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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, 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 6.7 to 10.8% reduction in the low birth weight rate, with larger impacts for births to African American mothers. These impacts are evident with difference-in-difference models and event study analyses. Our results suggest that part of the mechanism for this improvement in birth outcomes is the result of more prenatal care and less negative health behaviors (smoking). We find little role for changes in health insurance. We contribute to the literature by establishing that an exogenous increase in income can improve health, and illustrating a health impact of a non-health program. More generally, we demonstrate the potential for positive external benefits of the social safety net.

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An online appendix is available at:
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1. Introduction

In the U.S. and Europe, legislators and voters are once again debating the role of government; and in particular what the safety net should include. Evaluations of the costs and benefits of the safety net are often limited to examinations of labor supply and poverty. In our research, we seek to illustrate and quantify the potential for health impacts of non-health programs. In so doing, we hope to demonstrate the potential for positive external benefits of the social safety net. In this paper, we examine the impact of the Earned Income Tax Credit on a key marker of lifetime health and economic success—infant birth weight.

The Earned Income Tax Credit (EITC) provides a refundable transfer to lower income working families through the tax system. As a consequence of legislated expansions in the EITC (in 1986, 1990, and 1993) and the dismantling of welfare through the 1996 welfare reform, the EITC is now the most important cash transfer program for these families (Bitler and Hoynes 2010). 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[1]. The income transfers are significant; for example, among families with two or more children eligibility extends to annual earnings over $40,000 and the average credit (in 2008) for these recipient families is $2,563. The release of the new "supplemental poverty measure" reveals that the EITC lifts 6 million persons (including 3 million children) from poverty, more than any other program (Short 2010). The introduction and expansion of "in-work" assistance, developed in the U.S., is being adopted across many other 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 (see reviews in Eissa and Hoynes 2006, Hotz and Scholz 2003). 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

[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). In the aftermath of the Great Recession, in 2010 and 2011, Food Stamps costs increased substantially to $72 billion.

health benefits of non-health programs in the safety net.[2]

Using the EITC to examine impacts of income 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. Further, our research design identifies increases in income from tax reforms, allowing us to leverage 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 micro data, covering the full census of births beginning in 1984. We begin 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 (Currie 2011). We also explore other birth outcomes such as pre-term birth, weight-for-gestational age and Apgar score. We examine possible mechanisms for the changes in infant health by examining impacts on maternal health behaviors (smoking and drinking during pregnancy) and maternal health utilization behaviors (pre-natal care). In addition, using the Current Population Survey, we explore the possible role played by employment-induced changes in maternal health insurance.

We use three 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

[2] 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.

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 by year for the three different expansions and increases with family size for the 1993 expansion and later.

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 a 6.7 to 10.8 percent reduction in the low birth weight rate. Our results suggest that part of the mechanism for this improvement in birth outcomes is the result of more prenatal care, and less negative health behaviors (smoking). We find little role for changes in health insurance. Overall, our work provides 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, Section 7 we discuss mechanisms, and Section 8 we present robustness checks. In Section 9 we provide a discussion and Section 10 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 and the number of qualifying children. 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 child (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.[3]

The reach and importance of the credit has changed substantially over its history. Figure 1 presents the real maximum EITC credit (in 1999 dollars) by tax year and family size for our analysis

[3] 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.

period, 1983 to 1999. During this time, the EITC expanded dramatically through three tax acts: the 1986 Tax Reform Act (TRA86) and the Omnibus Reconciliation Acts of 1990 and 1993 (OBRA90, OBRA93). Importantly, the tax reforms, as illustrated in Figure 1, generate differential expansions based on family size (no children, 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. The figure also illustrates that the 1990 and 1993 expansions were phased in over several tax years.

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 (Internal Revenue Service 2011a).

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 (Dehejia and Lleras-Muney 2004) on infant health. Given limited evidence on this important issue, our paper provides noteworthy evidence on the potential health benefits of increases in income.[4]

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 Schmeider 2009). Infant health improvements may work through other channels as well, for instance reducing stress (e.g., financial stress) experienced by the mother leading to a direct and beneficial impact on birth weight (Aizer et al 2009, Camacho 2008, Evans and Garthwaite 2010). Patel (2011) also showed that EITC transfers to single women are, in part, spent on purchasing an automobile, which in of itself could

[4] 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 Evans2006), 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 and a “health index” (Lindahl 2005).

increase access to prenatal care and decrease stress.

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, Meyer and Rosenbaum 2001). Eissa and Hoynes (2004) 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). Less is known about the relationship between maternal employment and own or child health (Baum 2005, Del Bono et al 2008). However, Gelber and Mitchell (2011) find that the EITC leads to an increase in market time and a reduction in leisure, but no change in time spent with children.

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 due to an increase in the opportunity cost of the mother's time. Therefore, a third possible channel for the effect of the EITC on birth weight is through changes in 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. The available evidence suggests that the EITC does not impact fertility (Baughman and Dickert-Conlin, 2009) or family formation (Dickert-Conlin 2002; Ellwood, 2000, Herbst 2011).[5]

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

[5] 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).

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 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.[6] Our paper makes several contributions to this emerging literature. 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

[6] Other studies use tax-reform induced changes in after-tax income to examine impacts on other child outcomes. Dahl and Lochner (forthcoming) 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.

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.[7] There are also missing values for birth weight, parity, race, and marital status, but these are rare and not systematically occurring across states. We limit the sample to mothers age 18 and older with singleton births who are not missing values for birth weight or parity.[8]

We collapse the data to cells defined by state, month-year, parity of birth (1st, 2nd, 3rd, 4th 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 4th or greater cell), the fraction of births below 2,500 grams (also the fraction below 1500, 2000, 3000, 3500, and 4000 grams), as well as other outcomes such as prenatal care and health behaviors and the 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]

[7] 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. 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.

[8] We limit to singleton births because of systematically lower birth weight for multiple births. Our results are not sensitive to this sample selection.

[9] As with general individual tax 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"

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 2, which is reprinted from LaLumia (2011), shows that more than 50 percent of EITC tax refunds are received in February. So, for example, most tax year 1990 refunds are received in February 1991 (or shortly thereafter). We also assume that this cash is spent over the subsequent 12 months. 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.

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 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.

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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.

[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. Our results are robust to using observed gestation to assign effective tax year (discussed below).

[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).

A handful of studies examine the impact of the EITC on spending and find that expenditures increase more in the first quarter (quarter of EITC receipt) than later quarters (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 explore this and other alternative assumptions about the timing of EITC income and fetal sensitivity below in Section 8.1. We also compare the impact of the EITC through the maternal "labor supply channel" versus "income" channel. None of the alternatives we consider change the substantive results derived from our baseline assumptions.

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.[12]

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:

$$Y_{pjst} = \alpha + \delta After_t * Parity2plus_p + \beta X_{st} + \gamma_p + \eta_s + \delta_t + \phi_j + \varepsilon_{pjst} \quad (1)$$

where Y_{pjst} 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.[13] X_{st} includes controls for unemployment rate, welfare reform and Medicaid or SCHIP eligibility and we include fixed

[12] 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 (2011).

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

effects for demographic group ϕ_j, parity γ_p, state η_s and 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, we compare 2nd and higher order 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 second specification we include *After* * *Parity2* and *After* * *Parity3plus* (maintaining first births as the control). Finally, in our third specification, we limit the sample to 2nd and higher order births and include *After* * *Parity3plus*, thus effectively using 2nd births as a control for third and higher order births.

Identification in this model requires that in the absence of the EITC expansion the control group (e.g. first births) would have similar trends to the treated group (e.g., second or later births). To 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 second and third specifications). We then plot the year times *Parity2plus* interactions. This allows 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) $$Y_{pjst} = \alpha + \delta Maxcredit_{pt} + \beta X_{st} + \gamma_p + \eta_s + \delta_t + \phi_j + \varepsilon_{pjst}$$

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.

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 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 error). The first column indicates that second parity or higher births, relative to first births, were 0.36 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 second model (shown in column 3) decomposes the policy impact into second births and third or higher order births. The results are consistent with expectations: low birth weight status is reduced by roughly 0.53 percentage points for third or higher births versus 0.16 for second births (each compared to first births). We can also compare 3rd and higher order births to second births which eliminates the possibility of 1st births being an inadequate control (perhaps due to less knowledge about the EITC). These results (in column 5) show that low birth weight status is reduced roughly 0.34 percentage points relative to the mean of 10.7 percent. Table 2 also shows estimates for models where we add state controls for Medicaid expansions, welfare reform, and state level unemployment rates within the state by year cell (columns 2, 4 and 6). There is little change to the coefficients from adding these additional controls (we control for these variables in the remaining analysis).

A possible concern is that the regression results from Table 2 could be driven by pre-existing differential trends in health 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 particular, 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 3a, 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 3b shows the event study coefficients for the second model, where we plot the interactions of the year dummies with *Parity2* and *Parity3plus*. Here, as in Figure 3a, the pre-trend is quite flat. We include on Figure 3b 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; this limits our sample to three years of pre-trends.[14]

The third 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 this model 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 between third and higher births relative to second births was not part of the earlier 1990 EITC expansion, we can utilize data for more pre-years in the event study. Figure 3c shows the event study for this 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

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

weight births corresponding to the increase in eligibility for maximum EITC benefits. As with Figure 3b, 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.[15]

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 0.73 percentage points (relative to a mean of 14.4 percent), more than four times higher than the effect on white mothers (0.13 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 are more likely to receive larger EITC benefits. We return to this below when we discuss the magnitudes of these findings. Interestingly, smaller treatment effects are experienced by Hispanic mothers than non-Hispanic mothers (-0.13 versus -0.41 in the 2nd+/1st parity model). Perhaps this is because Hispanic children tend have better baseline birth outcomes (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. In addition, 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).[16]

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 benefits received. 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

[15] The event study shows that the improvement in low birth weight continues after OBRA93 is fully phased in. This may be explained by the EITC-generated improvement in maternal health that is brought into the pregnancy after the expansion is fully phased in (Evans and Garthwaite 2010).

[16] We also explored differences by gender of birth, finding no significant differences in birth outcomes. Dahl and Lochner (forthcoming) and Milligan and Stabile (2008) use tax reforms to identify changes in income on child outcomes and find larger effects for boys. However, they examine the impacts of income on outcomes of existing children and thus may capture gender biases in allocation of the additional income. In our case, many/most mothers may not know the sex during pregnancy so the differences in outcomes would come from biology rather than behavioral differences.

eligibility.[17] These results, along with the effects on infant health, are shown in Table 4. Panel A presents results corresponding to our model comparing 2nd and higher order births to first births. The first row transcribes from Tables 2 and 3 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 simulated EITC benefits.[18] 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. This IV-type interpretation suggests what the impact of EITC income would be, under the assumptions that (1) take-up of EITC was 100 percent[19], and (2) EITC income was the only mechanism through which the policy impacted birth outcomes. 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 and Panel C presents estimates for the comparisons of 3rd and higher order births compared to 2nd births.

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[17] 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.

[18] We take our CPS sample which proxies a "new births" sample (describe above) and regress the real EITC simulated benefit on dummies for *Parity2plus* (for our 2+/1 model) or *Parity2* and *Parity3plus* (for 3+/2/1 model), *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.

[19] 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.

The results in Table 4 show that for our high impact sample a \$1000 EITC TOT (2009 \$) leads to a 0.68 percentage point reduction in percent low birth weight, a 6.7 percent effect. The corresponding \$1000 TOT effect for whites is -0.28 percentage points or a 3.4 percent decline and for blacks a TOT effect of -1.17 percentage points or an 8.1 percent decline.

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.

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, we 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 columns (2) and (6) 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.

Figure 4 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 = -0.354). 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 the Parity 2+ vs. 1 model (Figure 4a) and the parity 3+ vs. 2 model (Figure 4b). 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. You can also see "placebo" estimates, for example the estimate for highly educated women (labeled "ed16+") which has a very small EITC treatment and a wrong signed (insignificant) and small coefficient on low birth weight.

One drawback of the analysis in Figure 4 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 two or more children (at least one less than 6).[20] We use the woman's characteristics to impute the EITC amount using TAXSIM (as above). We regress the predicted EITC on state fixed effects and full set of interactions of the demographic predictors.[21] We 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).[22] 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 5. 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). (Note we have plotted the percentage point *reduction* in low birth weight so that the EITC income treatment and the estimated treatment effects both increase with decile.) The results for the 2+/1 model (Figure 5a) shows the treatment effect growing with the EITC treatment, with significant impacts for the top 3 deciles. For the 3+/2 model (Figure 5b) the effects are significant for the 7th and highest decile.

6.3 Panel FE estimates using multiple EITC expansions

We can 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

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[20] 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.

[21] Demographic predictors include education group (<12, 12, 12+), race (white, black, other), marital status (married, single) and age (18-24, 25-34, 35+). We do not use Hispanic status because it is missing for some states in the Natality sample.

[22] The deciles are assigned taking account of the number of births in each cell. By construction the decile assignment does not vary by year (and only by state-demographic cell).

the maximum credit (in 1000s of 1995$), which varies by tax year and parity. We estimate equation (2) which is weighted using the number of births in a cell and standard errors are clustered on state.

Table 5 shows results from estimating the model on our high impact sample, single women with a high school education or less and for white and black subsets of the high impact sample. 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 (column 1). As above, the results show larger effects for blacks (-0.52) than for whites (-0.12).

Due to the 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 (in columns 2, 4, and 6) show substantially larger estimated treatment effects for models with parity linear trends. While we may be "overfitting" the parity-time relationship, we view the robustness to including the parity trends as an important result.

The results in Table 5 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 facilitate comparison to the difference-in-difference 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). For the full high impact sample, the point estimate suggests that a $1000 increase in the maximum credit leads to a $330 increase in EITC income (the tables shows the estimate of 0.330), 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 difference-in-difference estimates above.

The estimates (without parity linear trends) are very 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 6.7% for the DD), 3.4% for whites (3.4% in the DD), and 7.2% for blacks (8.1% for the DD). We also estimated models by decile of predicted EITC treatment, showing larger estimated effects in the higher deciles (available on request).

Given the similarity of the panel fixed effects results to the OBRA93 results, we return to the OBRA93 design for the remainder of the paper.

6.4 Other outcome variables

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 impact of the EITC expansion on the distribution of birth weight, following Almond, Hoynes, and Schanzenbach 2011we 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 the 2^{nd}+/1^{st} parity model (Figure 6a) and the 3^{rd}+/2^{nd} parity model (Figure 6b). 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 and very small effects at the top. For the 2^{nd}+/1^{st} parity model, the effects on very low birth weight (<1,500 grams) are not statistically significant.

Many studies also examine mean birth weight and we do so here for our high impact sample in Table 6. We estimate that for all high impact mothers the EITC expansion, comparing second and higher order births to first births, leads to an increase in mean birth weight of 10 grams (or 6.9 grams for the 3+/2 model). As shown in Appendix Table 1, these impacts are larger for Black mothers (18 grams), and for Non-Hispanic mothers (11 grams). Appendix Table 2 presents our birth weight results along with the EITC income results, so as to be able to gauge the magnitudes of our estimated impacts. For our basic 2+/1 model, we find that an increase of $1000 of EITC income (2009$ 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, Hoynes, and Schanzenbach 2011). Again, this impact is larger for Black mothers ($1000 TOT effect is 28.7 grams or a .9 percentage point increase) than for White mothers ($1000 TOT effect is 9.25 grams or a 0.28 percentage point increase), and larger for 3rd births (23.3 grams or 0.73 percent) relative to 2nd births.[23]

In addition to birth weight, we can examine other birth outcomes such as pre-term birth (born before the 37th week of pregnancy), small for gestational age (below the 10th percentile of birth weight for gestational age), and Apgar score (score below 8 [of 10]). [24] These results are presented in the remaining columns of Table 6 and show that the EITC expansions improved infant health along each of these dimensions: a reduction in prematurity, an improvement in weight-for-gestational age, and an improvement in 5 minute APGAR scores. All of these effects are found for the 2+/1 model and the 3+/2 model, are statistically significantly distinguishable from zero, and are fairly modest in magnitude. On the whole, these outcomes tell the same story as the analysis of low birth weight. Further, as shown in Appendix Table 1, the estimates are consistently larger for Blacks and Non-Hispanics compared to whites.

7. Mechanisms of impact

What are the mechanisms by which an expanded EITC leads to improved infant health? Some of the possible mechanisms can be examined directly. For instance, Patel (2011) documents the relationship between the EITC and family expenditures. It is plausible that this increased consumption may lend itself to a healthier fetal environment through improved nutrition or reductions in stress and other environmental threats. Alternatively, it is possible that the EITC could lend itself to improvements in

[23] We also estimated event study models for mean birth weight and we present those in Appendix Figure 1. The event study for parity 2+ vs. 1 (Appendix Figure 1a) suggests a possible pre-existing trend. Appendix Figure 1b splits this out by parity. Here we see (solid lines) that there is a modest (but similar) pre-trend in mean birth weight for both parity groups. As such, for mean birth weight, we feel more confident in our model using parity 2 births as a control group for parity 3+ births. Appendix Figure 1c presents the event study graph from our 3+/2 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.

[24] Unfortunately, due to data limitations we were unable to estimate the impact on infant mortality, an important outcome measure that would be complementary to the outcomes we present here. The national linked birth-death vital statistics records are unavailable for the years 1992, 1993, and 1994; and they lie in the middle of the OBRA93 expansions. When we re-estimate our birth weight models excluding these years, the main results become too noisy to provide much insight. We are unable to use the unlinked death and birth records to compute cohort-based measures of mortality, because the death records do not record the child's parity. Without this information we cannot assign a treatment status to deaths. We have explored the possibility of pursuing California vital statistics records (which have linked births and infant deaths over the full period). When we re-estimate our birth weight models on California data only, the results do not hold up within this state (point estimates are small, and confidence intervals are large).

health care utilization during pregnancy, leading to improved health outcomes.

To explore the impact of the EITC on health care, we examine prenatal care as an intermediate outcome. We consider several measures of prenatal care, and results are presented in Table 7 for our high impact sample. The results are for the most part consistent in magnitude and significance across both of our difference-in-difference research designs, and so we discuss only the 3+/2 results here.

The EITC expansion led to an increase in prenatal care across all measures. There is an increase in 0.62 percentage points of receiving any prenatal care, and a similar magnitude in receiving care before the third trimester. This increase is large compared the "no care" average outcomes (11.5% do not receive prenatal care before their 3rd trimester, and 4% do not receive any care). The impact on the number of visits, 0.09, is precisely estimated and fairly modest compared to the mean (9.8 visits). Finally, we see an improvement in the Kessner Index for adequate care.

In addition to examining prenatal care, we use information on self-reported smoking and drinking in the birth certificate data as additional intermediate outcomes of interest. We find reductions in "any smoking" of 1.2 percentage points, which is reasonably large compared to baseline smoking rates of about 30%. We find reductions in "any drinking" of about 1.1 percentage points (compared to mean drinking rates of about 3.3%).

Given that the EITC leads to increases in employment, one possible pathway for affecting infant health is through changes in maternal health insurance. The national birth records that are our primary data set do not have this information, so we use the March Current Population Survey for this purpose. We use information on whether a woman has any insurance, public insurance or private insurance as outcome variables, and analyze these with our 2+/1 and 3+/2 models. For this analysis we treat the number of children in the household as the key variable determining the EITC schedule.[25] Demand for insurance may be directly influenced by the presence of children in the household, and if this influence is changing over time this threatens the 2+/1 model, which compares families with children against those without. As such we place greater trust in the 3+/2 model which compares families with two or more

[25] In the CPS, we do not observe a new births sample. Instead here we adopt the usual EITC research design and use the observed number of children to assign treatment. Thus our 2+/1 sample compares women with children to women without children. Our 3+/2 design compares women with two or more children to women with one child.

children against those with one child. Finally, we also examine the subset of women who have children younger than age 6, since we expect their economic and demographic circumstances to be closer to the birth population of our main results.

Our results on health insurance for the high impact sample are in Table 8. In the first column we show employment as the outcome variable, confirming the typical result that EITC causes large and statistically significant increases in (annual) employment. The remaining columns of the table show that the EITC expansion led to a decrease in the woman's Medicaid coverage and an increase in her private insurance coverage. Examining the 3+/2 model for all mothers, we estimate a 3.7 percentage point reduction in Medicaid coverage and a 3.2 percentage point increase in private insurance. These numbers can be compared with a 6 percentage point increase in employment. The offsetting impacts lead, overall, to an estimate of no change in the probability of any insurance (and a decline in coverage in the 2+/1 model).[26] When we examine the sample of women with young children, we see the same qualitative impacts. The point estimates are smaller (2.1 percentage point reduction in Medicaid coverage and 2.8 percentage point increase in private coverage), and the standard errors are a bit larger.

How should we interpret the reductions in low birth weight in light of these estimates? First, we see the reduction in smoking rates as being a plausible channel for the birth weight improvements[27] and we also find the increase in the chance of having a prenatal visit to be consistent with this pattern. One possibility is that the EITC-generated increases in income lead to more access to prenatal care (and thus less smoking) and better birth outcomes. Another possibility is that the EITC-generated increase in employment leads to dropping Medicaid insurance for private insurance. The insurance specifications suggest that overall insurance coverage is not changed by the EITC expansion (and if anything declines in the 2+/1 model) but if the private insurance is higher "quality," lowering barriers for mothers to schedule doctor's appointments, this may result in earlier prenatal care (for some mothers), which leads to reductions in smoking and improved birth outcomes.

[26] Meyer and Rosenbaum (2008) find a similar result.

[27] Almond, et. al. (2005, Table VI) estimate that smokers have 3.5%pts higher incidence of low birth weight. We estimate in Table 7 a reduction in smoking of 1.2–1.9 %pts, which suggests a reduction in low birth weight incidence due to the smoking channel of 4 to 7 per 10,000 births. This is a modest but meaningful portion of our main estimates in Table 2 of an overall reduction in low birth weight incidence of 34 per 10,000 births.

This discussion of channels of impact is speculative on our part, and our research design does not let us distinguish it from alternative plausible channels. For example, it may be the case that employment itself leads to reductions in smoking, or instead that early prenatal care leads to reductions in low birth weight. Further, it may be that the increases in income lead to reductions in birth weight through other channels, perhaps including better nutrition.[28]

8. Extensions and robustness checks

8.1 Sensitivity to model of timing of impact

Recall that in our main results we assign the "EITC treatment" assuming that the income arrives in February (our "cash-in-hand" assumption), is available throughout the next 12 months, and the tax treatment as of the beginning of the third trimester is what matters for birth outcomes. Here we examine the sensitivity of our results to alternative models of the timing of the impact. We explore three alternatives: (1) when the "sensitive" period is for fetal development during gestation; (2) when the EITC income is received/spent and is likely to impact fetal health; and (3) impacts arising from the timing of the arrival of additional income through "labor supply" (versus time timing of arrival of the additional income through the EITC refund).

To examine alternative assumptions about the sensitive period for fetal development, we return to our data at the month-year (by state by parity by demographic group) level. We assume that the EITC income is received beginning in a February and spent evenly throughout the subsequent year. For a given birth, we can construct the EITC exposure during the infant's first trimester, second trimester, and third trimester as the average credit across the given months of pregnancy. For example, a March 1995 birth will have for its third trimester exposure one month of tax year 1993 policy (reflecting fetal exposure during January 1995, hence EITC received during February 1994, which comes from tax policy set in 1993), and two months of tax year 1994 policy (reflecting fetal exposure during February and March 1995, reflecting cash received in February 1995, which comes from tax policy in 1994). We also examine

[28] We would like to be able to measure insurance status linked with birth outcomes, and some state-level data (CA, MI) have this linkage in their records. However, when we limited our basic models to estimate on just CA or just MI, the main results do not hold up (with overly large and uninformative confidence intervals).

similar exposure in the first and second trimesters (as well as a model allowing for sensitivities in all three trimesters).

Results of these models are in Table 9. In this table we present results from our maximum EITC credit panel fixed effects specification, with low birth weight as the outcome variable.[29] Results for our 3+/2 difference-in-difference model (reported in Appendix Table 3) are qualitatively similar. Each row presents results from a separate model. The first row copies the results from Column 1 of Table 5. The next three rows assume that a child's "sensitive trimester" is the averaged over the 3rd, 2nd, and 1st, respectively. They all have similar point estimates and standard errors. The next row includes a variable for each trimester's exposure. The point estimates in this model are noisy and we cannot reject equality of coefficients; . the evidence is suggestive that the impact may be strongest in the first trimester.

To examine alternative assumptions about the timing of EITC income's impact on infant health, we next assume that all EITC income is spent in the February of receipt. We then assign this income to an infant based on whether and when February falls within their gestation. Results of these models are presented in rows 6 and 7 of Table 9. Again, the point estimates here are similar to that from our base model. Also, we cannot distinguish among the three trimesters to determine if one is especially important. (For this timing model, a birth will experience exposure at most during one of its trimesters – whichever one contains February. As such, we do not need to sum up the coefficients in row 7.)

Finally, we examine potential impacts through the labor supply and earned income channels of the EITC. To do this, we assign to each month the EITC treatment status of the *current (January-December) tax year*, to reflect the labor supply incentives and corresponding impact on earned income.[30] We then assign this treatment to each birth based on which months fall into which trimester. Results of this model are presented in rows 8 and 9 of Table 9. In row 8, we see results similar to the baseline model. In row 9, we "horse race" the labor supply channel against the core cash-in-hand channel. In this model the cash-in-hand coefficient retains its significance (and increases somewhat in magnitude), while the

[29] Since this examination of timing requires averaging across months of exposure, it seemed more natural to use the maximum credit model.
[30] For example, a March 1995 birth will have for its third trimester exposure three months tax year 1995 policy (reflecting fetal exposure during January-March 1995) and six months of tax 1994 policy (reflecting fetal exposure during June – December 1994).

labor supply coefficient becomes positive. Our reading of this evidence is that it is difficult to separately identify these two channels with the low-frequency tax changes that identify our model.

8.2 Potential 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 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 10 presents the results of the difference-in-difference analysis for the high impact sample where the dependent variable is the log of the number of births in the (state-parity-year-demographic) cell. We provide estimates of comparing 2nd and higher order births to 1st births, and 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 the remaining columns of Table 10, we examine the impact of the EITC expansion on the *composition* of births by estimating 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 changes in these observables we might be concerned about changes in 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 the models, with some models showing the treatment correlated with more births to disadvantaged mothers and others showing treatment correlated with fewer disadvantaged births. We are encouraged by the fact that, as shown in event study version of these results in Figure 7 (for the 3+/2 model), the changes in the demographics are changing smoothly through the 1993 expansion while our main results (Figure 3) 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.[31]

[31] Nonetheless, we show in Appendix Figure 2 that our main event study results for low birth weight are robust to adding controls for demographic-group times linear year.

8.3 Robustness and Sensitivity

Our results are robust to changes in the specific sample selection that we use for the analysis. In Appendix Table 4, we present estimates of the 2+/1 and 3+/2 difference-in-difference models for low birth weight. In column 1, we drop Mexican born women, who are at higher risk of being undocumented and ineligible for the EITC. In column 2, we use reported gestation to assign treatment (rather than assuming a 9-month gestation) and in column 4 drop observations with weight inconsistent with gestation. In column 3 we drop fourth and higher parities and in the last two columns we balance the panel to include states reporting education in all years (column 5) and states that did not impute marital status in all years (column 6). The qualitative results are unchanged across these different samples.

Our core research design is based on a “parity times tax year” identification strategy. Consequently, our strategy could be susceptible to bias arising from parity-specific spurious trends in birth weight that are coincident with the policy change. We have already tested this in two ways: First the event study models provide a direct test of the validity of the design by examination of pre-trends. Second, we showed that the maximum credit panel fixed effects model are robust to adding parity linear trends (the results with trends are, if anything, larger than our base case results). To examine this further, we examine a series of “pair-wise” comparisons of different parity births. Some of these comparisons (e.g. 2 vs. 1, 3 vs. 2) embed a treatment and some (e.g. 4 vs. 3) form a “placebo test” for our estimation method.

We present results in Appendix Table 5 for the high impact sample. In the first row we compare second births (treated under the “one child” EITC schedule) to first births (untreated), and so on. The first two rows reinforce our main findings – there was a relative improvement in low birth weight for 2nd births compared to 1st births, and also for 3rd births compared to 2nd births. The remaining rows of the table compare pairs of birth parities which are both “treated” and we expect to find no estimated effect for these comparisons. This appears to be true for 5th vs. 4th and 6th and higher vs. 5th. However, we do find that low birth weight improved more for fourth births than for third births, which is not consistent with our expectation. To investigate this finding further, we estimated an event study model for this comparison. This analysis indicates that the 4 vs. 3 difference begins in 1995 and grows after that. Additionally we predict EITC income “treatment” for 4 vs. 3 in the CPS (as we did above in Table 4). We found that the OBRA93 expansion was associated with an increase in predicted EITC income for fourth births compared to third, even though the policy change was the same for these two groups. This suggests that women with 4th births were becoming more disadvantaged (e.g. lower earnings) relative to women with 3rd births.

The gap between 4th and 3rd births does raise a cautionary note about potential parity-specific trends in birth weight, and our analysis should be interpreted in light of this caution. We believe that despite this, the preponderance of evidence indicates that the EITC does improve child health. First, the timing of these spurious trends does not correspond cleanly with the policy change. Second, in our “maxcredit” models, results are robust to inclusion of parity-specific trends. Third, the fact that predicted EITC income increases for 4th vs. 3rd births is consistent with the EITC being part of the channel for the improved birth weight of 4th born children.

9. Discussion

In this paper 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 (comparing 2nd and higher parities to 1st born children; and comparing 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 insignificant and small positive 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. However, our event study analysis shows that for low birth weight the preexisting trends are flat, for both the 2+/1 and the 3+/2 comparisons. This coupled with the falsification tests for “untreated” demographic groups, provides reassurance that we are estimating the causal impact of the expansions.

We find that for single low education (<= 12 years) mothers, a policy-induced treatment on the treated increase of $1000 in EITC income is associated with a 6.7 to 10.8 percent reduction in the low birth weight rate. Is this a meaningfully large impact? We address this question in two ways.

First, these estimates fit in well with the (small) literature on the impacts of income on low birth weight. Almond et al (2011) find that a $1000 (2009$) TOT impact of food stamps leads to an 4 percent reduction in low birth weight for whites and a 2 percent reduction for blacks, somewhat smaller than our 8 (3) percent reduction for blacks (whites). Hoynes et al (2011) find that WIC leads to a 10-20% reduction in low birth weight.

As a second way to assess the magnitude of the impacts, we aim to measure the dollar benefit of reductions in low birth weight. We take estimates from Almond et. al. (2005) who estimate cross-section and twin fixed effects models to measure the association between newborn hospital charges and birth weight. Using the results from their Table 5, we assign to each birth in our sample "excess hospital costs" (beyond those of regular birth weight births), and then estimate our difference-in-difference models with the excess costs as the outcome variable. We examine two measures of excess costs: one coming from Almond et. al.'s "pooled" model (analogous to a cross section association), and one coming from their "fixed effects" model.[32] We then take the resulting estimates and scale up using the "first stage" from Table 4 ($1000/$521 = 1.92 for our 2+/1 models, $1000/$294 = 3.40 for our 3+/2 models) to get a "$ benefits of reduced hospital charges per $1000 EITC received".

We find in our 2+/1 models that the TOT per $1000 EITC impact is a reduction of $80 using the "pooled" cost estimates, and $20 using the "fixed effects" estimates. In our 3+/2 models the TOT per $1000 EITC impact is a reduction of $245 in charges using the "pooled" estimates, and $95 using the "fixed effects estimates. These models are estimated with imprecision, with only the $245 estimate statistically significant at conventional levels. Taken as a whole, this suggests that the benefits from reducing hospital costs may be on the order of 2%-25% of the direct EITC outlays for these women. This does not count fully the health benefits from the improvements in birth weight. Hospital charges are just

[32] The pooled model is more representative of estimates from the epidemiological and public health literature, but Almond et. al. indicate that the twin fixed-effects model will have less omitted variable bias.

one of potentially many measurable benefits of reductions in low birth weight, and so these estimates are lower bounds. We note that this component of the EITC benefits accrue only to those mothers/infants born, which are a small fraction of women receiving EITC benefits. So the aggregate impact of this channel is small compared to overall expenditures.

10. 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 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 6.7 to 10.8% reduction in the low birth weight. 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. Notably, while some of these benefits are internal (to the family), given the substantial life time costs of low birth weight, nontrivial external benefits are captured. Importantly, these impacts are typically not taken into account given the non-health nature of the program and should be considered in discussions of the value of the safety net. The results also speak to the debate as to whether income affects health, by providing an estimate of a relatively large and exogenous increase in income on infant health.

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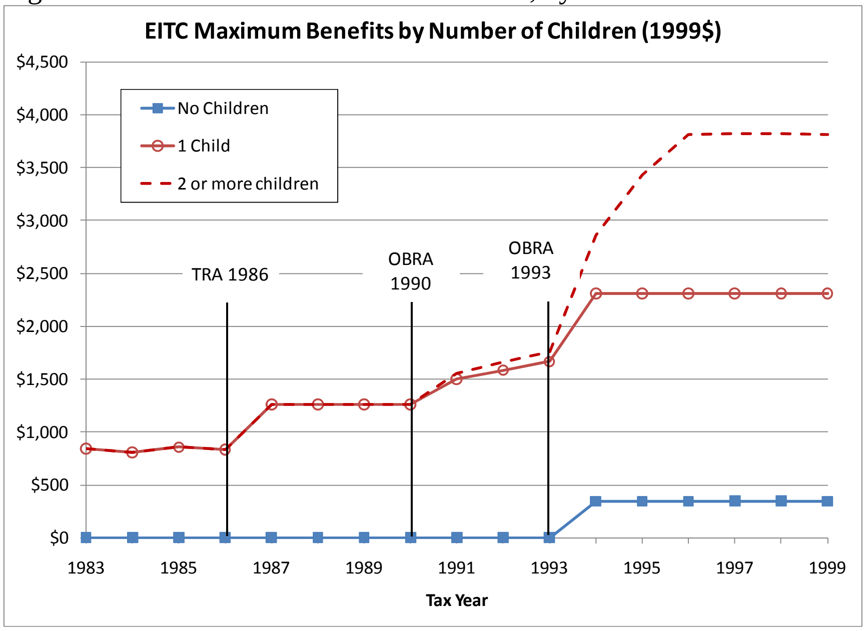
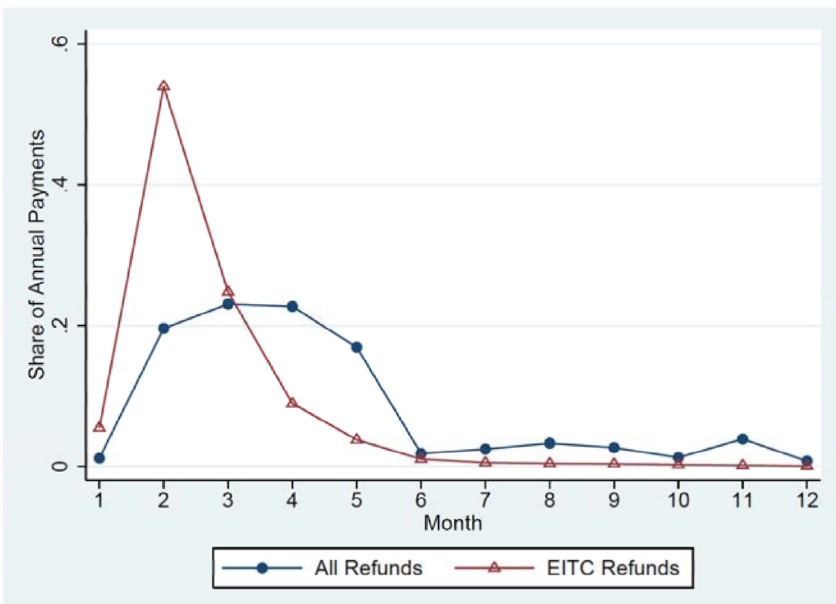
Figure 1: Maximum Credit for Federal EITC, by Tax Year and Number of Children



Source: Tax Policy Center (2011).

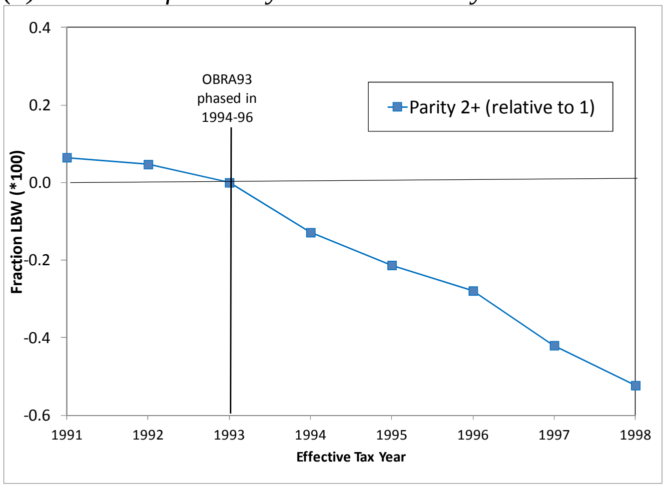
Figure 2: Distribution of Tax Refunds, by Calendar Month



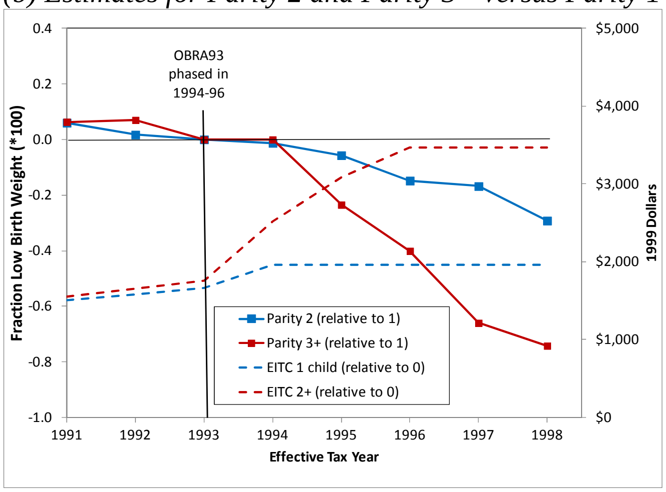
Note: Reprinted from Lalumia (2011). Figure shows 10 years averages using Monthly Treasury Statements 1998-2007.

Figure 3: Event Time Estimates of OBRA93 on Low Birth Weight and EITC Income, Single Women with a High School Education or Less

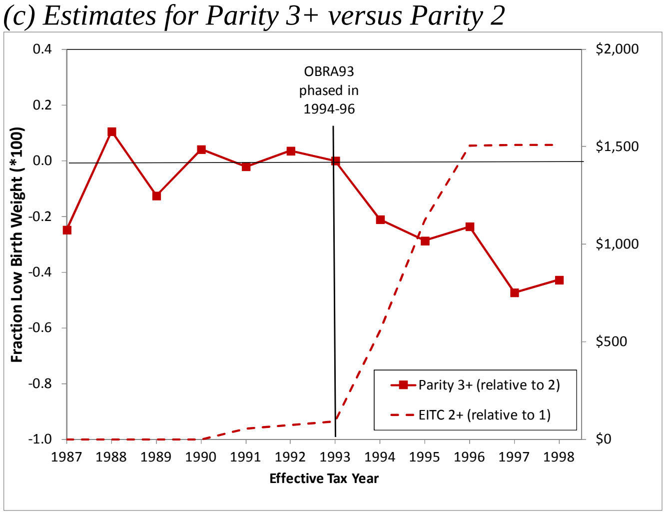
(a) Estimates for Parity 2+ versus Parity 1



(b) Estimates for Parity 2 and Parity 3+ versus Parity 1



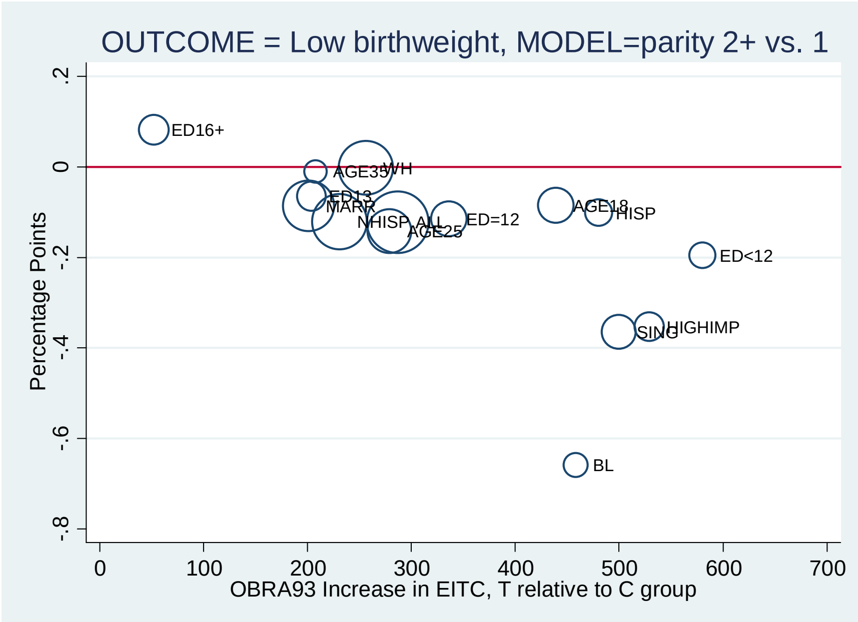
(c) Estimates for Parity 3+ versus Parity 2



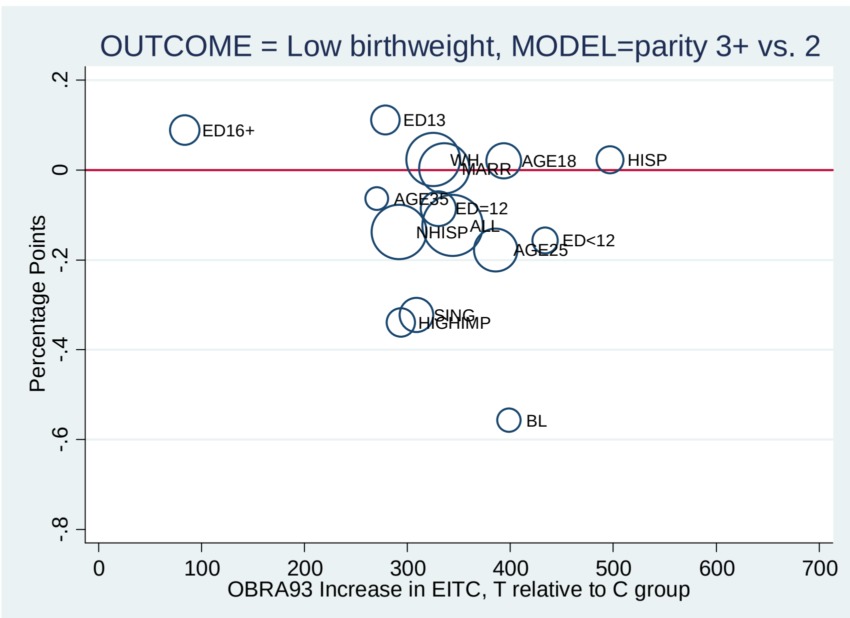
Notes: Each figure plots coefficients from an event-study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. higher order parity relative to lower order parity). The specification includes fixed effects for year, state, parity, demographic group and state-year 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 4: Demographic Subgroup Estimates of OBRA93 and Magnitude of EITC Treatment

(a) Estimates for Parity 2+ vs. Parity 1

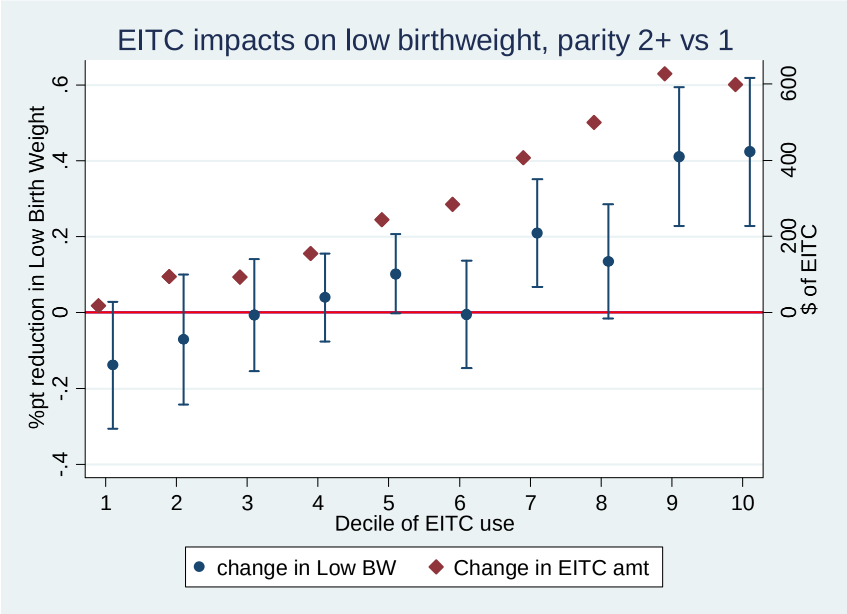


(b) Estimates for Parity 3+ vs. Parity 2

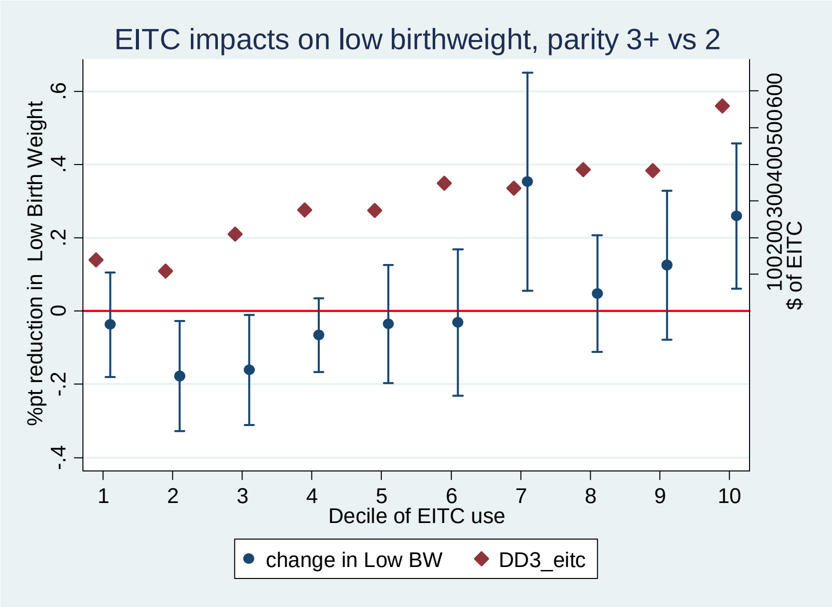


Notes: Each point on the graph represents DD regression estimates for a specified demographic subgroup. The x-axis provides the DD estimate of EITC income (using the CPS/TAXSIM sample) and the y-axis provides the DD estimate on LBW. The size of the points reflects group's cell size.

Figure 5: Difference-in-Difference Estimates of OBRA93, by Decile of Predicted EITC Treatment
(a) Estimates for Parity 2+ vs. Parity 1



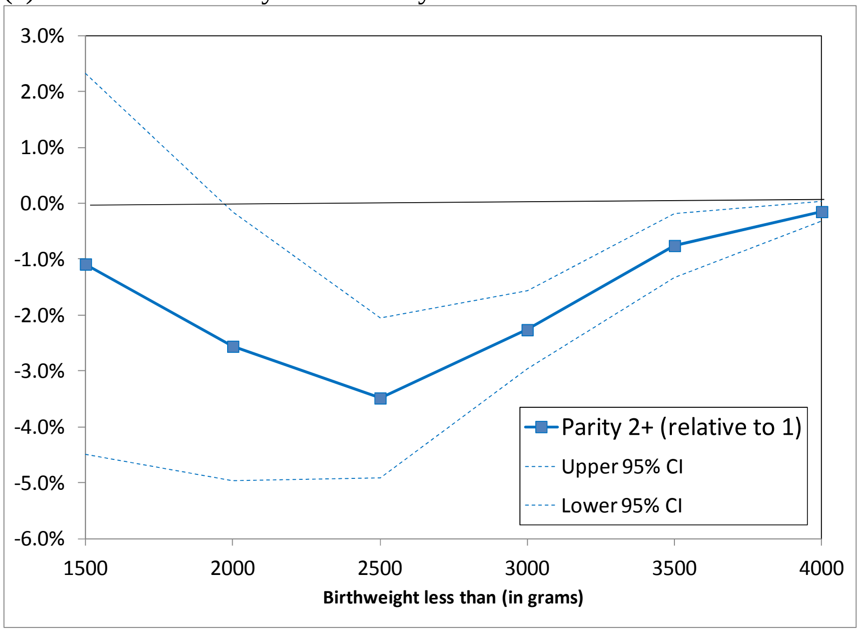
(b) Estimates for Parity 3+ vs. Parity 2



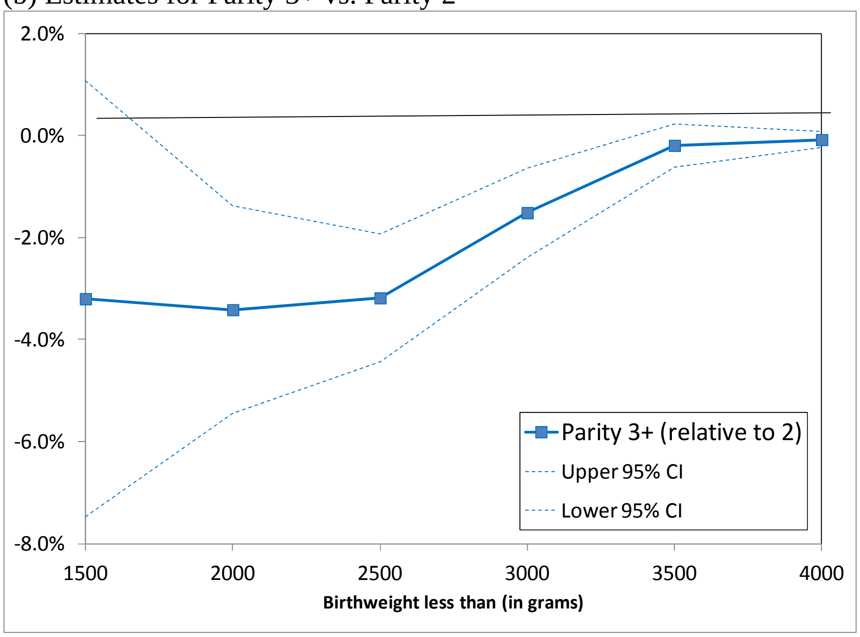
Notes: Each point on the graph represents DD regression estimates for 10 deciles of predicted EITC use. EITC deciles are assigned based on demographics and state using a prediction model estimated using the March CPS combined with TAXSIM. By construction the deciles do not vary by year or parity. The 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 6: Difference-in-Difference Estimates of OBRA93 on the Distribution of Birth Weight, Single Women with a High School Education or Less

(a) Estimates for Parity 2+ vs. Parity 1



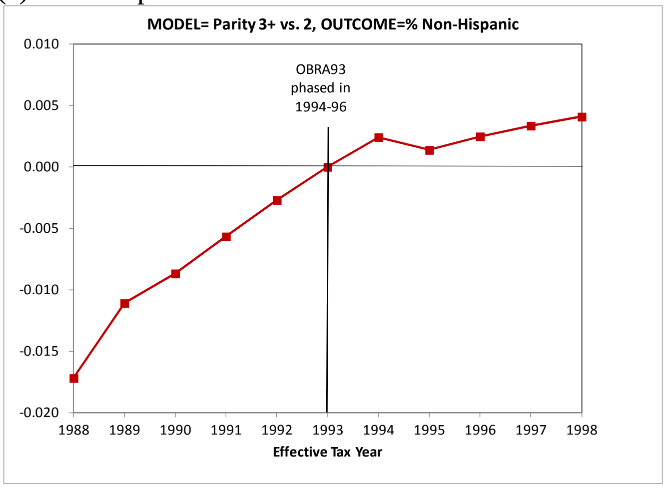
(b) Estimates for Parity 3+ vs. Parity 2



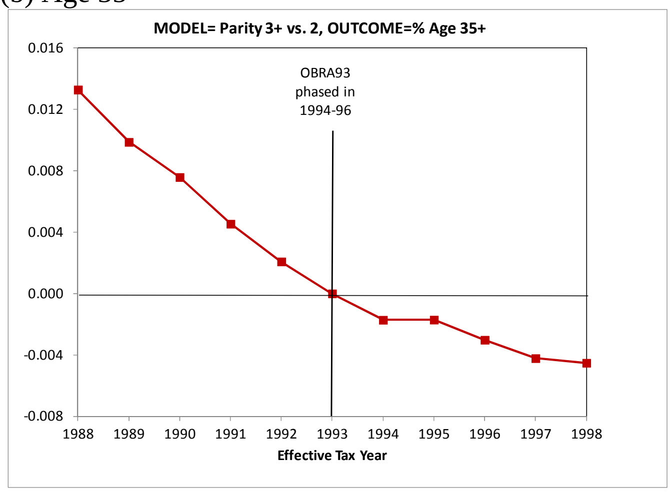
Notes: The graph shows estimates and 95 percent confidence intervals for the difference-in-difference estimate of the impact of EITC on the fraction of births that is below each specified number of grams. The specification is given by column (2) in Table 2.

Figure 7: Event Time Estimates of OBRA93 on Composition of Births, Single Women with a High School Education or Less (parity 3+ versus parity 2 model)

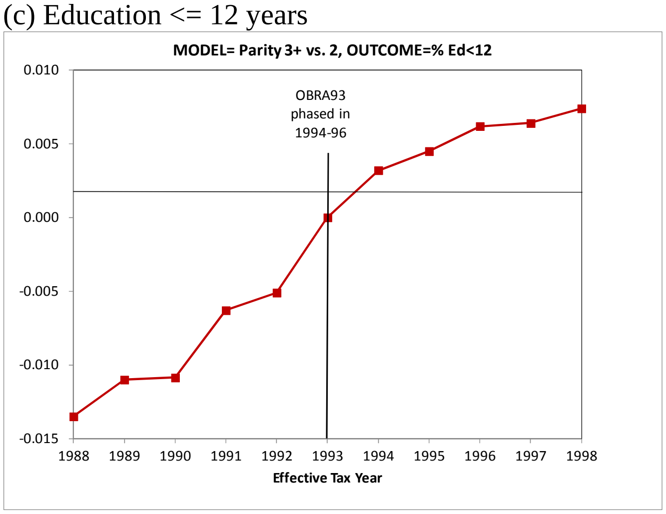
(a) Non-Hispanic



(b) Age 35+



(c) Education <= 12 years



Notes: Each figure plots coefficients from an event-study analysis where the coefficients are year dummies interacted with the treatment indicator (e.g. 3+ parity relative to parity 2). The specification includes fixed effects for year, state, parity, and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates.

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

Model:	Parity 2+ vs. 1		Parity 2, 3+ vs. 1		Parity 3+ vs. 2	
Parity2+ * After	-0.355***	-0.354***				
	(0.075)	(0.074)				
Parity=2 * After			-0.164**	-0.164**		
			(0.072)	(0.072)		
Parity3+ * After			-0.529***	-0.528***	-0.342***	-0.340***
			(0.091)	(0.090)	(0.069)	(0.068)
State x year control variables		X		X	X	X
Mean of the dep. variable	10.2	10.2	10.2	10.2	10.7	10.7
N	47,687	47,687	47,687	47,687	35,467	35,467

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

	White	Black	Non-Hispanic	Hispanic
Model: Parity 2+ vs. 1				
Parity2+ * After	-0.132*	-0.728***	-0.413***	-0.130*
	(0.072)	(0.143)	(0.099)	(0.070)
Mean of the dep. variable	8.14	14.43	11.24	7.04
N	21,775	13,780	26,066	14,823
Model: Parity 2, 3+ vs. 1				
Parity=2 * After	-0.114*	-0.310**	-0.187**	-0.0600
	(0.065)	(0.144)	(0.090)	(0.078)
Parity3+ * After	-0.151	-1.040***	-0.625***	-0.191**
	(0.093)	(0.160)	(0.123)	(0.087)
Mean of the dep. variable	8.14	14.43	11.24	7.04
N	21,775	13,780	26,066	14,823
Model: Parity 3+ vs. 2				
Parity3+ * After	-0.0231	-0.715***	-0.407***	-0.121
	(0.071)	(0.121)	(0.094)	(0.092)
Mean of the dep. variable	8.23	14.92	12.12	6.78
N	16,247	10,273	19,611	10,951

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			
Treatment Effect	-0.354	-0.132	-0.728
EITC increase (2009$)	$521	$471	$624
Treatment on Treated per $1000 (2009$)	-0.68	-0.28	-1.17
ToTper $1000 (2009$), % impact	-6.69%	-3.44%	-8.09%
B. PARITY=2, PARITY 3+ vs PARITY 1			
Treatment Effect (2 vs. 1)	-0.164	-0.111	-0.31
EITC Increase (2009$) (2 vs 1)	$373	$335	$445
Treatment on Treated per $1000 (2009$)	-0.44	-0.33	-0.70
ToTper $1000 (2009$), % impact	-4.33%	-4.07%	-4.83%
Treatment Effect (3+ vs. 1)	-0.528	-0.151	-1.04
EITC Increase (2009$) (3+ vs 1)	$667	$615	$749
Treatment on Treated per $1000 (2009$)	-0.79	-0.25	-1.39
ToTper $1000 (2009$), % impact	-7.79%	-3.02%	-9.62%
Mean of the dependent variable	10.2	8.1	14.4
C. PARITY 3+ vs. PARITY 2			
Treatment Effect (3+ vs. 2)	-0.340	-0.023	-0.715
EITC Increase (2009$) (3+ vs 2)	$294	$281	$304
Treatment on Treated per $1000 (2009$)	-1.16	-0.08	-2.35
ToTper $1000 (2009$), % impact	-10.81%	-0.99%	-15.76%
Mean of the dependent variable	10.7	8.2	14.9

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).

Table5: 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 ($1000 of 95$)	-0.307***	-0.774***	-0.119**	-0.0842	-0.518***	-1.359***
	(0.0659)	(0.127)	(0.0528)	(0.107)	(0.115)	(0.179)
Parity * linear time		X		X		X
State x year controls	X	X	X	X	X	X
Mean of dependent variable	11.21	11.21	8.810	8.810	14.76	14.76
N	81782	81782	37335	37335	23746	23746
1st stage impact of maxcredit on ave EITC	0.330	0.330	0.330	0.330	0.330	0.330
Treatment on Treated per $1000 (2009$)	-0.66	-1.67	-0.26	-0.18	-1.12	-2.93
ToT per $1000 (2009$), % impact	-5.9%	-14.9%	-2.9%	-2.1%	-7.6%	-19.8%

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 6: Difference-in-Difference Estimates of OBRA93 on Other Birth Outcomes, Single Women with a High School Education or Less

	Mean Birthweight	Preterm birth <37 wks	Weight for age below 10th p.	5-minute Apgar <8	5-minute Apgar, average
Model: Parity 2+ vs. 1					
Parity2+ * After	9.95***	-0.191**	-0.362***	-0.185***	0.011***
	(2.05)	(0.0798)	(0.0759)	(0.048)	(0.003)
Mean of the dep. variable	3206	14.32	14.66	3.61	8.89
N	47,687	47,613	47,593	45,820	45,820
Model: Parity 3+ vs. 2					
Parity3+ * After	6.81***	-0.194**	-0.154**	-0.103**	0.007**
	(1.741)	(0.0866)	(0.0648)	(0.051)	(0.002)
Mean of the dep. variable	3206	15.76	13.98	3.38	8.91
N	47,687	35,417	35,404	34,076	34,076

Notes: Each column is from a separate DD regression 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. Observations differ across the outcomes due to incomplete data on these outcomes for all state-years (and some missing data on gestation).

Table 7: Difference-in-Difference Estimates of OBRA93 on Pregnancy Behaviors, Single Women with a High School Education or Less

	Prenatal care began before 3rd tri	Prenatal care, number visits	Any prenatal care	Kessner Index, Inadequate care	Kessner Index, Good or better	Any Smoking	Any Drinking
Model: Parity 2+ vs. 1							
Parity2+ * After	0.634***	0.123***	0.570***	-1.105***	0.135	-1.930***	-1.060***
	(0.175)	(0.0226)	(0.105)	(0.198)	(0.205)	(0.152)	(0.128)
N	47,246	47,110	47,110	46,957	46,957	45,554	46,128
Mean, dep. Var	91.45	10.27	96.92	12.06	58.21	25.74	2.603
Model: Parity 3+ vs. 2							
Parity3+ * After	0.652***	0.0984***	0.616***	-0.880***	0.347**	-1.163***	-1.086***
	(0.175)	(0.0160)	(0.119)	(0.168)	(0.119)	(0.205)	(0.161)
N	35,141	35,040	35,040	34,922	34,922	33,885	34,312
Mean dep var.	89.42	9.797	95.92	15.13	53.40	28.69	3.320

Notes: Each column is from a separate DD regression 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. Observations differ across the outcomes due to incomplete data on these outcomes for all state-years.

Table 8: Difference-in-Difference Estimates of OBRA93 on Health Insurance and Employment, Single Women with a High School Education or Less (Current Population Survey)

	Employed Last Year	HI Coverage: Mediaid	HI Coverage: Any Private	HI Coverage: Any
Model: any children vs. none				
anykids * after	0.077***	-0.068***	0.023***	-0.035***
	(0.008)	(0.008)	(0.009)	(0.008)
Mean Dep Var	0.70	0.29	0.38	0.71
N	51,317	51,317	51,317	51,317
Model: 2+ children vs. 1				
2+kids * after	0.060***	-0.037***	0.032***	-0.001
	(0.011)	(0.110)	(0.011)	(0.010)
Mean Dep Var	0.65	0.40	0.32	0.74
N	30,313	30,313	30,313	30,313
Model: 2+ Children vs. 1 (and presence of young child)				
2+kids * after	0.048***	-0.021	0.028**	0.009
	(0.015)	(0.016)	(0.014)	(0.014)
Mean Dep Var	0.59	0.49	0.23	0.73
N	16,744	16,744	16,744	16,744

Notes: Each column is from a separate DD regression applied to 1992-1998 CPS (covering income years 1991-1998). Observations are at the individual level and the models include fixed effects for effective tax year, parity, state, demographics, and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. Estimates are weighted using the CPS weights and are clustered on state. Standard errors are in parentheses.

Table9: Maximum Credit Estimates of EITC on Low Birth Weight using Alternative Assumptions about Timing, Single Women with a High School Education or Less

		Assign treatment based on exposure in:			
Model	When do I get the money?	1st trimester	2nd trimester	3rd trimester	3rd tri, labor supply
Assign EITC in 7th month (base case)	Refund-cash, all year			-0.307***	
				(0.0659)	
Assign EITC based on 3rd trimester	Refund-cash, all year			-0.304***	
				(0.0671)	
Assign EITC based on 2nd trimester	Refund-cash, all year		-0.314***		
			(0.0667)		
Assign EITC based on 1st trimester	Refund-cash, all year	-0.332***			
		(0.0688)			
Horse race all trimesters	Refund-cash, all year	-0.419*	-0.145	0.231	
		(0.237)	(0.351)	(0.246)	
All cash in Feb, based on 3rd trimester	Refund-cash, Feb only			-0.458***	
				(0.095)	
All cash in Feb, horse race all trimesters	Refund-cash, Feb only	-0.191	-0.318***	-0.581***	
		(0.129)	(0.134)	(0.136)	
Labor supply model, 3rd trimester	Labor supply/earnings				-0.263***
					(0.066)
Horse race rebate credit & labor supply	Refund-cash & labor supply			-0.605**	0.306**
				(0.126)	(0.132)

Notes: Each row 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.

Table 10: Difference-in-Difference Estimates of OBRA93 on Number and Composition of Births, Single Women with 12 years of education or less

		Fraction in state-year cell:						
	Log(Births)	black	white	Non-Hispanic	18-24	24-34	35+	Ed<12
Model: Parity 2+ vs. 1								
Parity2+ * After	-0.020	-0.010	0.009	-0.017*	-0.005**	-0.001	0.006***	-0.004
	(0.014)	(0.009)	(0.010)	(0.010)	(0.002)	(0.002)	(0.001)	(0.010)
Mean of Dep Var		0.323	0.641	0.738	0.648	0.300	0.0520	0.432
N	37639	1632	1632	1632	1632	1632	1632	1632
Model: Parity 3+ vs. 2								
Parity3+ * After	-0.017	-0.003	0.004	0.006***	-0.025***	0.016***	0.009***	-0.011**
	(0.016)	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.001)	(0.004)
Mean of Dep Var		0.370	0.593	0.726	0.523	0.402	0.075	0.484
N	25419	1224	1224	1224	1224	1224	1224	1224

Notes: Each column is from a separate DD regression using the Natality data for effective tax years 1991-1998. Column 1 use data at the year-state-parity-demographic cell level and the specification includes fixed effects for effective tax year, parity, state, and demographic group and state-year controls for Medicaid/SCHIP, welfare reform and unemployment rates. The remaining columns use data at the year-state-parity level and the specification includes fixed effects for effective tax year, parity, state, and state-year controls for Medicaid/SCHIP, welfare reform and unemployment. Estimates are clustered on state. Standard errors are in parentheses.