program such as the EITC could have unintended negative side effects.

Keywords: Obesity, Child Development.

JEL classification: I18, I14

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1 Introduction

The gap in obesity rates between low- and high-income children is well documented in the literature (Anderson and Butcher, 2006; Kumanyika and Grier, 2006; Strauss and Pollack, 2001). A simple mean comparison indicates that the obesity rate among low-income children tends to be higher than among high-income children. Meanwhile, a social safety net program such as the Earned Income Tax Credit (EITC) program transfers income in the hopes of improving low-income families' overall well-being. Given such high obesity rates among low-income children and the range of illnesses obesity is linked to, does such transfer income improve child health outcomes?

This paper investigates what effect a policy-driven change in income has on childhood obesity. I exploit large income shocks generated by changes to the EITC program in the 1980s and 1990s. Since the income changes driven by the EITC expansion were exogenous to children's weight, they provide an opportunity to study the causal effect of income. Using the National Longitudinal Survey of Youth 1979 (NLSY79), I find that a policy-driven increase in income had an adverse impact on the weight of children among the EITC-eligible families. The finding suggests that a non-health program such as the EITC could have an unintended negative consequence.

My empirical analysis proceeds in three steps. First, I use ordinary least squares (OLS) to understand the general relationship between income and childhood obesity. Next, to account for the omitted variable bias in OLS estimates, I employ a difference-in-difference (DD) strategy that exploits a larger income shock to families with two or more children compared with families with one child after the largest EITC expansion (Omnibus Budget Reconciliation Act of 1993 or OBRA 93). The DD estimates likely contain the effect of maternal employment in addition to the effect of policy-driven income, because the expansions also generated a maternal labor supply response. Therefore, I employ the first-differenced instrumental variable (IV) strategy using the change in the EITC amount as an instrument for the change in family income to isolate only the policy-driven income effect. Since the EITC income is determined by family income itself, the instrument using current income violates the exogeneity condition of the instrument. Therefore, I calculate the EITC income using current income predicted by lagged income, thereby removing any income changes originating from a maternal employment change.

Because of four critical features, I use the NLSY79 and its Child/Young Adult supplement. First, the panel nature of the data set enables removal of any individual differences among children

that are fixed over time through the first differencing strategy. Second, I can match children to their mothers. Extensive information on a child's development can be linked to the mother's current and historical characteristics. Third, the data set contains a detailed measure of family income and demographic characteristics in order to simulate the EITC amount for which each family is eligible. The restricted access information about a respondent's resident state enables even more precise calculations of the eligible EITC amount.[1] Finally, children's height and weight are directly measured by interviewers; hence, they are more accurate than self-reported measures commonly available in other health data sets.

Both OLS and DD estimates imply an adverse effect of income on childhood obesity. The OLS estimates show a positive relationship between the EITC income and childhood obesity. However, the relationship is likely to be spurious. The DD estimates suggest that children from families who experienced a larger income shock gained more weight after the OBRA 93. Children from families with two or more children (the treatment group) experienced a 3 percentage point increase in the probability of obesity compared with children from families with one child (the control group).

The IV estimates reaffirm the finding from the OLS and DD estimates. The estimates show that a particular type of income shock – such as the one induced by an expansion of the EITC program – leads to children's weight gain. In particular, a $1,000 increase in policy-driven income leads to an increase in body mass index (BMI) by 0.02% of a standard deviation or a 0.5 percentage point increase in obesity rates. The finding is robust across specifications that account for possible confounding factors and use various functional forms of an instrument.

I further explore potential explanations for why a policy-driven increase in income leads to children's weight gain. Since only low-income families are affected by the EITC expansions, the mechanism behind the children's weight gain is related to unique characteristics of the EITC income and low-income families. The lump sum and temporary nature of the EITC income likely increases a family's food expenditure and encourages a volatile food consumption pattern. Moreover, the finding demonstrates that children's net caloric intake increases when families receive additional income. Such an increase in caloric intake may be driven by low-income parents' lack of nutrition knowledge, limited access to quality child care, a budget constraint, or preference for high-caloric

[1] Many states provide additional credits to families eligible for the federal EITC. The payout amount varies by state but is generally a specific percentage of the federal EITC amount. For more information on the state EITC rates, see NBER webpage (http://users.nber.org/∼taxsim/state-eitc.html). See Hotz and Scholz (2003) for additional information on the EITC program in general.

food.

The main finding offers an important lesson that a non-health program could unintentionally affect health outcomes of the recipients. Although a large body of literature demonstrates that an expansion of the EITC program succeeded in incentivizing unemployed low-income mothers to enter the workforce, the finding of my paper suggests that it had an adverse impact on at least some group of children.[2] Therefore, it is critical to consider all possible avenues of the program's effect to understand the full consequences of a public policy.

The paper contributes to the literature in three ways. First, it adds to the current body of knowledge on the role of family income in child development by examining the income effect on children's body weight, one specific, yet crucial aspect of child development (Case et al., 2002; Dahl and Lochner, 2012). Given that a number of social safety net programs transfer income in the hopes of improving family's well-being, it is important to understand the impact income has on children's weight. Second, although studies investigating the causal effect of income on adolescent weight (Akee et al., 2013) or adult weight (Schmeiser, 2009) exist, no study has yet examined the causal effect on children's weight. Because obese children tend to become obese adults (Krebs and Jacobson, 2003), in order to prevent obesity and other related illnesses among adults, it is important to understand what makes children obese. Furthermore, this is the first paper to study the effect of a non-health program on childhood obesity to the best of my knowledge. A growing number of studies examine how a non-health program – such as the EITC or Aid to Families with Dependent Children (AFDC) – affects health outcomes (Almond et al., 2011; Averett and Wang, 2013; Currie and Cole, 1993; Hoynes et al., 2012; Kehrer and Wolin, 1979). The paper adds to the literature by providing new evidence on the effect of the EITC expansion on childhood obesity.

This paper proceeds as follows. Section 2 describes the EITC expansions and discusses its effect on child obesity. Section 3 presents the empirical framework. The data set is described in Section 4. Section 5 reports the results, and Section 6 discusses implications of the main finding. Section 7 concludes.

[2]See Eissa and Hoynes (2004), Eissa and Liebman (1996), Ellwood (2000), Meyer (2002), Meyer and Rosenbaum (2001).

2 Expansion of the EITC Program

2.1 The EITC Expansion as an Exogenous Variation in Income

The EITC program experienced several significant expansions in the late 1980s and 1990s. The first increase in benefit amount occurred after the passage of the Tax Reform Act of 1986 (TRA 86). The Omnibus Budget Reconciliation Act of 1990 (OBRA 90) initiated a separation in credit amount for taxpayers with two or more children from taxpayers with one child although the difference was modest (see figure 1). The EITC benefit rose once again with the OBRA 93, which provided an especially large increase in benefits for families with two or more children.

The expansion of the EITC program generated an exogenous variation in family income. Policymakers' desire to move people out of social safety net programs and into the workforce motivated legislative actions – such as the TRA 86, OBRA 90, OBRA 93 – that led to the EITC expansions. As a result, the amount of EITC benefits grew dramatically for all eligible families (Figure 2). For instance, the maximum benefit amount that families with two or more children received more than quadrupled to $3,771 in 2001 from $852 (in year 2000 dollars) in 1985. The EITC expansion provided an exogenous income variation since the underlying motivation of the expansion was irrelevant to factors affecting children's weight.

Among the three expansions, the OBRA 93 provides an identifying variation for the DD strategy. The OBRA 93 generated a large benefit increase for families with two or more children compared with families with one child. Figure 1 demonstrates that the maximum benefit amount for families with two or more children increased by $2,034 (2000 dollars) compared with only $632 for families with one child. Since the EITC-eligible families could not have predetermined the number of child births with an expectation of the legislative action, the differing size of an income shock the OBRA 93 generated serves well as a natural experiment. A number of studies take the same approach to examine the effect of the EITC expansion on various outcomes (Averett and Wang, 2013; Evans and Garthwaite, 2010; Hoynes et al., 2012).

The TRA 86 and the OBRA 90 provide additional exogenous variations in income for the IV strategy. Although the size of the increase in benefit amount was not as substantial as that generated by the OBRA 93, both legislative actions still provided exogenous income variations. I construct an instrument that consists of a change in the EITC amount that families experienced because of the expansions. I leave the detailed discussion of constructing a valid instrument for the

next section.

2.2 The Effect of the EITC Expansions on Children's Weight

The EITC expansions affect children's weight through three channels. The first channel is by directly increasing the amount of the EITC benefits. Even if a family's earned income does not change, the family's total after-tax income increases over time because of several program expansions. Second, some families may also experience additional income gain because of an increase in maternal employment. The empirical evidence suggests that the expansions prompt a maternal labor supply response, which leads to higher earned income among some families (Eissa and Hoynes, 2004; Eissa and Liebman, 1996; Ellwood, 2000; Meyer, 2002; Meyer and Rosenbaum, 2001).[3] Finally, an increase in maternal labor supply reduces the amount of time mothers spend with children. The reduction in maternal time with children likely has an adverse impact on children's weight. As mothers enter workforce, their time preparing home cooked meals and taking children out for physical activities decline, adversely affecting children's weight (Anderson et al, 2003; Liu et al, 2009; Ruhm, 2008; Ziol-Guest et al, 2012). Children from low-income families are especially vulnerable to such weight gain since alternative options to maternal care is worse than for children from higher income families.

The effect of income on children's weight, however, is ambiguous. For instance, additional financial resources can lead to a reduction in children's weight by allowing income constrained families to buy healthier food (Inglis et al., 2009) and by reducing children's stress levels (Gundersen et al., 2011). On the other hand, additional income can increase the frequency of restaurant meals consumed (Lee and Brown, 1986) and induce more sedentary behaviors leading to an opposite outcome.

In this paper, I attempt to isolate only the effect of the first channel. Since a number of social safety net programs transfer cash with the hopes of improving low-income families' overall well-being, it is important to ask what happens to a child health when such benefit amount increases. Furthermore, studying how policy-driven income affects childhood obesity can shed light on potential ways to curb obesity and reveal the full consequences of similar income transfer programs.

[3]The EITC expansion leads to an increase in labor supply by single mothers and a decrease in labor supply by married mothers.

3 Empirical Framework

The empirical strategies utilize policy induced variations in income to estimate the effect of income on children's weight. The OLS estimate contains the effect of all three channels and is likely to be biased; hence, I employ the DD strategy to alleviate the potential endogeneity issue. Since I cannot disentangle the three channels from DD estimates, I use the first-differenced IV strategy to isolate only the policy-driven effect of income.

3.1 Modeling Children's Weight Gain

The weight of a child is determined by a number of observable and unobservable factors. The basic model for estimating the impact of family income on children's weight is,

$$y_{it} = I_{it}\alpha + X'_{it}\beta + w'_i\gamma + \mu_i + \epsilon_{it} \tag{1}$$

where y_{it} is a measure of child i's body weight at time t, I_{it} is a total family income net of taxes and including the EITC amount for the previous calendar year, and X_{it} denotes a vector of time-variant characteristics, such as number of siblings, a parent's age, and a mother's marital status (see Table 1).[4] w_i is a vector of observable time-invariant characteristics, such as a child's race and gender, and μ_i is unobservable time-invariant characteristics.

Implicit in equation (1) is that the income effect estimated here refers to changes in outcome brought on by a transitory income shock rather than a permanent income shock. Empirical evidence illustrates that a permanent income is more strongly correlated with child outcomes than a transitory income since parents make investment in children looking at a long-term horizon (Case et al., 2002). However, the causal effect of permanent income is challenging to estimate because of the difficulty of finding a valid exogenous variation in permanent income. Therefore, I only estimate the effect of current income in this paper.

The OLS model in equation (1) potentially suffers from an endogeneity issue. Since children from low-income families are exposed more to environments that adversely affect their weight, causality of income cannot be inferred from equation (1). For instance, if a school lunch causes weight gain, and a greater proportion of low-income children eat school lunch, then family income and children's weight would exhibit a negative relationship. However, family income does not

[4] To construct I_{it}, the federal and state tax liabilities are subtracted from, and the non-taxable income and the simulated EITC amount are added to the TAXSIM generated adjusted gross income (AGI).

directly increase children's weight in this case; thus, a change in family income without a change in school lunch participation will not generate any weight change.

In order to remove a potential source of endogeneity, I estimate the modified first-differenced model as follows:

$$\Delta y_{it} \quad = \quad \Delta I_{it}\alpha + \Delta X'_{it}\beta + w'_i\gamma + \Delta\epsilon_{it}, \tag{2}$$

where $\gamma = \gamma_t - \gamma_{t-1}$. The specification allows the changes in children's weight to vary by observable permanent characteristics. In the model (2), μ_i is eliminated, thereby removing the bias that could result from its correlation with I_{it}.

Although using the first-differenced model removes any time-invariant differences among children, it is not necessarily better than the OLS model. A family income measure contains measurement error because it is self-reported. The attenuation bias from measurement error is worse in the differenced estimates than in the OLS estimates. Moreover, even without an attenuation bias, the differenced estimates could still be biased because of a potential correlation between a change in income (ΔI_{it}) and a change in an unobservable shock affecting children's weight ($\Delta\epsilon_{it}$). For instance, a shock to parental health could affect both family income and children's weight.

3.2 Using the EITC Expansion to Estimate the Income Effect

3.2.1 Difference-in-Difference Model

In order to estimate a causal effect of income, I employ a DD strategy. Taking advantage of the large separation in benefit amount based on the number of dependent children, I estimate the following reduced-form model.

$$\begin{aligned} y_{it} = \theta * (Two\ plus\ children) + \lambda(Two\ plus\ children) * (after\ treatment) \\ +\delta_t + X'_{it}\rho + \mu_i + \xi_{it} \end{aligned} \tag{3}$$

where $Two\ plus\ children$ is a dummy variable for families with two or more children, δ_t reflects year dummies, and X_{it} is the same vector of controls as in equation (1).[5] Following Eissa and Hoyne's (2004) specification, I use 1985–1993 as the pre-treatment periods and 1994–2001 as the post-treatment periods. In some specifications, I also include child fixed effects, μ_i. The parameter of interest is λ.

[5] I use a linear probability model instead of a probit model since both models give similar results but estimates from the former are easier to interpret (Angrist and Pischke, 2008).

The preferred model controls for both time trend and group characteristics. The DD estimate relies on the assumption that, in the absence of a treatment, the trend for body weight is constant across children from families with two or more children (treatment group) and children from families with one child (control group). The preferred model includes time trend as well as group characteristics, thus, relaxing the assumption that a composition of treatment and control groups does not change over time.

3.2.2 Instrumental Variable Model

I use the IV strategy to identify only the policy-driven income effect, which is challenging in the DD or the first-differenced framework. The first-differenced estimate is likely to be biased because of endogeneity. The DD estimate likely contains effects besides the income effect. Therefore, I use the first-differenced IV strategy to resolve issues arising from DD and the first-differenced strategy.

Because the EITC amount is determined by the endogenous variable itself, constructing a valid instrument is not a trivial task. In order for an instrument to be valid for equation (2), it must be correlated with a change in family income (ΔI_{it}) but not correlated with a change in other unobservables affecting children's weight ($\Delta\epsilon_{it}$). Since family income contains the EITC amount, the first condition is easily satisfied by using a change in the EITC amount as an instrument. For instance, an instrument in the following form:

$$\begin{aligned} \Delta z_i^{IV}(P_{i,t}, P_{i,t-1}) &= z_{i,t}(P_{i,t}) - z_{i,t-1}(P_{i,t-1}) \\ &= (EITC_t \textit{ based on } inc_t) - (EITC_{t-1} \textit{ based on } inc_{t-1}) \end{aligned} \tag{4}$$

with $z_{i,t}$ as the EITC amount and $P_{i,t}$ as pretax income, satisfies the first condition. However, such an instrument violates the exogeneity condition. The EITC amount for those who qualify is determined by their earned income; thus, any endogenous income change leads to a change in the EITC amount that is correlated with unobservable factors affecting children's weight. For instance, if parents become ill and family income decreases as a result, this, in turn, will generate a change in the EITC amount (Δz_i^{IV}) that is correlated with a change in parental behaviors ($\Delta\epsilon_{it}$).

An ideal instrument, then, consists only of the EITC change generated by a structural change and other exogenous income shocks. An exogenous income shock originates from fluctuations in inflation rates or business cycles whereas an endogenous income shock occurs because of a factor affecting both family income and children's weight, such as a parental health shock. The amount

of the EITC a family receives could change over time because of (1) a structural change to the program, (2) an endogenous income shock, or (3) an exogenous income shock. Only (1) and (3) are part of a valid instrument.

One way to remove an endogenous income shock is to use predicted income instead of current income when calculating the current EITC amount. By assuming that income evolves in a predictable manner over two periods, only the EITC change generated by a structural program change is included as an instrument. In practice, a current income is predicted with an indicator for a positive lagged income and a fifth-order polynomial in lagged income.[6]

In what follows, I present three distinct forms of a valid instrument. If estimates are consistent across the models using different instruments the result is more strongly corroborated. The simplest way to construct a valid instrument is by assigning the maximum EITC benefit that each family is eligible for based only on the number of children and tax year. This is also the form of an instrument that Schmeiser (2009) uses. The first instrument is,

$$\begin{aligned} \Delta z_i^{IV1} &= Maximum\ \ z_{i,t}(\hat{E}[P_{i,t}|P_{i,t-1}]) - Maximum\ \ z_{i,t-1}(P_{i,t-1}) \qquad (5) \\ &= [Maximum\ EITC_t\ \textit{based on predicted inc}] - [Maximum\ EITC_{t-1}\ \textit{based on inc}_{t-1}] \end{aligned}$$

where $z_{i,t}(\hat{E}[P_{i,t}|P_{i,t-1}])$ is an EITC amount calculated using a predicted income instead of current income.

The Δz_i^{IV1} satisfies the exogeneity condition of a valid instrument but is weakly correlated with the endogenous variable. Since the instrument strictly consists of a change in the maximum benefit amount due to a structural program change, it is not correlated with an unobservable shock. On the other hand, the instrument is a crude measure. The method assigns the same treatment to all families regardless of their income levels, when in reality the change in EITC amount each family receives varies based on their income. For instance, only about 13% of eligible families receive the maximum benefit amount in the current sample (Appendix Table 1).

To improve upon the crudeness of a measure, an alternative form of an instrument can be constructed by first dividing families into multiple income groups and assigning a different level of treatment to each group afterwards. In practice, families are categorized into four income groups with equal ranges that do not vary over time. The benefit amount corresponding to the midpoint

[6] By imposing such a functional form, the instrument implicitly assumes that income evolves in an expected way, and, thus, lagged income gives a good prediction of the current income. An alternative functional form is to set $\hat{E}[P_{i,t}|P_{i,t-1}] = P_{i,t-1}$, thereby holding family income constant for two consecutive years.

income level of each group is calculated thereafter for different numbers of children. Depending on the group each family belongs to and the number of children, the benefit amount is assigned accordingly. This way, a family is assigned with one of the eight benefit amounts every year. The strategy yields the following form of an instrument:

$$\begin{aligned} \Delta z_i^{IV2} &= z_{i,t}(Group(\hat{E}[P_{i,t}|P_{i,t-1}])) - z_{i,t-1}(Group(P_{i,t-1})) \\ &= [EITC_t \textit{ based on middle inc of group}] - [EITC_{t-1} \textit{ based on middle inc of group}] \end{aligned} \tag{6}$$

Although Δz_i^{IV2} refines the treatment compared to the first instrument, it is still somewhat of a crude measure since families are assigned with only one of eight treatment amounts.

The third instrument follows the form proposed by Dahl and Lochner (2012).

$$\begin{aligned} \Delta z_i^{IV3} &= z_{i,t}(\hat{E}[P_{i,t}|P_{i,t-1}]) - z_{i,t-1}(P_{i,t-1}) \\ &= [EITC_t \textit{ based on predicted inc}] - [EITC_{t-1} \textit{ based on } inc_{t-1}] \end{aligned} \tag{7}$$

where $z_{i,t}(\hat{E}[P_{i,t}|P_{i,t-1}])$ is the EITC amount specific to each family based on their predicted income and family structure. The TAXSIM generated EITC amount is family specific: therefore, the treatment is more precise than the previous two instruments. Furthermore, the instrument also exploits variations in the state EITC amount by calculating a treatment amount specific to each family.

In summary, the IV model yields,

$$\Delta y_{it} = \Delta I_{it}\alpha + \Delta X'_{it}\beta + w'_i\gamma + \Delta\epsilon_{it} \tag{8}$$

where ΔI_{it} is instrumented by the three aforementioned instruments (Δz_i^{IV}).[7] If estimates are consistent across the models using different forms of an instrument, then it provides stronger support for the finding.

4 Data

The main source of data is the National Longitudinal Survey of Youth 1979 (NLSY79) and its Child/Young Adult supplement. The NLSY79 gathered information from people aged 14 to 21 as

[7] One of the potential issues with the proposed instruments is that all of them are still a function of a lagged pretax income ($P_{i,t-1}$) so the coefficient estimate could still be biased because of a serial correlation in income. Dahl and Lochner (2012) suggest including a polynomial function of lagged pre-tax income to mitigate the issue. The inclusion of such a polynomial function does not change the main finding.

of December 31, 1978, representing the civilian, non-institutionalized population residing in the U.S. The survey was conducted annually until 1994 and biennially afterwards. The original sample consists of 12,686 individuals, 6,283 of whom are women. The Child/Young Adult supplement consists of children born to the female respondents of the NLSY79. The supplement collected child development information – including children's height and weight – biennially since 1986. Children from the Child/Young Adult supplement are matched to their mothers in the NLSY79 to create the sample used in this paper.

One of the major advantages of the data set is its extensive information on various income components, which permits a calculation of the eligible EITC amount.[8] The NLSY79 began collecting information on whether or not a respondent filed for the EITC and the amount she received only beginning with the year 2000. Because the collected information is self-reported, it is missing in most cases. Therefore, the eligible EITC amount for each family is calculated using the National Bureau of Economic Research's TAXSIM program.

By simulating the eligible EITC amount, I assume that all eligible individuals claim the benefits and fully comply with the program rules. Studies support the assumption of full program compliance. For instance, Scholz (1994) asserts that 80 to 86 percent of the taxpayers eligible for the EITC appeared to have received the credit in 1990. A recent study on the EITC compliance by the Internal Revenue Service finds that most EITC recipients are compliant, and even those who claim benefits despite their ineligible status tend to have similar characteristics as the eligible population (IRS 2002a).

Another important advantage of the NLSY79 is its accurate measures of children's adiposity. The Child/Young Adult supplement contains information on children's height and weight, which are directly measured by an interviewer in most cases. I create a normalized BMI measure with a mean of zero and a standard deviation of one based on the age- and sex-specific standard (denoted as ZBMI). The ZBMI provides a better sense of children's weight status than BMI since the range for *normal* BMI varies by age and gender.[9] For instance, a 4-year old boy with a BMI of 18.5 is considered "obese" whereas a 10-year old boy with the same BMI is "normal." I also use an

[8] Both a respondent's and her spouse's wages and salaries, unemployment income, farm or business income, welfare (Aid to Families with Dependent Children (AFDC), food stamps, Supplemental Security Income), veteran benefits, worker compensation, disability benefits, and other income (child support, savings, rental, social security) are self-reported in all survey years and are used to construct the pre- and post-tax income levels.

[9] Children with BMI greater than the 95th percentile based on the 2000 CDC sex- and age-specific growth chart are categorized as "obese."

indicator variable for a child's BMI that surpasses the 85th percentile (overweight or obese) as a dependent variable in some specifications since obesity is a rather extreme health condition that is less likely to have been prevalent when the majority of samples were collected.

After imposing minor sample restrictions, the final sample consists of 24,414 child-year observations spanning from 1986 to 2002. The observations with a marital status change within a two-year period are excluded from the sample since a change in marital status could impact both family income and children's weight. Moreover, only children whose ages range from four to fourteen years old are included in the sample. Imposing additional sample restrictions to minimize measurement error in family income does not change the main results.[10]

The summary statistics are reported in Table 1. The dependent variables are presented in Panel A, and Panel B displays a list of regressors. The sample mainly consists of young children, the average age of whom is eight. Since the NLSY79 oversampled blacks and hispanics, the sample contains a greater than population average number of black and hispanic children. The average family income is about $34,000 in year 2000 dollars, which is slightly less than the 1999 national median income level of $42,000. Approximately one third of children in the sample are eligible for the EITC. These children are more likely to be black or hispanic, obese with a higher BMI, and from lower income families. Their mothers are also less likely to have been married in the previous year and to have lived with both biological parents at age 14.

As Panel B demonstrates, several control variables are missing observations in the sample. Hence, a dummy variable for a missing observation is constructed, and I fill in the missing values using values from the surrounding years when possible. Of particular importance is the imputation method for missing income. Using the panel nature of the data, I regress income on age and age squared only for individuals over the age of 22 to impute the missing income observations.[11] The imputation procedure has a negligible effect on the estimates.

To control for potential confounding factors, some specifications include yearly state unemployment rates, a welfare reform indicator, and food stamp program (also known as the Supplemental Nutrition Assistance Program or SNAP) rule change indicators. The yearly state unemployment rates are obtained from the Bureau of Labor Statistics' Local Area Unemployment Statistics

[10] Following Dahl and Lochner (2012), children from military or over-sampled poor white families, children whose family income exceeds $150,000, whose two-year change in family income is larger than $40,000, or whose two-year change in welfare amount is greater than $2,500 without a corresponding income change were all excluded in the more restrictive sample.

[11] More detailed information on the imputation procedure can be found in the appendix of Dahl and Lochner (2012).

whereas the dates for the AFDC waiver and Temporary Assistance to Needy Families (TANF) implementation are from Crouse (1999).[12] The food stamp program policy variables are from the U.S. Department of Agriculture's Economic Research Service SNAP Policy Database. I extract only the policy variables that researchers find to affect the program participation: the broad based categorical eligibility, finger printing, simplified reporting procedure, and short certification periods (Hanratty, 2006; Kabbani and Wilde, 2003; Klerman and Danielson, 2011).

One final note addresses a more technical issue. Since respondents are asked about their income in the previous year, a child's adiposity measure is regressed on one-year lagged family income although it is denoted as a contemporaneous income. Furthermore, the lagged family income for the first-differenced model is a two-year instead of a one-year lag since children's adiposity measures are surveyed biennially.

5 The Effect of the EITC Income on Childhood Obesity

This section begins by presenting the OLS estimates which illustrate a positive relationship between the EITC income and childhood obesity. I then report the DD and IV estimates. All of the estimates imply that a policy-driven increase in income leads to weight gain among children from the EITC-eligible families.

5.1 Ordinary Least Squares Estimates

The OLS model demonstrates that the relationship between income and children's weight differs depending on the type of income. The total family income is negatively correlated with children's body weight in column (1) of Table 2 whereas the EITC income is positively correlated in column (2). The results suggest that permanent income may affect children's weight differently from policy induced temporary income.

The observed relationship between income and children's weight is likely to suffer from omitted variable bias. The negative relationship between total family income and children's weight could result from the unobservable differences among children along family income level. The positive relationship between the EITC income and children's weight could be driven by high obesity rates

[12] The observations with missing geocode information were filled in using the surrounding years. If the observations were still missing geographic information after the imputation procedure, they were assigned with average yearly national unemployment rates or the last year welfare reform occurred. The welfare reform indicator is constructed to equal one for all of the years that the AFDC waiver or TANF was in place.

of children from the EITC-eligible families compared with children from higher income families receiving no EITC benefits.

In order to account for the potential source of bias in the relationship between EITC income and children's weight, I consider two approaches. First, I restrict the sample to only children whose families are most likely to be affected by the EITC changes. Following Eissa and Hoyne (2004), I limit the sample to children whose mothers have a high school education or less in columns (3) and (4). Second, I employ the first-differenced model in column (4). The first-differenced model removes any time-invariant heterogeneities among children and thus, alleviates the potential endogeneity issue.

The positive relationship between the EITC income and children's weight is fairly consistent across the two approaches. Although the estimates become less precise because of a smaller sample size, the overall estimates from the restricted sample do not change much in column (3). The first-differenced estimates in column (4) also demonstrate a positive relationship between the EITC income and children's weight.[13] However, since the potential bias is somewhat exacerbated in the first-differenced model, I present results from the DD model in the following section.

5.2 Difference-in-Difference Estimates

The summary statistics of the restricted sample (children whose mothers have a high school education or less) by family size highlight the importance of controlling for group characteristics in the DD model. Table 3 indicates that mothers with one child are slightly younger, less likely to be married, and more likely to be from a better socioeconomic background. Children from one-child families are also slightly younger, less likely to be a racial minority, but more likely to be overweight or obese. Because of the observable differences between two groups, the preferred DD model controls for group characteristics as well as the time trend.

The DD estimates in Table 4 indicate that children from families with two or more children experienced a larger weight gain compared with children from one-child families after the OBRA 93. Specifications A through C of column (1) indicate that the proportion of children categorized as overweight or obese (BMI $\geq$ 85th percentile) increased by four to five percentage points more in the treatment group (families with two or more children) than in the control group. Considering that 25% of children in the pre-OBRA 93 sample were overweight or obese, the four percentage

[13] Although it is not shown here, the fixed effect model also gives similar estimates as the first-differenced model.

point increase is fairly sizable. Moreover, the three percentage point increase in obesity rates in column (2) is also fairly large given that the pre-treatment average obesity rate was 13%. Figure 3 further demonstrates how the BMI distribution of a treatment group developed a thicker right tail and, hence, a greater proportion of overweight or obese children following the 1993 expansion.

The weight gain resulting from the expansion had a positive impact on some children but a negative impact on others. Column (3) of Table 4 shows that the expansion reduced the proportion of underweight children by 2 to 3 percentage points. Therefore, some children became healthier as they gained more weight after the OBRA 93. On the other hand, the expansion potentially had an adverse impact on other children's overall health.[14] Since obesity is linked to a range of illnesses, an increased proportion of overweight or obese children suggests an adverse health effect of the expansion.[15] An increased proportion of mothers reporting that their children suffered from an illness requiring medical attention (column (4)) also indicates that the overall health of children likely deteriorated after the expansion.

Of particular concern during the sample time period is the policy changes that could have separately affected children's weight. If potential confounders are not accounted for in the model, the effect of other policy changes could be mistakenly attributed to the effect of the EITC income. One of the potential confounders is welfare reform. Several states began modifying provisions of the existing welfare program in an effort to encourage employment, which eventually culminated in the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA 96). Studies show that a large number of previously unemployed mothers joined the workforce because of the reform, and this could have had a separate effect on children's weight.[16] Another potential confounder is a change in the food stamp program. In the late 1990s, access to the food stamp program became more restrictive. Although the eligibility restrictions were targeted exclusively toward recent immigrants and able-bodied adults without dependents and, thus, did not directly affect the sample at hand, the prevailing political sentiment at the time could have deterred some eligible families from participating (Figlio et al., 2000; Zedlewski and Brauner, 1999).

[14] Since Hoynes et al. (2012) state that the EITC had a positive impact on infant health, their study initially seems contradictory to this paper's finding. However, a closer look at the study reveals that the authors used the incidence of low birthweight as a measure for the infant health. Therefore, both studies concur that the EITC income increased children's body weight.

[15] Medical research links obesity to type 2 diabetes, heart diseases, and osteoarthritis (Haslam and James, 2005; Mokdad et al., 2003; Sharma, 2003).

[16] See Blank (2002), Meyer and Rosenbaum (2000), Meyer and Rosenbaum (2001), and Schoeni and Blank (2000) for detailed information on policy changes during the 1990s.

Although the effect of food stamp program participation on childhood obesity is ambiguous (Gibson, 2004; Gibson, 2006; Hofferth and Curtin, 2005; Kreider et al., 2012; Schmeiser, 2012), the changing food stamp program rules should be addressed in the empirical model to mitigate the potential source of bias. For instance, if families who were previously enrolled in the food stamp program stopped participating as the EITC program expanded, a change in children's weight because of a change in food stamp program participation might be wrongly attributed to the effect of the EITC expansion.

Controlling for potential confounders has a modest effect on the estimates. Specification D accounts for potential confounders by including additional variables, such as yearly state unemployment rates, welfare reform indicators, and indicators for food stamp program rules. The estimates are robust to the specification controlling for potential confounders. While the DD estimates provide a useful insight into the impact of the EITC expansion, part of the estimated effect is driven by low-income mothers' labor supply response. To examine only the policy-driven income effect, I present the IV results in the following section.

5.3 Instrumental Variable Estimates

The IV estimates indicate that a policy-driven increase in income leads to weight gain by children from the EITC-eligible families. Using a simulated change in the EITC amount as an instrument for the change in after-tax family income, Table 5 shows separate IV estimates for each of the three instruments. The first two columns present estimates from using the first instrument in equation (5), whereas the next four columns correspond to estimates using equation (6) and (7) respectively. The estimates vary little across the models using different instruments. Table 5 shows that a $1,000 increase in policy-driven income generates an increase in BMI by 0.02% to 0.03% of a standard deviation or an increase in the probability of obesity by 0.2 to 0.5 percentage points. Considering that the average obesity rate of the sample is 16%, the effect is sizable. The probability of being overweight or obese increases by 0.1 to 0.4 percentage points although most of the estimates are not precise. Restricting the sample to only those children whose mothers have a high school education or less barely changes the estimates in columns (2), (4), and (6). Since the IV results are similar across all models, I treat the regression model (5) as the baseline model for all subsequent analysis.

Although the instruments in Table 5 remove any income effect emerging from a maternal employment response, they do not account for a change in maternal time spent with children. Table 6

addresses the issue by including various measures of parental employment. The second specification includes total number of hours mothers worked during a current year while the third specification controls for total number of hours that both parents worked during a current year. Neither measure of labor supply has a significant impact on the estimates. To address the concern that it is not the contemporaneous labor supply but labor supply cumulated over time that affects children's weight, specification D includes an average number of hours per week mothers worked since a child's birth. Resulting estimates are slightly smaller than the baseline estimates but not significantly different. On the other hand, the general consensus in the literature is that the female labor supply response to the EITC expansions was only at the extensive margin (employment) rather than the intensive margin (number of hours). Therefore, I include mother's labor force participation status in specification E. Specification F contains total number of hours mothers worked in addition to the labor force participation status. The estimates from both specifications are similar to the baseline estimates.

The adverse effect of policy-driven income is robust across different specifications controlling for potential confounding factors. Table 7 assesses the importance of confounding factors in the estimated income effect. An inclusion of year dummies to account for the secular time trend in obesity rates results in slightly smaller coefficients (specification B). Specification C restricts samples to only years prior to the PRWORA 96 since it was one of the most significant legislative acts during the time period. The resulting estimates are larger than the baseline estimates but not by a statistically significant amount. Last, I include yearly state unemployment rates, welfare reform indicators, and food stamp policy indicators in specification D. Specification E includes year dummies in addition to the control variables in specification D. Both of the specifications yield consistent estimates, strongly corroborating the finding from DD estimates.

I further explore the potentially heterogenous impact of family income on children's weight in Table 8. Previous studies suggest that income has a larger effect on the development of low-socioeconomic children and younger children (Akee et al., 2013; Dahl and Lochner, 2012). Schmeiser (2009) also finds that income affects women's weight but not men's. To demonstrate the effect income has on children's weight by their socioeconomic status, age, race, or gender, Table 8 reports IV estimates by the subgroups. Only the coefficients from using an obesity indicator as a dependent variable are presented. As expected, a greater proportion of children from low-socioeconomic backgrounds are obese and are eligible for the benefits. Unlike the income effect on adult weight,

the effect on children's weight is similar across gender (see column (1) of Table 8). The weight of female children is slightly more responsive to income than that of male children, which may be a precursor to the heterogenous income effect among adults. The difference between female and male children, however, is not statistically significant. On the other hand, family income does have a larger effect on younger children and children from more disadvantaged backgrounds. For instance, income has a greater effect on children who are younger than ten years old than on those who are at least ten years old (column (3)). This is consistent with the studies highlighting the importance of parental resources, especially during the early years of a child's life (Duncan et al., 1998; Levy and Duncan, 1999). Racial minority children (column (2)) and children from a single parent household (column (4)) are not only more likely to be obese than their counterparts, but their body weight is also more responsive to income changes.

6 Discussion of the Main Findings

The IV estimates indicate that an increase in policy-driven income leads to weight gain by children from the EITC-eligible families. Although the finding is consistent with other studies investigating the causal effect of income on body weight (Akee et al., 2013; Schmeiser, 2009), it seems rather counterintuitive since more income is generally considered beneficial. In order to understand why policy-driven income has an adverse impact on children's weight, this section begins by considering the unique features of the EITC income. Then, I propose several hypotheses related to the features. Last, I discuss policy implications of my finding.

Compared to income earned through salaries or wages, the EITC income has three unique features. First, the EITC benefit is disbursed annually in a lump sum after the tax return is filed. While recipients can collect their benefits at a monthly frequency using the EITC Advance program, only a small percentage of people takes advantage of the option.[17] Second, the EITC income is temporary income. In order to be eligible for the benefit, one must have both earned and AGI below a threshold that varies by year and by number of qualifying children.[18] Once children become non-dependents, or if earned income exceeds the threshold, families are no longer eligible for the benefit. Finally, only low-income working families receive the EITC benefit.

The lump sum and temporary nature of the EITC income reveals how beneficiaries of the

[17] Only 1.1 percent of the EITC recipients took advantage of the advance payment option in 1998.

[18] Earned income includes wages and salary, business self-employment income, and farm self-employment income. The AGI is calculated by subtracting a tax payer's eligible deduction amount from earned income.

expansions spend their additional income. People tend to spend their tax refunds on nondurables, such as food away from home, apparel, vacation, and paying down debts (Agarwal et al., 2007; Johnson et al., 2006; Souleles, 2002). Romich and Weisner (2000) also point out that families often prioritize expenses related to children when budgeting the EITC income. Furthermore, families receiving additional food stamp or cash benefits tend to increase their overall food expenditure (Hoynes and Schanzenbach, 2009). Therefore, beneficiaries of the EITC expansions likely increase their overall food expenditure by taking children out to restaurants more frequently or buying them a larger quantity of high-caloric snacks. On the other hand, the lump sum nature of the EITC income can generate an irregular food consumption pattern. Studies on food stamp consumption demonstrate that people spend most of their benefits immediately upon receipt, leading to a volatile food security status and inducing unhealthy binge eating behaviors (Townsend et al, 2001). A similar mechanism may be at work with the EITC income.

Regardless of how recipients spend their additional income, the main finding clearly demonstrates that children from the EITC-eligible families consume more calories when their families receive extra money. Why do families consume more calories instead of spending extra money on higher quality low-caloric food? The first explanation involves low-income families' lack of nutrition knowledge. Research shows that a person's diet and nutrition knowledge is closely related to the quality of his diet (Axelson et al., 1985; Blaylock et al., 1999; Wardle et al., 2000). Low-income parents may have limited information on what types of food are healthy or how to cook healthily. As a result, with additional income, low-income families simply eat more of what they have been eating, which is high-caloric unhealthy food (Beydoun and Wang, 2008; Gibson et al., 1998; Wolfe and Campbell, 1993;). An alternative argument is that parents are knowledgable about nutrition but cannot provide healthy food to children because of lack of resources. Low-income working mothers face more barriers in caring for children than higher income mothers. They are more likely to rely on an informal child care arrangement because of limited access to quality center-based care. Children who are either at home alone or at a substandard child care facility consume more high-caloric food without proper supervision. The third explanation is a family's budget constraint. Even with additional income, families still cannot afford to buy more expensive healthier alternatives. Finally, the last explanation lies with taste or preference. The increase in caloric intake can simply be driven by a family's taste for high-caloric food (Richards et al., 2007).

The policy implication of the finding varies depending on the underlying mechanism behind

children's high caloric intake. If a lack of information is the main mechanism, public health efforts can be directed at educating low-income families on diet and nutrition. Community outreach programs that teach families nutritional contents of food or quick ways to prepare healthy meals could be an effective strategy. On the other hand, if limited access to quality child care is the main force behind children's weight gain, public policies helping working mothers would be the most effective solution (Fuller et al, 2002). For instance, government can increase access to quality child care by offering a sizable subsidy to low-income working mothers or tax incentives to facilities that operate quality child care in low-income neighborhoods. Finally, if a budget constraint is what drives children's high caloric intake, government can make healthier food more affordable by subsidizing it or by providing families with a larger cash transfer.

The difference between the effect of cash versus the effect of an in-kind transfer further offers potential policy implication. Programs such as the SNAP provide in-kind transfers to reduce food insecurity whereas the EITC program provides cash in order to improve a recipient's overall well-being.[19] How families respond to different types of transfers could guide policymakers in designing an effective policy. Hoynes and Schanzenbach (2009) state that a dollar in cash and a dollar in food stamp benefits have the same effect of increasing an overall food expenditure. Their finding suggests that an increase in food stamp benefits may have a similar impact on children's weight as does an increase in policy-driven income.[20]

Given that only low-income families are affected by the EITC expansions, and the policy-driven income is a temporary income shock, the paper's finding has limited generalizability. The income effect identified here is a local average treatment effect. Consequently, the positive IV coefficient captures the income effect on children only from low-income families and is unlikely to extend to those from higher income families. Besides, despite the plausible difference between a permanent versus a temporary income shock, empirical evidence suggests that both types of shocks induce a similar response, at least among low-income families. The finding that policy-driven income increases the weight of children from low-income families is consistent with the inverse U-shaped relationship between permanent family income and children's weight (Jo, 2014).

[19]The food stamp program provides coupons (nowadays an electronic debit card), which can only be used to purchase food.

[20]Researchers generally find that participating in the food stamp program increases the weight of female adults (Chen et al., 2005; Meyerhoefer and Pylypchuk, 2008), but the effect on children's weight has been mixed because of the difficulty in addressing selection bias (Gibson, 2004; Gibson, 2006; Hofferth and Curtin, 2005; Kreider et al., 2012; Schmeiser, 2012).

The main finding of my paper implies that, despite its success in increasing female labor force participation, the EITC expansion potentially had an adverse impact on children's health. The expansion has been previously shown to improve maternal health (Averett and Wang, 2013; Evans and Garthwaite, 2010) and infant health (Hoynes et al., 2012); on the contrary, the finding here suggests that the expansion adversely impacted children's health by increasing childhood obesity. Moreover, the IV result suggests that part of the increase was generated by a policy-driven increase in income.

7 Conclusion

Unlike the Medicaid program, which has an explicit goal of improving a recipient's health, a cash transfer program like the EITC has a more general purpose of improving a recipient's overall well-being. Given such high obesity rates among children from low-income families and the significance of the EITC program for low-income families, it is crucial to understand what role policy-driven income plays in increasing childhood obesity. In this paper, I examine the impact of policy-driven income on childhood obesity by exploiting the expansions of the EITC program in the 1980s and 1990s.

Family income and children's weight overall exhibit a negative association among families from all income levels. However, the EITC income and children's weight illustrate a positive association. The DD estimates imply that a child's probability of obesity increased by 3 percentage points following the largest EITC expansion. Since part of the observed effect is plausibly driven by an increase in maternal employment, I employ an IV strategy to identify only the income effect generated by exogenous policy changes. The IV estimates indicate that a $1,000 increase in family income generates an approximately 0.02% of a standard deviation increase in BMI or a 0.5 percentage point increase in the probability of obesity. The finding is robust across various specifications accounting for other policy changes and parental working hours.

The paper's main finding implies that the EITC has an adverse impact on childhood obesity. However, there are several caveats. First, the identified income effect has a limited generalizability because of certain characteristics of the EITC income. The finding only applies to children from the EITC-eligible families and to a particular type of income shock. On the other hand, even though the finding suggests a potentially negative health consequence for some children, I do not argue against the EITC program. Previous studies find a positive effect of the EITC on multiple

dimensions, hence, the program's benefits outweigh its potential harms. The paper highlights the importance of investigating even seemingly irrelevant outcomes to understand the full consequence of a program.

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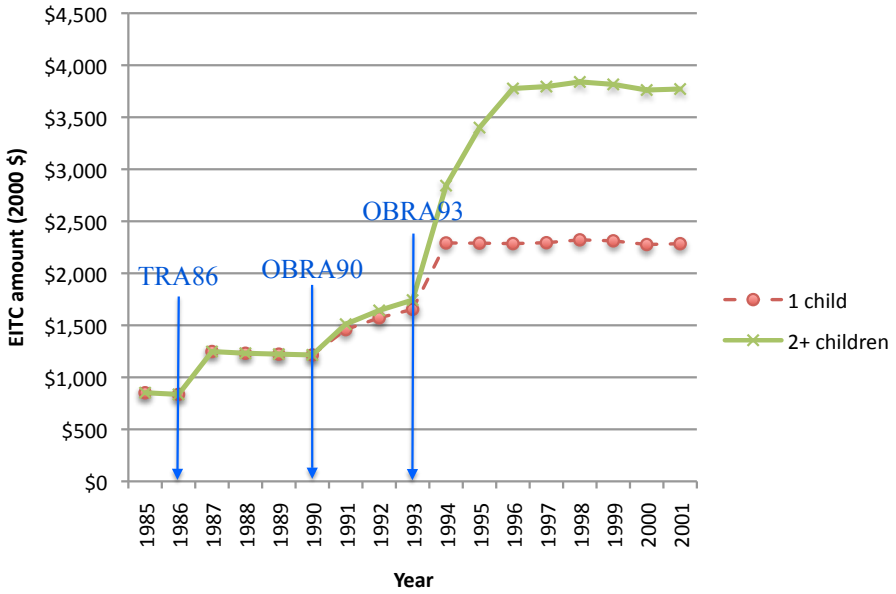
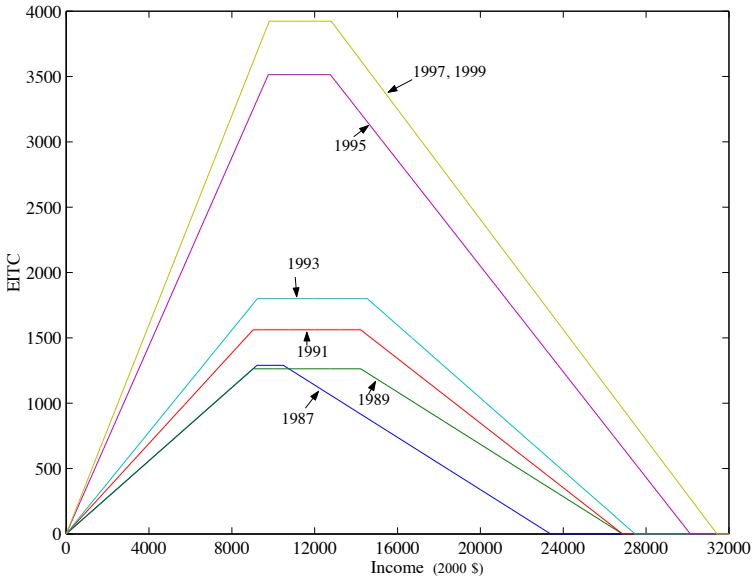
Figure 1: Federal Earned Income Tax Credit (EITC) Max Benefit Amount



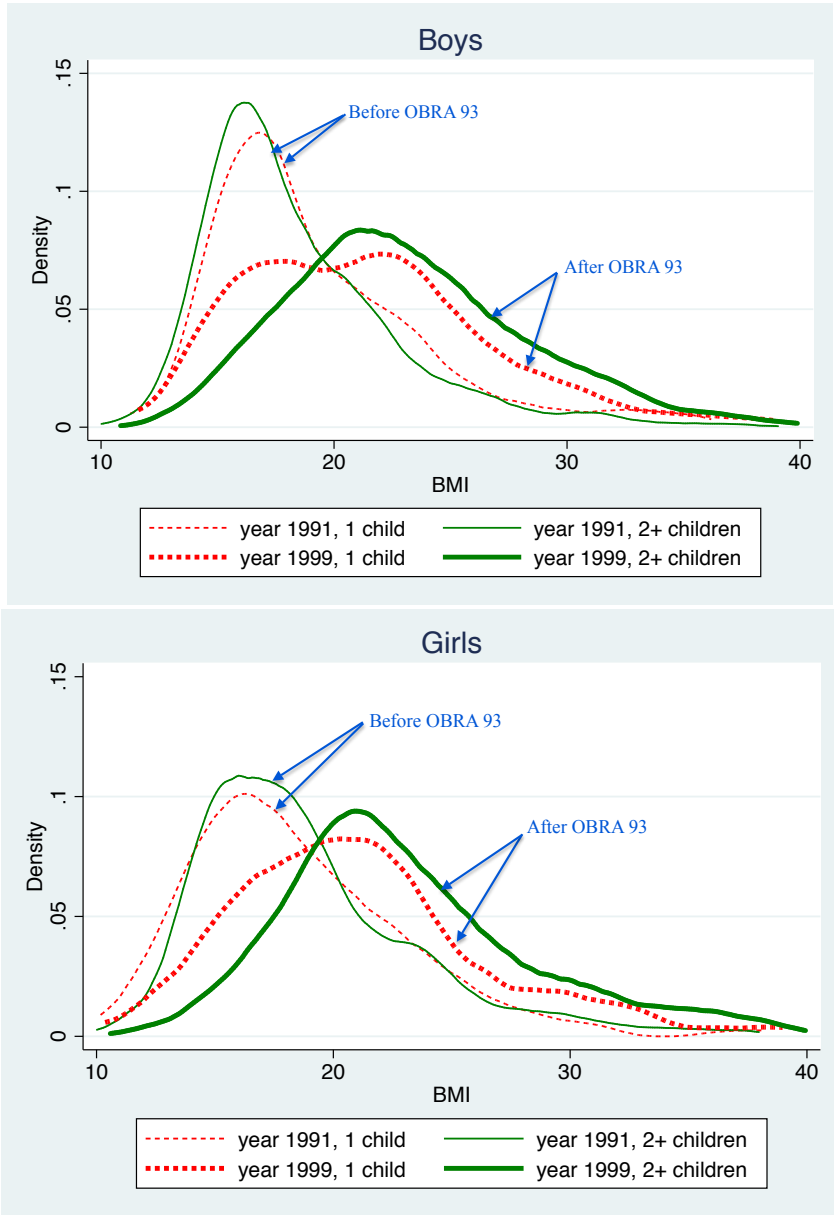
Source: Author's own calculations using the IRS form 1040 for each year

Figure 2: Federal EITC Schedules for Families with Two or More Children



Source: Dahl and Lochner (2012)

Figure 3: BMI Distribution Before and After OBRA 93



Source: Author's own calculations

Table 1: Summary Statistics

Variable	All Sample (1)	Not Eligible (2)	Eligible (3)
Panel A. Dependent variables			
BMI (body mass index)	18.30	18.07	18.85
ZBMI (normalized BMI)	0.26	0.23	0.32
% obese	0.15	0.15	0.16
% overweight	0.14	0.14	0.15
$\geq$ 85th BMI percentile	0.29	0.29	0.32
Panel B. Regressors			
Family income ($1K, year 2000 dollars)	34.4	41.48	17.41
Male	0.51	0.51	0.50
Black	0.30	0.25	0.42
Hispanic	0.20	0.19	0.21
Age	8.50	8.36	8.86
No sibling	0.13	0.12	0.15
One sibling	0.40	0.42	0.35
$\geq$ two siblings	0.47	0.46	0.49
Mother's age	32.78	33.00	32.23
Father's age	35.00	35.26	33.94
Mother was married last year	0.67	0.77	0.45
Number of adults in household	1.89	1.94	1.76
Mother lived with both parents at age 14	0.65	0.68	0.57
Highest grade completed by mother's father	8.64	9.19	7.31
Highest grade completed by mother's mother	9.72	10.07	8.88
Proportion of self-reported adiposity measures	0.25	0.26	0.23
Missing observation indicators			
Earning	0.03	0.05	0.00
Father's age	0.04	0.04	0.04
Mother lived with both parents at age 14	0.00	0.00	0.00
Number of adults in household	0.00	0.00	0.00
Highest grade completed by mother's father	0.16	0.14	0.21
Highest grade completed by mother's mother	0.06	0.06	0.08
Self-reported adiposity measures	0.14	0.14	0.14

Notes: Summary statistics presented here only includes observations used in the baseline IV regression. Statistics for spouse-related variables are calculated conditional on mothers being married. Whether a child is "obese" or not is determined using the 2000 Centers for Disease Control sex- and age-specific growth chart. If a child's BMI is greater than the 95th percentile, then he/she is considered "obese." If it is between the 85th and 95th percentiles, he/she is categorized as "overweight." A family income variable is net of all taxes and includes the EITC benefit as well as non-taxable income.

Table 2: The Impact of Family Income on Children's Weight: OLS and First-Differenced Estimates

	OLS	**OLS**	**OLS**	**Diff**
	(1)	(2)	(3)	(4)
		Panel A. ZBMI		
Family income ($10K)	-0.014***			
	(0.005)			
EITC ($10K)		0.386**	0.246	0.290*
		(0.166)	(0.193)	(0.176)
		Panel B. Obese		
Family income ($10K)	-0.005***			
	(0.001)			
EITC ($10K)		0.060	-0.012	0.046
		(0.047)	(0.055)	(0.048)
		Panel C. $\geq$ 85th		
Family income ($10K)	-0.005***			
	(0.002)			
EITC ($10K)		0.109*	0.044	0.004
		(0.058)	(0.068)	(0.055)
Sample	all	all	$\leq$HS	$\leq$HS
N	24,414	24,414	16,001	16,001

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* The Ordinary Least Squares (OLS) regression models (1)–(3) include year fixed effects and all of the control variables listed in Panel B of Table 1. The first-differenced (Diff) regression model (4) is estimated using the first-differences with gender, race, age, and year dummy variables. The fixed effect model, not shown here, gives similar estimates as those from the first-differenced model. Standard errors are clustered at the family level.

Table 3: Summary Statistics for the Treatment Group (Children from Families with Two or More Children) and the Control Group (Children from Families with One Child)

	1 Child	**2+ Children**	**Difference**
	(1)	(2)	(3)
BMI	18.27	18.16	0.12
	(0.17)	(0.14)	(0.44)
% obese	0.17	0.14	0.04***
	(0.01)	(0.00)	(0.01)
$\geq$ 85th BMI percentile	0.32	0.27	0.05***
	(0.01)	(0.00)	(0.01)
Family income ($1,000)	23.84	25.32	-1.48**
	(0.46)	(0.15)	(0.48)
Male	0.50	0.51	-0.01
	(0.01)	(0.00)	(0.01)
Black	0.29	0.34	-0.06***
	(0.01)	(0.00)	(0.01)
Hispanic	0.18	0.23	-0.05***
	(0.01)	(0.00)	(0.01)
Children's age	7.74	8.18	-0.43***
	(0.09)	(0.03)	(0.10)
Mother's age	29.10	29.65	-0.55***
	(0.09)	(0.03)	(0.09)
Mother was married last year	0.47	0.60	-0.13***
	(0.01)	(0.00)	(0.01)
# of adults in household	1.99	1.90	0.08**
	(0.03)	(0.01)	(0.03)
Mother lived with both parents at age 14	0.63	0.58	0.06***
	(0.01)	(0.00)	(0.01)
Highest grade completed by mother's father	8.31	7.24	1.07***
	(0.12)	(0.04)	(0.13)
Highest grade completed by mother's mother	9.35	8.76	0.59***
	(0.09)	(0.03)	(0.10)

Note: ** p<0.10, ** p<0.05, *** p<0.01.* The table demonstrates the differences in average group characteristics between the treatment and control groups.

Table 4: The Impact of Income on Children's Health Outcomes
(Children Whose Mothers Have a High School Education or Less): DD estimates

	$\geq$ 85th	**Obese**	**Underweight**
	(1)	(2)	(3)
A. No controls	0.052***	0.021	-0.033***
	(0.018)	(0.014)	(0.009)
B. No controls, child FE	0.038*	0.031**	-0.023**
	(0.020)	(0.015)	(0.010)
C. With controls, child FE	0.039**	0.030**	-0.021**
	(0.020)	(0.015)	(0.010)
D. With controls, child FE, state unemp.	0.039**	0.030**	-0.021**
rate, welfare & food stamp policies	(0.020)	(0.015)	(0.010)
Pre-OBRA 93 average (1985–1993)	0.25	0.13	0.10

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* Each column and row combination represents a separate regression from using the DD equation (3) of Section 3. Specification A presents a simple DD estimate without any control variables, which is equivalent to the simple mean comparison. Specifications B through D include child fixed effects (FE). Specifications C and D include child and family characteristics listed in Panel B of Table 1 to account for the group differences. Standard errors are clustered at the family level.

Table 5: The Impact of Family Income on Children's Weight: IV Estimates

	IV1		**IV2**		**IV3**	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. ZBMI			
Family income ($10K)	0.179***	0.280***	0.153***	0.183***	0.197***	0.196***
	(0.045)	(0.073)	(0.047)	(0.070)	(0.054)	(0.069)
			Panel B. Obese			
Family income ($10K)	0.028***	0.038**	0.024**	0.017	0.046***	0.034*
	(0.011)	(0.017)	(0.012)	(0.019)	(0.014)	(0.018)
			Panel C. $\geq$ 85th			
Family income ($10K)	0.031**	0.044**	0.014	0.007	0.025	0.010
	(0.013)	(0.022)	(0.014)	(0.021)	(0.016)	(0.022)
sample	all	$\leq$HS	all	$\leq$HS	all	$\leq$HS
First stage F	92.19	74.29	109.57	87.04	109.78	77.96
N	24,414	16,001	24,414	16,001	24,414	16,001

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* All of the regression models include children's gender and race indicators. An inclusion of the first-differenced control variables or a fifth polynomial function of lagged income $f(P_{i,t-1}) = I(P_{i,t-1} > 0) + P_{i,t-1} + P^2_{i,t-1} + P^3_{i,t-1} + P^4_{i,t-1} + P^5_{i,t-1}$ does not change the main result. Standard errors are clustered at the family level.

Table 6: Exploring the Role of Parental Employment in Estimated Effect

	ZBMI	**Obese**	**$\geq$ 85th**
	(1)	(2)	(3)
A. Baseline	0.197***	0.046***	0.025
	(0.054)	(0.014)	(0.016)
B. Total # of hours worked per year by mother (divided by 100)	0.217***	0.050***	0.024
	(0.064)	(0.016)	(0.019)
C. Total # of hours worked per year by parents (divided by 100)	0.236***	0.053***	0.027
	(0.067)	(0.017)	(0.020)
D. Avg # of hours mothers worked per week since a child's birth	0.194***	0.046***	0.019
	(0.055)	(0.014)	(0.017)
E. Mother's employment status	0.199***	0.046***	0.024
	(0.056)	(0.015)	(0.017)
F. Mother's employment status & total # of hours worked	0.217***	0.050***	0.024
	(0.064)	(0.016)	(0.019)

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* All of the regression models include children's gender and race indicators. The baseline IV regression model (specification A) is obtained from column (5) of Table 5. Standard errors are clustered at the family level.

Table 7: IV Estimates Accounting for the State Policy Changes

	ZBMI	**Obese**	**$\geq$ 85th**
	(1)	(2)	(3)
A. Baseline	0.197***	0.046***	0.025
	(0.054)	(0.014)	(0.016)
B. Year dummies	0.124**	0.043***	0.020
	(0.052)	(0.014)	(0.016)
C. Prior to the PRWORA 96 (1986–1996)	0.323***	0.061***	0.055**
	(0.086)	(0.020)	(0.025)
D. State unemployment rate, welfare & food stamp policies	0.187***	0.047***	0.024
	(0.053)	(0.014)	(0.016)
E. Year dummies, state unemployment rate, welfare & food stamp policies	0.124**	0.044***	0.021
	(0.052)	(0.014)	(0.016)

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* All of the regression models include children's gender and race indicators. The baseline IV regression model (specification A) is obtained from column (5) of Table 5. Standard errors are clustered at the family level.

Table 8: Heterogeneous Income Effect on Children's Weight (dependent variable=obese)

	Child's Gender (1)	**Child's Race** (2)	**Child's Age** (3)	**Mother's Marital Status** (4)	**Mother's Education** (5)	**Pretax Income** (6)
	Female	*Black/Hispanic*	*<10yrs old*	*Not married*	*≤ HS*	*<$35K*
Income ($10K)	0.058***	0.082**	0.067***	0.107**	0.034*	0.032***
	(0.019)	(0.032)	(0.021)	(0.048)	(0.018)	(0.010)
% obese	0.15	0.19	0.15	0.19	0.16	0.17
% eligible	0.29	0.36	0.26	0.49	0.34	0.54
First stage F	88.99	44.57	87.27	29.75	77.96	419.32
N	12,008	12,084	13,814	7,742	16,001	12,361
	Male	*White*	*≥10yrs old*	*Married*	*> HS*	*≥$35K*
Income ($10K)	0.035*	0.026**	0.020	0.016	0.049***	0.004
	(0.019)	(0.012)	(0.018)	(0.010)	(0.018)	(0.007)
% obese	0.16	0.13	0.16	0.14	0.15	0.14
% eligible	0.28	0.20	0.31	0.19	0.17	0.02
First stage F	65.49	71.34	80.39	92.91	51.94	149.70
N	12,406	12,330	10,600	16,672	8,413	12,053

Notes: ** p<0.10, ** p<0.05, *** p<0.01.* The table presents coefficient estimates from the baseline IV regression model by various subgroups. The "% obese" and "% eligible" denotes "the proportion of obese children" and "the proportion of children whose families are eligible for the EITC benefit," respectively. Standard errors are clustered at the family level.

Appendix

Appendix Table 1: Proportion of Eligible Sample at Each Region of the EITC Schedule

Year	# of children	% eligible	Phase-in	Flat	Phase-out
1985	3,096	0.29	0.52	0.12	0.32
1987	3,096	0.33	0.46	0.02	0.52
1989	2,968	0.32	0.33	0.17	0.50
1991	3,301	0.30	0.32	0.21	0.47
1993	3,630	0.30	0.30	0.23	0.47
1995	3,626	0.30	0.27	0.11	0.61
1997	3,333	0.29	0.31	0.10	0.59
1999	2,423	0.27	0.26	0.13	0.61
2001	2,037	0.22	0.28	0.07	0.64
Total	27,510	0.29	0.34	0.13	0.52