

Income, the Earned Income Tax Credit, and Infant Health[†]

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This paper uses quasi-experimental variation from federal tax reform to evaluate the effect of the EITC on infant health outcomes. We find that the EITC reduces the incidence of low birth weight and increases mean birth weight: a \$1,000 treatment-on-the-treated leads to a 2 to 3 percent decline in low birth weight. Our results suggest that the candidate mechanisms include more prenatal care and less negative health behaviors (smoking). Additionally, we find a shift from public to private insurance coverage, and for some a reduction in insurance overall, indicating a potential change in the quality and perhaps quantity of coverage. (JEL H24, I12, I38, J13)

In the United States 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 nonhealth 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

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¹The figures for the EITC are for tax year 2008, the most recent program data available (Internal Revenue Service 2011). TANF expenditures are for 2009 and consist of total cash expenditures (Office of Family Assistance 2009). Food stamp expenditures are for 2009 (US Department of Agriculture 2011). In the aftermath of the Great Recession, in 2010 and 2011, Food Stamps costs increased substantially to \$72 billion.

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 2011). The introduction and expansion of “in-work” assistance, developed in the United States, 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 US 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 health benefits of nonhealth 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.³

We use the US 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). Low birth weight status is additionally a key risk factor for infant mortality, with mortality being 24 times greater for those in the low birth weight category compared to other infants (Mathews and MacDorman 2013). We also explore other birth outcomes, such as preterm 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 (prenatal care). In addition, using the Current Population Survey, we explore the possible role played by employment-induced changes in earnings and maternal health insurance.

²For example, Almond, Hoynes, and Schanzenbach (2011) and Hoynes, Schanzenbach, and Almond (2013) 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, Rehkopf, and Xuan (2010), who examine the impact of the EITC on birth outcomes. These papers are discussed below.

³An important caveat to this is that the EITC expansion also impacted labor supply, and we are unable to fully disentangle the effects of this from the effects of income.

We use three quasi-experimental estimation strategies. First, we begin with a difference-in-differences analysis of the most recent and largest EITC reform, OBRA 1993. This commonly used approach in the EITC and labor supply literature leverages variation over time and across family size. Second, we use an event study design, along with comparison groups, to analyze the impacts of the 1993 expansion. This approach allows us to explicitly examine the validity of the control group by examining differences in pretrends 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 focus on single women and explore differences in estimates across groups more and less likely to be impacted by the EITC, using mother's education, age, and race. 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 the effects of the EITC expansion on after-tax income 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 or less) mothers, a policy-induced treatment on the treated increase of \$1000 in after-tax income is associated with a 0.17 to 0.31 percentage point decrease in low birth weight status. Given roughly 10.7 percent of treated children were low birth weight, this represents a 1.6 percent to 2.9 percent decline. Our results suggest that part of the mechanism for this improvement in birth outcomes is the result of more prenatal care, and less negative maternal health behaviors (smoking). Additionally, we find a small decrease in insurance coverage for some and affected families experienced a shift from public to private insurance. This change suggests that another potential mechanism is a change in the quality and, for some, quantity of 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 I we describe the EITC and the tax reform induced changes in the credit over our sample time frame. In Section II we review the background literature and discuss the possible channels through which the EITC may impact infant health. In Section III we describe the data and in Section IV we describe our empirical model. In Section V we present our results, Section VI we discuss mechanisms, and Section VII we present robustness checks. In Section VIII we provide a discussion and Section IX we conclude.

I. 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. Based on the 2011 EITC schedule, the flat region begins for one-child families who have an income of \$9,100 (12,780 for families with two or more children) and extends to a family income of \$16,690 (for both). When in the flat region, the family receives the maximum credit (in 2011 \$3,094 for one child and \$5,112 for two or more children), while in the phase-out region, the credit is phased out at the phase-out rate (16 and 21 percent). While the generosity of the credit varies with number of children, it does not vary with marital status; taxpayers pool their earnings and income and apply their combined resources to determine eligibility and credit amounts.⁴

The reach and importance of the credit has changed substantially over its history. Figure 1 presents the real maximum EITC credit (in 1999 dollars) by tax year and family size for our analysis 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.⁵

⁴Beginning 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.

⁵The OBRA93 reform changed the tax code in a number of other ways. Such changes included increasing the top marginal tax rate, removing the tax cap on Medicare, and reforms to how deductions are itemized. However, these reforms are not expected to differentially affect families with two or more versus one child, and were unlikely to influence the mothers in our high impact sample.

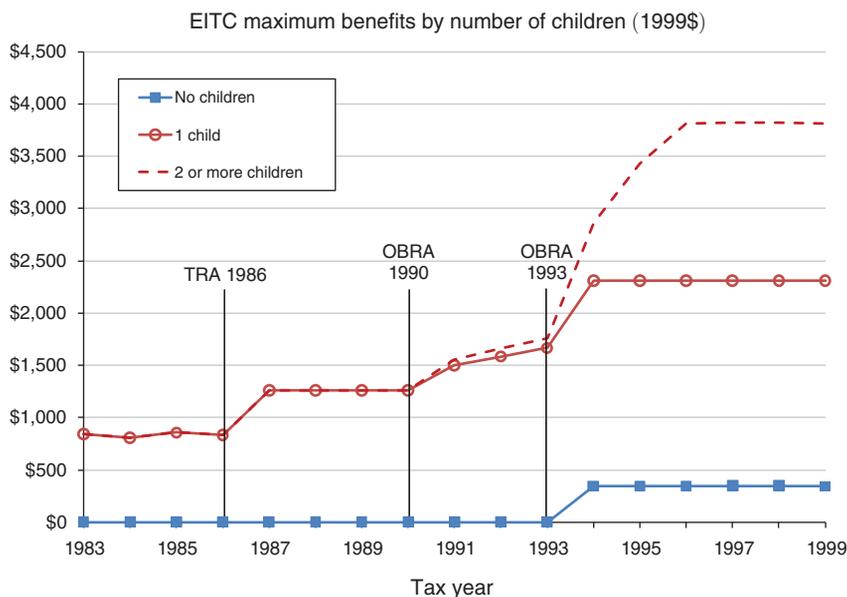


FIGURE 1. MAXIMUM CREDIT FOR FEDERAL EITC, BY TAX YEAR AND NUMBER OF CHILDREN

Source: Tax Policy Center (2011)

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 2011).

II. 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 socioeconomic 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 (Currie and Almond 2011; Currie 2011). As stated in the recent and comprehensive survey by Currie and Almond (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 and 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. 2012). 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.⁶

The increase in after-tax income could also lead to changed behaviors, such as smoking or drinking, which lead to well-documented decreases in birth weight (Currie, Neidell, and Schneider 2009). Consumption of these unhealthy products might increase if they are normal goods. Given that we also think infant health is a normal good, unhealthy behaviors may decrease if the income elasticity of infant health is large enough to dominate that of smoking. 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, Stroud, and Buka 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 and of itself, could 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

⁶Some studies provide credible evidence on the impact of income on dimensions of health other than infant health. These studies leverage income variation from a wide range of sources and examine, for example, unanticipated social security payments and mortality (Snyder and Evans 2006), 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).

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, Ermisch, and Francesconi 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. These employment effects may bring with them changes in infant health. Employment opportunities may make smoking less accessible through workplace smoking bans. Increases in employment (through changes in insurance) or income may increase access to prenatal care, which reduces smoking (Evans and Lien 2005).

In addition, because the EITC is tied to the presence and number of children, an expansion in the credit could theoretically lead to increases in fertility. On the other hand, the work-inducing aspect of the EITC suggests that it could lead to reductions in fertility 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).⁷

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

Ours is not the first paper to analyze the health impacts of the EITC. Evans and Garthwaite (2010) use a difference-in-differences 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-differences design, and concludes that the EITC leads to a 7 to 14 gram increase in average birth weight. Strully, Rehkopf, and Xuan (2010) find that the presence of state

⁷This 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).

EITCs leads to a 15 gram increase in average birth weight.⁸ Our paper makes several contributions to this emerging literature. First, we present results from several identification strategies, including an OBRA93 difference-in-differences 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 EITC-induced change in after-tax income 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.

III. Data

Our main data is the US Vital Statistics Natality Data, which consists of micro data on the full census of births from the National Center for Health Statistics. We use data covering births from 1983–1999. The data include birth weight, gender, live birth order (parity), and state and month of birth. There are also (limited) demographic variables including the age, race, ethnicity, education, and marital status of the mother. Education and ethnicity of the mother are missing in some state-years, but by 1992, all states provide education, and by 1993, all states provide ethnicity.⁹ 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 single mothers age 18 and older with singleton births who are not missing values for birth weight or parity.¹⁰ Single filers with children account for about three-quarters of the EITC credit payments (Bitler, Hoynes, and Kuka 2013).

We estimate models based on cells aggregated by state, year, and demographics of the mothers. In particular, we collapse the data to cells defined by state, year, parity of birth (first, second, third, fourth or greater birth to a mother), education of mother (<12, 12, 13–15, >16, missing), race of the mother (white, black, other),¹¹ ethnicity of the mother (Hispanic, non-Hispanic, missing), marital status (married, not married, missing), and age of mother (18–24, 25–35, 35+). For each cell we calculate average birth weight, average parity (for fourth or greater cell), the fraction

⁸Other studies use tax-reform induced changes in after-tax income to examine impacts on other child outcomes. Dahl and Lochner (2012) 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.

⁹In 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 started reporting education. By 1991 New York City reported education, 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.

¹⁰We limit to singleton births because of systematically lower birth weight for multiple births. Our results are not sensitive to this sample selection.

¹¹The other race category includes Asians, Native Americans, and other nonwhite/nonblack races.

of births below 2,500 grams (also the fraction below 1,500, 2,000, 3,000, 3,500, and 4,000 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.”¹²

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 (2013), 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.¹³ This is motivated by evidence that the third trimester of pregnancy is important for birth weight production.¹⁴

Combining these “cash in hand” and sensitive developmental stage assumptions, we assign births to the 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

¹²As with general individual tax exemptions, a new birth is counted as a child for the tax year regardless of when they are born (Department of Treasury 2011). However, as we describe below, given our “cash on hand” assumption, the birth is assigned the tax schedule for the tax year one or two years prior to the calendar year of the birth. Therefore, the number of children that is relevant for assigning the appropriate tax schedule is the number of children in the family prior to this birth.

¹³By using three months prior to birth, we assume pregnancy is a nine-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).

¹⁴For example, the cohort exposed to the Dutch Famine in the third trimester had lower average birth weight than cohorts exposed earlier in pregnancy (Painter, Roseboom, and Bleker 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, Stein, and Susser (1980).

TABLE 1—ILLUSTRATION OF CASH-IN-HAND ASSIGNMENT OF EFFECTIVE TAX YEAR

Birth month and year		Beginning of third trimester		Effective tax year
Month	Year	Month	Year	
January	1990	10	1989	1988
February	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
September	1990	6	1990	1989
October	1990	7	1990	1989
November	1990	8	1990	1989
December	1990	9	1990	1989
January	1991	10	1990	1989
February	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
September	1991	6	1991	1990
October	1991	7	1991	1990
November	1991	8	1991	1990
December	1991	9	1991	1990
January	1992	10	1991	1990
February	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
September	1992	6	1992	1991
October	1992	7	1992	1991
November	1992	8	1992	1991
December	1992	9	1992	1991

“treatment” spilling over into the last measured “control” year (or vice-versa), and this should attenuate our estimated impacts.

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, Phillips, and O’Connor 2000; Gao, Kaushal, and Waldfogel 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 VIIA. 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, and age of mother).

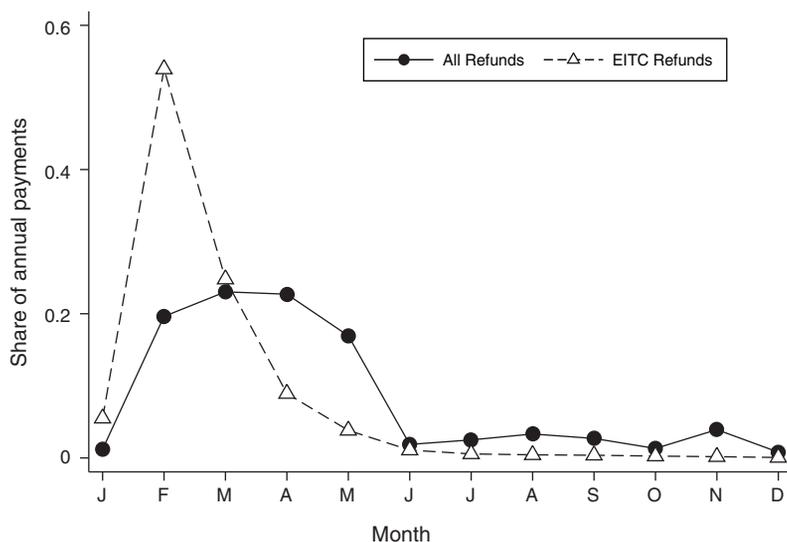


FIGURE 2. DISTRIBUTION OF TAX REFUNDS, BY CALENDAR MONTH

Note: Figure shows 10 years averages using Monthly Treasury Statements 1998–2007.

Source: Reprinted from Lalumia (2013)

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.¹⁵ Our primary identification strategy is based on parity-by-year. However, we wish to control for demographic group flexibly with fixed effects (and sometimes to stratify on demographic group). Also, some of our potentially confounding policy control variables are defined at the state-year level. These two facts motivate our choice of cell aggregation. We have explored alternative choices of cell aggregation, and these do not impact our results.

Our analysis also utilizes the March Current Population Survey (CPS). We use this data to quantify the effect of the EITC expansion on family after-tax income, employment and health insurance (the vital statistics does not report earnings, income or EITC receipt). We use after-tax income to interpret the magnitude of our findings on low birth weight. Our CPS analysis uses a sample of single women ages 18–45, and for each woman we identify the income and family members that comprise her “tax filing unit.”¹⁶ Our measure of family pretax income includes earnings

¹⁵The 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).

¹⁶We assign tax filing units as follows. For single women who are heads of household or heads of a subfamily, we assign to the woman the income and number of children in her family (either primary family if head of household or subfamily if head of subfamily). If the woman is not a head of household or head of subfamily, we assign her zero children and her income is limited to her own personal income.

and government cash transfers.¹⁷ Tax payments and, in particular, the EITC are not captured in the CPS. We use NBER's TAXSIM¹⁸ to assign income taxes and the amount of the EITC, assuming complete take-up of the credit.¹⁹ Using this we construct after-tax (and transfer) income equal to gross pretax income less federal income taxes (including the EITC). As discussed above, the woman is "treated" by the EITC expansion prior to birth of the child. The CPS does not report "future births" and instead we use women 18–45 as the group "at risk" of having a child. Thus, a woman with one child is assigned "parity 2" because should she have a birth, it would be her second. More generally, in the CPS, parity is assigned to be equal to her number of children plus one.

IV. Empirical Methods

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

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

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 . Demographic groups j are defined by education category by age group by race by ethnicity as defined above in Section II. We include data for effective tax years 1991–1998, and *After* equals one for effective tax years 1994 through 1998.²⁰ X_{st} includes controls for unemployment rate, welfare reform and Medicaid or SCHIP eligibility, and we include fixed effects for demographic group ϕ_j , parity γ_p , state η_s , and 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. We have also explored alternative levels of clustering, including: demographic group level, parity-by-year, two way clustering on demographic group and state, and two way clustering on parity and state. Statistical inference results are robust to these alternative clustering choices.

In our first specification, we compare second- 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

¹⁷Government cash transfers include social security, SSDI, UI, workers compensation, veteran's benefits, AFDC/TANF, SSI, and general assistance. In the CPS food stamp benefits are recorded at the household level, and it is not straightforward to assign food stamp income at the family level. Our government transfer (and after tax income) measure thus exclude Food Stamps. We have run models in which we assign household food stamp receipts to all families in the household, with qualitatively similar results.

¹⁸We use marital status, number of dependents, and income and earnings to calculate taxes (including EITC) based on tax filing units described above. Providing this information, TAXSIM returns the EITC and other federal tax obligations. The NBER TAXSIM model is described in Feenberg and Coutts (1993).

¹⁹Scholz (1994) analyzes tax year 1990 and estimates take-up rates between 80-86 percent. 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.

²⁰In this design, we do not include data on years prior to 1991 because of the prior expansion in OBRA90.

were exposed to the relatively small childless EITC credit. To examine predictions concerning the differential expansion in the two-child versus one-child EITC, in our second specification we include $After \times Parity2$ and $After \times Parity3plus$ (maintaining first births as the control). Finally, in our third specification, we limit the sample to second- and higher order births and include $After \times Parity3plus$, thus, effectively using second 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 pretrends.

In our second quasi-experimental model we take advantage of the full set of tax reforms that 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 (first, second, third or more). We then estimate

$$(2) \quad Y_{pjst} = \alpha + \delta Maxcredit_{pt} + \beta X_{st} + \gamma_p + \eta_s + \delta_t + \phi_j + \varepsilon_{pjst}$$

$Maxcredit$ 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.

V. Results

A. OBRA 93 Treatment—Main Estimates for Low Birth Weight

We begin by presenting results for the OBRA93 difference-in-differences 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). For example, for the 1998 tax year (using the CPS augmented with TAXSIM generated federal income tax calculations), we find that about 42 percent of single women 18–45 with a child under age 3 (a 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

TABLE 2—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF OBRA93 ON LOW BIRTH WEIGHT, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

Model	Parity 2+ versus 1		Parity 2, 3+ versus 1		Parity 3+ versus 2	
Parity2+ × After	-0.355*** (0.075)	-0.354*** (0.074)				
Parity=2 × After			-0.164** (0.072)	-0.164** (0.072)		
Parity3+ × After			-0.529*** (0.091)	-0.528*** (0.090)	-0.342*** (0.069)	-0.340*** (0.068)
State × year control variables		X		X		X
Mean of the dependent variable	10.2	10.2	10.2	10.2	10.7	10.7
Observations	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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

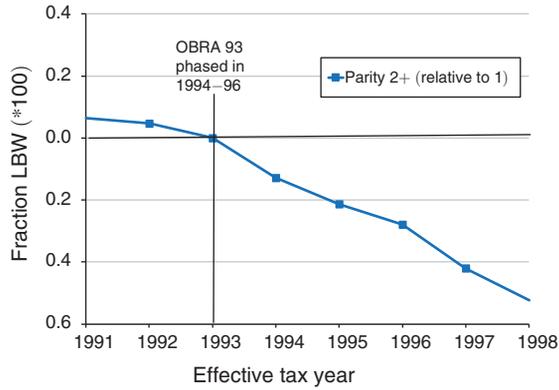
*Significant at the 10 percent level.

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 third and higher order births to second births, which eliminates the possibility of first 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).²¹

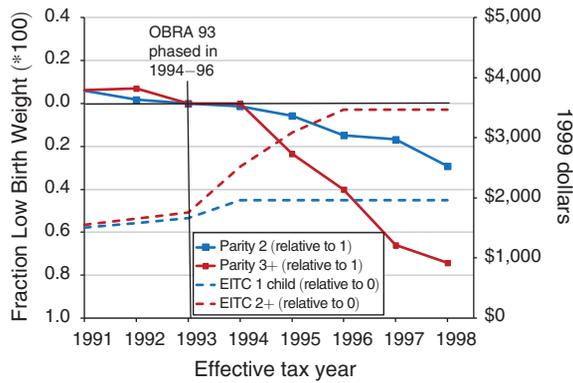
A possible concern is that the regression results from Table 2 could be driven by preexisting differential trends in health by parity of birth. For example, if the incidence of low birth weight for higher birth orders was already declining before the OBRA93 expansion then our estimates could be biased 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

²¹Table 2 shows a total of 47,687 observations. The total number of cells for the high impact sample are 88,128. Many cells are empty, especially due to the low incidence of missing demographics. See Appendix B for more information.

Panel A. Estimates for Parity 2+ versus Parity 1



Panel B. Estimates for Parity 2 and Parity 3+ versus Parity 1



Panel C. Estimates for Parity 3+ versus Parity 2

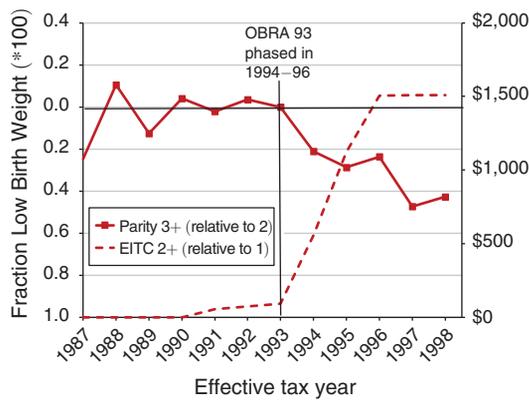


FIGURE 3. EVENT TIME ESTIMATES OF OBRA93 ON LOW BIRTH WEIGHT AND EITC INCOME, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

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 are based on the March CPS and the EITC is calculated using TAXSIM. See text for details.

prior to the OBRA93 expansion. The figure suggests there was little to no pretrend 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 3, panel B 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 3, panel A, the pre-trend is quite flat. We include on Figure 3, panel B 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 (second versus third 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.²²

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 3, panel C 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 preperiod shows no confounding pre-trend and there is a sharp decline in the incidence of low birth weight births corresponding to the increase in eligibility for maximum EITC benefits. As with Figure 3, panel B, 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.²³

We provide additional context for these results in online Appendix Figure 1 panels A–D. In these figures we show raw trends in the rates of low birth weight by parity, over the period 1981–1999. In each figure we show low birth weight probabilities relative to the 1993 level. Online Appendix Figure 1, panel A has results for our high-impact group, and shows two main findings. First, the changes we observe in our experimental period occur within long-run decreases in rates of low birth weight for this population (although in the pre-1991 period there are many other policies changing, including earlier expansions of the EITC). Second, the raw trends for 1991–1998 show the same pattern as in our main event study results in Figure 3, panels A–C; thus these results are not strongly impacted by the covariate controls in the model. In contrast, the trend for all births is one of increasing rates of low birth weight (online Appendix Figure 1, panel B). The long-run trend for white high-impact mothers (online Appendix Figure 1, panel C) is similar to that

²² In our panel fixed effects model below we take advantage of the full set of credit expansions.

²³ 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).

TABLE 3—DIFFERENCE-IN-DIFFERENCES 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
<i>Panel A. Model: Parity 2+ versus 1</i>				
Parity2+ × After	−0.132* (0.072)	−0.728*** (0.143)	−0.413*** (0.099)	−0.130* (0.070)
Mean of the dependent variable	8.14	14.43	11.24	7.04
Observations	21,775	13,780	26,066	14,823
<i>Panel B. Model: Parity, 2, 3+ versus 1</i>				
Parity=2 × After	−0.114* (0.065)	−0.310** (0.144)	−0.187** (0.090)	−0.0600 (0.078)
Parity3+ × After	−0.151 (0.093)	−1.040*** (0.160)	−0.625*** (0.123)	−0.191** (0.087)
Mean of the dependent variable	8.14	14.43	11.24	7.04
Observations	21,775	13,780	26,066	14,823
<i>Panel C. Model: Parity 3+ versus 2</i>				
Parity3+ × After	−0.0231 (0.071)	−0.715*** (0.121)	−0.407*** (0.094)	−0.121 (0.092)
Mean of the dependent variable	8.23	14.92	12.12	6.78
Observations	16,247	10,273	19,611	10,951

Notes: Each column is from a separate DD regression of the percent low birth weight 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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

for all high-impact mothers, while for high-impact black mothers (online Appendix Figure 1, panel D), the trends are less monotonic. All the figures show that there are large changes in parity gaps in low birth weight that occur during the 1980s. It would be valuable, but outside of the scope of this study, to understand these changes better.

Table 3 shows heterogeneity in effects by race and Hispanic origin within the high-impact sample.²⁴ 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 decline relative to a mean of 8.1 percent). Interestingly, smaller treatment effects are experienced by Hispanic mothers than by non-Hispanic mothers (−0.13 versus −0.41 in the second+/first parity model). This is consistent with the finding that Hispanic children 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

²⁴We do not present models for "other" race (12,132 obs) or "missing" Hispanic status (6,798 observations). With these two additional subgroups, the observations for race and ethnicity sum to the total number of observations in Table 2.

larger fraction of Hispanics are undocumented immigrants who do not qualify for the EITC (Department of the Treasury 2011).²⁵

To interpret these results, we examine the effects of the EITC expansion on after-tax incomes of single, low-education women. Here, we use the CPS because, as we discussed above, neither income nor EITC benefits is reported in the vital statistics. In particular, we use data on single, low-education women ages 18–45 in the March CPS for 1992–1999 (corresponding the tax years 1991–1998). For each woman in the sample, we construct a measure of after-tax (family) income equal to earnings plus government cash transfers minus federal income taxes, including the EITC (details are above in Section III). We use the CPS data and estimate a difference-in-differences model identical to that estimated in the vital statistics data (e.g., equation 1) with the only difference being the use of micro-data rather than the cells we use with the vital statistics. As described in Section III, in the CPS “parity” is equal to the number of children plus one (if you had had one child and you had a new birth it would be your second child).

Combining results on income from the CPS with results on birth weight from the vital statistics helps us to interpret the improvements in low birth weight in light of the economic impact of the expansions. This is somewhat in the spirit of a Two Sample Instrumental Variables estimation, and in discussing these results, we use “treatment on the treated” (TOT) language. However, we believe that the exclusion restriction required for a strict interpretation of this as the causal effect of income is too strong (for example, as discussed in Section III, it is plausible that the EITC expansion could impact infant health through other channels such as labor supply). And so the spirit in which we combine results from the CPS and vital statistics samples is one of “providing context.” Even so, we would like for the populations represented in the two datasets to be similar. They are not the same, since the CPS is not a sample of women giving birth. And it is possible that the income responsiveness of women who will soon give birth may be different than that of other women. To address this concern, we have estimated a propensity-score re-weighting variation on our CPS income models. In this variation, we re-weighted the CPS observations to be representative of the age, education, race, and ethnicity profiles of the mothers in the vital statistics sample. This re-weighting had little impact on our results and as such we use the unweighted CPS results, to keep things as simple as possible.²⁶

Before presenting the TOT results, we begin by discussing the “first-stage” effect of the EITC expansion on family economic outcomes. These results are presented in panel A of online Appendix Table 1. We begin, in column 1, by presenting

²⁵We also explored differences by gender of birth, finding no significant differences in birth outcomes. Dahl and Lochner (2012) 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.

²⁶The main exception, in which the re-weighting *did* matter was for black mothers, when comparing parity three or more to parity two households. For this group, the unweighted model estimates an increase in after tax income of \$628 (std. err. \$656). The weighted model estimates a loss of income of \$201 (std. err. \$564). Although these point estimates are economically meaningfully different, they are both noisy, and statistically indistinguishable from zero or each other.

estimates of the expansion on women's employment (at all last year). It is encouraging that these show large and statistically significant effects on employment given this well-established finding in the literature (Eissa and Liebman 1996; Meyer and Rosenbaum 2001). The remaining columns present estimates of the EITC expansion on earnings, select government transfers of interest (AFDC/TANF, SSI, Food Stamps), the EITC, and total after tax income. The table shows that the EITC expansion led to meaningful and statistically significant increases in earnings and EITC credits. The expansion was also associated with a large reduction in public assistance, including AFDC/TANF and Food Stamps. This is consistent with the evidence on the transition from welfare to work that resulted from the combination of welfare reform and the expansion of the EITC (Grogger 2003, Patel 2011). Finally, the results show that the expansion was associated with a statistically significant increase in SSI income. This, on first glance, seems counterintuitive from the perspective of the EITC's expected effects. However, this period did see a large increase in SSI receipt (Duggan and Kearney 2007) and part of the growth of the SSI child caseload appears to be a response to other changes in the safety net, including welfare reform (Schmidt and Sevak 2004). Overall, the take-away from online Appendix Table 1 is that the EITC expansion led to substantial effects on after-tax income, with the largest changes coming from the increase in earnings, the increase in the EITC, and the reduction in AFDC/TANF. For example, the 2+/1 parity results show that the expansion leads to an increase of \$1,401 in earnings, \$665 in the EITC, and a reduction in AFDC/TANF of \$698; overall after-tax income increases by \$1,558. Notably, looking at the increase in the family's EITC alone would yield an incomplete, and underestimated, estimate of the change in family resources.²⁷

We combine the estimated impacts on income along with the effects on infant health in Table 4. We begin with panel A, which presents results corresponding to our model comparing second and higher order births to first births, and column 1, which presents these results for the high impact sample of single women with 12 or fewer year of education. The first row repeats the results from Table 2, with the estimated impact of the EITC expansion on low birth weight (0.354 percentage point reduction). The second row shows estimates of the impact of the expansion on after-tax income from online Appendix Table 1 (\$1,558 increase). The third row presents the impact of a \$1,000 treatment on the treated (TOT) estimate obtained by dividing the first row by the second row and then multiplying by 1,000, and the fourth row converts this to a percent impact by dividing by group mean. For the full high-impact sample, our results imply a TOT per \$1,000 impact of 0.23 percentage points, or a 2.2 percent reduction in LBW.²⁸

This IV-type interpretation of our results suggests what the impact of EITC and EITC-induced income would be, under the assumptions that income is the only mechanism through which the policy impacted birth outcomes (and take-up of the EITC is 100 percent). We view this as an overly restrictive assumption, in light of a

²⁷We thank two referees for making this point.

²⁸The percent reduction is calculated from a 0.23 percentage point TOT impact on a mean low birth weight rate of 10.2 percent.

TABLE 4—MAGNITUDES IN OBRA93 MODELS, LOW BIRTH WEIGHT

	All	White	Black
<i>Panel A. Parity 2+ versus Parity 1</i>			
Treatment effect	-0.354	-0.132	-0.728
Increase in after tax income (2009\$)	\$1,558	\$1,543	\$956
Treatment on treated per \$1,000 (2009\$)	-0.23	-0.09	-0.76
ToTper \$1,000 (2009\$), percent impact	-2.2%	-1.1%	-5.3%
Mean of the dependent variable	10.2	8.1	14.4
<i>Panel B. Parity=2, Parity 3+ versus Parity 1</i>			
Treatment effect (2 versus 1)	-0.164	-0.111	-0.31
Increase in after tax income (2009\$) (2 versus 1)	\$991	\$961	\$524
Treatment on treated per \$1,000 (2009\$)	-0.17	-0.12	-0.59
ToTper \$1,000 (2009\$), percent impact	-1.6%	-1.4%	-4.1%
Treatment effect (3+ versus 1)	-0.528	-0.151	-1.04
Increase in after tax income (2009\$) (3+ versus 1)	\$2,047	\$2,134	\$1,189
Treatment on treated per \$1,000 (2009\$)	-0.26	-0.07	-0.87
ToTper \$1,000 (2009\$), percent impact	-2.5%	-0.9%	-6.1%
Mean of the dependent variable	10.2	8.1	14.4
<i>Panel C. Parity 3+ versus Parity 2</i>			
Treatment effect (3+ versus 2)	-0.340	-0.023	-0.715
Increase in after tax income (2009\$) (3+ versus 2)	\$1,081	\$1,146	\$628
Treatment on treated per \$1,000 (2009\$)	-0.31	-0.02	-1.14
ToTper \$1,000 (2009\$), percent impact	-2.9%	-0.2%	-7.6%
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 \$1,000 increase in EITC income (row 1/row 2 \times 1,000). Row 4 provides the percent TOT impact (row 3/mean).

possible labor supply channel, but still think that the numbers offer a useful scaling of the coefficients.

Panel B of Table 4 presents similar results for our separate comparisons of second and third and higher order births against first children, and panel C presents estimates for the comparisons of third and higher order births compared to second births. Among all high-impact women, panel B in Table 4 shows that the EITC treatment for third and higher order births is larger than for second births (\$2,047 versus \$991). Interestingly, the scaled estimates (TOT) are also larger for the third than second births (2.5 percent versus 1.6 percent reduction per \$1,000 of treatment). Table 4 thus helps us to understand why the coefficients in Tables 2 are larger for third than for second births: it is both because the third births received a larger treatment and also because their outcomes appear to be more sensitive to the treatment. Overall, the results for all high-impact women show a narrow range of estimates of the TOT per \$1,000 of 1.6 to 2.9 percent (reduction in LBW).

The remainder of the table presents similar estimates by race for the high-impact sample. As shown in Table 3, the effects of the EITC expansion on LBW is larger for black, single, low-educated women, compared to white, single, low-educated women. Interestingly the CPS analysis, as shown in panels B and C of Appendix Table 1, shows a smaller first-stage effect on after-tax income for black women (compared to white). Thus, the estimated treatment on the treated results are larger for black compared to white women. For example, the comparison of second and

higher births compared to first births shows a 5.3 percent reduction for blacks and a 1.1 percent reduction for whites (percent impact of a \$1,000 TOT).

This finding of a larger effect of after-tax income on LBW for blacks versus whites could be explained by a couple of factors. First, even within this “high-impact” group, the black women are more disadvantaged than the white women. The black high-impact group has almost double the baseline low birth weight (14.4 versus 8.1 percent, Table 4), and this may imply a differential sensitivity of low birth weight to underlying health inputs. Additionally, the black high-impact sample has 20 percent lower baseline after-tax income compared to the whites (online Appendix Table 1), and thus the effect of an increase in income may be larger for this group. Second, online Appendix Table 1 presents the “first stage” effects by race and shows that the larger first stage for high-impact whites is primarily the result of a larger effect of the EITC expansion on earnings (compared to black high-impact women). The changes in government transfers and the EITC are similar across these groups. These differential results by race may therefore suggest that there are effects on birth weights that operate through work behavior.²⁹

B. Impacts of OBRA93 across Subgroups

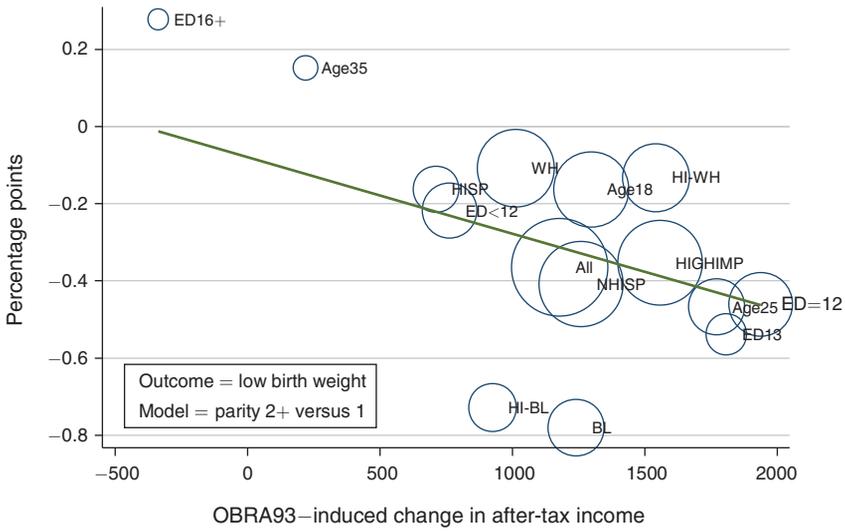
To obtain more insight into these effects, we have estimated the results on subgroups of the data. For this analysis we use all single women (regardless of their education). Within the full sample we estimate models on the following subgroups: education categories (<12, 12, 13–15, 16 or more); race (white, black;³⁰ ethnicity (Hispanic, non-Hispanic); age group (18–24, 25–34, 35+); and (for continuity) the high-impact sample. Note that we run subgroup regressions on most, but not all, of the demographic categories (we omit the relatively small cells with “other” races and those “missing” information on Hispanic ethnicity). 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-differences impact on after-tax 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 after-tax 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 ($x = \$1,558$, $y = -0.354$). 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+ versus 1 model (Figure 4a). Subgroups with large estimated impacts on low birth weight are also those with large impacts on the after-tax income, while subgroups with smaller increases in after-tax income also have smaller impacts on low birth weight. You can also see “placebo” estimates, for example, the estimate for highly

²⁹Interestingly, the literature on the effects of the EITC on labor supply does not include estimates by race. When we broaden the sample to all single women, the first stage for blacks is larger than whites (as can be seen in Figure 4 panel A below).

³⁰We also perform this analysis on the “other” race subgroup. Since the sample size for these cells are small and the standard errors are large, we do not include them in our subgroup analysis.

Panel A. Estimates for Parity 2+ versus Parity 1



Panel B. Estimates for Parity 3+ versus Parity 2

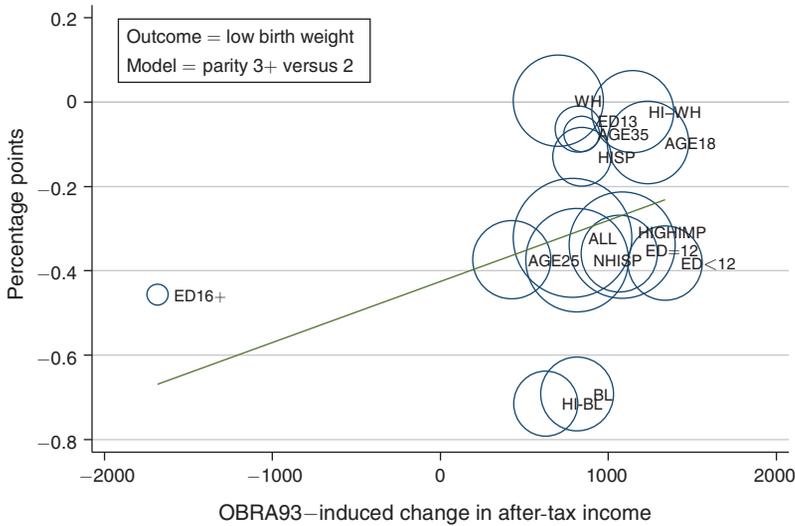


FIGURE 4. DEMOGRAPHIC SUBGROUP ESTIMATES OF OBRA93 AND MAGNITUDE OF EITC TREATMENT

Notes: Each point on the graph represents DD regression estimates for a specified demographic subgroup. The x-axis provides the DD estimate of after-tax 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.

educated women (labeled “ed16+”), which has a very small EITC treatment and a wrong signed (insignificant) and small coefficient on low birth weight.

The parity 3+ versus 2 model (Figure 4, panel B) is more mixed—college educated mothers have a “wrong-signed” income effect, which influences the look of the graph beyond their importance in the birth sample. Among the other groups,

TABLE 5—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 (\$1,000 of 95\$)	-0.307*** (0.0659)	-0.774*** (0.127)	-0.119** (0.0528)	-0.0842 (0.107)	-0.518*** (0.115)	-1.359*** (0.179)
Parity \times linear time		X		X		X
State \times year controls	X	X	X	X	X	X
Mean of dependent variable	11.21	11.21	8.81	8.81	14.76	14.76
Observations	81,782	81,782	37,335	37,335	23,746	23,746
First stage impact of maxcredit on average income	1.29	1.29	1.29	1.29	1.29	1.29
Treatment on treated per \$1,000 (2009\$)	-0.17	-0.43	-0.07	-0.05	-0.29	-0.75
ToT per \$1,000 (2009\$), percent impact	-1.5%	-3.8%	-0.7%	-0.5%	-1.9%	-5.1%

Notes: Each column is from a separate regression of the percent low birth on the EITC maximum credit applied to Natality data for effective tax years 1984–1998. Observations are at the year-state-parity-demographic cell level. All models include fixed effects for effective tax year, parity, state, demographic group and state-year controls for Medicaid/SCHIP, welfare reform, and unemployment rates. Estimates are weighted using the number of births in the cell and are clustered on state. Standard errors are in parentheses. The “first 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 \$1,000 (estimate/first stage \times 1,000) and the last row provides the percent TOT impact.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

all experience both improvements in income and improvements in infant health. These results parallel those in Table 3, with black moms experiencing the greatest health returns, and white and Hispanic moms experiencing more muted birth weight improvements.

C. 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 the maximum credit (in 1,000s of 1995 US dollars), 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 \$1,000 (1995 US dollars) 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-specific linear trends (in year). 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-differences results (Table 4) because here we are using the maximum benefit program parameter. To facilitate comparison to the difference-in-differences results we return to the CPS data linked with TAXSIM and estimate a “first stage” model where we regress after-tax income on the maximum credit (along with controls for year, parity, demographics). For the full high-impact sample, the point estimate suggests that a \$1,000 increase in the maximum credit leads to a \$1,290 increase in after-tax income (the tables shows the estimate of 1.29), reflecting the fact that the EITC “crowds” in earnings. We use this to construct an IV-type estimate of the impact of \$1,000 of after-tax 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-differences estimates: a \$1,000 of EITC expansion reduces the incidence of low birth weight by 1.5 percent for the full high-impact sample (compared to 2.2 percent for the DD), 0.7 percent for whites (1.1 percent in the DD), and 1.9 percent for blacks (5.3 percent for the DD).

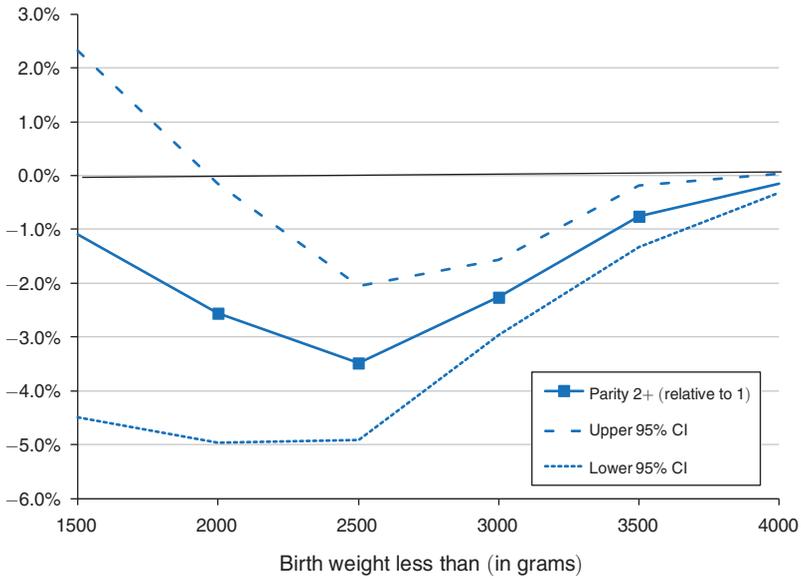
Given the qualitative similarity of the panel fixed effects results to the OBRA93 results, we return to the OBRA93 design for the remainder of the paper.

D. 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 (2011), we estimated a series of difference-in-differences 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 percent confidence intervals for the second+/first parity model (Figure 5, panel A) and the third+/second parity model (Figure 5, panel B). 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 second+/first 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 online Appendix Table 2, these impacts are larger for Black mothers (18 grams), and for Non-Hispanic mothers (11 grams). Online Appendix Table 3 presents our birth weight results along with the after-tax income results, so as to be able to gauge the magnitudes of our estimated impacts. For our basic 2+/1 model, we

Panel A. Estimates for Parity 2+ versus Parity 1



Panel B. Estimates for Parity 3+ versus Parity 2

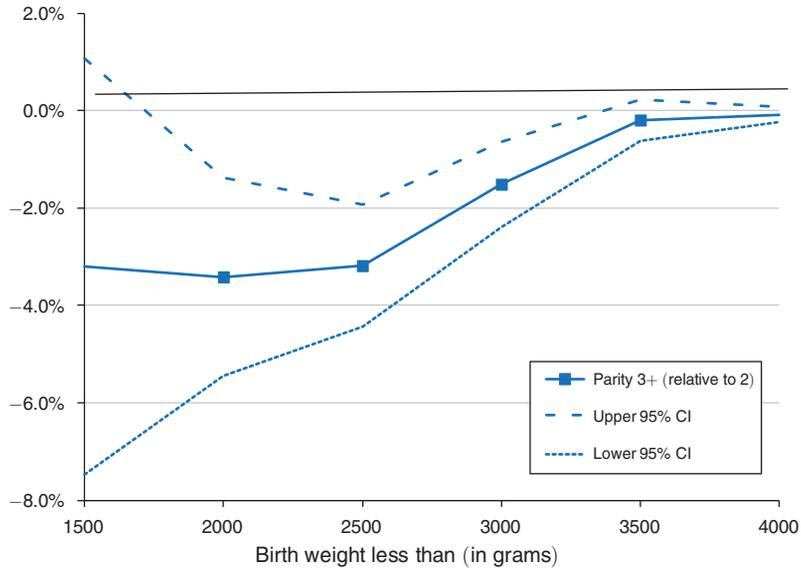


FIGURE 5. DIFFERENCE-IN-DIFFERENCES ESTIMATES OF OBRA93 ON THE DISTRIBUTION OF BIRTH WEIGHT, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

Notes: The graph shows estimates and 95 percent confidence intervals for the difference-in-differences 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.

TABLE 6—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF OBRA93 ON OTHER BIRTH OUTCOMES, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

	Mean birth weight	Preterm birth <37 wks	Weight for age below 10th p.	5-minute Apgar <8	5-minute Apgar, average
<i>Panel A. Model: PARITY 2+ versus 1</i>					
Parity2+ × After	9.95*** (2.05)	-0.191** (0.0798)	-0.362*** (0.0759)	-0.185*** (0.048)	0.011*** (0.003)
Mean of the dependent variable	3,206	14.32	14.66	3.61	8.89
Observations	47,687	47,613	47,593	45,820	45,820
<i>Panel B. Model: PARITY 3+ versus 2</i>					
Parity3+ × After	6.81*** (1.741)	-0.194** (0.0866)	-0.154** (0.0648)	-0.103** (0.051)	0.007** (0.002)
Mean of the dependent variable	3,206	15.76	13.98	3.38	8.91
Observations	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).

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

find that an increase of \$1,000 of income (2009\$ TOT) is associated with an increase in mean birth weight of 6.4 grams for a 0.2 percent effect. This TOT percent effect is significantly smaller than the 2.2 percent impact for low birth weight and 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 (\$1,000 TOT effect is 18.7 grams or a 0.6 percent increase) than for White mothers (\$1,000 TOT effect is 2.8 grams or a 0.1 percent increase), and is similar for third births (6.3 grams or 0.2 percent) relative to second births.³¹

In addition to birth weight, we can examine other birth outcomes such as preterm birth (born before the 37th week of pregnancy), small for gestational age (below the tenth percentile of birth weight for gestational age), and Apgar score (score below 8 [of 10]).³² 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

³¹We also estimated event study models for mean birth weight and we present those in online Appendix Figure 2. The event study for parity 2+ versus 1 (Appendix Figure 2a) suggests a possible preexisting trend. Online Appendix Figure 2b splits this out by parity. Here we see (solid lines) that there is a modest (but similar) pretrend 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. Online Appendix Figure 2c presents the event study graph from our 3+/2 model. The pretrends appear to be flat, and there is a jump in mean birth weight (the outcome) following the expansion.

³²Due to data limitations we are 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, which are critical for our OBRA93 expansion. The unlinked death records are not sufficient because they do not record the child's parity, which is also critical to our design.

improvement in five 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 online Appendix Table 2, the estimates are consistently larger for Blacks and Non-Hispanics compared to whites.

VI. 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 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-differences 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 percent do not receive prenatal care before their third trimester, and 4 percent 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 prenatal care.³³

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 percent. We find reductions in “any drinking” of about 1.1 percentage points (compared to mean drinking rates of about 3.3 percent).

Given that the EITC leads to increases in employment, one possible pathway for affecting infant health is through changes in maternal health insurance. The vital statistics data 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 difference-in-differences models. Our results on health insurance for the

³³This index is based on trimester of initiation of prenatal care and number of visits, normalized by gestational age. It is coded as “adequate” (which requires first trimester initiation, and e.g., 15 or more prenatal care visits for a birth at 40 weeks gestation), “inadequate” (which requires third trimester initiation, and e.g., four or fewer prenatal care visits for a birth at 40 weeks gestation), and “indeterminate.” For specific classification see the online Appendix in McDonald and Coburn (1988).

TABLE 7—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF OBRA93 ON PREGNANCY BEHAVIORS, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

	Prenatal care began before 3rd trimester	Prenatal care, number visits	Any prenatal care	Kessner index, inadequate care	Kessner index, good or better	Any smoking	Any drinking
<i>Panel A. Model: Parity 2+ versus 1</i>							
Parity2+ × After	0.634*** (0.175)	0.123*** (0.0226)	0.570*** (0.105)	-1.105*** (0.198)	0.135 (0.205)	-1.930*** (0.152)	-1.060*** (0.128)
Observations	47,246	47,110	47,110	46,957	46,957	45,554	46,128
Mean dependent variable	91.45	10.27	96.92	12.06	58.21	25.74	2.603
<i>Panel B. Model: Parity 3+ versus 2</i>							
Parity3+ × After	0.652*** (0.175)	0.0984*** (0.0160)	0.616*** (0.119)	-0.880*** (0.168)	0.347** (0.119)	-1.163*** (0.205)	-1.086*** (0.161)
Observations	35,141	35,040	35,040	34,922	34,922	33,885	34,312
Mean dependent variable	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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

TABLE 8—DIFFERENCE-IN-DIFFERENCES 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: Medicaid	HI coverage: any private	HI coverage: any
<i>Panel A. Model: Parity 2+ versus 1</i>				
anykids × after	0.073*** (0.008)	-0.056*** (0.007)	-0.010 (0.009)	-0.041*** (0.008)
Mean dependent variable	0.71	0.29	0.38	0.71
Observations	51,317	51,317	51,317	51,317
<i>Panel B. Model: Parity 3+ versus 2</i>				
2+kids × after	0.077*** (0.012)	-0.037*** (0.013)	0.036*** (0.012)	0.003 (0.012)
Mean dependent variable	0.66	0.47	0.28	0.77
Observations	22,124	22,124	22,124	22,124

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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

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. Examining the 3+/2 model, we estimate a 3.7 percentage point reduction in Medicaid coverage and a 3.6 percentage point increase in private insurance; with zero impact for any health insurance coverage. Examining the 2+/1 models, we find a 5.6 percentage point reduction in Medicaid, no significant change in private insurance, and an overall 4.1 percentage point reduction in any insurance.³⁴

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,³⁵ 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.³⁶ 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. Bolstering our belief that prenatal care and other behaviors are part of the mechanism for our results, we find that the effects of the EITC expansion on prenatal care and smoking are larger if we time the EITC treatment to be earlier in the pregnancy (e.g., first or second trimester rather than our default definition of third trimester exposure). See online Appendix Table 4 for these results.

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.

³⁴ Meyer and Sullivan (2008) find a similar result. 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 children against those with one child.

³⁵ Almond, Chay, and Lee (2005, Table VI) estimate that smokers have 3.5 percentage points higher incidence of low birth weight. We estimate in Table 7 a reduction in smoking of 1.2–1.9 percentage points, 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.

³⁶ Some EITC-eligible women (particularly those with lower incomes) may have access to prenatal care through the Medicaid expansions for pregnant women (Gruber 1997, Currie and Gruber 1996). However, given the relatively low income limits, a nontrivial portion of EITC-eligible women will not be eligible for Medicaid. Additionally, there is some evidence that Medicaid eligible women have difficulty getting prenatal care (Office of Inspector General 1990).

VII. Extensions and Robustness Checks

A. 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. We have examined the sensitivity of our results to alternative models of the timing of the impact. We explore three alternatives: (i) when the “sensitive” period is for fetal development during gestation; (ii) when the EITC income is received/spent and is likely to impact fetal health; and (iii) impacts arising from the timing of the arrival of additional income through “labor supply” (versus timing of arrival of the additional income through the EITC refund). Next, we briefly summarize results for (i) and (ii); details and full results are available in Hoynes, Miller, and Simon (2012).

To examine alternative assumptions about the sensitive period for fetal development, we used data at the month-year (by state by parity by demographic group) level to create treatment assignment variables for EITC exposure in the first, second, and third trimesters. We then used these measures in a “horse race” model. The point estimates in the joint model are noisy, and we cannot reject equality of coefficients.³⁷

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 (rather than spread throughout the year). We then assign this income to an infant based on whether and when February falls within their gestation. Results of this model 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.³⁸ Row 1 presents the baseline model (Table 5) and the “cash in February” model is presented in row 2 of Table 9. The point estimates here are similar to that from our base model, but somewhat less precise. When we try a “horse race” model for exposure in the three trimesters, we cannot statistically distinguish whether one is especially important. Results for our 3+/-2 difference-in-differences model are qualitatively similar.

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.³⁹ We then assign this treatment to each birth based on which months fall into which trimester. Results of this

³⁷There is limited independent variation in the three trimester exposure measures, which leads to a problem of multicollinearity. As such our experiment may not be ideally suited to make these distinctions. When we “residualize” our trimester measures (residuals based on regressions on the fixed effects and other covariates), the correlations between the three variables range from 0.967 to 0.985. The unconditional correlations are even higher.

³⁸Since this examination of timing requires averaging across months of exposure, it seemed more natural to use the maximum credit model.

³⁹For example, a March 1995 birth will have for its third trimester exposure three months of 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).

TABLE 9—MAXIMUM CREDIT ESTIMATES OF EITC ON LOW BIRTH WEIGHT USING ALTERNATIVE ASSUMPTIONS ABOUT TIMING, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS

Model	When do I get the money?	Assign treatment based on exposure in:			
		First trimester	Second trimester	Third trimester	Third trimester labor supply
Assign EITC in seventh month (base case)	Refund-cash, all year			-0.307*** (0.066)	
All cash in February, based on third trimester	Refund-cash, Feb only			-0.458*** (0.095)	
Labor supply model, third trimester	Labor supply/earnings				-0.263*** (0.066)
Horse race rebate credit & labor supply	Refund-cash & labor supply			-0.605** (0.126)	0.306** (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.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

model are presented in rows 3 and 4 of Table 9. In row 3, we see results similar to the baseline model. In row 4, 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 labor supply coefficient becomes positive. This may suggest that while the income from the EITC is protective of infant health, the labor supply is detrimental. It may also just be the case that it is difficult to separately identify these two channels with the low-frequency tax changes that identify our model.

B. 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-differences 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 second and higher order births to first births, and comparing third and higher order births to second 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

TABLE 10—DIFFERENCE-IN-DIFFERENCES ESTIMATES OF OBRA93 ON NUMBER AND COMPOSITION OF BIRTHS, SINGLE WOMEN WITH 12 YEARS OF EDUCATION OR LESS

	log(births)	Fraction in state-year cell:						Ed<12
		black	white	Non-Hispanic	18–24	24–34	35+	
<i>Panel A. Model: Parity 2+ versus 1</i>								
Parity2+ × After	−0.020 (0.014)	−0.010 (0.009)	0.009 (0.010)	−0.017* (0.010)	−0.005** (0.002)	−0.001 (0.002)	0.006*** (0.001)	−0.004 (0.010)
Mean of dependent variable		0.323	0.641	0.738	0.648	0.300	0.0520	0.432
Observations	37,639	1,632	1,632	1,632	1,632	1,632	1,632	1,632
<i>Panel B. Model: Parity 3+ versus 2</i>								
Parity3+ × After	−0.017 (0.016)	−0.003 (0.003)	0.004 (0.003)	0.006*** (0.002)	−0.025*** (0.004)	0.016*** (0.004)	0.009*** (0.001)	−0.011** (0.004)
Mean of dependent variable		0.370	0.593	0.726	0.523	0.402	0.075	0.484
Observations	25,419	1,224	1,224	1,224	1,224	1,224	1,224	1,224

Notes: Each column is from a separate DD regression using the Natality data for effective tax years 1991–1998. Column 1 uses 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.

***Significant at the 1 percent level.

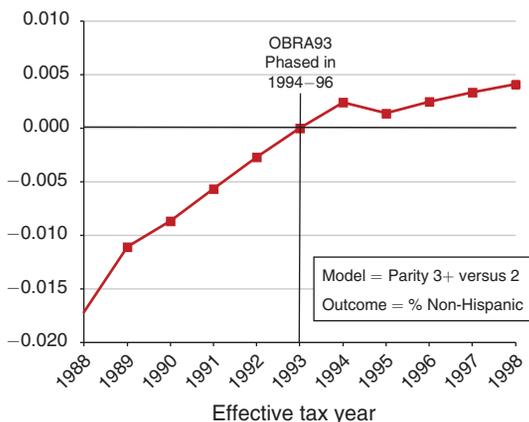
**Significant at the 5 percent level.

*Significant at the 10 percent level.

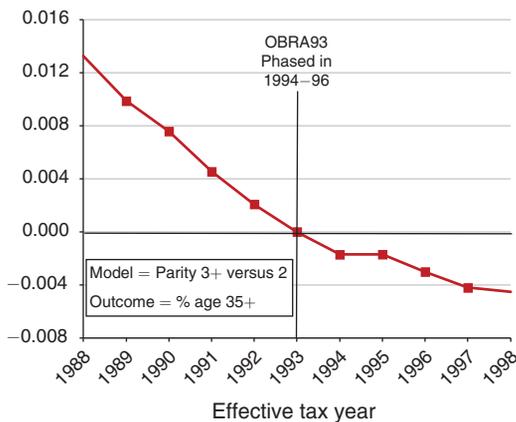
that these are all characteristics that we control for in our main regressions above. So changes in these variables are not a threat to our design. But if treatment is related to changes in these observables we might be concerned about changes in unobservables. The table shows evidence of some small, 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 the event study version of these results in Figure 6 (for the 3+/2 model), the changes in the demographics do not have the same “thumbprint” as our main event study results. Overall, these demographics are changing smoothly through the 1993 expansion (perhaps flattening after 1993), while our main results (Figure 3) show a flat pretrend and then 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.⁴⁰

⁴⁰Nonetheless, we show in online Appendix Figure 3 that our main event study results for low birth weight are robust to adding controls for demographic-group times linear year.

Panel A. Non-Hispanic



Panel B. Age 35+



Panel C. Education <= 12 years

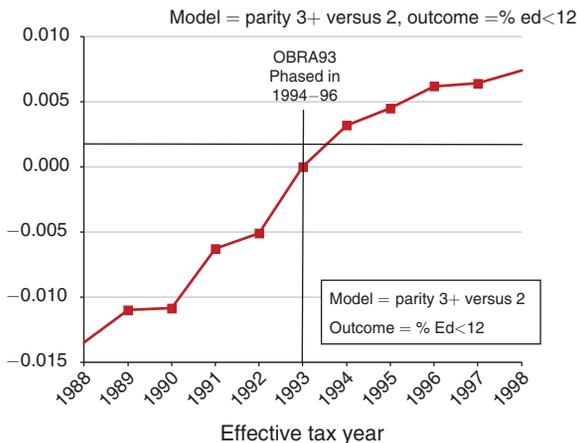


FIGURE 6. EVENT TIME ESTIMATES OF OBRA93 ON COMPOSITION OF BIRTHS, SINGLE WOMEN WITH A HIGH SCHOOL EDUCATION OR LESS (*parity 3+ versus parity 2 model*)

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.

C. *Robustness and Sensitivity*

Our results are robust to changes in the specific sample selection that we use for the analysis. In online Appendix Table 5, we present estimates of the 2+/1 and 3+/2 difference-in-differences 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 nine-month gestation), and in column 4 we 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 pretrends. 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 versus 1, 3 versus 2) embed a treatment and some (e.g., 4 versus 3) form a “placebo test” for our estimation method.

We present results in online Appendix Table 6 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 second births compared to first births, and also for third births compared to second births. The remaining rows of the table compare pairs of birth parities that are both “treated,” and we expect to find no estimated effect for these comparisons. This appears to be true for fifth versus fourth and sixth and higher versus fifth. 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 versus 3 difference begins in 1995 and grows after that.

The gap between fourth and third 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. And second, in our “maxcredit” models, results are robust to inclusion of parity-specific trends.

VIII. 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 second and higher parities to first-born children; and comparing third and higher parities to second-born children). In addition, when we examine groups (e.g., high-education mothers) for whom we expect (and find) little income impact, we see insignificant and small (and for some opposite signed) effects 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 or less) mothers, a policy-induced treatment on the treated increase of \$1,000 in after-tax income is associated with a 1.6 to 2.9 percent reduction in the low birth weight rate. Is this a meaningfully large impact? We address this question in three ways.

First, these estimates fit in well with the (small) literature on the impacts of income on low birth weight. Almond, Hoynes, and Schanzenbach (2011) find that a \$1,000 (2009\$) TOT impact of Food Stamps leads to a 4 percent reduction in low birth weight for whites and a 2 percent reduction for blacks, in the range of our 5 (1) percent reduction for blacks (whites). Hoynes, Page, and Stevens (2011) find that WIC leads to a 10–20 percent reduction in low birth weight.

A second potential comparison with our estimates is that of the observational relationship between income and low birth weight. The Vital Statistics data does not report income so we are unable to use our main data to establish the cross sectional relationship. Instead, we use estimates from Huckfeldt (2013), which uses data from the Pregnancy Risk Assessment Monitoring System (PRAMS). PRAMS combines data from birth certificates, hospital discharges (on the birth), and interviews. Huckfeldt uses data for 2004 and 2005 (only years with income data) including a sample of 17,865 births to single, low-education women. He estimates an OLS regression of low birth weight on family income dummies (only ranges of income are provided), which we combine with CPS estimates of mean income within these categories to determine the OLS relationship between income and low birth weight. His results show that moving from the bottom income category (\$0–\$9,999, CPS average = \$3,956) to the next category (\$10,000–\$14,999 CPS average = \$13,984) implies a reduction in low birth weight of 0.11 percentage points per \$1,000 increase in income. Moving to the third category (\$15,000 to \$19,999 CPS average = \$19,653) from the second implies 0.14 percentage points of reduced low birth weight per \$1,000 of increased income. And moving from the third to the fourth category (\$20,000 to \$24,999, CPS average = \$25,356) implies 0.11 percentage point improvement per \$1,000 increase in income. Huckfeldt also adds in demographic controls and expands the analysis to include married and higher education mothers with only small changes to the correlations.

In comparison to these OLS results, our normalized per \$1,000 income “TOT” results are somewhat larger, at 0.23 percentage points reduction in low birth weight per \$1,000. However, we view the two approaches as giving comparable orders of

magnitude. Why might our “IV” results be larger than the OLS ones? The typical candidate explanations apply. First, measurement error may lead to attenuation bias in the OLS results. Second, there may be a downward bias in the OLS results due to omitted variables bias. For example, if the OLS variation in income is more closely correlated with “work stress” (unlike the EITC expansion, there is less scope for “windfall income” in the typical low-income earner’s world), this may lead to a downward bias to OLS. Third, the EITC impacts more than income, and these channels may not be present in the OLS relationship. Fourth, these two estimates come from different samples, use different measurement approaches, and each includes nontrivial sampling error—so the differences may be due to chance. Finally, there may be some important difference in the LATE which is picked up by the EITC policy variation, compared to that of the OLS regressions.

As a third 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, Chay, and Lee (2005) who estimate crosssection 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-differences models with the excess costs as the outcome variable. We examine two measures of excess costs: one coming from Almond, Chay, and Lee’s “pooled” model (analogous to a cross section association), and one coming from their “fixed effects” model.⁴¹ We then take the resulting estimates and scale up using the “first stage” from Table 4 ($\$1,000/\$1,558 = 0.64$ for our 2+/1 models, $\$1,000/\$1,081 = 0.93$ for our 3+/2 models) to get a “\$ benefits of reduced hospital charges per \$1,000 after-tax income received.”

We find in our 2+/1 models that the TOT per \$1,000 after-tax income impact is a reduction of \$28 using the “pooled” cost estimates, and \$7 using the “fixed effects” estimates. In our 3+/2 models the TOT per \$1,000 EITC impact is a reduction of \$67 in charges using the “pooled” estimates, and \$26 using the “fixed effects estimates. These models are estimated with imprecision, with only the \$67 estimate statistically significant at conventional levels. If you normalize by only EITC income (rather than after-tax income), this results in larger but still modest estimates of the hospital cost benefits due to EITC dollars.⁴² This does not count fully the health benefits from the improvements in birth weight. Hospital charges are just one of potentially many measurable benefits of reductions in low birth weight, and so these estimates are lower bounds. Finally, 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.

⁴¹The pooled model is more representative of estimates from the epidemiological and public health literature, but Almond, Chay, and Lee indicate that the twin fixed-effects model will have less omitted variable bias. The direct estimates of averted costs are for the 2+/1 comparison $-\$43$ (s.e. 29) for the pooled model, and $-\$10.4$ (s.e. 16.7) for the fixed effects model. For the 3+/2 comparison, the averted costs are $-\$72$ (s.e. 35) for the pooled model and $-\$28$ (s.e. 20) for the fixed effects model.

⁴²Taken as a whole, these results suggested that the benefits from reducing hospital costs may be on the order of 2 percent – 25 percent of the direct EITC outlays for these women.

IX. Conclusion

This paper evaluates the health impact of a central piece in the US 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 the EITC expansions reduced 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 after-tax income is associated with a 1.6 to 2.9 percent reduction in the low birth weight rate. These impacts are evident with difference-in-differences models and event study analyses, and show larger impacts for births to African American mothers. We believe that these effects are largely due to the sizeable increase in income for eligible families; but they could be operating through other important policy-induced channels such as changes to labor supply. Overall, however, our results suggest that there are nontrivial 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 nonhealth 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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