Do Cash Transfer Programs Improve Infant Health: Evidence from the 1993 Expansion of the Earned Income Tax Credit

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Abstract

In this paper I examine the effect of the Earned Income Tax Credit (EITC) on birth outcomes by exploiting the 1993 expansion, which increased benefits disproportionately for families with two or more children. Using both difference-in-difference and difference-in-difference-in-difference approaches, I find that the expansion had small positive and statistically significant effects on birth weight, and that the expansion reduced the incidence of low birth weight. I also examine whether the increase in income as a result of the expansion led to greater investment into health inputs by testing whether the expansion increased the number of prenatal visits among eligible mothers. I find the expansion had no effect on prenatal care decisions.

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1. Introduction

Economists have recently become interested in the relationship between socioeconomic status and health. A large literature documents that those with higher socioeconomic status have better health outcomes. Adler et al. (1994) establish that the positive relationship between income and health is not limited to the lower end of the income distribution, but persists throughout. This relationship begins early in life—Case et al. (2003) find that the gradient is present as early as childhood, and Finch (2003) provides evidence that there is health a gradient among infants.

If one is concerned with reducing disparities in health across income classes, it is important to understand which policy tools are effective in improving health. Some argue that an effective mechanism to achieve this goal is the use of in-kind transfer programs that provide direct access to healthcare for the poor. Others argue that cash transfer programs that give extra income to poor households offer the opportunity to purchase healthcare and health related goods, relieve stressful financial burdens that may be detrimental to health, and may reduce the consumption of cigarettes. Understanding the different health effects each type of program generates is important for policymakers interested in reducing inequalities in health. This paper adds to the literature by providing an evaluation of effect of the largest cash transfer program, the Earned Income Tax Credit (EITC), on birth outcomes.

There is research devoted to examining the efficacy of in-kind transfer programs such as Medicaid, Women, Infants, and Children (WIC), and SCHIP in programs in improving health outcomes. Such programs make healthcare more accessible to low-income families, and increase the consumption of medical care among eligible families (Currie and Gruber, 1996). Research on such programs finds that Medicaid expansions in the 1990's reduced infant mortality and the incidence of low birth weight (Currie and Gruber, 1994). The effect of the WIC program is less clear—Kowaleski-Jones and Duncan (2002) find that participation in WIC improves birth weights, while Joyce et al. (2005) find that WIC has little effect on infant health.

There has not been as much examination of the health effects of cash transfer programs. Most of the work done has examined the AFDC/TANF program, which has diminished in importance over time. Notably, Currie and Cole (1993) examine whether participation in the AFDC program affects birth weights, and find that enrolling in AFDC has no effect for infants of non-white mothers, and positive effects for infants of white mothers. The literature leaves important questions about the ability of cash transfer programs to improve health unanswered. Namely, no one has examined the health effects of the EITC, which is the largest cash transfer program. Additionally, there has been little work examining whether the extra income provided by cash transfer programs has been used to invest in health inputs such as visits to the doctor.

This paper makes two contributions to the literature. First, I provide an evaluation of the effect of the EITC on a number of birth outcomes by exploiting the 1993 expansion of the EITC, which disproportionately increased benefits for families with 2 or more children. Second, I examine whether the expansion of the EITC led to greater investment in health by low income families by testing whether the expansion increased the number of prenatal doctor visits or changed tobacco use patterns during pregnancy among affected mothers.

Using a commonly employed difference-in-difference estimation strategy, I detect small but statistically significant improvements in birth weight, a reduction in the incidence of low birth weight, an increase in the number of prenatal doctor visits, and a reduction in smoking during pregnancy. When I employ a difference-in-difference-in-difference methodology that has never been used in the EITC literature, I find the expansion had positive and significant effects on birth weight that are larger than the difference-in-difference estimates, statistically significant reductions in the incidence of low birth weight, and no effect on prenatal visits and smoking. Such results indicate the expansion slightly improved birth weights, and that the standard difference-in-difference methodology associated with the 1993 expansion may be flawed.

The paper proceeds as follows. Section 2 discusses the EITC program, and explains why we might expect it to affect birth outcomes. Section 3 explains my identification strategy and describes the data. Section 4 presents results for the effect of the expansion on health outcomes and the use of health inputs during pregnancy. Section 5 tests my results by using alternative identification strategies. Section 6 concludes.

2. The EITC expansion and the 1993 expansion

A. History and Background of the EITC

The EITC is the nation's largest cash transfer program, with transfers totaling $36 billion in 2004. While the program's goal is to generate income for poor families, it is different from traditional cash assistance programs in that transfers are made through a refundable tax credit. This unique structure, which requires families to have positive earned income to be eligible, has helped the program gain in popularity due to its more stringent work requirements for the program's recipients. Created in 1975, the EITC has been expanded in terms of benefit size and eligibility requirements numerous times since its inception.

The structure of the payment schedule is as follows: for a certain income range commonly referred to as the phase-in range (up to $10,020 for a filing unit with two or more dependent children in 2001), recipients receive a subsidy that is proportional to their earned income. At the end of the phase-in range, the size of the credit reaches its maximum ($4008 for the same filing unit in 2001). For incomes above the phase-in range, the maximum credit is paid until a certain income threshold ($13,090 in 2001), when the credit is slowly phased out as earned income increases. At the eligibility-restricting income ($32,121 in 2001), the size of the credit reaches zero. Figure 1 illustrates the payment schedule for families with one and 2 or more children in both 1993 and 1996.

The EITC has been expanded a number of times since its inception. When the program was created in 1975, filing units eligible for the EITC were those that met the income requirements and had at least one dependent child.[1] The size of the credit did not depend on the number of dependent children in the filing unit, and benefit levels were not indexed to inflation. Thus, the program diminished in importance over time as the size of the benefits was wiped out by inflation. The Tax Reform Act of 1986 (TRA86) indexed the EITC to inflation, leading to an expansion in the maximum credit for all eligible families.

The second major expansion, which is the focus of this paper, occurred in 1993. The Omnibus Budget Reconciliation Act (OBRA 1993) dramatically changed the EITC in a couple of ways. First, it expanded the maximum credit size for all eligible families by increasing the amount of the subsidy in the phase-in range. Second, it generated significant differences in benefits for families with 2 or more children and families with 1 child. As a result of the expansion, the maximum credit increased from $1,511 to $3,556 for families with 2 or more children, compared with an increase from $1,434 to $2,152 for families with one child. The

[1] A dependent child is defined as a biological, adopted, or step-child of the taxpayers. After 1991, the child had to live with the parent for more than half the year to be defined as dependent.

subsidy in the phase-in range increased from 19.5 to 40 percent of earned income for families with 2 or more children, compared to an increase from 18.5 to 34 percent for families with one child. Figure 1 compares the benefit levels for families with 1 child against families with 2 or more children before and after the expansion by plotting adjusted gross income on the horizontal axis and the size of the credit on the vertical axis. Note that before the expansion, benefit sizes were nearly identical, but after the expansion had taken full effect in 1996, benefit levels for families with 2 or more kids were 65 percent ($1404) greater.

The 1986 and 1993 expansions have helped the EITC grow into the nation's largest cash assistance program. The size of the EITC and its unique incentive structure make it one of the most studied government programs.

A large majority of the research on the EITC has focused on labor supply, since those who argue for the EITC over other cash assistance programs state that the EITC will encourage poor families to work. Results dictate that the EITC increases both the likelihood that a single mother will work, and the number of hours a single mother will work conditional on working (Meyer and Rosenbaum, 2001; Eissa and Liebman, 1996). Among married women, results dictate that the EITC reduces labor force participation (Eissa and Hoynes, 2004). Other research on the EITC has examined the program's effect on fertility (Baughman and Dickert-Conlin, 2006) and marriage and family formation (Dickert-Conlin and Houser, 2002), finding that the program has little effect on both outcomes. Surprisingly, no one has examined the effect of the EITC on health outcomes. I will now describe how we might expect the EITC to affect infant health.

B. The EITC and Health

We might expect the EITC to affect health through three different pathways. First, the EITC increases income through cash transfers. If higher income causes better health, we would expect to see health improvements as a result of the income expansion provided by the EITC. To understand how large the income effects of the EITC could be, consider an EITC-eligible family with 2 dependent children earning the maximum credit in 2001. Such a family would earn between \$10,020 and \$13,090 in 2001 in the absence of the EITC. With the EITC the family's income increases by \$4009, a growth between 30 and 40 percent.

There has been little work that directly tests the causal nature of the relationship between income and health. There are, however, a number of explanations for why we might think that such a relationship may exist. Traditional arguments state that those with higher income have greater access to care and greater opportunity to purchase care. If the use of medical care improves health outcomes, then we would expect that increasing income could improve health outcomes by allowing for greater use of medical care. This argument assumes that medical care is a normal good with a relatively large income elasticity, and that the use of medical care improves health. However, there is some dispute about the ability of medical care to improve health (McKeown, 1979) and not too much evidence of how the use of medical care responds to changes in income.

Another explanation for why higher income might improve infant health that is growing in popularity argues that one's relative social status might affect health outcomes. These arguments are motivated by the fact that there is no relationship between national income and national health outcomes across countries, but the relationship between income and health is persistent within countries. Further, the health/income gradient is present in countries with universal health care, where access to care should be equal across income classes. Marmot et al. (1997) and others argue that if low social status causes stress, which is detrimental to health, then increases in income that might result in increases in social rank might improve health.

Finally, increases in income may affect health outcomes through pathways related to the consumption of unhealthy goods. Some suggest that higher income might reduce cigarette smoking, a leading cause of low birth weight infants. Such an argument only follows if consumption of cigarettes directly depends on income—that lower income individuals smoke more is not sufficient to prove this, since low income might be correlated with unobserved characteristics that affect the likelihood an individual smokes. Wasserman et al. (1991) provide evidence that this is the case by establishing that cigarettes are an inferior good.

In addition to the income effects of the EITC, a large literature has established that the EITC affects labor supply decisions. If one's labor decisions affect health outcomes, then we would expect the EITC to influence health outcomes through this channel. It is hard to test the effects of labor supply on health, so hypothesizing whether the health effects of the EITC generated by labor supply changes are positive or negative is difficult. If working during pregnancy has a negative effect on birth outcomes, then it is plausible that the labor supply effects of the EITC may counteract the income effects. The limited research examining this issue shows that labor force participation during pregnancy has little effect on birth outcomes (Baum, 2004). If this is true it is likely that any effects of the EITC on birth weight, my outcome of interest, should be attributable to something other than the EITC's labor supply effects.

One last way the EITC might affect health is through fertility incentives. Under the EITC payment schedule, families with 2 or more children receive a larger credit than families with one child, creating a financial incentive for EITC eligible mothers with one child to have a second child. If the mothers who respond to this incentive are a non-random sample of EITC eligible mothers with 1 child, then the EITC would change the composition of the mothers giving birth. If the children of these mothers have different birth outcomes, we would expect the EITC to influence infant health. It is unclear whether this fertility incentive would have a positive or negative effect on birth weights.

The theoretical predictions of the effect of the EITC are ambiguous. It is unlikely that the income effects of the EITC would reduce health, but it is possible that the labor supply and fertility effects the program creates might be detrimental to infant health. I estimate the overall effects of the EITC on infant health, and do not try to isolate the income, labor supply, and fertility effects. I do provide tests for whether there was a change in both the consumption of health care and the use of cigarettes as a result of the expansion. I will now describe my empirical methods for identifying the effect of the EITC on birth outcomes.

3. Data and Identification Strategy

To generate an estimate of the effect of the 1993 EITC expansion on birth outcomes, one could examine whether a specific birth outcome for a group treated with an increase in EITC benefits improved after the implementation of the expansion. The problem with such an identification strategy is that the method fails to isolate the effect of the expansion from other secular trends in birth outcomes. If factors unrelated to the EITC were causing birth weights to decrease right around 1993, such a method might estimate that the EITC expansion reduced birth weights, when in fact the expansion may have prevented birth weights from decreasing even further.

To circumvent this problem, I will employ a difference-in-difference framework. This estimation strategy attempts to mimic a randomly assigned clinical trial in that it examines the outcomes of two groups, one of which is "treated" with an EITC expansion. The group that is not treated, known as the control group, is intended to represent the secular trends in the outcome of interest that would have occurred among the treatment group in the absence of an intervention. To isolate the effect of the EITC expansion from secular trends, one can look at the change in the difference in birth outcomes between the treatment and control groups before and after the expansion. Such a method is preferred to the method previously described in that the control group can help identify the secular trends in the outcome of interest, allowing us to distinguish between the effect of the treatment and other effects not related to the treatment.

In the context of the 1993 EITC expansion, I use two different treatment/control specifications. In both models, I select families with 2 or more children as the treatment group. Such families received the largest increase in benefits as a result of the 1993 expansion. In the first model, I select mothers giving birth to their first or second child as the control group. Interpretation on the estimate generated for this model is difficult, because the mothers in the control group giving birth to their second child were treated with an expansion as well. Thus, the estimate generated does not measure the full effect of the 1993 expansion for families with 2 or more children, but instead measures the effect of the increase in benefits that went above and beyond that received by mothers with one child.

In the second specification, I select mothers giving birth to their first child as the control group. The interpretation on this estimate is easier, because mothers giving birth to their first child received a near negligible treatment in 1993, so they should represent what would have happened to the treatment group in the absence of any treatment at all. Thus, this estimate measures the full effect of the 1993 expansion on mothers with two or more children.

Providing estimates for both models is useful, however, because the actual treatment in the first model is smaller than that in the second, so we should expect to see larger estimates of the treatment effect in the second specification. Figure 3 presents time-series trends for a number of infant health outcomes and maternal health behavior during pregnancy for the treatment group and the control group in the second model. The pictures on Figure 3 indicate that the treatment group had some improvement in birth weight, the incidence of low birth weight, and smoking prior to the expansion, but there was not much change in doctor visits.

The general difference-in-difference model will estimate equation (1):

$$Y_{ijt} = \mu*POST_t*THIRDKID_{ijt} + \gamma*POST_t + \eta*THIRDKID_{ijt} + \beta*X_{ijt} + \theta_j + \lambda_t + \delta_{jt} + \varepsilon_{ijt} \quad (1)$$

where *i*, *j*, and *t* index persons, states, and time . Y is the birth outcome of interest, X is a vector of demographic characteristics, including a complete set of dummies for mother's age and birth parody, θ_j are state effects, and λ_t are year effects.[2] δ_{jt} are time specific factors within each state that might affect birth weights: the percentage of the population that is black, the percentage of the population that is foreign born, the unemployment rate, and per capita disposable income. POST is a dummy variable that equals 1 if the birth occurred in the group deemed post-expansion; this group varies based on the model being estimated. I will discuss how the values of this variable change in the Section 4. THIRDKID is a dummy variable that equals 1 if the

[2] The year effects subsume the POST variable, and the set of birth parity dummies subsumes the THIRDKID variable

mother giving birth is part of the treatment group—these are mothers who are giving birth to their third or higher child. The coefficient μ on the interaction term POST*THIRDKID is the coefficient of interest. It represents the effect of the expansion on those who were likely to receive the EITC[3], and our estimate $\hat{\mu}$ will be unbiased if the treatment and control groups have been properly identified. I will now describe the data I use to estimate this equation.

A. Data

Data for this project come from the Natality Detail File, an annual census of births in the United States provided by the National Center for Health Statistics (NCHS). The data in the file come directly from birth certificates, and provide detailed information about birth outcomes, demographic characteristics of the mother, the number of previous live births of the mother, maternal smoking habits, and prenatal care usage. The data do not contain information about income or EITC-eligibility, so I am forced to use a proxy for EITC eligibility. A common proxy in the literature is years of education: Eissa and Hoynes (2004) use mothers with less than a high school degree to estimate EITC eligibility, and Baughman and Dickert-Conlin (2007) use mothers with no years of college. I follow Eissa and Hoynes and proxy EITC eligibility with having less than a high school degree, since they find that 58 percent of married women with less than a high school degree are EITC-eligible, compared with only 19 percent for those with a high school degree.

To construct my dataset, I gather all relevant demographic information: year, state, mother's age, marital status, years of education, number of previous live births, and race. I drop

[3] Note that this coefficient does not measure the effect of the expansion on those who *actually received* the expansion. I deal with this problem in section 4.

observations missing any of this information. This resulted in losing around 6 percent of all births from years 1989-1996 from the dataset. Additionally, I drop any missing values from the outcome of interest variable to construct outcome specific datasets. For my analysis, I collapse the individual level data into cells based on year, state, mother's race, mother's age, number of past live births, and marital status, and weight each cell according to the number of mothers who share those characteristics.[4] Table 1 presents summary statistics for the birth weight, number of prenatal visits, and smoking during pregnancy datasets. Births in which the mother is less than 18 years old or younger are also dropped, since such mothers would not receive EITC benefits. All differences in the number of observations across these five datasets are attributable to my dropping of observations missing values on the outcome of interest in that specific dataset. Note that the means do not dramatically change from birth weigh to prenatal care datasets, indicating that the observations dropped are a representative sample of those without missing information in the dataset.[5]

I restrict my analysis to the years 1989-1996 for a number of reasons. First, prior to 1989 information about maternal smoking during pregnancy was not reported on birth certificates, making it difficult to gather estimates for this outcome. Additionally, using years 1989-1993 as pre-treatment years provides a sufficiently long pre-treatment period. Second, the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which dramatically changed the welfare state in the United States, occurred in 1996. Due to the magnitude of the changes generated by this welfare reform, I choose to avoid the years after its

[4] An example of a cell would be a 23-year old married, white mother with 11 years of education giving birth to her 3rd kid in North Carolina in 1994

[5] The race means change in the tobacco dataset because CA, NY, IN, and SD did not report smoking during pregnancy for much of the sample

passage to avoid any confounding effects. Lastly, since the 1993 expansion was phased in from 1994-1996, I drop the births in 1994 from the analysis since it is unlikely that most EITC eligible mothers would have received the expanded credit during pregnancy. I will now present results using the difference-in-difference framework.

4. Results

The results presented in this section are the difference-in-difference estimates of the effect of the 1993 expansion on birth weights, incidence of low birth weight, number of prenatal visits, and tobacco use during pregnancy. For each of the outcomes of interest, the dummy variable POST equals one for years 1995 and 1996 and zero in all other years.

Tables 2-5 present the results for the four outcomes of interest. The dependent variables in each respective table are: birth weight (in grams), a dummy variable for whether the infant was born low birth weight (<2500 grams), the total number of prenatal visits during pregnancy, and a dummy variable for whether the mother smoked during pregnancy. The regressions in Tables 3 and 5 are estimated as linear probability models for ease of the interpretation.

In each table, the estimates on the coefficient POST*THIRDKID are the difference-in-difference estimates for the treatment effect of the EITC expansion. The first two columns present estimates for a pooled sample of single and married women, while the third and fourth columns present estimates for single women and the last two present estimates for married women. Columns 1, 3, and 5 present results of my first specification, where the control group includes mothers giving birth to their first or second child. Columns 2, 4, and 6 present results for my second specification, where the control group is limited to mothers giving birth to their first child. Robust standard errors clustered by state are presented in parentheses below the coefficient estimates.

Under the first specification, for the pooled sample of both single and married women, the expansion is estimated to have increased birth weights by 3.63 grams, but is not statistically significant. Under the second specification, the estimate increases to 7.30 grams, and is statistically significant at the 1 percent level. This is a result we would expect if we anticipate that the 1993 expansion improved birth outcomes, because as mentioned earlier the treatment effect in the first model is the effect of the benefits to the treatment group above and beyond what was received by mothers giving birth to their first child; the treatment effect in the second model measures the full effect of the expansion. We see similar differences between the two specifications for both the single and married regressions as well.

We see statistically significant reductions in the incidence of low birth weight across all groups. The effect is largest among single women, with the expansion estimated to have reduced the incidence of low birth weight by .44 percent under the second specification. Again, we see a larger effect when the comparison group is first born children for each sample; when the comparison group is first and second born children, the estimated effect of reducing the incidence of low birth weight falls by 29 percent in the pooled sample.

In looking at the expansion’s effect on the number of doctor visits, an indicator of the investment in infant health, we see very little change after the expansion. There were no statistically significant effects among all women or single women, and small but statistically significant increases in prenatal visits among married women under both specifications. Estimates indicate that the expansion increased the number of prenatal doctor visits by 0.051 for married women under the first model, and by 0.095 visits under the second model. The mean number of visits for the treatment group in 1993 was approximately 10, so results show that the expansion increased the number of prenatal visits by approximately 1 percent.

Results for smoking during pregnancy indicate that the expansion reduced the proportion of mothers that smoked during pregnancy. Estimates for the pooled sample under the second specification reveal that that expansion reduced the likelihood that a mother smoked during pregnancy by 0.7 percent. The estimates for single and married women are a larger—1 percent and 1.15 percent, respectively. To put the magnitude of these estimates into context, it should be noted that 30 percent of mothers in the treatment group in 1993 smoked during pregnancy.

A. Conversion of Coefficients

One important note from my estimation strategy is that not all mothers in the treatment group are actually EITC eligible. Equation 1 represents the reduced form equation estimating the effect of the expansion on people who are likely to be eligible for the EITC. The treatment effect of interest in this paper is the effect of the EITC expansion on people who were both eligible for the EITC and received the credit. I do not have a first stage regression to estimate the likelihood of EITC take-up among less-than-HS educated mothers, so I turn to an alternate method of estimating the "treatment on the treated."

To produce this estimate, I turn to data from the 1997 March Current Population Survey (CPS), which surveys households about labor market conditions in 1996, to compute the proportion of my treatment group that is actually eligible for the EITC. While this approximation method does not take into account the fact that some EITC-eligible households will not take-up the credit, it should be noted that EITC take-up rates are extremely high, so my estimates will come close to the actual EITC take up rate among the treatment group. From the CPS data, I find that 55 percent of women between the ages of 18 and 45 with at least 2 children and less-than 12 years of education are EITC eligible. Among married women, 63 percent are EITC eligible, and among single women 44 percent are EITC eligible.

To obtain my estimates of the treatment on the treated, I convert the difference-in-difference estimates using the delta method. The delta method says that if for some random variable Z~N(μ, σ^2), and some continuous function g(.), then g(Z)~N($g(\mu), g'(\mu)^2 * \sigma^2$). I define g($\hat{\mu}$) $= \frac{1}{\alpha}\hat{\mu}$, where $\hat{\mu}$ is the reduced form treatment effect estimated in equation 1, and α is the proportion of the treatment group that I estimate is eligible for the EITC. The coefficient estimate $\hat{\mu}$ in equation 1 (the reduced form estimate of the treatment effect) is a normally distributed random variable, and g is clearly a continuous function, so the conditions required to use the delta method are met.[6]

Table 10(a) presents the converted difference-in-difference estimates for each of the outcomes of interest under the second specification, where the members of the control group did not receive any EITC expansion. The rows indicate the outcome of interest, and the columns present estimates broken down by marital status. This table allows us to understand the differing effects of the 1993 expansion on single and married women. This is important to note because the expansion increased labor supply among single women, but decreased labor supply among married women. While there is not enough information to conclude whether these differences are attributable to fundamental differences between the two groups or due to the differing

[6] It follows from the delta method that the adjusted coefficients and their standard errors should be equal to the reduced form coefficient and standard error estimates multiplied by the reciprocal of the fraction of the treatment group that took up the EITC.

reactions in the labor market of the two groups, we should note the effects on the two groups are drastically different for birth weights.

5. Alternate Specification

A. Concerns with Difference-in-Difference estimates

While the difference-in-difference estimation strategy offers a number of advantages, it relies on the critical assumption that the outcomes of the control group represent what would have happened to the treatment group in the absence of treatment. If such an assertion proves false, then the difference-in-difference model will provide a biased estimate of the treatment effect. In this paper, the primary assumption is that the trends in birth outcomes among first born children represent the trends that would have occurred among third and higher born children in the absence of the expansion of the EITC.

The labor supply literature on the EITC has specified treatment and control groups based on the number of children within a household to generate difference-in-difference estimates, and this method is generally accepted. However, mothers giving birth to their first kid have different observed characteristics than mothers giving birth to their third or higher kid. Table 6 presents means and standard deviations for a number of relevant observed characteristics and birth outcomes among mothers in both the treatment and control groups before the expansion. Note that mothers in the treatment group are on average 4 years older than mothers in the control group, have less schooling, are more likely to be married, and are more likely to be non-white. While these differences are not proof that the driving assumption is false, they do raise concerns that the time trends of the control group may not mimic the time trends of the treatment group. What should really make us skeptical of the difference-in-difference estimates, however, is direct evidence that the treatment and control groups' time trends in birth weights were not identical prior to the expansion. If such behavior occurred over the period of interest, the difference-in-difference estimates I present are biased.

The primary concern with the difference-in-difference estimates is that the treatment groups' birth outcomes were improving relative to the control group prior to the EITC expansion. Under such a scenario, my estimates of the treatment effect would include the effect of the treatment plus the secular change in the difference between the two groups' birth outcomes, making the estimates biased. To test whether this is the case, I estimate equation 4, which includes a "false treatment" placebo term and an interaction term.

$$Y_{ijt} = \tau * PLACEBO_t * THIRKID_{ijt} + \chi * PLACEBO_t + \eta * THIRDKID_{ijt} + \beta * X_{ijt} + \theta_j + \lambda_t + \delta_{jt} + \varepsilon_{ijt} \quad (4)$$

The PLACEBO variable equals 1 for all years after the "false expansion," and zero for the years prior to the false expansion. The sample on this regression is restricted to the year 1989-1993, which are my pre-treatment observations. If there were no differing trends between the treatment and control group prior to the 1993 expansion, we should expect the coefficient τ to be zero. Table 7 presents estimates of equation 4 for all three marital status pools, when the "false expansion is defined to have occurred in 1991 for one model and 1992 for the other. In each case, we see that among both the pooled sample and single women there were positive and statistically significant changes in birth weight for the treatment group relative to the control group prior to the expansion. Such results strongly hint that the difference-in-difference model is not sufficient to estimate the true treatment effect, calling for an alternative identification strategy.

Difference-in-Difference-in-Difference Strategy

To alleviate this concern, I employ a difference-in-difference-in-difference framework. Such a model exploits the fact that not all mothers in the Natality Detail dataset are EITC-eligible. By comparing the time trends of the treatment and control groups among non-EITC mothers to those of EITC mothers, I can "difference out" the secular changes in outcomes of the treatment and control groups over time, thus eliminating the bias from the estimate. This is done by subtracting the "treatment effect" estimated for non-EITC mothers from the treatment effect estimated for EITC mothers. The assumption of this model is that the difference in the time trends of first born and third and higher born children is the same for both EITC and non-EITC eligible mothers. The difference-in-difference-in-difference model is presented by equation 5:

$$Y_{ijt} = \acute{\omega}*EITC*POST_t*THIRDKID_{ijt} + \rho*EITC*POST + \psi*EITC*THIRDKID + \alpha*POST_t*THIRDKID_{ijt} + \varphi*EITC + \gamma*POST_t + \eta*THIRDKID_{ijt} + \beta*X_{ijt} + \theta_j + \lambda_t + \delta_{jt} + \varepsilon_{ijt} \quad (5)$$

The variable EITC indicates whether the mother is likely to be EITC-eligible, and equals 1 if the mother has less-than a high school degree. The coefficient $\acute{\omega}$ represents the effect of the EITC expansion on those who were likely to receive the treatment. If the assumption driving the estimate of the difference-difference-in-difference model is correct, then $\acute{\omega}$ should present an unbiased estimate of the treatment effect.

Table 8 presents regression estimates of equation 5 for each of the four outcomes of interest for the pooled sample of single and married women. The treatment group is third and higher born kids, and the control group is first born kids. In these regressions, the population consists of women without a high school degree and women with a college degree. I dropped women with between 12 and 15 years of education because more than 22 percent of such women are eligible for the EITC, which means some of the trends among high school educated women would reflect the changes due to the EITC expansion, thus confounding the effects of adding the third difference to the model.[7] Only 8 percent of women with a college degree are EITC-eligible, so the expansion should not dramatically affect this group.

Note that the estimate of the treatment effect on birth weight increases dramatically—the previous difference-in-difference estimate was 7.38 grams, while the DDD estimate is 13.8 grams. Further, the difference-in-difference estimate of the treatment effect falls outside of the 95 percent confidence interval of the DDD estimate, indicating that the two models yield fundamentally different results. For incidence of low birth weight, we see similar results; the DDD estimates the treatment effect to be larger, indicating the time trends between the treatment and control group in the difference-in-difference model were not the same. For prenatal visits, the DDD estimates yield qualitatively similar results to the DD model—the expansion had no effect on the number of prenatal visits. For tobacco use during pregnancy, the DDD estimates do not detect a statistically significant effect of the expansion, which is a qualitatively different result from the difference-in-difference, which found that the expansion led to a reduction in smoking during pregnancy.

Table 10(b) presents the adjusted DDD estimates, taking into account the fact that equation 5 estimates the reduced form equation, which does not estimate the treatment-on-the-treated. Adjustments were again made using the delta method, following the same process described in Section 4. Again note the large difference between the estimates of the effect of the

[7] Baughman and Dickert-Conlin (2008) find that 22 percent of women with some college but no degree are EITC eligible. Assuming women with a HS degree but no college are more likely to be EITC-eligible, this number will be greater than 22 percent.

expansion on single women relative to married women. While this paper does not seek to explain this difference, it is a question that may be asked in future research.

B. Specification check on the DDD

The fact that the DDD estimates are significantly different from the difference-in-difference estimates raises concern about which estimate to trust. From the placebo tests previously conducted, we have strong reason to suspect that the difference-difference estimate is biased. To try to correct this problem, I employed the DDD model to difference out the time trend differences between first born and third and higher born children. Such a method is only effective if the time trends differences between first and third born children are the same for EITC and non-EITC eligible mothers. To test whether this is the case, I again employ a placebo test on the DDD model. To do so, I estimate equation 6:

$$Y_{ijt} = \pi{*}EITC{*}PLACEBO_t{*}THIRDKID_{ijt} + \chi{*}EITC{*}PLACEBO + \psi{*}EITC{*}THIRDKID + \kappa{*}PLACEBO{*}THIRDKID + \varphi{*}EITC + \eta{*}THIRDKID_{ijt} + \beta{*}X_{ijt} + \theta_j + \lambda_t + \delta_{jt} + \varepsilon_{ijt} \quad (6)$$

If the coefficient π is zero, then prior to the expansion the non-EITC group is mimicking the EITC group. If this coefficient is not zero, then we have cause for concern that the assumption of the DDD model is wrong.

Table 8 presents the regression estimates for equation 6. Note that in all 6 regressions, the coefficient on the placebo treatment effect term is not statistically different from zero. These results indicate that prior to the expansion, the time trend differences between first born and third and higher born children of less than high school educated mothers and college educated mothers were similar, offering support to the argument that the DDD are unbiased. There should be some concern about the accuracy of the DDD estimates for single women, considering the magnitude of the coefficient—it is approximately 10 grams for both falsification tests. It is the only placebo estimate that is as large as the placebo estimates from the difference-in-difference model, indicating that adding the third difference may not be as useful for single mothers. While we cannot reject the null hypothesis that there were changes in time trends prior to the expansion that will contaminate the DDD model, it appears as though there were some pre-expansion changes that should make us concerned. It should be noted, however, that the DDD estimate of the treatment effect for single women[8] is more than double the estimates produces by the placebo model.

This DDD model reveals two important facts. First, the 1993 EITC expansion had a positive and statistically significant effect on birth weights, reduced the incidence of low birth weight among affected mothers, but did not affect the likelihood of smoking during pregnancy or the number of prenatal visits. Second, the results of the DDD model changed significantly from the difference-in-difference estimates, indicating that the assumption that mothers with no children provide a good control group for mothers with 2 or more kids may be flawed. Unlike the difference-in-difference estimates, which failed falsification tests, the DDD estimates were robust to the placebo tests, indicating that these are more reliable. While this paper only examines this assumption in the context of birth outcomes, the failure of the difference-in-difference model should be noted when evaluating the validity of difference-in-difference estimates of the expansion on other outcomes.

6. Conclusion

[8] This result is not presented in any of the tables. The DDD model estimates a 23.7 gram increase in birth weight among single women with less-than-a-high-school degree after the expansion.

This paper provides an evaluation of the effect of the 1993 expansion of the EITC. To estimate the effects of the expansion, I employ both a difference-in-difference model and a difference-in-difference-in-difference model. Results for the difference-in-difference model indicate that the expansion had small positive and statistically significant effects on birth weight, reduced the incidence of low birth weight and the proportion of women that smoke during pregnancy, and had little effect on the number of prenatal visits. The DDD model confirms that the expansion had positive effects on birth weight, but the results are significantly larger than the DD estimates. The model also finds that the expansion reduced the incidence of low birth weight, but had little effect on smoking during pregnancy and the number of prenatal visits. Further, when adjusting for the fact that not all women in the treatment group were actually treated with the expansion, I find that the effect of expansion was different on single and married women. While the expansion increased birth weights for both groups, the effects were nearly double for single women. While this paper does not seek to explain this difference, it raises an interesting question for future research.

Such results indicate that the 1993 expansion was effective in increasing birth weights, though the magnitude of the effects is relatively small. Further, the differing results from the difference-in-difference and DDD estimates indicate that specifying treatment and control groups based on the number of children a mother has may not satisfy the crucial assumption of a difference-in-difference model—that the treatment and control groups follow the same trends over time. While this paper only proves this to be true for birth outcomes, other researchers using such an identification strategy should do so with caution.

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Figure 1: Effect of 1993 EITC Expansion

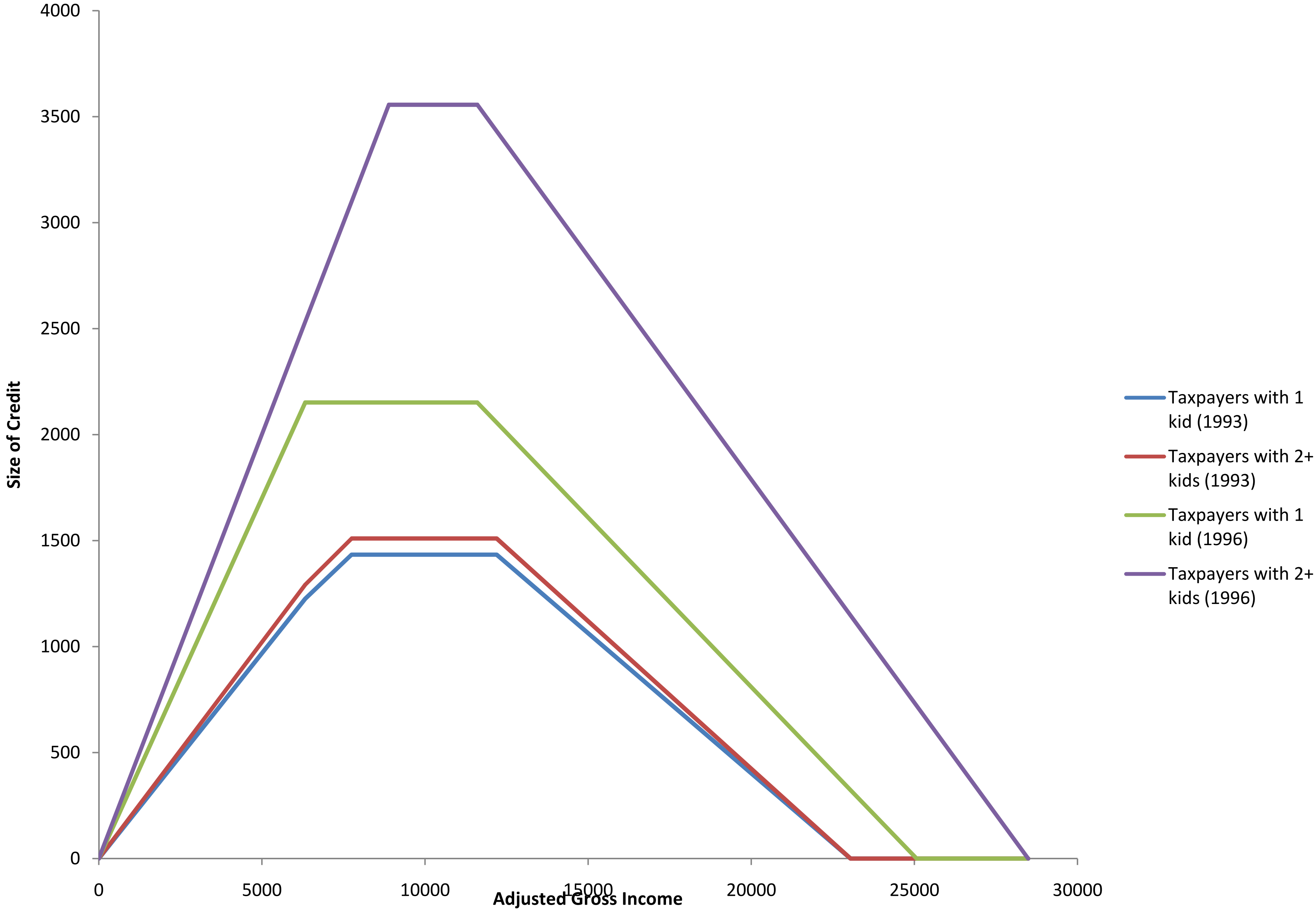

Figure 2: Treatment and Control Group Trends Over Time

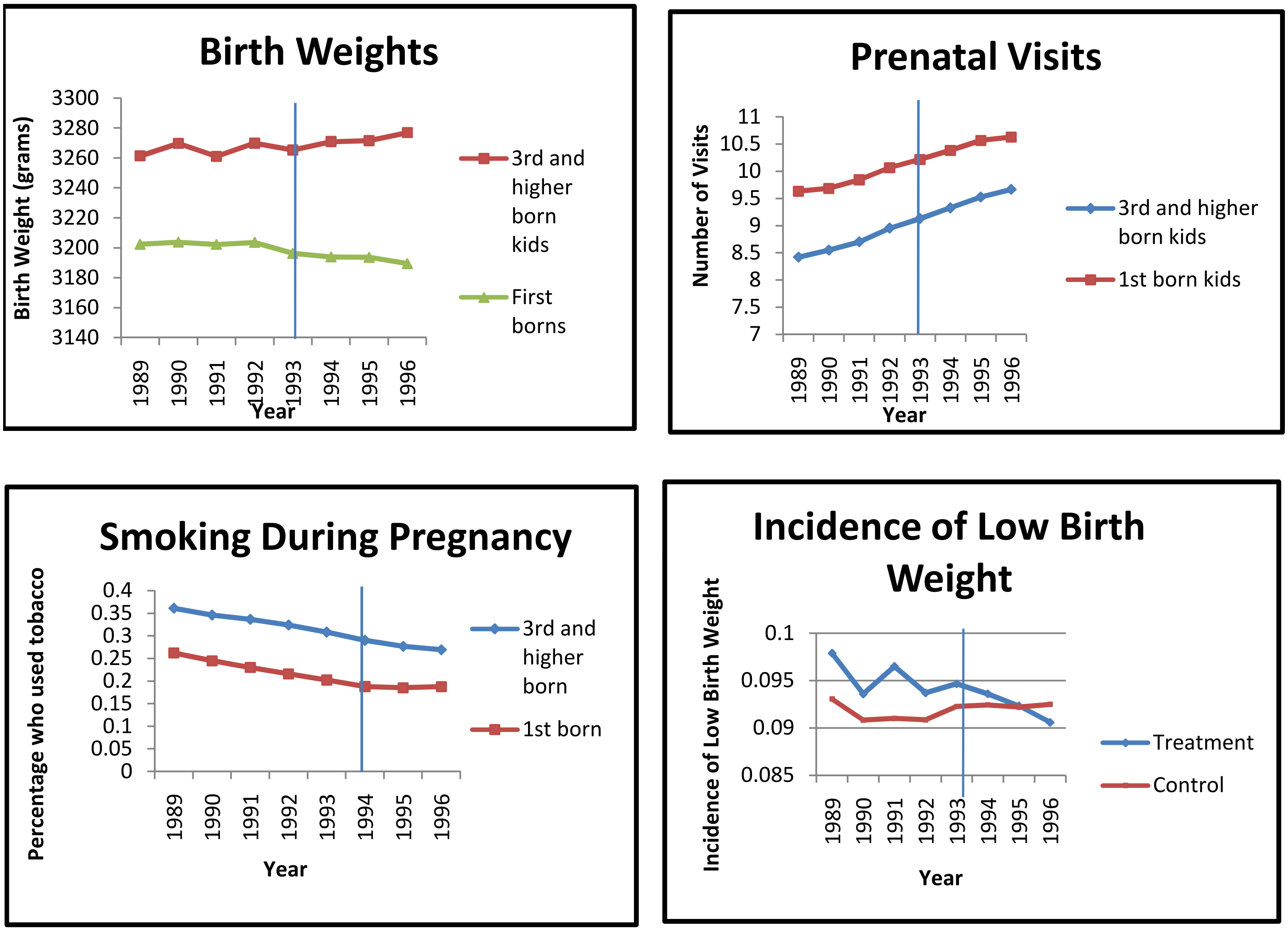

Table 1: Summary Statisitcs

Variable	Birth Weight	Prenatal Visits	Tobacco Use
	Dataset		
Mom Age	24.47686	24.47376	23.96721
	(5.51)	(5.51)	(5.34)
Number of past births	1.517895	1.51403	1.480204
	(1.53)	(1.53)	(1.50)
Years of Education	8.920412	8.921095	9.325409
	(2.45)	(2.45)	(2.09)
Married	0.512832	0.5149805	0.5070627
	(0.50)	(0.50)	(0.50)
White	0.388925	0.3921038	0.4791318
	(0.49)	(0.49)	(0.50)
Black	0.1680666	0.1655417	0.2100721
	(0.37)	(0.37)	(0.41)
Hispanic	0.4007747	0.4002537	0.2765427
	(0.49)	(0.49)	(0.45)
Asian	0.0166686	0.0165471	0.0093917
	(0.13)	(0.13)	(0.10)
Birthwt	3257.445	---	---
	(281.44)		
Low-birth weight	0.0866031	---	---
	(0.13)		
Gestation Age	---	---	---
Prenatal Visits	---	9.610971	---
		(2.33)	
Tobacco Use?	---	---	0.3034312
			(0.28)
Observations	4936901	4773940	3344247

Standard Deviations in Parentheses

Notes : Table presents summary statisitcs for the datasets used to estimate the effect of the expansion for each of the four outcomes of interest. Data are taken from the Natality Detail file, and span the years 1989-1996, with observations in 1994 dropped from the dataset.

Table 2: Results of 1993 Expansion on Birth Weights

	All Marital		Single		Married	
	(1)	(2)	(3)	(4)	(5)	(6)
	All kids	**No 2nd kid**	**All kids**	**No 2nd kid**	**All kids**	**No 2nd kid**
POST*THIRDKID	3.63	7.30	7.26	10.75	4.16	9.85
	(2.23)	(2.69)***	(4.19)*	(5.14)**	(2.80)	(3.10)***
POST	(18.23)	(22.65)	(11.03)	(21.06)	(24.40)	(26.55)
	(6.01)***	(7.88)***	(6.50)*	(7.18)***	(8.35)***	(7.77)***
THIRDKID	(151.88)	(146.11)	26.39	(125.67)	(128.02)	(135.24)
	(120.64)	(122.38)	(.)	(95.38)	(291.70)	(294.59)
Percentage black	1.12	2.45	(2.61)	(3.53)	3.81	6.13
	(4.38)	(3.96)	(5.45)	(5.71)	(4.44)	(4.65)
Percentage foreign	6.37	5.62	10.90	10.79	3.68	2.68
	(3.30)*	(3.33)*	(4.06)***	(4.14)**	(3.14)	(3.57)
Unemployment	(0.57)	(1.19)	3.66	2.71	(1.14)	(1.13)
	(3.44)	(3.76)	(4.74)	(5.04)	(2.48)	(2.86)
Disposable income	1.44	0.61	3.88	4.05	4.66	3.11
	(1.57)	(1.54)	(2.29)*	(2.50)	(1.59)***	(1.98)
White	27.92	24.70	(32.24)	(34.99)	75.56	71.89
	(23.21)	(22.58)	(21.15)	(21.13)	(24.03)***	(23.06)***
Black	(209.87)	(219.95)	(229.41)	(239.50)	(138.90)	(148.02)
	(21.85)***	(21.37)***	(19.54)***	(19.45)***	(20.90)***	(20.42)***
Asian	(53.38)	(56.74)	(137.58)	(142.71)	(31.24)	(35.43)
	(17.84)***	(17.47)***	(18.86)***	(20.30)***	(17.38)*	(16.75)**
Hispanic	68.21	68.54	15.83	14.22	115.58	116.95
	(27.06)**	(27.07)**	(28.09)	(28.75)	(23.47)***	(23.18)***
Constant	3251.09	3121.30	3255.29	3311.18	2931.51	3172.66
	(32.39)***	(143.95)***	(203.22)***	(212.57)***	(170.20)***	(52.27)***
Observations	781535	622312	361191	287884	420344	334428
R-squared	0.17	0.16	0.18	0.17	0.10	0.10

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Estimates are from OLS regression estimating Equation 1. Birth weight (in grams) is the dependent variable. The "All kids" column are estimates when the control group is first and second born children. The "No 2nd kid" columns are estimates from where only first born children are the control group. Observations from 1994 are dropped from the analysis.

Table 3: Effect of the 1993 Expansion on Low Birth Weight

	All Marital		Single		Married	
	(1)	(2)	(3)	(4)	(5)	(6)
	All kids	**No 2nd kid**	**All kids**	**No 2nd kid**	**All kids**	**No 2nd kid**
POST*THIRDKID	-0.002	-0.0028	-0.0033	-0.0044	-0.0022	-0.0035
	(0.0007)***	(0.0009)***	(0.0013)**	(0.0017)**	(0.0006)***	(0.0007)***
POST	0.0053	0.0066	0.0037	0.006	0.0086	0.0084
	(0.0022)**	(0.0026)**	-0.0025	(0.0035)*	(0.0021)***	(0.0023)***
THIRDKID	-0.1105	-0.1123	0.0034	-0.1188	0.2606	0.2621
	(0.0372)***	(0.0381)***	-7.8443	(0.0304)***	(0.1546)*	(0.1550)*
% Black	-0.0017	-0.0022	-0.0006	-0.0006	-0.002	-0.0028
	-0.0023	-0.0023	-0.0032	-0.0032	-0.0013	(0.0017)*
% Foreign	-0.003	-0.0029	-0.0045	-0.0044	-0.0021	-0.0022
	(0.0013)**	(0.0013)**	(0.0018)**	(0.0019)**	(0.0009)**	(0.0010)**
Unemployment	-0.0004	-0.0005	-0.0011	-0.0012	-0.0004	-0.0006
	-0.001	-0.0011	-0.0019	-0.0021	-0.0008	-0.0008
Disp. income	-0.0004	-0.0002	-0.0013	-0.0012	-0.0013	-0.001
	-0.0008	-0.0008	-0.001	-0.001	(0.0006)**	-0.0006
White	0.014	0.0158	0.0193	0.0202	0.0094	0.0114
	(0.0023)***	(0.0028)***	(0.0041)***	(0.0044)***	(0.0019)***	(0.0023)***
Black	0.0845	0.0891	0.079	0.0826	0.0673	0.0712
	(0.0042)***	(0.0050)***	(0.0046)***	(0.0051)***	(0.0045)***	(0.0052)***
Asian	-0.0075	-0.0071	0.0126	0.0148	-0.0055	-0.0053
	(0.0036)**	(0.0040)*	(0.0065)*	(0.0071)**	(0.0024)**	(0.0027)*
Hispanic	-0.0079	-0.0077	-0.0064	-0.0065	-0.0114	-0.0114
	(0.0026)***	(0.0027)***	-0.0059	-0.0062	(0.0015)***	(0.0018)***
Constant	0.0748	0.1474	0.1125	0.1063	0.1744	0.0953
	(0.0144)***	(0.0781)*	-0.1169	-0.117	(0.0480)***	(0.0121)***
Observations	781535	622312	361191	287884	420344	334428
R-squared	0.08	0.08	0.1	0.09	0.04	0.04

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Linear Probability Model estimating the effect of the 1993 EITC expansion on the incidence of low birth weight. The dependent variable is an indicator equalling one if the child's birth weight was less than 2500 grams. The "All kids" column are estimates when the control group is first and second born children. The "No 2nd kid" columns are estimates from where only first born children are the control group. Observations from 1994 are dropped from the analysis.

Table 4: Effects of 1993 Expansion on Prenatal Visits

	All Marital		Single		Married	
	(1)	(2)	(3)	(4)	(5)	(6)
	All kids	**No 2nd Kids**	**All kids**	**No 2nd Kids**	**All kids**	**No 2nd Kids**
POST*THIRDKID	-0.004	0.007	-0.025	-0.009	0.051	0.095
	(0.0240)	(0.0290)	(0.0400)	(0.0470)	(0.023)**	(0.031)***
POST	0.839	0.779	1.082	0.995	0.389	0.337
	(0.181)***	(0.163)***	(0.177)***	(0.133)***	(0.129)***	(0.137)**
THIRDKID	-0.890	0.441	0.422	1.781	-1.844	-1.846
	(.)	(0.4770)	(0.214)*	(827.9630)	(0.991)*	(1.043)*
% Black	-0.320	-0.301	-0.349	-0.329	-0.314	-0.307
	(0.176)*	(0.1830)	(0.176)*	(0.185)*	(0.1900)	(0.1990)
% Foreign	0.248	0.249	0.215	0.210	0.340	0.355
	(0.078)***	(0.081)***	(0.095)**	(0.098)**	(0.085)***	(0.088)***
Unemployment	0.067	0.091	0.140	0.169	0.042	0.056
	(0.0490)	(0.046)*	(0.060)**	(0.058)***	(0.0470)	(0.0470)
Disp. income	-0.247	-0.248	-0.257	-0.259	-0.154	-0.155
	(0.070)***	(0.069)***	(0.091)***	(0.092)***	(0.070)**	(0.069)**
White	1.214	1.139	1.342	1.349	1.072	0.977
	(0.146)***	(0.166)***	(0.118)***	(0.118)***	(0.101)***	(0.122)***
Black	-0.502	-0.562	-0.082	-0.113	-0.051	-0.119
	(0.132)***	(0.150)***	(0.1320)	(0.1380)	(0.0970)	(0.1190)
Asian	0.457	0.406	0.189	0.155	0.159	0.107
	(0.071)***	(0.066)***	(0.1270)	(0.1440)	(0.081)*	(0.061)*
Hispanic	-0.148	-0.149	0.089	0.123	-0.339	-0.341
	(0.1350)	(0.1430)	(0.1170)	(0.1100)	(0.075)***	(0.084)***
Constant	25.2440	24.7910	25.9280	12.3050	11.2210	23.8650
	(6.236)***	(6.465)***	(5.966)***	(1.849)***	(1.362)***	(6.694)***
Observations	765405	608752	353022	280980	412383	327772
R-squared	0.30	0.29	0.32	0.32	0.29	0.27

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: OLS regression estimating the effect of the 1993 EITC expansion on thenumber of prenatal visits to the doctor. The dependent variable is the number of visits a mother reported on her birth certificate. The "All kids" column are estimates when the control group is first and second born children. The "No 2nd kid" columns are estimates from where only first born children are the control group. Observations from 1994 are dropped from the analysis.

Table 5: Effects of the 1993 expansion on Tobacco Use

	All Marital		Single		Married	
	(1)	(2)	(3)	(4)	(5)	(6)
	All kids	**No 2nd kid**	**All kids**	**No 2nd kid**	**All kids**	**No 2nd kid**
*POST*THIRDKIL*	-0.0022	-0.007	-0.0042	-0.0106	-0.0059	-0.0115
	(0.0026)	(0.0022)***	(0.0031)	(0.0033)***	(0.0024)**	(0.0025)***
POST	-0.0476	-0.0266	-0.0275	-0.048	-0.0423	-0.0453
	(0.0074)***	(0.0088)***	(0.0103)**	(0.0099)***	(0.0074)***	(0.0086)***
THIRDKID	0.3254	0.013	-0.0051	-0.0689	0.2972	0.3052
	(0.1951)	(13.9112)	(.)	(.)	(0.2898)	(0.2923)
% Black	-0.0239	-0.0226	-0.0234	-0.0211	-0.0203	-0.0197
	(0.0075)***	(0.0080)***	(0.0065)***	(0.0076)***	(0.0073)***	(0.0072)***
% Foreign	0.0032	0.0033	-0.0002	-0.0007	0.009	0.0096
	(0.0030)	(0.0032)	(0.0038)	(0.0043)	(0.0032)***	(0.0033)***
Unemployment	0.0059	0.0059	0.0057	0.0056	0.0072	0.0069
	(0.0047)	(0.0048)	(0.0058)	(0.0060)	(0.0055)	(0.0056)
Disp. income	0.0025	0.0024	0.0022	0.0017	0.005	0.0045
	(0.0032)	(0.0034)	(0.0034)	(0.0037)	(0.0033)	(0.0035)
White	0.2862	0.2918	0.3267	0.3277	0.2674	0.2723
	(0.0279)***	(0.0285)***	(0.0409)***	(0.0417)***	(0.0208)***	(0.0215)***
Black	0.0793	0.0978	0.0436	0.0558	0.0618	0.0791
	(0.0311)**	(0.0320)***	(0.0427)	(0.0436)	(0.0251)**	(0.0261)***
Asian	-0.1485	-0.143	-0.145	-0.1353	-0.0996	-0.093
	(0.0317)***	(0.0323)***	(0.0450)***	(0.0465)***	(0.0211)***	(0.0213)***
Hispanic	-0.087	-0.0775	-0.0924	-0.0859	-0.081	-0.0724
	(0.0312)***	(0.0314)**	(0.0447)**	(0.0455)*	(0.0221)***	(0.0223)***
Constant	0.8408	0.7611	0.7853	0.7248	-0.4731	0.663
	(0.2744)***	(0.2908)**	(0.2425)***	(0.2773)**	(0.1624)***	(0.2842)**
Observations	650650	513600	302248	238950	348402	274650
R-squared	0.46	0.42	0.52	0.48	0.5	0.46

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Linear Probability Model estimating the effect of the 1993 EITC expansion on the incidence of smoking during pregnancy. The dependent variable is an indicator equalling one if the reported smoking during pregnancy on her birth certificate. The "All kids" column are estimates when the control group is first and second born children. The "No 2nd kid" columns are estimates from where only first born children are the control group. Observations from 1994 are dropped from the analysis.

Table 6: Treatment vs. Control Group

Variable	1st Borns	3rd and Higher Borns
Mom Age	25.1836	29.2431
	(5.2018)	(5.3165)
Mom Educ	12.9986	11.8531
	(2.4686)	(2.9801)
Married	0.7055	0.7201
	(0.4558)	(0.4489)
White	0.6878	0.5644
	(0.4634)	(0.4958)
Black	0.1307	0.2009
	(0.3371)	(0.4007)
Hispanic	0.1354	0.1909
	(0.3421)	(0.3930)
Asian	0.0310	0.0221
	(0.1733)	(0.1469)
Birth Weight	3310.5280	3354.5760
	(166.8030)	(296.1865)
Low Birth Weight	0.0710	0.0771
	(0.0656)	(0.1195)
Observations	7000588	4991283

Standard Deviations in Parentheses

Table 7: Falsification Tests for Difference-in-Difference Model on Birth Weights

	1991 False Expansion			1992 False Expansion		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Marital	Single	Married	All Marital	Single	Married
PLACEBO*THIRDKID	4.99	11.82	5.18	6.42	8.52	4.16
	(1.79)***	(2.58)***	(3.13)	(1.95)***	(2.29)***	(2.55)
PLACEBO	-9.76	-12.29	-12.27	-24.19	-10.68	-11.58
	(4.91)*	(7.77)	(3.51)***	(7.44)***	(7.16)	(3.82)***
THIRDKID	-142.54	73.63	65.67	10.48	77.97	66.31
	(123.96)	(.)	(.)	(20573.36)	(.)	(.)
% Black	22.55	21.19	19.68	2.46	21.42	19.62
	(12.75)*	(14.52)	(11.48)*	(4.22)	(14.58)	(11.47)*
% foreign born	5.03	7.11	1.49	5.8	7.14	1.45
	(4.04)	(6.69)	(3.28)	(3.45)*	(6.68)	(3.27)
Employ	2.27	7.2	-0.69	-1.73	7.39	-0.66
	(2.67)	(3.88)*	(2.28)	(4.16)	(3.88)*	(2.28)
Disp. Inc.	1.94	0.05	2.66	0.76	0.12	2.65
	(3.00)	(3.74)	(3.20)	(1.59)	(3.73)	(3.21)
White	16.46	-44.33	64.1	26.21	-44.22	63.98
	(24.59)	(20.66)**	(28.59)**	(22.75)	(20.70)**	(28.58)**
Black	-237.34	-258.15	-158.27	-217.03	-258.06	-158.42
	(23.29)***	(19.35)***	(25.16)***	(21.55)***	(19.38)***	(25.15)***
Asian	-73.56	-166.98	-50.28	-51.75	-166.72	-50.53
	(19.61)***	(17.78)***	(21.96)**	(17.45)***	(17.81)***	(21.99)**
Hispanic	57.45	2.68	106.81	71.15	2.75	106.67
	(30.21)*	(30.29)	(29.38)***	(27.21)**	(30.32)	(29.38)***
Constant	2394.06	3339.72	3181.86	3118.16	3336.46	3181.26
	(465.82)***	(84.38)***	(76.45)***	(149.91)***	(84.53)***	(76.45)***
Observations	441972	202057	239915	712935	202057	239915
R-squared	0.17	0.18	0.10	0.16	0.18	0.10

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Regression where the dependent variable is birth weight. 2nd born children are dropped from the analysis. Years included in the sample are 1989-1993. The first three columns simulate a 1991 expansion, and the second three simulate a 1992 expansion. PLACEBO equals 1 for all years after the simulated expansion.

Table 8: Difference-in-Difference-in-Difference Estimates of the Effect of the Expansion

	Birth Weight		LBW		Visits		Tobacco	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	DDD	DD	DDD	DD	DDD	DD	DDD	DD
EITC*THIRDKID* POST	13.8	---	-0.0033	---	-0.028	---	-0.0014	---
	(3.17)***	---	(0.0011)***	---	(0.037)	---	(0.0033)	---
EITC*POST	11.65	---	-0.0042	---	0.676	---	-0.0285	---
	(2.17)***	---	(0.0008)***	---	(0.126)***	---	(0.0092)***	---
EITC*THIRDKID	-85.82	---	0.0136	---	-0.476	---	0.0402	---
	(14.16)***	---	(0.0022)***	---	(0.144)***	---	(0.0046)***	---
THIRDKID*POST	-6.04	7.30	0.0005	-0.0028	0.055	0.007	-0.0007	-0.007
	(1.78)***	(2.69)***	(0.0006)	(0.0009)***	(0.020)***	(0.0290)	(0.0009)	(0.0022)***
POST	-31.57	(22.65)	0.0093	0.0066	0.143	0.779	-0.0345	-0.0266
	(5.68)***	(7.88)***	(0.0018)***	(0.0026)**	(0.124)	(0.163)***	(0.0110)***	(0.0088)***
THIRDKID	7.49	-146.11	-0.0019	-0.1123	0.473	0.441	0.1755	0.013
	(25,199.33)	(122.38)	(.)	(0.0381)***	(96.898)	(0.4770)	(.)	(13.9112)
EITC	-138.84	---	0.0406	---	-1.391	---	0.347	---
	(9.61)***	---	(0.0032)***	---	(0.180)***	---	(0.0183)***	---
% Black	1.09	2.45	-0.0014	-0.0022	-0.245	-0.301	-0.0029	-0.0226
	(2.91)	(3.96)	(0.0010)	-0.0023	(0.111)**	(0.1830)	(0.0053)	(0.0080)***
% Foreign	2.06	5.62	-0.0012	-0.0029	0.141	0.249	0.0055	0.0033
	(2.67)	(3.33)*	(0.0008)	(0.0013)**	(0.062)**	(0.081)***	(0.0023)**	(0.0032)
Unemployment	1.02	(1.19)	-0.0003	-0.0005	0.096	0.091	0.0142	0.0059
	(2.04)	(3.76)	(0.0007)	-0.0011	(0.044)**	(0.046)*	(0.0090)	(0.0048)
Disp. Inc.	2.7	0.61	-0.0003	-0.0002	-0.148	-0.248	0.0057	0.0024
	(1.33)**	(1.54)	(0.0005)	-0.0008	(0.057)**	(0.069)***	(0.0051)	(0.0034)
White	98.54	24.70	-0.0002	0.0158	0.965	1.139	0.1482	0.2918
	(18.06)***	(22.58)	(0.0026)	(0.0028)***	(0.083)***	(0.166)***	(0.0220)***	(0.0285)***
Black	-158.52	(219.95)	0.0715	0.0891	-0.305	-0.562	0.0356	0.0978
	(17.92)***	(21.37)***	(0.0041)***	(0.0050)***	(0.088)***	(0.150)***	(0.0257)	(0.0320)***
Asian	-62.57	(56.74)	0.0033	-0.0071	0.123	0.406	0.0109	-0.143
	(16.03)***	(17.47)***	(0.0028)	(0.0040)*	(0.053)**	(0.066)***	(0.0226)	(0.0323)***
Hispanic	121.72	68.54	-0.0184	-0.0077	-0.247	-0.149	-0.1401	-0.0775
	(21.90)***	(27.07)**	(0.0029)***	(0.0027)***	(0.106)**	(0.1430)	(0.0267)***	(0.0314)**
Constant	3,208.88	3121.30	0.0934	0.1474	11.939	24.7910	-0.2703	0.7611
	(127.20)***	(143.95)***	(0.0403)**	(0.0781)*	(1.125)***	(6.465)***	(0.7408)	(0.2908)**
Observations	802940	#########	802940	622312	787231	608752	672655	513600
R-squared	0.26	0.16	0.1	0.08	0.52	0.29	0.5	0.42

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: This table presents both difference-in-difference (DD) and difference-in-difference-in-difference (DDD) estimates of the effect of the 1993 expansion on the four outcomes of interest. The coefficient EITC*THIRDKID*POST is the DDD estimate in the odd-numbered columns, and the coefficient THIRDKID*POST is the DD estimate in the even numbered columns. The treatment group is third and higher born children, and the control group is first born. The population is a pooled sample of single and married women.

Table 9: Falsification Tests on Difference-in-Difference-in-Difference on Birth Weight

	1991 Placebo			1992 Placebo		
	(1)	(2)	(3)	(4)	(5)	(6)
	All marital	Single	Married	All marital	Single	Married
EITC*THIRDKID*PLACEBO	2.28	11.24	2.89	1.13	9.79	2.39
	(2.70)	(10.56)	(3.78)	(2.57)	(10.75)	(3.46)
NOHS*PLACEBO	6.06	-2.84	3.49	7.85	-1.17	5.05
	(2.05)***	(4.94)	(2.12)	(2.42)***	(7.19)	(3.10)
EITC*POST	6.06	-2.84	3.49	7.85	-1.17	5.05
	(2.05)***	(4.94)	(2.12)	(2.42)***	(7.19)	(3.10)
THIRDKID*PLACEBO	2.25	0.64	2.31	1.83	-1.2	2.02
	(1.96)	(11.23)	(1.83)	(2.26)	(10.60)	(2.21)
EITC*THIRDKID	-84.43	3.23	-81.65	-83.72	5.91	-80.98
	(14.33)***	(15.04)	(13.85)***	(13.90)***	(12.90)	(12.90)***
EITC	-142.9	-137.58	-109.86	-142.02	-138.51	-109.46
	(10.13)***	(10.99)***	(9.15)***	(9.81)***	(10.54)***	(9.05)***
PLACEBO	-12.79	-1.59	-13.84	-21.9	-4.03	-18.58
	(3.91)***	(5.70)	(2.29)***	(3.96)***	(8.07)	(3.42)***
White	80.27	-36.4	132.92	80.27	-36.3	132.8
	(21.52)***	(22.37)	(16.94)***	(21.49)***	(22.42)	(16.93)***
Black	-183.7	-252.29	-97.85	-183.69	-252.2	-97.97
	(20.96)***	(20.89)***	(17.56)***	(20.94)***	(20.92)***	(17.56)***
Asian	-84.37	-172.30	-44.29	-84.39	-172.09	-44.47
	(19.70)***	(19.19)***	(14.83)***	(19.66)***	(19.22)***	(14.84)***
Hispanic	102.96	6.45	149.15	102.97	6.51	149.04
	(26.11)***	(32.25)	(18.13)***	(26.05)***	(32.28)	(18.09)***
Constant	2,837.20	3,459.13	2,831.05	2,852.55	3,457.44	2,849.29
	(377.63)***	(91.37)***	(287.09)***	(386.64)***	(91.72)***	(295.64)***
Observations	564488	233608	330880	564488	233608	330880
R-squared	0.27	0.18	0.22	0.27	0.18	0.22

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: The first 3 columns define the variable PLACEBO as equal to one for all years past 1991, and the second three columns define PLACEBO as equal to one for all years past 1992

Table 10: Adjusted Treatment Effect Estimates

(A) Adjusted Difference in Difference Estimates

	All Marital	Single	Married
Birth Weight	13.27	24.44	15.63
	(4.899)***	(11.681)**	4.917***
Low Birth Weight	-0.005	-0.01	-0.0056
	(.0016)***	(.00377)***	(.0012)***
Prenatal Visits	0.0129	-0.0195	0.15
	(0.0530)	(0.1076)	(.0486)***
Tobacco	-0.0127	-0.024	-0.01831
	(.0040)***	(.0075)***	(.0039)***
% EITC eligible (1/α)	0.55	0.44	0.63
Scaling factor (α)	1.8182	2.2727	1.5873

(B) Adjusted Difference-in-Difference-in-Difference Estimates

	All Marital	Single	Married
Birth Weight	25.08	53.87	24.07
	(5.769)***	(17.528)***	(6.122)***
Low Birth Weight	-0.006	-0.0126	-0.005
	(.002)***	(.0075)*	(.0017)***
Prenatal Visits	-0.05	-0.159	0.078
	(0.0666)	(0.1162)	(0.05789)
Tobacco	-0.0026	0.0068	-0.0069
	(0.01)	(0.02)	(0.00)
% EITC eligible (1/α)	0.55	0.44	0.63
Scaling factor (α)	1.8182	2.2727	1.5873

Notes: Panel A presents delta method-adjusted difference-in-difference estimates of the treatment effect. Panel B presents the delta method-adjusted difference-in-difference-in-difference estimates. The control group for the DD estimates is first born children with less than a HS degree. The DDD estimates indicate the difference in the "treatment effect" between those those that received the expansion (no HS degree) and those that did not (college degree).