

Income, the Earned Income Tax Credit, and Infant Health

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ABSTRACT:

This paper evaluates the health impact of a central piece in the U.S. safety net for families with children: the Earned Income Tax Credit. Using tax-reform induced variation in the federal EITC and the presence and generosity of state EITC's, we examine the impact of the credit on infant health outcomes. We find that increased EITC income reduces the incidence of low birth weight and increases mean birth weight. For single low education (≤ 12 years) mothers, a policy-induced treatment on the treated increase of \$1000 in EITC income is associated with 7% reduction in the low birth weight rate. These impacts are evident with difference-in-difference models and event study analyses. We conclude that the sizeable increase in income for eligible families significantly improved birth outcomes for both whites and African Americans, with larger impacts for births to African American mothers.

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1. Introduction

The Earned Income Tax Credit (EITC) provides a refundable transfer to lower income working families through the tax system. Through expansions in the federal credit in 1986, 1990, and 1993, the EITC is now the most important cash transfer program for these families (Bitler and Hoynes 2011). In 2008, the EITC reached 25 million families at a total cost of \$51 billion, compared to \$9 billion in benefits for cash welfare (TANF) and \$50 billion for food stamps. The income transfers are significant; for example, among families with two or more children eligibility extends to annual earnings of about \$39,000 and the average credit for these recipient families is \$2,563¹. This policy, developed in the U.S., is being adopted across many other developed countries around the world (Owens 2005).

Following the rapid expansion of the EITC and its now central place in the U.S. safety net, a substantial literature has examined the impact of the EITC on a wide variety of outcomes such as labor supply, poverty, consumption, marriage, and fertility.² Our paper enters at this point and examines the potential health benefits of this important income transfer program. In particular, we examine the impact of the EITC on infant health outcomes, including birth weight and low birth weight. This adds to a small, but growing, literature on the potential health benefits of non-health programs in the safety net.³

Using the EITC to examine impacts on infant health is attractive for several reasons. First, the EITC generates sizable increases in household after-tax income. As we discuss below, the EITC increases income through both the tax credit and incentivized increases in earnings.

¹ The figures for the EITC are for tax year 2008, the most recent program data available (Internal Revenue Service, 2011a). TANF expenditures are for 2009 and consist of total cash expenditures (U.S. Department of Health and Human Services 2011). Food stamp expenditures are for 2009 (U.S. Department of Agriculture, 2011).

² See the reviews of the EITC literature in Eissa and Hoynes (1996) and Hotz and Scholz (2003).

³ For example, Almond, Hoynes and Schanzenbach (2010, 2011) examine the health impacts of the Food Stamp Program and Bitler, Gelbach and Hoynes (2005) examine the health impacts of welfare reform. Closer to this study are Evans and Garthwaite (2010) who examine the impact of the EITC on maternal health and Baker (2008) and Strully et al (2010) who examine the impact of the EITC on birth outcomes. These papers are discussed below.

Further, our research design harnesses the increases in income deriving from tax reforms, allowing us to identify exogenous increases in income. This is important because there are few quasi-experiments that identify exogenous changes in income (see discussion in Almond, Hoynes, and Schanzenbach 2011). This approach, allows us not only to analyze the impact of the EITC on health, but also speak to the more general question of the impacts of income on health.

We use the U.S. Vital Statistics data, covering the full census of births beginning in 1984, a few years before the 1986 expansion in the EITC, through 1998, a few years after the 1993 expansion in the EITC is fully phased in. Using the national natality data, we examine the impacts of the EITC on birth weight and low birth weight (weighing less than 2,500 grams). These outcomes are standard measures of infant health, and are highly predictive of longer term adult health and economic outcomes. For a recent review see Currie 2011. We also explore possible mechanisms for these changes by examining impacts on behaviors (smoking and drinking during pregnancy) as well as utilization of prenatal care, pre-term birth, and Apgar score.

We use four quasi-experimental estimation strategies. First, we begin with a difference-in-difference analysis of the most recent and largest EITC reform, OBRA 1993. This commonly used approach in the EITC and labor supply literature leverages variation over time and across family size. Second, we use an event study design, along with comparison groups, to analyze the impacts of the 1993 expansion. This approach allows us to explicitly examine the validity of the control group by examining differences in pre-trends across groups. In the third approach, we expand the time frame to encompass the 1984, 1990, and 1993 EITC expansions. To do so, we estimate a panel fixed effects model where we measure the generosity of the EITC using the maximum EITC credit. This measure of the EITC varies over years for the three different expansions. It also increases with family size for the 1993 expansion and later. Finally, in the

fourth approach we take advantage of the considerable variation across states due to state “add-on” EITCs. This approach allows us to expand on the year-by-number of children identification design used by us and in many other EITC papers. To address the potential endogenous changes in the composition of birth, due to EITC-induced fertility or survivor bias, we examine impacts on fertility rates and the observable characteristics of the births.

In the empirical results, we explore differences in estimates across groups more and less likely to be impacted by the EITC, using mother’s education, marital status, age, race, and deciles of predicted EITC treatment. We also present various placebo results. To interpret the magnitude of our findings, we use the March Current Population Survey combined with the NBER TAXSIM model to compute average EITC benefits for the subsamples that we analyze in the natality data. We use these calculations to quantify the “treatments” received by different groups and thereby interpret differences in our estimated EITC impacts on infant health.

We find that increased EITC income reduces the incidence of low birth weight and increases mean birth weight. For single low education (≤ 12 years) mothers, a policy-induced treatment on the treated increase of \$1000 in EITC income is associated with 7% reduction in the low birth weight rate. These results provide important findings for evaluating the benefits of the social safety net as well as the more general question of how income affects health.

The remainder of our paper is organized as follows. In Section 2 we describe the EITC and the tax reform induced changes in the credit over our sample time frame. In Section 3 we review the background literature and discuss the possible channels through which the EITC may impact infant health. In Section 4 we describe the data and in Section 5 we describe our empirical model. In Section 6 we present our results and in Section 7 we conclude.

2. The Earned Income Tax Credit and Tax Reforms

The Earned Income Tax Credit began in 1975 as a modest program aimed at offsetting the social security payroll tax for low-income families with children and was born out of a desire to reward work. The EITC is refundable so that a taxpayer with no federal tax liability receives a tax refund from the government for the full amount of the credit. Taxpayers can elect to receive the credit throughout the year with their paychecks; but very few (less than 5 percent) avail themselves of this early payment option (Friedman 2000).

A taxpayer's eligibility for the EITC depends on their earned income (or in some cases adjusted gross income), and the number of qualifying children who meet certain age, relationship and residency requirements. First, the taxpayer must have positive earned income, defined as wage and salary income, business self-employment income, and farm self-employment income. Also, the taxpayer must have adjusted gross income and earned income below a specified amount. In 2011, the maximum allowable income for a taxpayer with one (two or more children) is \$36,052 (\$40,964) (Tax Policy Center 2011). Second, a taxpayer must have a qualifying child, who must be under age 19 (or 24 if a full-time student) or permanently disabled and residing with the taxpayer for more than half the year.

The amount of the credit to which a taxpayer is entitled depends on the taxpayer's earned income, adjusted gross income, and, since 1991, the number of EITC-eligible children in the household. There are three regions in the credit schedule. The initial phase-in region transfers an amount equal to a subsidy rate times their earnings. Since 1995, the subsidy rate is 34 percent for taxpayers with one child and 40 percent for taxpayers with two or more children. In the flat region, the family receives the maximum credit (in 2011 \$3,094 for one child and \$5,112 for two or more children), while in the phase-out region, the credit is phased out at the phase-out rate (16 and 21 percent). While the generosity of the credit varies with number of children, it does not

vary with marital status; taxpayers pool their earnings and income and apply their combined resources to determine eligibility and credit amounts.⁴

The reach and importance of the credit has changed substantially over its history. The program grew slowly from its introduction in 1975 until 1986, and in fact shrank in real terms due to inflation. The 1987 expansion of the EITC, passed as part of the Tax Reform Act of 1986 (TRA86), increased the generosity of the credit for the lowest-income workers and extended its benefits beyond the poorest (see Eissa and Liebman 1996). TRA86 returned the real maximum credit to its 1975 level, and as part of a more widespread change in TRA86 indexed the EITC for inflation. By 1988, taxpayers with incomes between \$11,000 and \$18,576 became eligible for the credit and faced its phase-out marginal tax rate for the first time. The credit was extended again in 1991, as part of the Omnibus Reconciliation Act of 1991. The largest single expansion over this period was contained in the Omnibus Reconciliation Act of 1993 (OBRA93) legislation. The 1993 expansion of the EITC, phased in between 1994 and 1996, led to an increase in the subsidy rate from 19.5 percent to 40 percent (18.5 to 34 percent) and an increase in the maximum credit from \$1,511 to \$3,556 (\$1,434 to \$2,152) for taxpayers with two or more children (taxpayers with one child). The phase-out rate was also raised, from 14 percent to 21 percent (13 to 16 percent) for taxpayers with two or more children (taxpayers with one child). Overall, the range of the phase-out was expanded dramatically, such that by 1996 a filing unit with two children would still be eligible with income levels of almost \$30,000. In addition, OBRA93 introduced a small credit was created for families with no children.

Figure 1 summarizes these expansions in the EITC and shows the real maximum credit (in 1999 dollars) by tax year and family size for our analysis period, 1983 to 1999. Importantly, this illustrates the differential magnitude of the expansions based on family size (no children,

⁴ Beginning in 2002, the phase-out range was increased for married taxpayers filing jointly. The values for these taxpayers were \$1,000 higher than for singles in 2002, and are \$5,080 higher in 2011.

one, two or more) that forms the basis of our quasi-experimental design. Families with no children are eligible for only a small credit (\$347 in 1999 dollars) beginning in 1993. Following OBRA93, families with two or more children experience increases in the maximum credit of \$2,160 (1999 dollars) compared to the much smaller change of \$725 for families with one child.

These expansions have led to a dramatic increase in the total cost of the EITC. As discussed in Eissa and Hoynes (2011), the total cost of the EITC increased steadily from less than 10 billion in 1986 (in 2004 dollars) to more than 40 billion in 2004 (2004 dollars). In fact, between 1990 and 1996 the program more than doubled in real terms. In 2008, the most recent year for which data is available, the EITC was received by 25 million families for a total cost of more than 50 billion dollars (Internal Revenue Service 2011a).

In addition to the federal credit, many states have introduced state EITCs. Rhode Island was the first state to create a state EITC, in 1986. As of 2011, a total of 25 states have introduced state EITC's that supplement the federal credit (Hatcher Group, 2011). Almost all states structure their credits as a share of the federal credit, varying between 5 percent or less in a handful of states (Illinois, Maine, North Carolina, Washington) to more than 40 percent (Wisconsin) and almost all make the credit refundable like the federal credit. Figure 2 shows the number of states offering EITCs. State EITCs have typically been introduced in periods of economic expansion, as illustrated by activity in the mid to late 1990s and the later part of the 2000s.

3. The EITC and Infant Health

The EITC may lead to changes in infant health through several channels including income, maternal labor supply, and fertility. Here we discuss these channels and in so doing, discuss the theoretical expectations and related empirical literature.

First, an expansion in the EITC leads to an exogenous and sizable increase in after-tax

income for low to moderate income families with children. Hence spending on all normal goods will increase, and assuming child health is a normal good, health inputs increase leading to an improvement in infant health (Currie 2009). It is well established that family socio-economic status is associated with better health (for example see Case, Lubotsky and Paxson 2002). However, due to many confounding variables (such as cognitive ability and other psychological and emotional skills, social class, early childhood conditions, as well as the potential for reverse causality) the literature provides few estimates of the causal impact of income on birth weight, or health more broadly (Almond and Currie 2011, Currie 2011). As stated in the recent and comprehensive survey by Almond and Currie (2011), “It is however, remarkably difficult to find examples of policies that increase incomes without potentially having a direct effect on outcomes.”

One approach is to use variation in social assistance policies to leverage exogenous variation in income. For example, Currie and Cole (1993) use a sibling fixed effect estimator and find that receipt of AFDC income has no impact on birth weight while Kehrer & Wolin (1979) find evidence that the Gary Income Maintenance experiment may have improved birth weight for some groups. More recently, Almond, Hoynes and Schanzenbach (2011) use the introduction of the Food Stamp Program and find that the near-cash transfer leads to an increase in birth weight, a reduction in low birth weight, and no change in neonatal infant mortality. Similarly, Hoynes, Page and Stevens (2011) find that exposure to WIC, a food and nutrition program for pregnant women and young children, leads to an improvement in infant health.

There is also evidence that conditional cash transfers in developing countries can improve birth outcomes (Barber and Gertler 2008, Amarante et al 2011). While not examining income per se, related work explores the impact of maternal education (Currie and Moretti 2003, McCrary and Royer 2011), layoffs (Lindo 2011) and recessions (Dehajia and Lleras-Muney

2004) on infant health. Given the limited evidence on this important issue, our paper provides noteworthy evidence on the potential health benefits of increases in income.⁵

The increase in after-tax income could also lead to increased behaviors such as such as smoking or drinking which lead to well documented decreases in birth weight (Currie, Neidell, and Schneider 2009). Infant health improvements may work through other channels as well, for instance reducing stress (e.g., financial stress) experienced by the mother which may have a direct and beneficial impact on birth weight (Aizer et al 2009, Camacho 2008, Evans and Garthwaite 2010).

A second possible channel operates through employment and earnings. Because the EITC is tied to work, the credit provides incentives to enter work for single parent (or single earner) families. However, secondary earners, such as some married women, face incentives to reduce work. The predictions for hours worked for all family types are more complex, but for most workers theory suggests an incentive to reduce hours if already in the labor market (Eissa and Hoynes 2006). There is consistent empirical evidence that the EITC encourages work among single mothers but little evidence that eligible-working women adjust their hours of work in response to the EITC (Eissa and Liebman 1996, Hotz, Mullin and Scholz 2002, Keane and Moffitt 2004, Meyer and Rosenbaum 2001). Eissa and Hoynes (1998) find that the EITC leads to a modest reduction in employment for married women and no change for married men. This discussion implies that the EITC may lead to an increase in income through an increase in own earnings (at least for single women) (Patel 2011). Increases in maternal employment may have independent impacts on stress and infant health (Baum 2005, Del Bono et al 2008).

⁵ Some studies provide credible evidence on the impact of income on dimensions of health other than infant health. These studies leverage income variation from a wide range of sources and examine, for example, unanticipated social security payments and mortality (Snyder and Evans 2005), the opening of Indian casino and mental health (Costello et al 2003), declines in agricultural income and mortality (Banerjee et al 2007), and receipt of an inheritance and self reported health (Meer, Miller and Rosen 2003), lottery winnings a “health index” (Lindahl 2005).

In addition, because the EITC is tied to the presence and number of children, an expansion in the credit could theoretically lead to increases in fertility. On the other hand, the work-inducing aspect of the EITC suggests that it could lead to reductions in fertility. Therefore, a third possible channel for the effect of the EITC on birth weight is through changes in births and the composition of births. Any increase in fertility for this relatively disadvantaged group would be expected to lead to a negative compositional effect and subsequent downward bias on the estimates. However, the existing evidence suggests that the EITC does not impact fertility (Baughman and Dickert-Conlin, 2009) or family formation (Dickert-Conlin 2002; Ellwood, 2000, Herbst 2011).⁶

Overall, given the balance of evidence and predictions, we expect that the EITC may improve infant health. The same forces that improve infant health, however, could also lead to a change in the composition of births. In particular, if improvements in fetal health lead to fewer fetal deaths, there could be a negative compositional effect on birth weight from improved survivability of “marginal” fetuses. This could bias downward the estimated effects of the EITC on birth weight. In any case, to evaluate such channels (and the related question of selective fertility), we test for impacts of the EITC on total births and the composition of births.

Ours is not the first paper to analyze the health impacts of the EITC. Evans and Garthwaite (2010) use a difference-in-difference analysis of OBRA93, relying on comparisons across women with one versus two or more children, to examine impacts on maternal health using biomarkers and self-reported health. Quite relevant for our work, they find evidence that the expansion of the EITC lowered the counts of the risky biomarkers in mothers, suggesting an income pathway for a reduction in stress. Baker (2008) also examines OBRA93 using a difference-in-difference design, and concludes that the EITC leads to a 7 to 14 gram increase in

⁶ This finding is consistent with the broader literature finding that the elasticity of fertility with respect to transfers from income support programs is very small (Moffitt 1998).

average birth weight. Strully et al. (2010) find that the presence of a state EITCs leads to a 15 gram increase in average birth weight.⁷ Given this emerging literature, our paper makes several contributions. First, we present results from several identification strategies, including an OBRA93 difference-in-difference design and a parametric design using the maximum credit to facilitate analysis of a longer time period with multiple tax reforms. Second, we present event study analyses as a direct test of the validity of our research design. Third, we richly analyze differences across subgroups based on mother's demographic characteristics and the magnitude of the EITC treatment. Using the CPS combined with TAXSIM, we are able to quantify the predicted EITC benefit for each group and therefore compare average treatment effects using a reasonable metric. Finally, we examine impacts on fertility and the composition of births to analyze the potential for endogenous fertility.

4. Data

Our main data is the U.S. Vital Statistics Natality Data, which consists of micro data on the full census of births from the National Center for Health Statistics. We use data covering births from 1983-1999. The data include birth weight, gender, live birth order (parity), and state and month of birth. There are also (limited) demographic variables including the age, race, ethnicity, education and marital status of the mother. Education and ethnicity of the mother are missing in some state-years, but by 1992 all states provide education and by 1993 all states provide ethnicity.⁸ There are also missing values for birth weight, parity, race, and marital status,

⁷ Other studies use tax-reform induced changes in after-tax income to examine impacts on other child outcomes. Dahl and Lochner (2009) find the increase in income through the EITC leads to improvements in child test scores. Milligan and Stabile (2008) use variation in child benefits across Canadian provinces finding that higher income leads to increases in child test scores and decreases in aggression.

⁸ In 1983 California, Texas, and Washington did not report education. In 1988 New York City also stopped reporting education, causing a sizable portion of births from New York State to be missing mother's education. California started reporting education in 1989, and in 1990 Texas. By 1991 New York City reported, and all states reported education in 1992. For Ethnicity: In 1983, there were 27 states that did not report Hispanic status.

but these are rare and not systematically occurring across states. We limit the sample to mothers age 18 and older who are not missing values for birth weight or parity.

We collapse the data to cells defined by state, month, parity of birth (1st, 2nd, or 3rd or greater birth to a mother), education of mother (<12, 12, 13-15, 16+, missing), marital status of mother (single, married, missing), race of the mother (white, black, other, missing), ethnicity of the mother (Hispanic, non-Hispanic, missing) and age of mother (18-24, 25-35, 35+). For each cell we calculate average birth weight, average parity (for 3rd or greater cell), the fraction of births below 2,500 grams (also the fraction below 1500, 2000, 3000, 3500, and 4000 grams) and number of births.

Once we have the data collapsed to cells, we assign the appropriate tax (EITC) schedule for the births. As illustrated in Table 1, assigning the appropriate EITC schedule amounts to assigning the “effective tax year” (what tax year the birth is “treated” by) and number of children to each birth. To do so, note first that the pregnancy is the “treated” time frame, so the number of children *prior* to the current birth will dictate the appropriate EITC schedule. For example, we assign a first-born child the EITC schedule for “no children,” while a third-born child would be given an EITC schedule for “two children.”⁹

We make two assumptions to assign each birth (or cell) to a given tax year. Our first assumption, which we refer to as “cash in hand”, assumes that the EITC’s impact on infant health runs through the cash available to the family which arrives with receipt of the tax refund.

Figure 3, which is reprinted from LaLumia (2011), shows that more than 50 percent of EITC tax

Alabama, Connecticut, Kentucky, North Carolina, and Washington began reporting in 1988, the majority of the other states followed in 1989 when only Louisiana, New Hampshire, and Oklahoma did not report. Louisiana began reporting in 1990, Oklahoma in 1991, and New Hampshire in 1993.

⁹ As with exemptions, a new birth is counted as a child for the tax year regardless of when they are born (Internal Revenue Service 2011b). However, as we describe below, given our “cash on hand” assumption, the birth is assigned the tax schedule for the tax year one or two years prior to the calendar year of the birth. Therefore, the number of children that is relevant for assigning the appropriate tax schedule is the number of children in the family *prior* to this birth.

refunds are received in February. So, for example, most tax year 1990 refunds are received in February 1991 (or shortly thereafter). Hence, in practice we assume that a birth is treated based on the tax code for the prior calendar year if their sensitive developmental stage occurs during February or later, and are treated based on the tax code of two calendar years ago if their sensitive developmental stage occurs during January. Second, we assume that the sensitive developmental stage is three months prior to birth.¹⁰ This is motivated by evidence that the third trimester of pregnancy is important for birth weight production.¹¹

Combining these “cash in hand” and sensitive developmental stage assumptions, we assign births to EITC tax year as follows: For births in the months of May – December (third trimester beginning in February through September), we assign the EITC parameters from the prior calendar year. For births in the months of January – April (third trimester beginning in October through January), we assign the EITC parameters from two calendar years ago. This timing is illustrated in Table 1, where we show our mapping from birth month into effective tax year for births in 1990 through 1992.¹²

The assumptions behind this mapping are unlikely to be precisely accurate. However, our identification strategy does not rely on high-frequency time variation. We are comfortable proceeding with these as a tractable and plausible assignment rule. To the extent that the

¹⁰ By using three months prior to birth, we assume pregnancy is a 9-month event, ignoring preterm births. We make this choice because gestation is not well measured and is missing for some state-years in our data.

¹¹ For example, the cohort exposed to the Dutch Famine in the third trimester had lower average birth weight than cohorts exposed earlier in pregnancy (Painter et al., 2005). In addition, Almond, Hoynes and Schanzenbach (2011) show that the impact of exposure to the food stamp program is greatest in the third trimester. Also see the review in Rush et al. (1980).

¹² In future work we will also examine the impact of the EITC through the maternal “labor supply channel”. In this assignment we will assume that the EITC’s labor supply impacts occur uniformly throughout the tax year. As such, infants born in April-December have had their sensitive developmental stage occur during the current calendar year, while those born in January-March have had their mother’s labor supply impacted by the prior calendar year. Additionally, the EITC credit depends on the number of children as of December 31 of the tax year (Internal Revenue Service 2011b). We assume that the mother’s work incentives anticipate her completed family size to be determined by the timing of the birth under consideration. So for births in the January-March, the EITC schedule should be based on the number of children not including the birth under consideration. For births April-December, the schedule should be based on the number of children including the birth under consideration.

assignment rule is inaccurate, this should result in some “treatment” spilling over into the last measured “control” year (or vice-versa), and this should attenuate our estimated impacts.¹³

With this timing established, we collapse the data further to cells based on effective tax year (and state, parity of birth, education, race, ethnicity, marital status and age of mother). To control for potential confounders, we add data on state by year unemployment rates, Medicaid/SCHIP income eligibility thresholds, and dummies for post welfare reform.¹⁴

5. Empirical Methods

We provide several quasi-experimental research designs beginning with a difference-in-difference analysis of the OBRA93 expansion. We choose the OBRA93 expansion because it is the largest expansion of the EITC and it generated differential expansions for different family sizes. We begin by estimating the following model:

$$(1) \quad Y_{pjtst} = \alpha + \delta \text{After}_t * \text{Parity2plus}_p + \beta X_{st} + \gamma_p + \eta_s + \delta_t + \phi_j + \varepsilon_{st}$$

where Y_{pjtst} is a measure of infant health (e.g., fraction low birth weight, average birth weight)

for the cell defined by parity p , demographic group j , in state s for effective tax year t . We

include data for effective tax years 1991–1998 and After equals one for effective tax years 1994

through 1998.¹⁵ X_{st} includes controls for unemployment rate, welfare reform and Medicaid or

SCHIP eligibility and we include fixed effects for demographic group ϕ_j , parity γ_p , state η_s and

¹³ A handful of studies examine the impact of the EITC on spending and find that the majority of the credit is spent in the first quarter (Barrow and McGranahan, 2000, Patel 2011, Smeeding et al., 2000, Gao et al 2009). Recognizing this, another approach to assigning EITC timing is to take advantage of within year variation in the treatment. We do not pursue this approach because it would be more sensitive to getting the timing right for the sensitive developmental stage and any within-year timing would be prone to spurious correlation with the systematic seasonality of births (Buckles and Hungerman 2008).

¹⁴ The state-year unemployment rates are from the Bureau of Labor Statistics (2011). The welfare reform dummy variable is equal to one if the state has implemented a waiver or passed TANF by the given year and comes from Bitler, Gelbach and Hoynes (2006). The Medicaid/SCHIP income eligibility threshold comes from Hoynes and Luttmer (forthcoming).

¹⁵ In this design, we do not include data on years prior to 1991 because of the prior expansion in OBRA90.

effective tax year δ_t . The estimates for this and all subsequent models are weighted using the number of births in the state-year-parity-demographic cell and the standard errors are clustered by state.

In our first specification, which we refer to as DD1, we compare 2nd and later births (*Parity2plus*) to first births, recalling that the EITC treatment corresponds to the number of children prior to the current birth. In this case first births are the control, because they were exposed to the relatively small childless EITC credit. To examine predictions concerning the differential expansion in the two-child vs. one-child EITC, in our DD2 specification we include *After * Parity2* and *After * Parity3plus*. Finally, in our DD3 specification, we limit the sample to 2nd and later births and include *After * Parity3plus*, thus effectively using 2nd births as a control for third or later births.

Identification in this model requires that in the absence of the EITC expansion the control group (e.g. first births for DD1) would have similar trends to the treated group (e.g., second or later births for DD1). To directly explore the validity of the design, we extend the OBRA93 analysis to an “event time” analysis. In practice, this means estimating (1) with a full set of year effects (which we already have) and year effects interacted with *Parity2plus* (with analogous models for the event time versions of DD2 and DD3). We then plot the year times *Parity2plus* interactions allowing for an examination of the pre-trends.

In our second quasi-experimental model we take advantage of the full set of tax reforms which have resulted in expansions of the EITC. We parameterize the EITC schedule using the maximum credit, which varies by effective tax year and birth order (1st, 2nd, 3rd or more). We then estimate:

$$(2) \quad Y_{pjt} = \alpha + \delta \text{Maxcredit}_{pt} + \beta X_{st} + \gamma_p + \eta_s + \delta_t + \phi_j + \varepsilon_{st}$$

Maxcredit is equal to the maximum EITC credit that the family can receive, given effective tax

year t and parity p . All other variables are defined the same as in (1) above. To implement our parametric maximum credit model, we use effective tax years 1983 –1998.

Both these designs are based on parity by year identification. In our third design, we instead use variation in the state EITC's. As shown in Figure 2, by 2010 about half the states have initiated EITC's, which are typically structured as a simple percentage of the federal credit. This state variation has been used in prior studies of the impact of the EITC on employment (Meyer and Rosenbaum 2001), poverty (Neumark and Wascher 2001) and infant health (Strully et al 2011¹⁶). We estimate:

$$(3) \quad Y_{pjtst} = \alpha + \delta Statecredit_{pst} + \beta X_{st} + \gamma_{pt} + \eta_s + \phi_j + \varepsilon_{st}$$

where *Statecredit* is equal to one plus the state percent add-on to the federal credit (which in some but not all states varies by number of children). This approach allows us to absorb parity times year fixed effects γ_{pt} . (Results for this model will be coming in a subsequent draft of the paper¹⁷.)

6. Results

6.1 OBRA 93 Treatment - Main Estimates for Low Birth Weight

We begin by presenting results for the OBRA93 difference-in-difference model using effective tax years 1991 –1998. Our main estimates are for a “high impact sample” consisting of single women with a high school education or less. This follows much of the EITC and labor supply literature which also focuses on this high impact group (e.g., Eissa and Liebman 1996, Meyer and Rosenbaum 2001). Using the March Current Population Survey and the NBER

¹⁶ Strully et al (2011) model the state EITC as a dummy variable equal to one if the state has an EITC program in place. This ignores the significant variation in the generosity across state credits (Williams et al 2010).

¹⁷ To take full advantage of this approach, we will use data through 2008 to capture the states that added EITC's in the end of the period. The public use natality data suppresses state codes after 2002, so the state analysis will use the restricted access data for the later years.

TAXSIM model, we find that for the 1998 tax year about 42 percent of single women 18-45 with a child under age 3 (our proxy for a “new births” sample) and a high school education or less are eligible for the EITC. This compares to 32 percent for single women with some college and 27 percent for married women with a high school education or less (with the same age of woman and age of child restrictions).

Results from estimating equation (1) for our high impact subsample are shown in Table 2. Each column of the table represents estimates from a separate regression where the dependent variable is the fraction low birth weight (multiplied by 100). We show only the coefficient on the treatment effect (and its clustered standard errors). The first column indicates that second parity or higher births, relative to first births, were 0.37 percentage points less likely to be low birth weight in the post-OBRA93 period (relative to the mean of 10.2 percent). Since the OBRA93 expansion was larger for families that already had two children the DD2 model (shown in column 3) decomposes how the policy impacted second births and third or higher births. The results are consistent with expectations given the larger expansion for third and higher births: Low birth weight status is reduced by roughly 0.56 percentage points for third or higher births versus 0.16 for second births (compared to first births). Table 2 also shows estimates for models where we add state controls for Medicaid expansions, welfare reform, state level unemployment rates, and mean parity within the state by year by demographic group cell (columns 2 and 4). There is little change to the coefficients from adding these additional controls.

A remaining concern is that the regression results from Table 2 could be driven by pre-existing differential trends by parity of birth. For example, if the incidence of low birth weight for higher birth orders was already declining before the OBRA93 expansion then our estimates could be biased upward. To address this concern we show results from an event study in Figure 4a. In particular, for the DD1 model, we estimate a model similar to specification (2) in Table 2

except we replace *After*Parity2plus* with a full set of year dummies interacted with *Parity2plus*. We plot the year by parity interactions in Figure 4, where we normalize the coefficients to 0 in 1993, the year prior to the OBRA93 expansion. The figure suggests there was little to no pre-trend before the expansion, validating the research design. In addition, the treatment effect grows with years since 1993 which is consistent with the phased-in expansion (see Figure 1). Figure 4b shows the event study coefficients for the DD2 model, where we plot the interactions of the year dummies with *Parity2* and *Parity3plus*. Here, as in Figure 4a, the pre-trend is quite flat. We include on Figure 4b the maximum EITC credit (in 1999 dollars) by year for second births and third or higher births (relative to first births). By including this measure of the relative expansions in the credit, we can see that the magnitudes of the event study coefficients across group (2nd versus 3rd or higher births) and across years are quite consistent with the law changes. In particular, the treatment effects are larger for the third and later births than for second births and the treatment effects increase with time since 1993. One limitation of these results is due to the earlier OBRA90 expansion in the EITC, we limit the sample to three years of pre-trends.¹⁸

The DD3 model, which uses second births as the control group rather than first births, offers an additional examination of the effects of increased income through the EITC. Recall in DD3 we compare third and higher order births to second births, taking advantage of the differential expansions for two or more children (Figure 1). In addition, because this differential expansion was not part of the earlier 1990 EITC expansion, for the event study we can utilize data for more pre-years. Figure 4c shows the event study of the DD3 model, where we estimate it using data for effective tax years 1987-1998. The results are very encouraging: The relatively long pre period shows no confounding pre-trend accompanied by a sharp decline in the incidence of low birth weight births corresponding to the increase in eligibility for maximum EITC

¹⁸ In our panel fixed effects model below we take advantage of the full set of credit expansions.

benefits. As with Figure 4b, we include the maximum EITC credit (in 1999 dollars) by year for third and higher births (relative to second births) and see that the magnitude and timing of the birth weight changes line up well with the timing of the EITC expansion.

Low birth weight (less than 2,500 grams) is a standard outcome for infant health. However, for our purposes it is rather arbitrary. To explore more fully the impacts of the EITC expansion on the distribution of birth weight, following Almond et al (2011) we estimated a series of difference-in-difference models for the probability that birth weight is below a given gram threshold: 1,500; 2,000; 2,500; 3,000; 3,500; and 4,000. We plot the estimates and their 95 confidence intervals for DD1 (Figure 5a) and DD3 (Figure 5b). For each gram threshold, we divide the estimate by the mean for that outcome (generating the percent effects in the graphs). The results generally show larger effects at the lower end of the birth weight distribution, which very small effects at the top. For DD1, the effects on very low birth weight are not statistically significant.

Table 3 shows heterogeneity in effects by race and Hispanic origin within the high impact sample. The EITC reduced the likelihood of having a low birth weight birth for black mothers of 0.74 percentage points (relative to a mean of 14.4 percent), more than four times higher than the effect on white mothers (0.169 percentage point decile relative to a mean of 8.1 percent). This reflects the fact that black mothers in comparison to white mothers (in the high impact sample) have on average lower income and therefore more likely to receive larger EITC benefits. We return this below when we discuss the magnitudes of these findings. Interestingly, smaller treatment effects are experienced by Hispanic mothers than non-Hispanic mothers (-0.18 versus -0.42 in the DD1 model). Perhaps this is because Hispanic children tend have better birth outcomes as it is (7 percent of Hispanic births in the high impact are low birth weight compared to 11.2 percent of non-Hispanic births), so there could be less room for improvement. A larger

fraction of Hispanics are undocumented immigrants who do not qualify for the EITC which could also attenuate the estimates (Internal Revenue Service 2011b).

To interpret these results it is helpful to know the average increase in dollars of income accruing to EITC eligible mothers (due to OBRA93). Because neither income nor EITC benefits is reported in the vital statistics it is impossible to use our estimation sample to directly quantify changes in EITC. The CPS has earnings and income but not EITC benefits; however, using the CPS combined with the NBER TAXSIM model it is possible to impute the average change in dollars of EITC eligibility.¹⁹ These results, along with the effects on infant health, are shown in Table 4. Panel A presents results corresponding to our DD1 model (2nd births and higher relative to first births). The first row transcribes from Tables 2 and 3 of the estimated impacts of the EITC expansion on low birth weight for all mothers, and for Black and White mothers. The second row shows estimates of the impact of the expansion on EITC dollars received by each of these groups (relative to first births). These are calculated by a simple pre-post treated/untreated difference in difference estimate with the outcome variable being the TAXSIM calculation for predicted EITC.²⁰ This row shows that among our high impact sample of single mothers with 12 or fewer years of schooling, Black mothers received the largest increase in average predicted EITC income from OBRA93 (\$624 in 2009 dollars compared to \$471 for white mothers).

The third row of Table 4 presents the “impact of \$1000 treatment on the treated” (TOT) estimate obtained by dividing the first row by the second row and then multiplying by 1000.

¹⁹ In particular, we use the March CPS for 1992-1999 (corresponding the tax years 1991-1998). To construct a sample resembling the “new births” natality sample, we limit the sample to women 18-45 with a child age less than three. We use the woman’s marital status and household income and earnings to calculate taxes. We assign dependents to be the number of children in the household minus one (e.g. the child<3 is the “new birth” and hence not counted in our tax calculation to mimic the treatment assignment in the natality data). Providing this information, TAXSIM returns the federal EITC that the observation is eligible for.

²⁰ We take our CPS sample which proxies a “new births” sample (describe above) and regress the real EITC imputed benefit on dummies for *Parity2plus* (for DD1) or *Parity2* and *Parity3plus* (for DD2), *After*, and interactions of parity and after. Exactly replicating equation (1) (adding state and year fixed effects, demographic dummies and state-year controls) has little impact on the estimates.

This IV-type interpretation suggests what the impact of EITC income would be, under the assumption that it were the only mechanism through which the EITC impacted birth outcomes (and take-up of EITC was 100 percent²¹). We view this as an overly restrictive assumption, but still think that the numbers offer a useful scaling of the coefficients. The fourth row presents the percent impact of the \$1000 treatment on the treated by dividing the third row by the group mean. Finally, Panel B of Table 4 presents similar results for our separate comparisons of 2nd and 3rd and higher order births against 1st children (our DD2 model).

The results in Table 4 show that for our high impact sample a \$1000 EITC TOT (2009 \$) leads to a 0.71 percentage point reduction in percent low birth weight, a 7 percent effect. The corresponding \$1000 TOT effect for whites is -0.36 percentage points or a 4.4 percent decline and for blacks a TOT effect of -1.19 percentage points or an 8.2 percent decile. Almond et al (2011) find that exposure to the food stamp program in the third trimester leads to an 8 percent TOT effect on low birth weight for whites and a 9 percent impact for blacks. The food stamp treatment (annualized) is somewhat larger, at \$2150 (2005\$) (Hoynes and Schanzenbach 2009).

Panel B in Table 4 shows the EITC treatment for 2nd births is smaller than 3rd or later births (\$373 versus \$667). Interestingly, the scaled estimates (TOT) are also smaller for the 2nd births than they are for the 3rd births. Table 4 thus helps us to understand why the coefficients in Tables 2 and 3 are smaller for 2nd than for 3rd births: it is both because the 2nd births received a smaller treatment, and also because their outcomes appear to be less sensitive to the treatment. Table 4 further shows that this effect is most pronounced for Black mothers. For White mothers, it appears that the differences are solely due to the size of the differences in the EITC treatment.

²¹ Scholz (1994) analyzes tax year 1990 and estimates take-up rates between 80-86%. Internal Revenue Service (2002) analyzes 1996 tax year and estimates range from 82 to 87 percent. The IRS study finds lower take-up rates for childless filers and Hispanics.

6.2 Impacts of OBRA93 across subgroups

To obtain more insight into these effects, we have estimated the results on subgroups of the data. For this analysis we use the full sample (that is, no longer condition on being in the high impact sample). Within the full sample we estimate models on the following subgroups: education categories (<12, =12, 12+); race (White, Black); ethnicity (Hispanic, Non-Hispanic); marital status (Single, Married); age group (18-24, 25-34, 35+); and (for continuity) the high impact sample. For each of these subgroups we estimate the impacts of the EITC expansion on probability of low birth weight using the specification in column (2) of Table 2, and we also estimate the difference-in-difference impact on EITC income (as in row 2 of Table 4) using the CPS sample.²²

Figure 6 presents results from this exercise. The x-axis shows the impact on EITC income and the y-axis shows the estimated impact on low birth weight. For example, the key result from Table 4 (“all high impact mothers”) is presented as a dot at (x = \$521, y = -0.369). The size of the dots represents the number of births for this subgroup. These scatterplots show a strong relationship between the magnitude of EITC treatment and impacts on low birth weight for DD1 (Figure 6a) and DD3 (Figure 6b). Subgroups with large estimated impacts on low birth weight are also those with large impacts on the EITC income while subgroups with smaller increases in EITC income also have smaller impacts on low birth weight.

One drawback of the analysis in Figure 6 is that the subgroups are overlapping. As an alternative, we take the full sample and divide it into 10 “deciles of predicted EITC treatment.” To do this we take the CPS 1997-2001 (tax year 1996-2000) and select women ages 18-45 with

²² For completeness we also estimated outcomes for subgroups missing education and Hispanic ethnicity though there were only a handful of these missing values. We do not report these below.

two or more children (at least one less than 6).²³ We use the woman’s characteristics to impute the EITC amount using TAXSIM (as above). We then regress the predicted EITC on state fixed effects and full set of interactions of education group (<12, 12, 12+), race (white, black, other), marital status (married, single) and age (18-24, 25-34, 35+).²⁴ We then apply these parameter estimates to the natality sample to obtain predicted EITC income. Using the predicted EITC we assign each demographic group-state cell to an “EITC decile” (1 being least impacted and 10 being most impacted).²⁵ We then estimate the difference-in-difference models for low birth weight and the EITC treatment for each decile (as we did for each demographic subgroup). We present the results for this analysis in Figure 7. The parameter estimates (and their 95 percent confidence intervals) are shown in blue circles (left y-axis scale) and the EITC treatment for each decile is shown in red diamonds (right y-axis scale). The results for DD1 (Figure 7a) shows the treatment effect grows with the EITC treatment, with significant impacts for the top 3 deciles. For DD3 (Figure 7b) the effects are significant for the 7th and highest decile.

6.3 Mean birth weight as dependent variable

Our analysis so far has focused on low birth weight (along with the one additional presentation of the distributional impacts). Many studies also examine mean birth weight, and we do so here for our high impact sample in Table 5. We estimate that for all high impact mothers the EITC expansion led to an increase in mean birth weight of 9 grams. The impacts were larger for 3rd and higher parities (14 grams), for Black mothers (18 grams), and for Non-Hispanic mothers (11 grams). These results are consistent with those found for low birth weight.

²³ This CPS sample is slightly different from the one described above. To assign predicted EITC we want a stable EITC schedule (not varying in real terms across years or with number of children) thus years 1996-2000 (after OBRA93 is fully phased in) and women with two or more children.

²⁴ We do not use Hispanic status because it is missing for some states in the Natality sample.

²⁵ The deciles are assigned taking account of the number of births in each cell. Note that, by construction, the EITC decile is fixed for the education-marital status-race-age group across all years and across all parities.

Figure 8a shows the event study corresponding to Column 1 of Table 5. The line suggests a possible pre-existing trend, and so raises concerns about the identification assumption needed for that result. Figure 8b splits this out by parity. Here we see (solid lines) that there is a modest pre-trend in mean birth weight for both parity groups, but that the pre-trend appears to be similar for both groups. As such, for mean birth weight, we feel more confident on our DD3 model, which uses parity 2 births as a control group for parity 3+ births. This figure also shows that the EITC receipt for each of these groups was similar (and low) prior to the OBRA 93 expansion. After the expansion 2nd children received a small increase, while 3rd and higher parities received a large increase. Figure 8c presents the event study graph from our DD3 model. The pre-trends appear to be flat, and there is a jump in both EITC income (the treatment) and in mean birth weight (the outcome) following the expansion.

Table 6 presents our DD1, DD2 and DD3 results along with the EITC income results, so as to be able to gauge the magnitudes of our estimated impacts. For our basic DD1 model, we find that an increase of \$1000 of EITC income (TOT) is associated with an increase in mean birth weight of 19 grams for a 0.6 percent effect. This TOT percent effect is significantly smaller than the 7 percent impact for low birth weight. This is consistent with other studies finding larger impacts in the lower tail of the birth weight distribution (e.g. Almond et al 2011). Again, this impact is larger for Black mothers (\$1000 TOT effect is 29 grams or a 1 percentage point increase) than for White mothers (\$1000 TOT effect is 12 grams or a 0.4 percentage point increase), and larger for 3rd births (21 grams or 0.7 percent) than for 2nd births (15 grams or 0.5 percent)

6.4 Summary of OBRA 93 results

So far we have been focusing on difference in difference estimates using the OBRA93

expansion of the EITC. This has enabled us to use a familiar and clear source of identification, to easily explore distributional impacts and impacts on subgroups, and to easily test some of the needed identification assumptions.

We find that the OBRA93 expansions are associated with decreases in low birth weight and increases in average birth weight. This is true for two different identification strategies (DD1, which compares 2nd and higher parities to 1st born children; and DD3, which compares 3rd and higher parities to 2nd born children). When we examine impacts by subgroup, we find that the groups who have largest improvements in child health have greater increases in EITC income (3rd and higher births), greater socioeconomic risk, or both (Black mothers). In addition, when we examine groups (e.g., high education mothers) for whom we expect (and find) little EITC income impact, we see insignificant and small negative effects (for some college) or positive and sometimes significant effects (for college graduates) on the low birth weight outcomes. This provides a “falsification test,” which is satisfied in our data.

One key limitation from this analysis is the maintained assumption that counterfactual differences in birth outcomes across parity groups would be constant over time. When considering average birth weight using the DD1 design, there is some evidence of a preexisting trend in differences across parity groups – which call this assumption into question for that particular analysis. However, for our low birth weight analysis (for both the DD1 and DD3 designs), and for our mean birth weight analysis using the DD3 design, the preexisting trends are flat. On the whole, we view these results as fairly robust and provide strong evidence that we are indeed identifying impacts of the EITC.

6.5 Panel FE estimates using multiple EITC expansions

We extend our analysis of the EITC by looking at a larger number of years and the three

different expansions. In particular, we use natality data spanning effective tax years 1984-1998 encompassing the expansions in 1986, 1990 and 1993. To parameterize the generosity of the EITC we use the maximum credit (in 1000s of 1995\$), which varies by tax year and parity. We estimate equation (2) which is weighted using number of births in a cell and standard errors are clustered on state.

Table 7 shows results from estimating the model on our high impact sample, single women with a high school education and for white and black subsets of the high impact sample. Because we are looking at a longer time span, with multiple EITC expansions, we can explore the sensitivity of the results to the inclusion of parity times year linear trends. The results for the full high impact sample show that a \$1000 dollar (1995\$) increase in the maximum credit leads to a 0.3 percentage point decline in the percent low birth weight. The greater number of years and expansions increases the precision of our estimates. Adding parity linear time trends increases that estimate substantially to more than 0.8 percentage points.

The results in Table 7 are not directly comparable to the magnitudes for difference-in-difference results (provided above in Table 4) because here we are using the maximum benefit program parameter. To compare these results to the DD results we return to the CPS data linked with TAXSIM and estimate a “first stage” model where we regress predicted EITC income on the maximum credit (along with controls for year, parity, demographics). The coefficient on that first stage regression is provided in the bottom of Table 7. For the full high impact sample, the point estimate suggests that a \$1000 increase in maximum credit leads to a \$330 increase in EITC income, reflecting the fact that a fraction of the sample is not eligible, or are eligible for an amount smaller than the maximum benefit. We use this to construct an IV-type estimate of the impact of \$1000 of EITC income (not max credit) on the percent low birth weight, as well as the percent impact. This is comparable to our DD estimates above.

Without trends our estimates are similar to, but slightly lower than, the difference-in-difference estimates: a \$1000 of EITC expansion reduces the incidence of low birth weight by 5.6% for the full high impact sample (compared to 7% for the DD), 3.4% for whites (4.4% in the DD), and 7.2% for blacks (8.2% for the DD). However, when we include trends the impacts of the EITC increase to a 16% reduction in low birth weight for low educated single mothers of all races and a 21% for low educated single black mothers. To better understand why our estimates are sensitive to these trends, in future work we will extend our event study through the full time period.

We also performed our decile analysis using the maximum credit panel fixed effect model. This is based on the full natality sample where deciles are constructed using the same method as described above for the DD models. Here, however, we drop states missing education data.²⁶ These estimates are shown in Figure 9. The blue circles provide estimates (and 95% confidence intervals) for estimates on the maximum credit in the panel fixed effects regression, by predicted EITC decile. The results show no impact of the EITC in the first five deciles, as would be expected if these groups had little/no exposure to the EITC. Starting with the fifth decile we begin to see increasingly larger effects. The effects in the ninth and tenth deciles seem to be larger than the 6th and 8th decile, though the size of the confidence intervals means the difference is not statistically significant.

6.6 Threats to identification: Endogenous Births

As discussed above, expansions in the EITC may lead to changes in the composition of births due to labor supply incentives and fertility. If, for example, expansions lead to an increase

²⁶ There is incomplete reporting of education for California, New York, Texas, and Washington so we drop these states entirely from this analysis (to balance the state panel). This was not an issue for the estimates in Table 7 because we simply include dummies for missing education as another demographic cell. However, when we assign EITC deciles we need education (e.g. there are no missing education cells in the CPS). This was not an issue in the OBRA93 analysis because all states report education by 1992.

in the births to more disadvantaged women, then our estimates will be biased downward. To explore this we apply the same identification strategy that we applied above to infant health and examine the impacts on the number and composition of births. Table 8 presents the results of the OBRA difference in difference analysis where the dependent variable is the log of the number of births in the cell. We provide estimates of DD1 (comparing 2nd and higher order births to 1st births) and DD3 (comparing 3rd and higher order births to 2nd births). Consistent with the existing literature, the results show small and statistically insignificant impacts on overall fertility. In Table 9, we go on to examine the impact of the EITC expansion on the composition of births by running a series of models with the dependent variable equal to the share of births in the cell that are born to women in a given demographic group. We examine impacts on the race, education, and age of the mother. Note that these are all characteristics that we control for in our main regressions above. So changes in these variables are not a threat to our design. But if treatment is related to these observables we might be concerned about unobservables. The table shows evidence of some small, but statistically significant effects of the treatment on the demographic characteristics. Interestingly, there is an inconsistent pattern across DD1 and DD3 (DD1 correlated with more births to disadvantaged mothers and DD3 correlated with fewer disadvantaged births). We are encouraged by the fact that, as shown in event study version of these results in Figure 10, the changes in the demographics are changing smoothly through the 1993 expansion while our main results (Figure 4) change sharply with the expansion in 1993. Given this, we do not see endogenous fertility to be a concern for the interpretation of our results.

7. Conclusion

This paper evaluates the health impact of a central piece in the U.S. safety net for families

with children: the Earned Income Tax Credit. Using tax-reform induced variation in the federal EITC and the presence and generosity of state EITC's, we examine the impact of the credit on infant health outcomes. We find that increased EITC income reduces the incidence of low birth weight and increases mean birth weight. For single low education (≤ 12 years) mothers, a policy-induced treatment on the treated increase of \$1000 in EITC income is associated with a 7% reduction in the low birth weight rate. These impacts are evident with difference-in-difference models and event study analyses. We conclude that the sizeable increase in income for eligible families significantly improved birth outcomes for both whites and African Americans, with larger impacts for births to African American mothers. Our results suggest that there are non-trivial health impacts of the EITC. Importantly, these impacts are typically not taken into account given the non-health nature of the program. The results also speak to the debate as to whether income affects health, by providing an estimate of relatively large and exogenous income on infant health.

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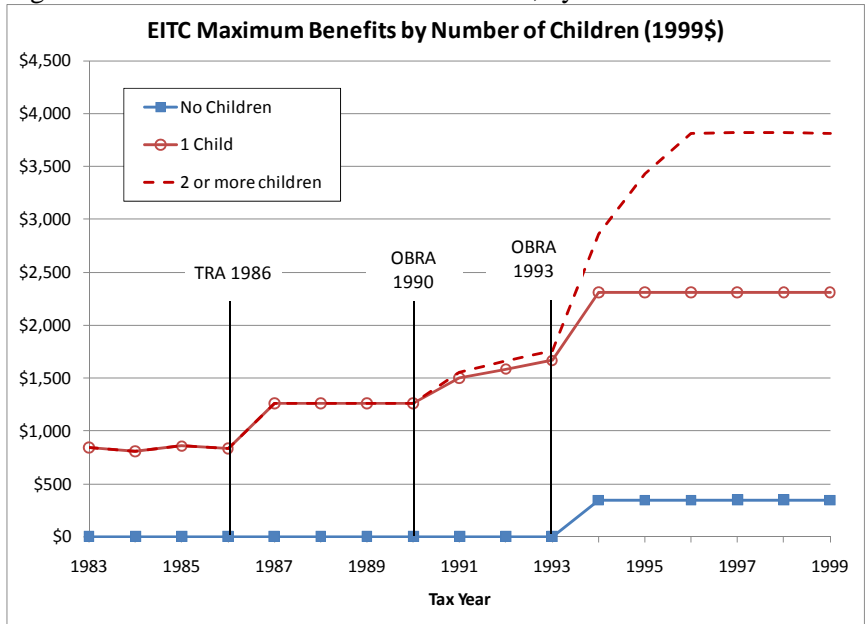
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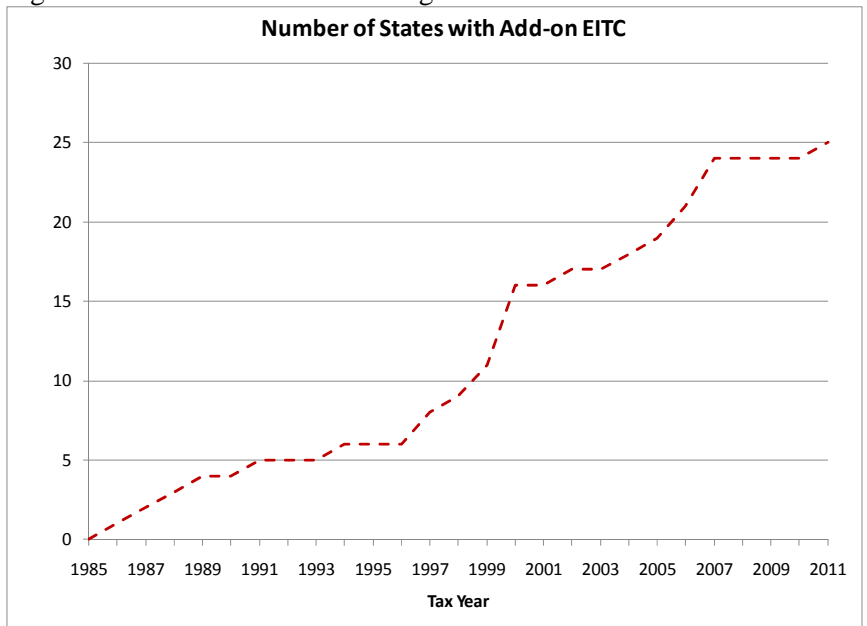
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Figure 1: Maximum Credit for Federal EITC, by Tax Year and Number of Children



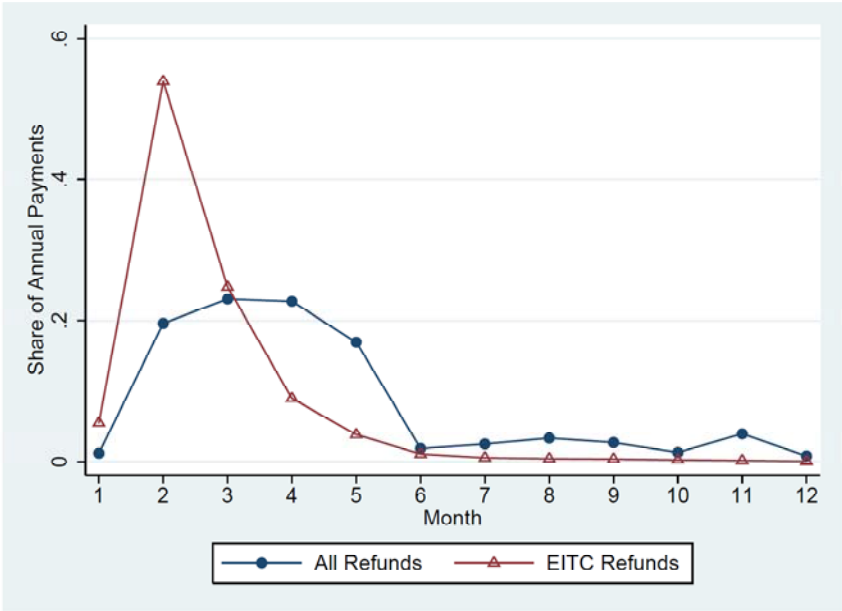
Source: Tax Policy Center (2011).

Figure 2: Number of States Offering State EITC



Source: Hatcher Group (2011).

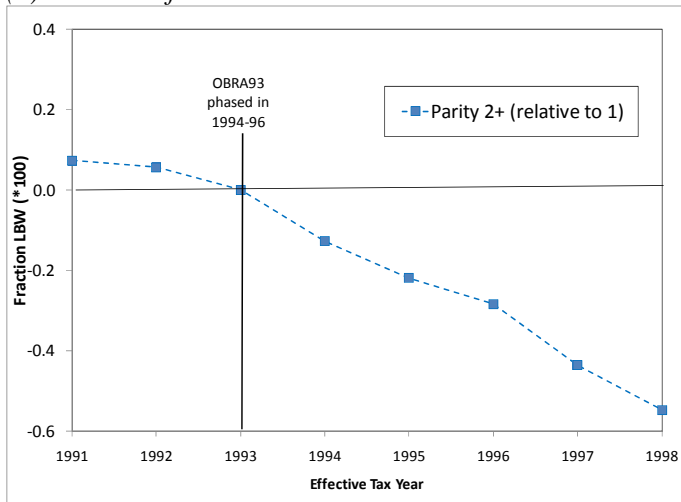
Figure 3: Distribution of Tax Refunds, by Calendar Month



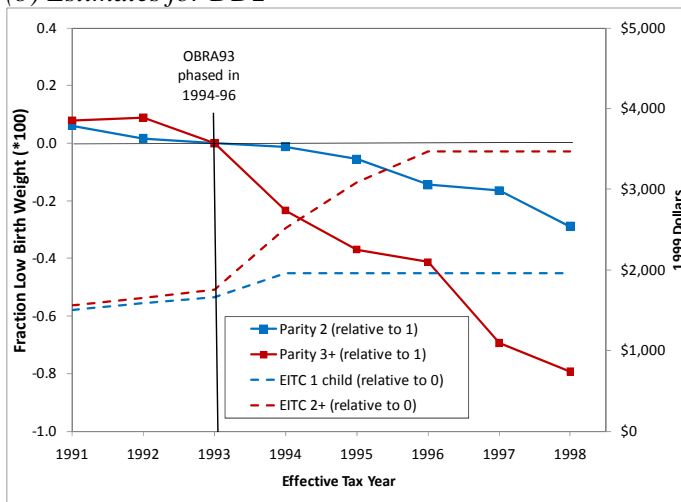
Note: Reprinted from Lalumia (2011). Figure shows 10 years averages using Monthly Treasury Statements 1998-2007.

Figure 4: Event Time Estimates of OBRA93 on Low Birth Weight and EITC Income, Single Women with a High School Education or Less

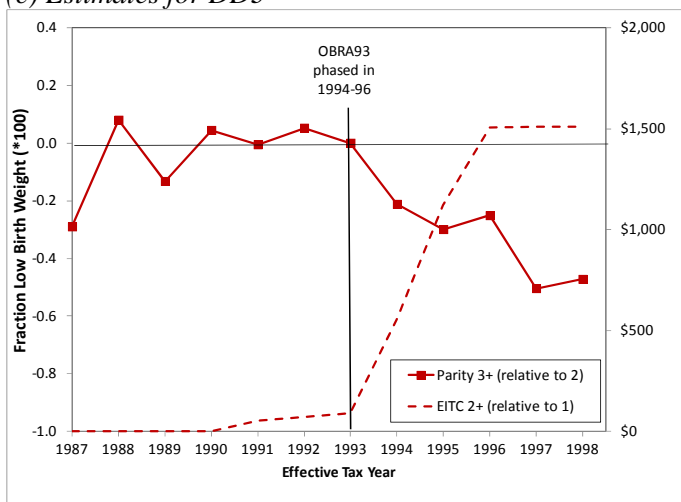
(a) Estimates for DD1



(b) Estimates for DD2



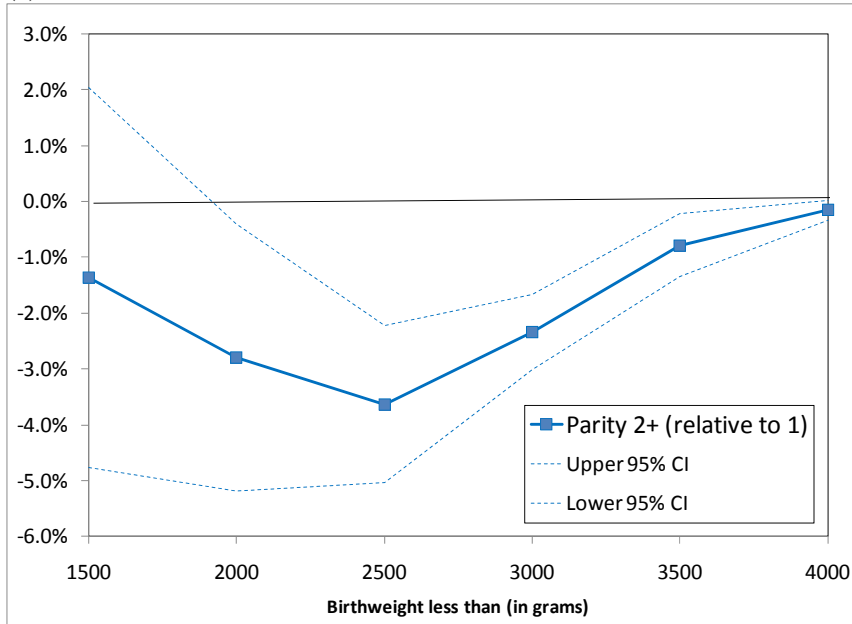
(c) Estimates for DD3



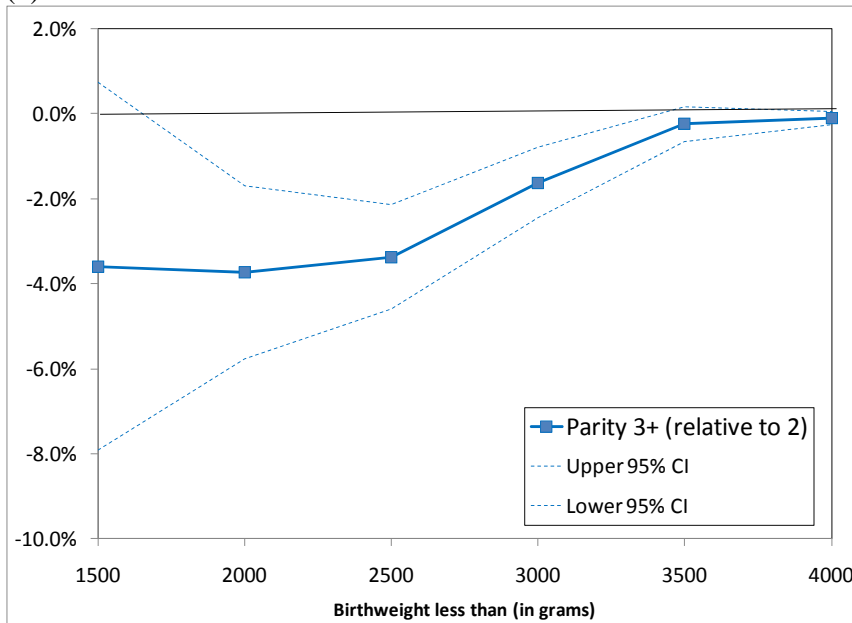
Notes: Each figure plots coefficients from an event-study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. higher order parity relative to lower order parity). The specification includes fixed effects for year, state, parity, demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. In panels (b) and (c) the figure provides DD estimates for low birth weight and predicted EITC income. Estimates for EITC income and based on the March CPS and the EITC is calculated using TAXSIM. See text for details.

Figure 5: Difference-in-Difference Estimates of OBRA93 on the Distribution of Birth Weight, Single Women with a High School Education or Less

(a) Estimates for DD1



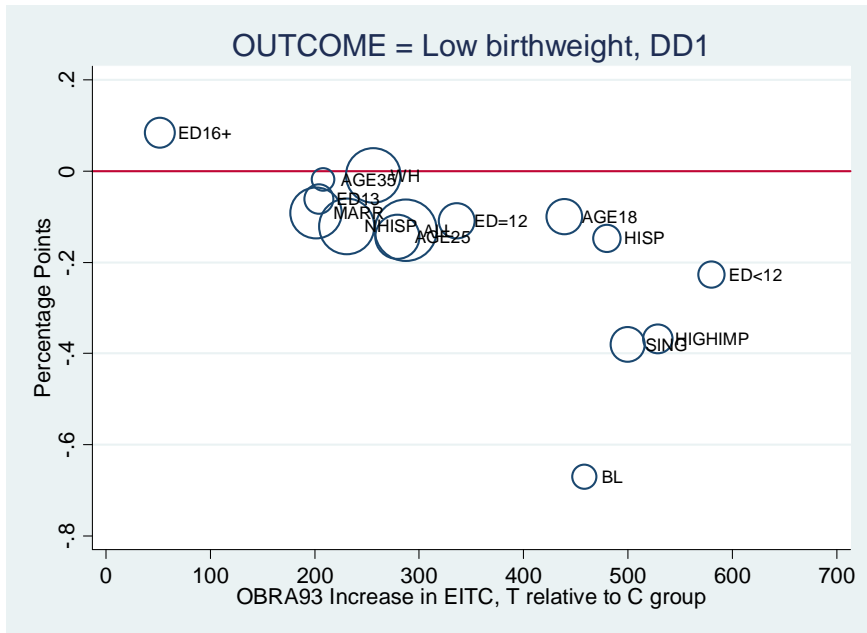
(b) Estimates for DD3



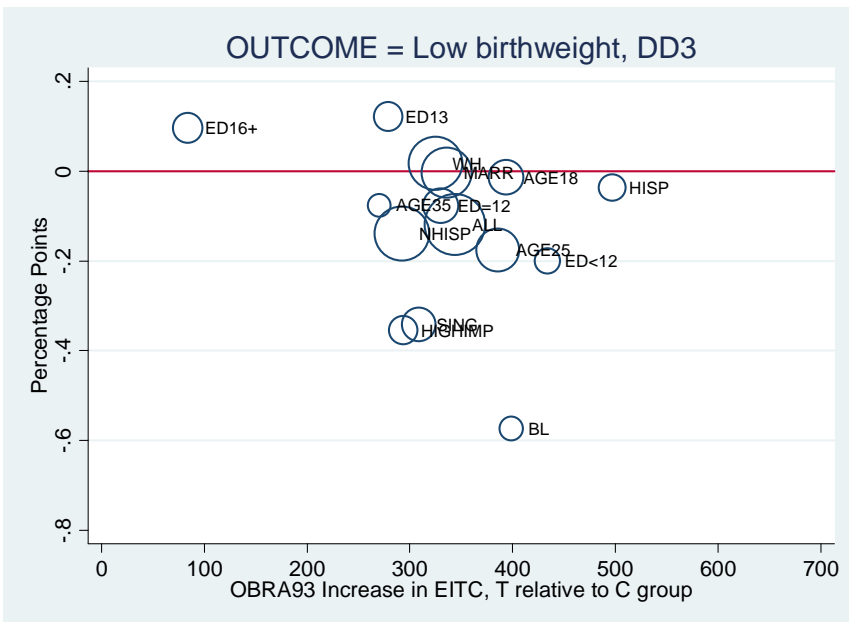
Notes: The graph shows estimates and 95 percent confidence intervals for the difference-in-difference estimate of the impact of EITC on the fraction of births that is below each specified number of grams. The specification is given by column (2) in Table 2.

Figure 6: Demographic Subgroup Estimates of OBRA93 and Magnitude of EITC Treatment

(a) Estimates for DD1



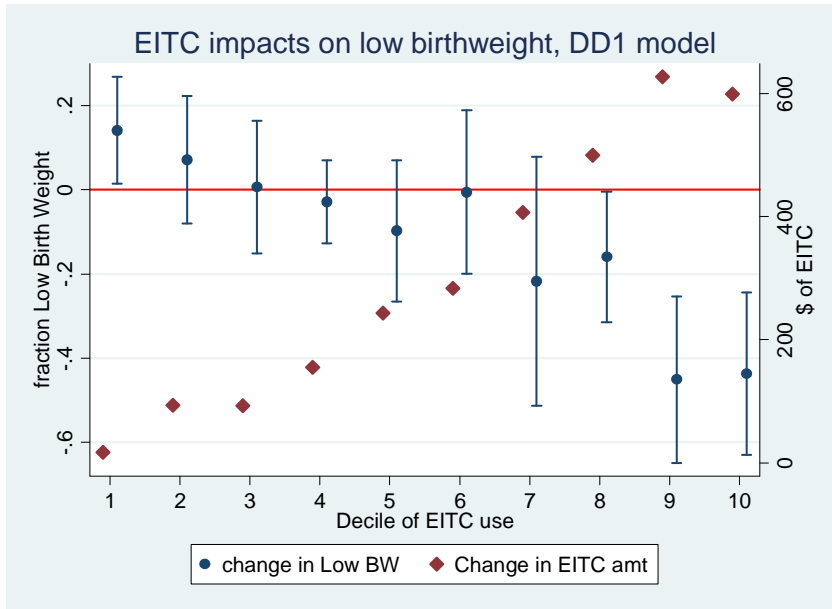
(b) Estimates for DD3



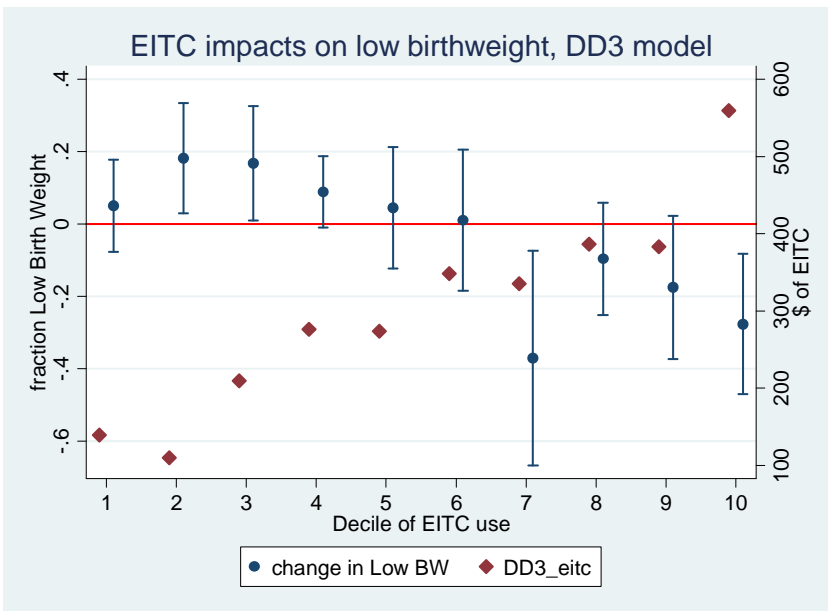
Notes: Each point on the graph represents DD regression estimates for a specified demographic subgroup. The x-axis provides the DD estimate of EITC income (using the CPS/TAXSIM sample) and the y-axis provides the DD estimate on LBW. The size of the points reflects group's cell size.

Figure 7: Difference-in-Difference Estimates of OBRA93, by Decile of Predicted EITC Treatment

(a) Estimates for DD1



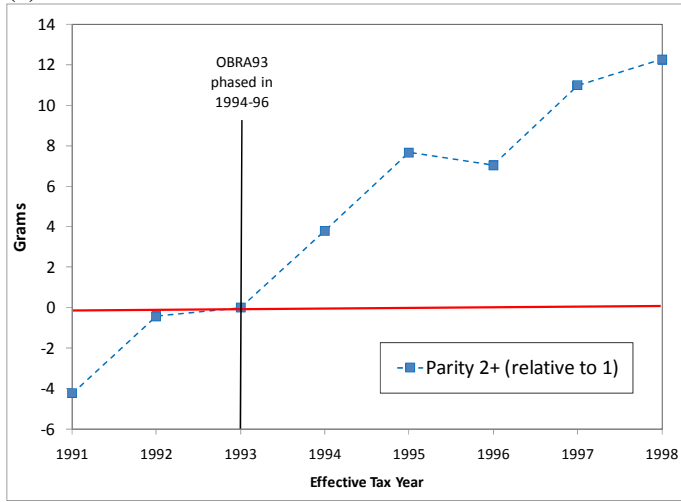
(b) Estimates for DD3



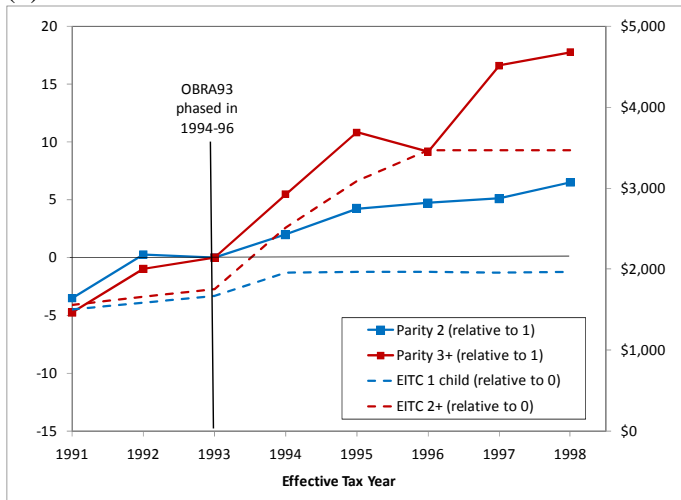
Notes: Each point on the graph represents DD regression estimates for 10 deciles of predicted EITC use. EITC deciles are assigned based on demographics and state using a prediction model estimated using the March CPS combined with TAXSIM. By construction the deciles do not vary by year or parity. The blue circles provide the DD estimate of LBW (and the 95% confidence interval) and the red diamonds provide the DD estimate on EITC income using the CPS/TAXSIM sample.

Figure 8: Event Time Estimates of OBRA93 on Mean Birth Weight, Single Women with a High School Education or Less

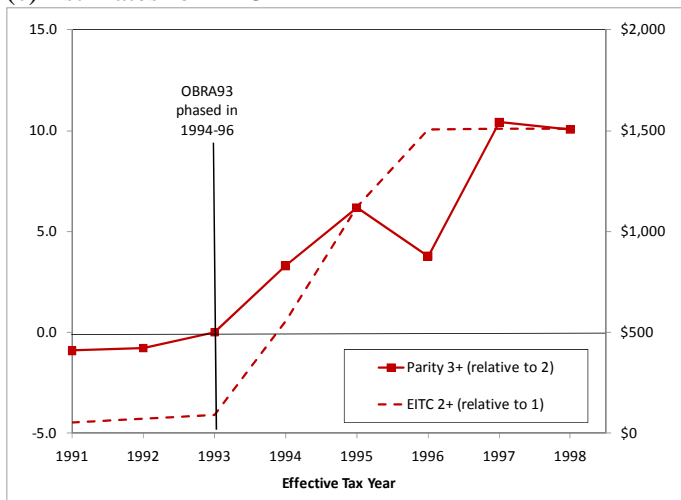
(a) Estimates for DD1



(b) Estimates for DD2

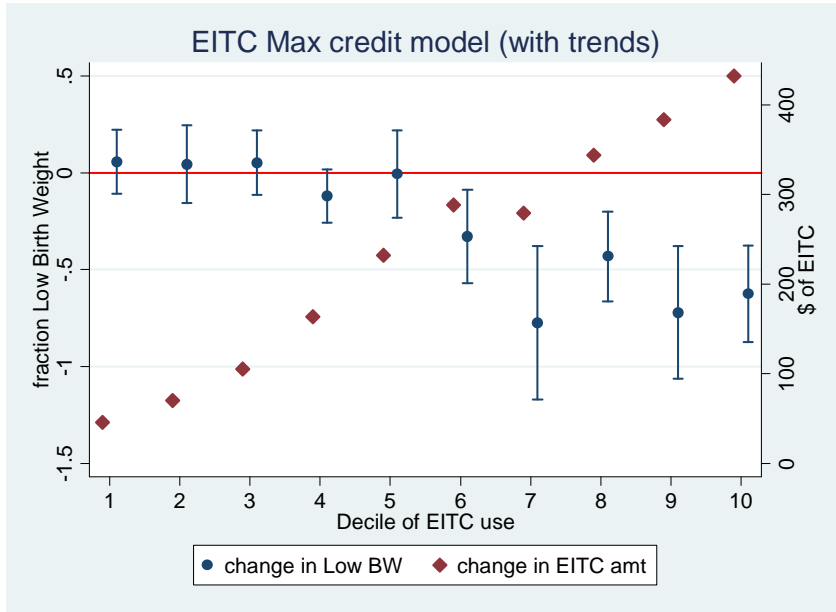


(c) Estimates for DD3



Notes: Each figure plots coefficients from an event-study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. higher order parity relative to lower order parity). The specification includes fixed effects for year, state, parity, demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. In panels (b) and (c) the figure provides DD estimates for average birth weight and predicted EITC income. Estimates for EITC income and based on the March CPS and the EITC is calculated using TAXSIM. See text for details.

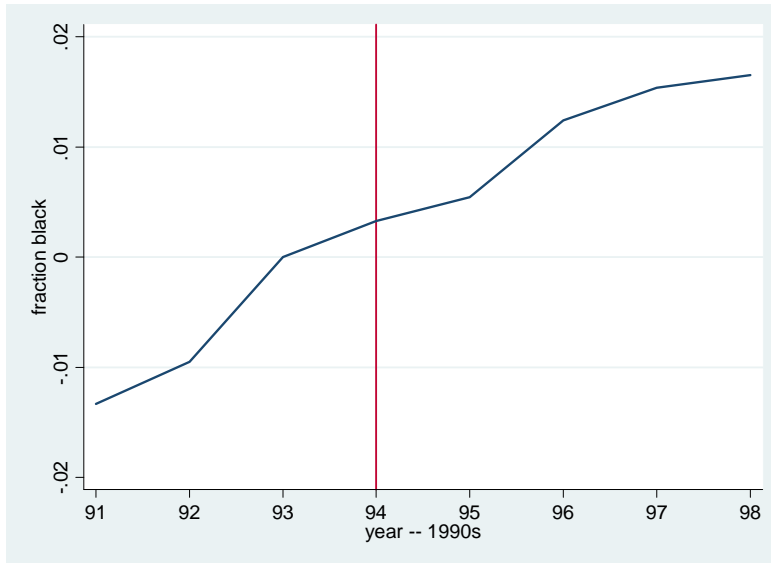
Figure 9: Maximum Credit Estimates of EITC on Low Birth weight, by Decile of Predicted EITC Treatment



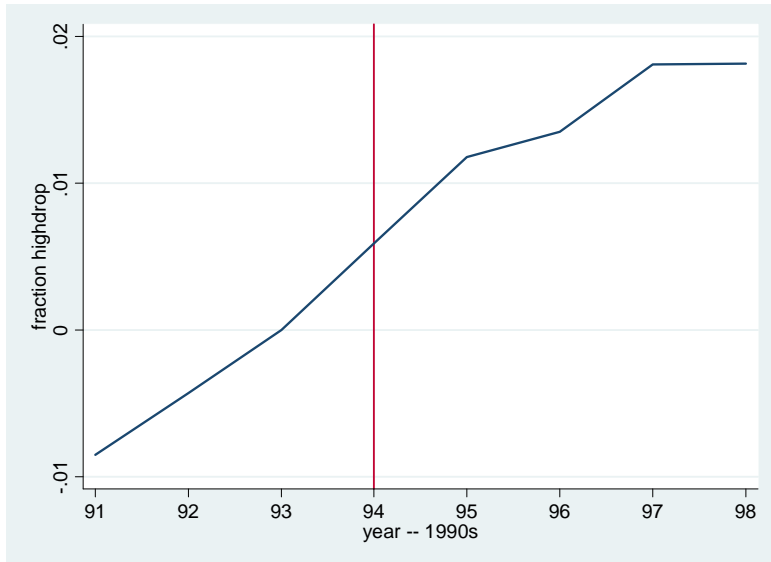
Notes: Each point on the graph represents a panel fixed effects estimate of the impact of the EITC maximum credit for 10 deciles of predicted EITC use. EITC deciles are assigned based on demographics and state using a prediction model estimated using the March CPS combined with TAXSIM. By construction the deciles do not vary by year or parity. The blue circles provide the estimate for LBW (and the 95% confidence interval) and the red diamonds provide the DD estimate on EITC income using the CPS/TAXSIM sample (from Figure 7).

Figure 10: Event Time Estimates of OBRA93 on Maternal Education and Race

(a) Outcome = share of births in cell to African American women



(b) Outcome = share of births in cell to women with <12 years of education



Notes: Each figure plots coefficients from an event-study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. higher order parity relative to lower order parity). The specification includes fixed effects for year, state, and parity, and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. See text for details.

Table 1: Illustration of Cash-in-Hand Assignment of Effective Tax Year

Birth month and year		Beginning of 3rd trimester		Effective tax year
Month	Year	Month	Year	
January	1990	10	1989	1988
Feburary	1990	11	1989	1988
March	1990	12	1989	1988
April	1990	1	1990	1988
May	1990	2	1990	1989
June	1990	3	1990	1989
July	1990	4	1990	1989
August	1990	5	1990	1989
Sept	1990	6	1990	1989
October	1990	7	1990	1989
November	1990	8	1990	1989
December	1990	9	1990	1989
January	1991	10	1990	1989
Feburary	1991	11	1990	1989
March	1991	12	1990	1989
April	1991	1	1991	1989
May	1991	2	1991	1990
June	1991	3	1991	1990
July	1991	4	1991	1990
August	1991	5	1991	1990
Sept	1991	6	1991	1990
October	1991	7	1991	1990
November	1991	8	1991	1990
December	1991	9	1991	1990
January	1992	10	1991	1990
Feburary	1992	11	1991	1990
March	1992	12	1991	1990
April	1992	1	1992	1990
May	1992	2	1992	1991
June	1992	3	1992	1991
July	1992	4	1992	1991
August	1992	5	1992	1991
Sept	1992	6	1992	1991
October	1992	7	1992	1991
November	1992	8	1992	1991
December	1992	9	1992	1991

Table 2: Difference-in-Difference Estimates of OBRA93 on Low Birth weight, Single Women with a High School Education or Less (Single Women with 12 years of education or less)

Parity2+ * After	-0.368***	-0.369***		
	(0.0744)	(0.0729)		
Parity=2 * After			-0.162**	-0.160**
			(0.0714)	(0.0713)
Parity3+ * After			-0.556***	-0.561***
			(0.0915)	(0.0875)
State x year control variables		X		X
Mean of the dep. variable	10.2	10.2	10.2	10.2
N	37,639	37,639	37,639	37,639

Notes: Each column is from a separate DD regression of the percent low birth applied to Natality data for effective tax years 1991-1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, demographic group. Columns (2) and (4) add state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses.

Table 3: Difference-in-Difference Estimates of OBRA93 on Low Birth Weight, Single Women with a High School Education or Less by Race and Ethnicity

	Race of mother				Hispanic Origin of Mother			
	White	Black	White	Black	Non-Hispanic	Hispanic	Non-Hispanic	Hispanic
Parity2+ * After	-0.169**	-0.741***			-0.422***	-0.182**		
	(0.0744)	(0.142)			(0.0979)	(0.0690)		
Parity2 * After			-0.111*	-0.308**			-0.182**	-0.0581
			(0.0643)	(0.144)			(0.0899)	(0.0767)
Parity3+ * After			-0.231**	-1.065***			-0.647***	-0.288**
			(0.102)	(0.159)			(0.121)	(0.0893)
State x year control variables	X	X	X	X	X	X	X	X
Mean of the dep. variable	8.14	14.43	14.43	10.16	11.24	7.04	11.24	7.04
N	16,870	10,938	16,870	10,938	19,864	11,969	19,864	11,969

Notes: Each column is from a separate DD regression of the percent low birth applied to Natality data for effective tax years 1991-1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses.

Table 4: Magnitudes in OBRA93 Models, Low Birth Weight

	All	White	Black
A. PARITY 2+ vs. PARITY 1			
DD Parameter estimate	-0.369	-0.169	-0.741
DD Increase in EITC (2009\$)	\$521	\$471	\$624
Treatment on Treated per \$1000 (2009\$)	-0.71	-0.36	-1.19
ToTper \$1000 (2009\$), % impact	-6.97%	-4.41%	-8.23%
B. PARITY=2, PARITY 3+ vs PARITY 1			
PARITY=2 DD Parameter estimate	-0.160	-0.111	-0.308
PARITY=2 DD Increase in EITC (2009\$)	\$373	\$335	\$445
Treatment on Treated per \$1000 (2009\$)	-0.43	-0.33	-0.69
ToTper \$1000 (2009\$), % impact	-4.22%	-4.07%	-4.80%
PARITY=3+ DD Parameter estimate	-0.561	-0.231	-1.065
PARITY=3+ DD Increase in EITC (2009\$)	\$667	\$615	\$749
Treatment on Treated per \$1000 (2009\$)	-0.84	-0.38	-1.42
ToTper \$1000 (2009\$), % impact	-8.28%	-4.61%	-9.85%
Mean of the dependent variable	10.16	8.14	14.43

Notes: Each column in each panel provides estimates for a separate DD regression. In each panel, the first row repeats LBW parameter estimates from Tables 2 and 3. The second row provides the DD estimate on EITC income from the CPS/TAXSIM data. Row 3 is the treatment on the treated estimate of a \$1000 increase in EITC income (row 1 / row 2 * 1000). Row 4 provides the percent TOT impact (row 3 / mean).

Table 5: Difference-in-Difference Estimates of OBRA93 on Mean Birth Weight, Single Women with a High School Education or Less by Race and Ethnicity

	All	Race of Mother		Ethnicity of Mother	
		White	Black	Non-Hispanic	Hispanic
Parity2+ * After	9.948*** (2.000)	5.516** (2.082)	18.16*** (3.120)	10.92*** (2.132)	3.849 (3.926)
Parity=2 * After	5.592** (1.696)				
Parity3+ * After	13.93*** (2.561)				
State x year control variables	X	X	X	X	X
Mean of the dep. variable	3206	3206	3273	3067	3273
N	37,639	37,639	16,870	10,938	19,864

Notes: Each column is from a separate DD regression of average birth applied to Natality data for effective tax years 1991-1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses.

Table 6: Magnitudes in OBRA93 Models, Mean Birth Weight

	All	White	Black
A. DD1: PARITY 2+ vs. PARITY 1			
DD Parameter estimate	9.948	5.516	18.16
DD Increase in EITC (2009\$)	\$521	\$471	\$624
Treatment on Treated per \$1000 (2009\$)	19.09	11.71	29.10
ToTper \$1000 (2009\$), % impact	0.60%	0.36%	0.95%
B. DD2: PARITY=2, PARITY 3+ vs PARITY 1			
PARITY=2 DD Parameter estimate	5.592	4.852	7.725
PARITY=2 DD Increase in EITC (2009\$)	\$373	\$335	\$445
Treatment on Treated per \$1000 (2009\$)	14.99	14.48	17.36
ToTper \$1000 (2009\$), % impact	0.47%	0.44%	0.57%
PARITY=3+ DD Parameter estimate	13.93	6.213	25.97
PARITY=3+ DD Increase in EITC (2009\$)	\$667	\$615	\$749
Treatment on Treated per \$1000 (2009\$)	20.88	10.10	34.67
ToTper \$1000 (2009\$), % impact	0.65%	0.31%	1.13%
C. DD3: PARITY 3+ vs. PARITY 2			
DD Parameter estimate	7.432	0.341	17.93
DD Increase in EITC (2009\$)	\$294	\$281	\$304
Treatment on Treated per \$1000 (2009\$)	25.28	1.21	58.98
ToTper \$1000 (2009\$), % impact	0.79%	0.04%	1.92%
Mean of the dependent variable	3206	3273	3067

Notes: Each column in each panel provides estimates for a separate DD regression. In each panel, the first row repeats average birth weight parameter estimates from Table 5. The second row provides the DD estimate on EITC income from the CPS/TAXSIM data. Row 3 is the treatment on the treated estimate of a \$1000 increase in EITC income (row 1 / row 2 * 1000). Row 4 provides the percent TOT impact (row 3 / mean).

Table 7: Maximum Credit Estimates of EITC on Low Birth weight, Single Women with a High School Education or Less by Race

	All		White		Black	
Maximum Credit (\$1000 of 95\$)	-0.291***	-0.835***	-0.124**	-0.136	-0.453***	-1.442***
	(0.0646)	(0.125)	(0.0531)	(0.110)	(0.113)	(0.174)
Parity * linear time		X		X		X
State x year controls	X	X	X	X	X	X
Mean of dependent variable	11.21	11.21	8.33	8.33	14.71	14.71
N	64,816	64,816	29,071	29,071	18,811	18,811
1st stage impact of maxcredit on ave EITC	0.330	0.330	0.330	0.330	0.330	0.330
Treatment on Treated per \$1000 (2009\$)	-0.63	-1.80	-0.28	-0.33	-1.06	-3.11
ToT per \$1000 (2009\$), % impact	-5.6%	-16.0%	-3.4%	-4.0%	-7.2%	-21.1%

Notes: Each column is from a separate regression of the percent low birth on the EITC maximum credit applied to Natality data for effective tax years 1984-1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses. The “1st stage impact of maxcredit on EITC” is the coefficient of a regression of the predicted EITC income on the maximum credit, estimated on the CPS/TAXSIM sample. The following two rows calculate the treatment on the treated impact per \$1000 (estimate / 1st stage * 1000) and the last row provides the percent TOT impact.

Table 8: Difference-in-Difference Estimates of OBRA93 on Log(Births)
(Single Women with 12 years of education or less)

	DD1	DD3
Parity2+ * After	-0.0199 (0.0141)	
Parity3+ * After		-0.0180 (0.0152)
State, year FE	X	X
State x year control variables	X	X
N	37639	25419

Notes: Each column is from a separate DD regression of the log(births) using to Natality data for effective tax years 1991-1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, and demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are clustered on state. Standard errors are in parentheses.

Table 9 Difference-in-Difference Estimates of OBRA93 on Composition of Births

	Fraction in state-year cell:										
	black	white	other	not hispanic	hispanic	hispanic missing	18-24	24-34	35+	dropout	high school
DD1											
Parity2+ * After	0.018*** (0.004)	-0.019*** (0.004)	0.0003 (0.0008)	0.0003 (0.001)	-0.0004 (0.0012)	0.0001 (0.0009)	0.018*** (0.003)	-0.009*** (0.002)	-0.008*** (0.001)	0.018*** (0.003)	-0.018*** (0.003)
Mean of Dep Var	0.323	0.641	0.0361	0.738	0.253	0.00906	0.648	0.300	0.0520	0.432	0.568
N	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224
DD3											
Parity3+ * After	-0.003 (0.002)	0.003 (0.002)	-0.0003 (0.0008)	0.008*** (0.002)	-0.007** (0.002)	-0.0007 (0.0006)	-0.026*** (0.003)	0.015*** (0.004)	0.011*** (0.001)	-0.012** (0.003)	0.012** (0.003)
Mean of Dep Var	0.370	0.593	0.0363	0.726	0.266	0.00863	0.523	0.402	0.0751	0.484	0.516
N	816	816	816	816	816	816	816	816	816	816	816

Notes: Each column is from a separate DD regression of the share of births to women with the given demographic group applied to Natality data for effective tax years 1991-1998. Observations are at the year-state-parity cell level. All models include fixed effects for effective tax year, parity, state, and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are clustered on state and weighted using the number of births in the cell. Standard errors are in parentheses.