The Earned Income Tax Credit and Infant Health Revisited

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Hoynes, Miller and Simon (2015), henceforth HMS, report that the national expansion of the Earned Income Tax Credit (EITC) is associated with decreases in low birth weight. We question their findings. HMS's difference-in-differences estimates are unidentified in some comparisons, while failed placebo tests undermine others. We contend that HMSs' associations are confounded by the waning of the crack epidemic. Data from New York City birth certificates show differential exposure to cocaine by race and parity and its link to low birth weight between 1983-1998. Identifying small, causal effects of a national policy at single point in time is exceedingly challenging.

JEL H24; I12; I38; J13.
Keywords: Low birth weight; earned income tax credits; difference-in-differences

Acknowledgements: We thank Robert Kaestner, Michael Grossman, Kitt Carpenter, David Jaeger and Hilary Hoynes for comments and Tim Moore for data on homicide rates. We are grateful to seminar participants at Baruch College's Marxe School of Public and International Affairs and Queen's University Belfast, the CUNY Graduate Center and the NBER Summer Institute.

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Identifying the effect of a national policy change on birth outcomes at a single point in time is exceedingly challenging. The potential for confounding from coincident changes looms large without a sharp discontinuity in the outcome, convincing evidence of plausible mechanisms, and a clear counterfactual. In an award-winning article, Hoynes, Miller and Simon (2015, henceforth HMS) use variation in the Earned Income Tax Credit (EITC) by family size pre and post the 1993 expansion to test the effect of an income transfer to working families on birth outcomes.

The question of whether income transfers can improve birth outcomes is fundamental to U.S. social policy. First, preterm birth (< 37 weeks gestation) is the most important predictor of infant mortality. Over two-thirds of all low birth weight births (< 2500 grams) are preterm. Second, there is a clear inverse gradient between adverse birth outcomes and socio-economic status. Third, the long-term effects of low birth weight on adult health appear significant (Almond and Currie 2011). With expenditures of over 63 billion dollars and 26 million recipients in 2013, the EITC is considered a highly successful anti-poverty program designed to encourage employment among low-income earners (Nichols and Rothstein 2016). Should the EITC also improve infant health, then its welfare-enhancing impact would be even greater.

HMS follow a large literature that evaluates the EITC on primarily employment by using a difference-in-difference (DD) design to compare the birth outcomes of single, less educated women who are having a first child with those having second and higher order births (Eissa and Liebman 1996; Meyer and Rosenbaum 2000; Meyer 2002; Eissa and Hoynes 2004; Eissa and Nichols 2005). The largest increase in the EITC occurred with Omnibus Budget Reconciliation Act of 1993 (OBRA93). HMS contrast the change in low birth weight from 1991-1993, their pre-period, to 1994-1998 the post period. They find that the change in the generosity of the EITC after 1993 lowers the incidence of low birth weight by 0.35 percentage points over a mean of 10.2 percent or a decline of 9 grams over a mean of 3206 grams by comparing single women with children to those without. HMS report associations between EITC and more prenatal care and less prenatal smoking as evidence of plausible mechanisms. HMS then extend the analysis to include the EITC expansions in 1986, 1990 and 1993. They find a $1000 increase the maximum available benefit lowers the rate of low birth weight by 0.30 percentage points, a finding consistent with the DD analysis from 1993. [1]

Despite an extensive set of analyses, the only comparison with a potentially credible design is between women of parity 2 to women of parity 3+. HMS's DD analysis of the effect of the EITC expansion in 1993 on low birth weight between women of parity 1 and 2 lacks a valid pre-period. Women of parity 2—the treatment group—experienced growth in available tax credits throughout the pre-period because the 1990 EITC expansion was phased in over three years (1991-1993). A less contaminated design would have been to use 1987-1990 as the pre-period and 1991-1998 as the post-period. There was no change in the EITC from 1987-1990, a continuous increase each year in available tax credits from 1991 to 1994 and no further relative expansion between women of parity 2 versus parity 1 from 1995 to 1998 (see Figure 1). We show, however, that profound differential trends by parity from 1987-1990 undermine any comparison between parities 1 and 2.

By contrast, the comparison between women of parity 2 to women of parity 3+ provides a more credible design. As can be seen in Figure 1, there is a large divergence in the available tax credit between women of parity 2 and parity 3 or higher after 1993 and barely any before. Most research on the EITC has focused on this expansion and with good reason (Meyer 2002; Eissa and Hoynes 2004; Hotz, Miller and Scholz 2006; Evans and Garthwaite 2015). By 1996 women of parity 3 or higher were eligible for $1,283 more in tax credits in 1995 dollars than women of parity 2 (Figure 1). HMS find that

[1] The paper by HMS is the first published analysis of the effect of the 1993 expansion of the EITC on infant health. Strully, Renkhop and Xuan (2010) use variation in state EITCs to analyze their association with birth weight. However, by 1993 only 4 states had a refundable tax credit (Maryland, Minnesota, Vermont and Wisconsin) while Rhode Island had a nonrefundable credit (Hotz and Scholz 2001). Moreover, the size of the state tax credits, were roughly one-fifth the magnitude of the federal tax credits and yet, Strully, Renkhop and Xuan (2010) report estimated effects on birthweight that are twice as large as HMS.

the EITC is associated with a decrease in low birth weight among women of parity 3+ relative to parity 2. The finding, however, is only true for black women who make up approximately 20 percent of EITC filers. There is no association among whites or Hispanics. But the results for black women fail a basic placebo test (Hotz, Miller and Scholz 2006). There should be no effect of the EITC on black women of parity 3 versus parity 4 or 4+ as the expansion in the EITC after 1993 was the same for each. And yet the improvement in the rate of low birth weight among black women of parity 4+ exceeds the improvement between black women of parity 3 versus 2. In addition, only the estimates for black women fail this specification test. There is no effect of the EITC between women of parity 3 and 4+ among whites and Hispanics, which is consistent with the null effect for both groups between women of parity 2 and 3+.

HMS's stated effect of the EITC on low birth weight among black women lacks a credible mechanism. Prenatal smoking is the most important modifiable risk factor for LBW. HMS's results indicate that prenatal smoking fell by 2.41 percentage points more among black women of parity 3+ relative to parity 2. This implies an income elasticity of -2.75 using HMS's reported change in income. This elasticity exceeds anything found in the literature and assumes that smoking is a vastly more inferior good among women of parity 3+ relative to women of parity 2. This finding also fails a placebo test in that smoking among women of parity 4 fell 1.2 percentage points more than women of parity 3.

So, what did cause the fall in the rate of low birth weight among black women of higher parity? We contend that HMS confound the waning of crack-cocaine epidemic in the early 1990s with changes in the EITC. We cite a vast clinical literature from the late 1980s and 1990s that shows marked differences in prenatal exposure to crack-cocaine by race and parity. As more direct evidence, we use New York City birth certificates that contain an indication of prenatal exposure to heroin and cocaine. New York City was one of the earliest and hardest hit areas by the crack epidemic. We show large differential trends in low birth weight and prenatal drug use by race and parity from 1984-1998 that track closely with the rise and fall in homicides over the same period. We then return to HMS's DD models and add the national homicide rates among young black males as a proxy for the evolution of crack cocaine markets in the U.S. (Blumstein 1995; Cork 1999; Fryer et al. 2010; Evans, Garthwaite and Moore 2016, 2018). The inclusion of homicide rates, eight data points at the national level, eliminates the effect of the EITC in HMS's specifications. Although not definitive, these additional data provide compelling evidence as to the possible confounding in HMS estimates.

II. The 1993 EITC Expansion: Parity 1 vs. Parity 2

Federal legislation resulted in the expansion of the EITC in 1986, 1990 and 1993. The expansions in 1990 and 1993 were phased in over the subsequent three years: 1991-1993 and 1994-1996. Up until 1994, the available tax credit through the EITC was almost identical for families with one or more children (parity 2+ as specified by HMS).[2] As shown in Figure 1, the available tax credit for women of parity 3+ increased greatly relative to women of parity 2 beginning in 1994. In that same year women of parity 1 became eligible for a tax credit for the first time. These staggered expansions make comparisons between women of parity 2 versus parity 1 almost impossible when the study period is limited to effective tax years 1991-1998 as used by HMS. From 1990 to 1993 the ETIC credit increased by $401 (in 1995 $) for women of parity 2 relative to parity 1. From 1994 to 1998 the relative increase was $264.[3] In other words, the increase in the EITC for HMS's treatment group, women of parity 2, relative to their counterfactual, women of parity 1, is larger in the pre-period than in their post period. To lessen the impact of the 1990 increase in the EITC among women of parity 2, HMS limit the pre-period to 1991-1993. The increase over this two-year pre-period becomes $179 but is still 68 percent of the post-EITC increase. This hardly rectifies the mis-specified pre-period. First, HMS have to assume that

[2] In vital statistics data, parity is number of previous live births. A woman giving birth for the first time would parity zero or nulliparous. HMS define parity as the current plus previous births. Thus, a pregnant woman carrying her first child is defined as parity 1 and a pregnant women with one previous birth is defined at parity 2 by HMS.
[3] As shown in Figure 1 EITC increases by $578 between 1994 and 1998 for women of parity 2 but the EITC increase by $314 for women of parity 1 over the same period. The net increase is $264 (578-314).

there are no lagged effects from the $222 increase in the EITC from 1990 to 1991, which is inconsistent with their statement regarding the 1993 expansion.[4] Second, HMS mapping from the child's birth date to the year the mother is exposed to the EITC, what HMS refer as the woman's "effective tax year," is an approximation at best. [5] For instance, HMS assume that women who gave birth in January through April of 1992 were subject to the EITC in 1990. But many of these women were likely working in 1991 and received the higher 1991 tax credit in 1992. We agree with HMS that mapping a pregnancy with exposure to the EITC is not precise, but limiting the pre-period to 1991-1993 does little to mitigate the cumulative exposure of their treatment group to increases in the EITC during their pre-period. It is also unnecessary. The years 1987-1990 provide a superior pre-period with 1991-1998 as the more appropriate post period (see Figure 1). Specifically, there is no change in the EITC from 1987 to 1990 between women of parity 2 versus parity 1. There is a continuous relative increase from 1991 to 1994 of $631 in 1995 dollars for women of parity 2 versus parity 1 that remains unchanged from 1995-1998. Indeed, the $631 increase in available tax credits for women of parity 2 compared to parity 1 is the largest relative expansion in a 4-year period between the two groups over the entire existence of the EITC.

To illustrate, we first replicate HMS's DD regression estimates comparing the rate of low birth weight among women of parity 2 relative to parity 1 using 1991-93 as the pre-period and 1994-1998 as

[4] In discussing the phase in of the 1993 expansion they write, "In addition, the treatment effects increase with years since 1993 which is consistent with the phased-in expansion" (HMS, p. 187).

[5] For instance, HMS assume that the effect of the EITC impacts infant health in the third trimester. The restriction has two implications. First, a third of women who gave birth in 1992 were assigned to the effective tax 1990 by HMS. Many were pregnant in 1991 but likely working and thus eligible for higher 1991 tax credit in February of 1992. We appreciate that there is no perfect mapping from birth year to tax year exposure which is all the more reason to use 1987-1990 as the pre-period during which there were no changes in tax credits. Second, the third trimester is not only point in a pregnancy that could benefit from an intervention. Neural tubal defects, which occur early in pregnancy, are vulnerable to smoking and drinking. Precursors of preterm birth such as vaginal bleeding, preeclampsia and gestational diabetes should be diagnosed and treated whenever they arise during pregnancy. This explains the current emphasis on pre-conception health (see https://www.acog.org/Clinical-Guidance-and-Publications/Committee-Opinions/Committee-on-Gynecologic-Practice/The-Importance-of-Preconception-Care-in-the-Continuum-of-Womens-Health-Care).

the post period. HMS aggregate individual-level birth certificate data for low birth weight into 47,687 non-zero cells for single women with no more than a high-school education defined by state, year, parity, maternal education, race, ethnicity and age from 1981 to 1998 and estimate the following regression (HMS, Appendix B).

$$(1)\ Y_{pjst} = \alpha + \delta\left(After_t * Treat_p\right) + \pi X_{st} + \rho_p + \varphi_j + \delta_t + \tau_s + \epsilon_{pjst}$$

Y_{pjst} is the rate of low birth by parity (ρ_i), demographic groups (φ_j), state (λ_s) and year (δ_t). X is set of state policies: Medicaid/SCHIP, welfare reform and the state unemployment rate. $Treat_p$ equals one for the parities experiencing expansion in available tax credits (i.e., parity 2) relative to the controls (i.e., parity 1). HMS limit the sample to single mothers with high school or less of completed schooling. We report the DD coefficient, δ, in the top panel of Table 1. Among all women, an increase in the EITC beginning in 1994 is associated with a 0.164 percentage point decline in the rate of low birth among women of parity 2 relative to parity 1 (HMS, Table 2). In Panel B we use the same pre-period as HMS but we drop California, New York, Texas and Washington because they did not report mother's education prior to 1989. The differences in the estimates between Panel A and B are modestly more supportive of HMS. In the bottom panel of Table 1, we show results from running the same specification but using 1987-1990 as the pre-period and 1991-1998 as the post-period. The protective effect of the EITC is more than twice as large for all women (-0.564), black women (-0.920) and white women (-0.325) than using 1991-1993 as the pre-period.

Estimating the effect of the EITC with a more coherent pre- and post-period appears to further support HMS's conclusions. A key assumption of the DD, however, is that trends between the "treated" and comparison group leading up to intervention should be parallel. As evidence, HMS estimate an event-study specification that shows leads and lags of the estimated DD.[6] We replicate their specification (Figure 2, Panel A) but then extend the data back to 1987 (Figure 2, Panel B). On the right vertical axis we show the relative increase in available tax credits in 1995 dollars to women of parity 2 relative to parity 1. Two points merit note. First, available tax credits increase continuously from 1990 to 1994 with no change either before or after these four years. Second, the rate of low birth weight among women of parity 2 relative to parity 1 declines continuously from 1987 to 1998 (Figure 2, Panel B) with no obvious discontinuity after 1990 or 1993. An F-test decisively rejects the null that coefficients on the interactions from 1987-1990 are different from zero ($F_{4,46}=5.73$). We show the same plots of the event-study coefficients by race in Figures 3. The decline in low birth weight prior to 1994 is even more pronounced among black women (Figure 3, Panel A).

We contend that it is not possible to isolate the effect of the 1993 EITC on the birth outcomes of women of parity 2 relative to parity 1 between 1991 and 1998. The continuous increase in available tax credits from 1991 to 1994 for women of parity 2 relative to parity 1 makes HMS's pre-period inappropriate in a DD design. If the goal of HMS's study is to assess the effect of exogenous increases in income on infant health, then a cleaner identification strategy is to use 1987-1990 as the pre-period and 1991-1998 as the post period. However, profound differential trends in low birth weight between women of parity 2 and 1 from 1987-1990 undermine this specification. We turn next to the comparisons between women of parity 2 versus parity 3+, the comparison that has been the major focus in the EITC literature.

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[6] (2) $Y_{pjst} = \alpha + \sum_{k=-2}^{5} [\delta_k(\mathbf{1}(\boldsymbol{After}_{(\boldsymbol{k=t-t^*})} * Treat_p)] + \pi X_{st} + \rho_p + \varphi_j + \delta_t + \tau_s + \epsilon_{pjst}$

$where\ 1\ is\ the\ indicator\ function\ and\ \ t* = 1993\ and\ t = 1991, \ldots 1998$

III. The 1993 EITC Expansion: Parity 2 vs. Parity 3+

The extensive literature on the employment effects of the EITC have used the large increases in the available tax credits to women of parity 3+ relative to women of parity 2 (Figure 1).[7] Between 1993-1996, the ETIC rose by $1,861 for women of parity 3+ and by $578 for women of parity 2, a net difference of $1,283 in 1995 dollars. HMS use equation (1) to estimate the difference in birth outcomes between women of parity 3+ versus parity 2 between 1991-1998. The pre-period in this design, 1991-93 is not confounded by differential exposure to the EITC of any substance, unlike the comparison for women between parity 2 and parity 1.

The results provide support for HMS's conclusion that the 1993 EITC expansion improved birth outcomes for low-income single women of parity 3+ relative to women of parity 2. In Table 2, we replicate HMS's results for all women and then by race/ethnicity (HMS Tables 2 and 3). Women of parity 3+ experience declines of 0.347 percentage points more than women of parity 2, a 3 percent decline given a mean rate of low birth weight of 10.2 percent. The results for all women, however, are driven by birth outcomes among black women for whom rates of low birth weight fell 0.715 percentage points more among women of parity 3+ relative to parity 2. Importantly, there is no association between the EITC and rates of low birth weight among white or Hispanic women. The point estimate for white women is so close to zero that even the upper 95 percent confidence interval suggests a clinically inconsequential effect.

The large increase in the 1993 EITC for women of parity 3 and higher provides a natural placebo test (Hotz, Miller and Scholz 2006). There should be no differential effect of the EITC on women on parity 3 versus those of parity 4 or parity 4+ as they all experienced the same increase in the available

[7] See Meyer 2002; Meyer and Rosenbaum 2000; Eissa and Hoynes 2003; Hotz, Miller and Scholz 2006; Evans and Garthwaite 2014)

tax credits.[8] HMS report this test for all women in their Appendix, which we have replicated in column (1) of Table 2, Panels B, C and D. Specifically, the rate of low birth weight declines by 0.225 percentage points more among women of parity 3 versus those of parity 2 after the 1993 expansion in the EITC. The effect is slightly larger between women of parity 4 versus parity 3 (-0.240) and even larger for women of parity 4+ versus those of parity 3 (-0.261). HMS acknowledge that failure of this placebo test points to differential trends in low birth weight by parity. They argue, however, that the "preponderance of evidence" supports their conclusion that EITC improved infant health.[9] What HMS don't point out is that only the estimates for black women fail this test (Table 2, column 2, Panels B, C and D). There is no differential effect of the EITC on white or Hispanic women of parity 3 versus parity 4 or parity 4+, which is consistent with the null findings for both groups between women of parity 3 versus parity 2 (Table 2, columns 3 and 4).

As a further examination of potentially differential trends among black women, we follow HMS and estimate event-study regressions of the low birth weight and plot the coefficients (see equation (2) in Footnote 6). Figure 4 Panel A replicate HMS's Figure 3 for women of parity 3+ to parity 2 but with the pre-period extended back to 1983. Figure 4 Panels B and C show the same event-study comparisons for black women of parity 4 versus parity 3 and then women of parity 4+ versus parity 3. The pattern in each figure is the same. There is a large differential increase in low birth weight of black women of the higher parity relative to the lower parity in each panel from 1983 and to roughly 1990 followed by a

[8] One might argue that differential increases in labor force participation and by women of parity 3 versus parity 4+ might explain the failed placebo tests. The effect of work on birth outcomes is ambiguous at best and an unlikely explanation for the failed test (Bonzini et al. 2007; Snijder et al. 2012).

[9] HMS write, " 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" (p. 205). As we show below, the "maxcredit" models show evident sensitivity to controls for trend.

differential decline after 1993. The event-study figures combined with the failed placebo tests underscore the threat of differential trends in low birth weight by parity for black women. We discuss potential confounders in Section VI.

IV. The EITCs of 1986, 1990 and 1993

The EITC was expanded three times between 1983 and 1998.[10] To evaluate the impact of all three expansions, HMS again aggregate individual-level birth certificate data to cells defined by state, year, parity, maternal education, race, ethnicity and age from 1983 to 1998 (HMS, Appendix B). They limit the sample to single, unmarried women who gave birth. To account for the multiple expansions HMS estimate the following equation:

$$(2)\ Y_{pjst} = \alpha + \delta Maxcredit_{pt} + \pi X_{st} + \rho_p + \varphi_j + \delta_t + \tau_s + \epsilon_{pjst}$$

Y_{pjst} is the rate of low birth by parity (ρ_p), demographic groups (φ_j), state (λ_s) and year (δ_t). X is set of state policies: Medicaid/SCHIP, welfare reform and the state unemployment rate. $Maxcredit_{pt}$ is the maximum tax credit available to eligible filers that varies by parity and year. HMS include an additional term (p_p* T) to control for linear trends by parity. The concern is well-founded. As the event study graphs show trends in low birth weight differ substantially by parity and race beginning in 1983.[11] The importance of correctly adjusting for the time-series pattern in low birth weight becomes apparent in Table 1. In column (1) we replicate HMS's results from the specification that does not include a linear trend in parity. The coefficient for all women indicates that a $1000 increase in the maximum available credit is associated with -0.304 percentage point decline in the rate of low birth weight. The effects for white women are substantially smaller [-0.117, column (4)], while those for black women are much

[10] The Tax Reform Act, 1986; Omnibus Budget Reconciliation Act, 1990; and the Omnibus Budget Reconciliation Act, 1993 (Nichols and Rothstein 2016).

[11] HMS show unadjusted trends in low birth weight by race and parity in their online Appendix which make the same point.

larger [-0.518, column (7)]. Inclusion of the linear trend term in parity alters the estimates substantively (Table 1, columns 2, 5 and 8). In this specification there is no association for white women whereas the coefficient for black women increases by 160 percent. According to these estimates, a $1000 increase in maximum available credit lowers the rate of low birth weight among black women by a sizeable 1.4 percentage points over a mean of 14.9. We next add an interaction of parity and time squared to equation (1) given the curvilinear relationship to low birth weight. All associations between *Maxcredit* and low birth weight are eliminated (Table 1, columns 3, 6 and 9).

One argument against trend terms interacted with parity is that we may have overfitted the data and absorbed the effect of the EITC with the trend terms. As a check we replicate the regressions in Table 3 but limit the sample to years 1983 to 1993. During this period the EITC was expanded twice in 1986 and 1990. The combined increase was $772 (in 1995 dollars) for women of parity 2 relative to parity 1, which is three times greater than the relative increase of $264 from 1993 to 1998. Over the same period the number of tax filers more than doubled.[12] Despite this expansion there is no effect of the EITC on low birth weight among all women, no effect among black women, but a statistically significant effect among white women. When we add linear trends by parity following HMS all associations are lost. We then test for quadratic trends in low birth weight by parity from 1983 -1993 given the non-linearities by parity shown in Figure 3. We continue to reject the null hypothesis that prior to the 1993 EITC expansion there were no differential quadratic trends by parity.

Although HMS focus in on the 1993 expansion, the analysis prior to 1994 is relevant for several reasons. First, there is no association between the EITC and low birth weight among black women prior to 1994. This is the only group for whom HMS find an association between the EITC and low birth weight using the superior experiment from the literature. Second, quadratic trends in low birth weight

[12] See https://www.taxpolicycenter.org/statistics/eitc-recipients (last accessed November 16, 2018)

by parity are present prior to the 1993 EITC expansion, which suggest violation of the parallel trend assumption in HMS's DD analysis. Third, the differential trends by parity prior to 1993 are consistent with the failed placebo tests between black women of parity 3 and 4 in the DD analysis above. Fourth, HMS find no effect of the 1993 expansion of the EITC among white and Hispanic women of parity 2 versus parity 3+. This is also consistent with the absence of an association among white women prior to 1994 when HMS control for linear trends in low birth weight by parity.

V. The Etiology of Low Birth weight and Plausible Mechanisms

The lack of an association between the EITC and low birth weight is consistent with the inability to prevent preterm birth as reported in the clinical literature. Low birth weight (infants less than 2,500 grams) consists of infants born preterm and/or those who were growth restricted. In 2015, 69 percent of all low birth weight births were preterm. In an exhaustive review of the literature on preterm birth, the Institute of Medicine (IOM) states in the abstract, "The current methods for diagnosis and treatment of preterm labor are currently based on an inadequate literature, and little is known how preterm birth can be prevented" (Institute of Medicine 2007, p.2). Nevertheless, HMS propose two mechanisms by which the EITC might protect against adverse birth outcomes: prenatal care and prenatal smoking.[13] We find prenatal care to be an implausible explanation. First, the association between prenatal care and birth outcomes in the public health literature is so confounded by selection bias that most estimates can be dismissed. The few randomized studies of augmented prenatal care have reported almost uniformly, no association with improved infant health (Collaborative Group on Preterm Birth Prevention 1993; Goldenberg and Rouse 1998; Goldenberg and Culhane 2007; Alexander and Kotelchuck 2001; Klerman et al. 2001; Carroli et al. 2001). The multicenter RCT of preterm prevention is a particularly germane example (Collaborative Group on Preterm Birth Prevention 1993). The intervention targeted women at

[13] Our discussion focuses on the sample of black women as there is little consistent evidence an association between the EITC and birth outcomes for either white or Hispanic women.

high risk for preterm birth. The treatment group received weekly examinations beginning in weeks 20-24 until delivery. Patients in the treatment group were also trained to recognize the signs of preterm labor. The intervention conferred no benefit despite a level of support that far exceeded routine prenatal care. Even based on the questionable estimates of prenatal effectiveness in the public health literature, the change in prenatal care associated with the EITC is inconsequential. HMS report, for example, that the EITC is associated with a change of 0.6 percentage point change in prenatal care visits before the third trimester. This is less than a one-percent increase evaluated at the mean, a change so small as to be clinically irrelevant. Lastly, trends in prenatal care vary by parity and the effect of care fails the placebo test between women of parity 3 and 4. [14]

Prenatal smoking, on the other hand, is considered the most important modifiable determinant of low birth weight (Floyd et al. 1993; Heath 2001). Based on HMS's data and specifications, the EITC lowered the prevalence of smoking by 2.41 percentage points among black women of parity 3 relative to parity 2 over a mean prevalence of 19 percent. The finding is implausible for two reasons. First, it assumes smoking is an inferior good among low income, single women for which there is little evidence. A meta-analysis by economists suggest that, on balance, smoking is a normal good (Gallet and List 2003). More implausibly, the implied income elasticity using HMS's data is -2.75, a huge response that is also outside the bounds of the literature for even a normal good let alone an inferior one. A second implausible pathway is working. Increased labor force participation associated with the EITC could reduce smoking through workplace smoking bans. But even the most generous increases in labor force participation can explain only a trivial decline in smoking.[15] Third about 15-20 percent of women quit

[14] We refer readers to Appendix Table 2.

[15] For instance, the EITC is also associated with increases in labor force participation of 10 percentage points among black women of parity 3 relative to parity 2 (HMS, Appendix Table 1). If these women worked at institutions with workplace smoking bans, smoking would be expected to fall. However, the decline given current research is too small to cause the decrease reported by HMS. Consider a population of 100,000 black women. Assume 20 percent or 20,000 smoke. 10,000 enter the workforce, given HMS's results, of which 2000 smoke.

smoking during pregnancy but the reason given are the fear of adverse birth outcomes, medical advice and nausea (Floyd et al. 1993). HMS are suggesting that the cash transfers from the EITC received mostly in February and spent largely on durables and transportation induce dramatically greater quit rates among women of parity 3 relative to parity 2. Fourth, the smoking estimates also fail the placebo test as black women of parity 4 quit more than black women of parity 3 (Appendix Table 1).

VI. Another Explanation

Despite the lack of interventions to prevent preterm birth, trends in low birth weight reported by HMS and documented here vary significantly by race and parity. One plausible factor consistent with differential trends in low birth weight by race and parity is the impact of the crack-cocaine epidemic of the 1980s and 1990s. There is broad consensus among social scientists that growth in crack-cocaine markets expanded rapidly in poor urban areas between 1984 and 1990. The spread of crack lead to a dramatic upsurge in homicide that was concentrated among young black males between the ages of 15-24 (Blumstein 1995; Cook and Laub 1998; Cork 1999; Blumstein, Rivara and Rosenfeld 2000; Grogger and Willis 2000; Fryer et al. 2013; Evans, Garthwaite and Moore 2016, 2018). Use of cocaine and its attendant lifestyle also had a profound impact on black women giving birth (Dunlap et al. 2006). Rates of low birth weight among *all black* women in New York City rose 2.6 percentage points between 1984 and 1988 from 10.5 to 13.1 percent, reversing a 21-year decline (Joyce 1990; Joyce and Racine 1993). In the largest population prevalence study ever undertaken, 29,494 women were tested for perinatal substances at 202 California hospitals in 1992. The percent of women exposed to cocaine at delivery was 13 times greater among black non-Hispanics (7.79 percent) than white, non-Hispanics (0.60

Based on Evans et al. (1999), 7000 women would end up working at sites with workplace bans of which 1400 smoke (0.20* 7000). Bans reduce smoking by 5 percentage points or by 350 (.05*7000). As a result, smoking in the population would fall from 20,000 to 19,650 or from 20 percent to 19.65 percent—a decline of 0.35 percentage points. Further even if all 2,000 black women that entered the workforce quit smoking this would only explain a decline in smoking prevalence for this group of 2.0 percentage points. Even with the most implausible assumptions, we cannot explain the 2.41 percentage point drop in smoking based on HMS data.

percent) and Hispanics (0.55 percent) (Vega et al. 1993). Prenatal exposure to crack was also concentrated among older, higher parity women. Hospital-based studies of prenatal cocaine exposure found the average age of users was between 25 and 29 and the average parity was 3 as computed by HMS (Hadeed and Siegel 1989; McCalla et al. 1991; Phibbs, Bateman and Schwartz 1991; Handler et al. 1991; Bateman et al. 1993; Eyler et al. 1994; Chazotte et al. 1995; Singer et al. 2002). A meta-analysis of 30 studies on the association between prenatal cocaine use and adverse birth outcomes reported that mean birth weight was 491 grams lower and that the odds of low birth weight was 3.4 times greater among women exposed relative to those unexposed to cocaine. The authors emphasize that tobacco was a frequent concomitant risk factor, which tightens the link to low birth weight (Gouin et al. 2011).

There are no national data on prenatal exposure to crack-cocaine let alone by race and parity. However, the New York City Department of Health collected information on drug use during pregnancy as part of its vital registration system for births. From 1980 to 1987 there was an indication for prenatal exposure to narcotics as a medical risk factor. Beginning in 1988, the indication was refined to include separate codes for heroin, cocaine and marijuana. We tabulate the percent of births with an indication of narcotic use during pregnancy from 1980-1987 and splice it with the percent of births with an indication of cocaine and heroin from 1988 to 2000 by race and parity.[16] In figure 5 Panel A we show the percent of low birth weight births and the percent of births exposed prenatally to illicit drugs. The data pertain to black women only with separate series for women of parity 2 and parity 3+. In Panel B of Figure 5 we have normalized the series to 1993. Both Panels demonstrate that the change in prenatal drug use rose faster among black women of parity 3+ relative to parity 2 beginning around 1985 as did

[16] Screens for exposure to cocaine as reported on birth certificates are based on self-reports and may include a physician's indication based on the medical chart. There is little doubt that the true prevalence is underreported (Behnke et al. 2013). Nevertheless, the birth certificate data likely reflect trends in exposure while not accurately reporting the level. There is, for example, unlikely to be many false positives of drug exposure given the stigma associated with the indication.

the rate of low birth weight. Figure 6 shows the same two panels for white women. The data are noisier with a less obvious distinction in low birth weight and drug use by parity.

We next overlay the differential rate of prenatal drug use between black women of parity 3+ relative to parity 2 with the NYC homicide rate and the national homicide rate among black males 15 to 24. All series are normalized to 1993. The point is twofold: first, all series begin to rise after 1985, peak around 1991 and decline thereafter. Second, to the extent that the rise in homicide rates reflects the spread of crack-cocaine markets within New York City and the nation, they are consistent with the clinical literature which reported greater exposure among black women of higher relative to lower parity.

Re-estimating the DDs with Homicide Rates

In this section we use the national homicide rate among black males 15-24 as a proxy for the spread of crack-cocaine markets in the 1980s and 1990s. We add this one variable to HMS's DD specification of low birth weight between women of parity 3+ and 2. The results in Table 4, Panel A show that there is no longer any association between 1993 EITC expansion and low birth weight among black women of parity 3+ relative to parity 2. The null results for white and Hispanic women reported by HMS remain as such. We then estimate the comparison between women of parity 4+ and parity 3. Recall that this placebo test found substantial differences between these two groups of black women when there should have been none. As we show in Panel B of Table 4, inclusion of the black male homicide rate eliminates this association as well. Lastly, we show event-study estimates between women of parity 3+ versus parity 2 by race from 1983-1998 in Figure 8. The top panel shows the black women and the bottom panel white women. We have added the national homicide rate for black and white males 15-24 over the same period. The time-series pattern for black women is similar to that for New York City (Figure 7). The homicide rate tracks the differential change in low birth weight between black women of parity 3+ versus parity 2. In the Figure for white women we use the same scale on both the left and right vertical axis to emphasize the huge difference in homicide rates by race.

A Preponderance of Evidence

Without actual data on prenatal exposure to cocaine, the use of black homicide rates to proxy for spread of crack cocaine exercise is not dispositive. Nevertheless, it in instructive to review the "preponderance of evidence" that suggests no association between the EITC and low birth weight. First, the increase in the EITC for women of parity 3+ relative to parity 2 after the 1993 EITC provides the only credible contrast. HMS's comparisons between women of parity 2 versus parity 1 lack a viable pre-period. Second, only black women of parity 3+ experience a decrease in low birth weight; there is no association among white or Hispanic women. Third, we find that the 1993 EITC lowered the rate of low birth weight more among black women of parity 4 and 4+ relative to parity 3, a finding that HMS concede points to differential trends by parity. Fourth, the null effect among whites and Hispanics between women of parity 3+ versus parity 2 is consistent with the lack of an effect in the placebo tests for these groups. Fifth there is no plausible mechanism. The decrease in smoking among black women of parity 3+ relative to parity 2 is too large to be credible. Their findings suggest smoking is an inferior good with an income elasticity of -2.75 among black women. This result also fails the placebo test as women of parity 4+ decrease smoking more than women of parity 3.

Lastly, we wipe out the association between the 1993 EITC and low birth weight among black women of parity 3+ relative to parity 2 by adding a national time-series of homicide rates to HMS's own sample and specification. The addition of these eight data points reflects a remarkable lack of robustness. Despite over 47,000 observations and a large number of fixed effects, identification is limited to variation in low birth weight by parity and year at the national level. Addition of the homicide rate is not the result of a specification search. Social scientists have frequently used the homicide rate of black males ages 15 to 24 as proxy for the spread of crack markets across the U.S. We provide additional support for this association with unique data on prenatal drug use in New York by race and parity from 1984-1998. The differential rise and fall low birth weight among black women of parity 3+ relative to parity 2 in New York City over the same period tracks both homicide rates and prenatal drug use closely. The association is further buttressed by an extensive clinical literature that documents the impact of crack cocaine on birth outcomes.

VII. Conclusion

We end by praising HMS for trying to test the effect of an exogenous income transfer on infant health. The EITC represents a sizable increase in income among working single women with children. Income transfers may indeed improve birth outcomes. What we have shown is efforts to identify small effects at the national level without a sharp discontinuity and plausible mechanisms are vulnerable to confounding from other events and policies.

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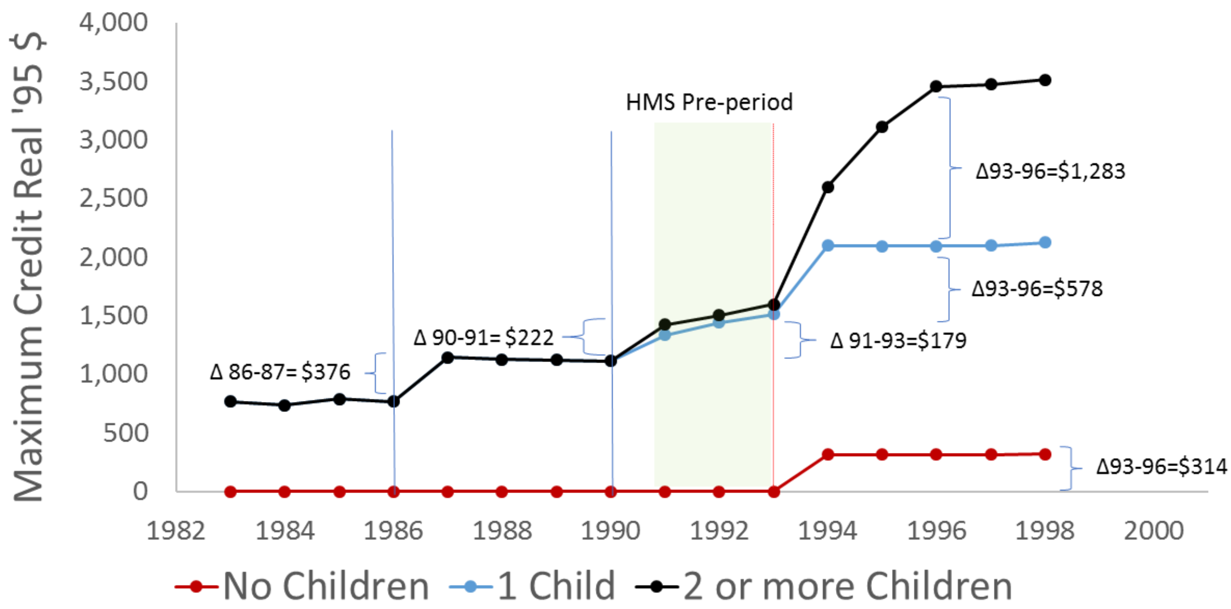
Tables and Figures

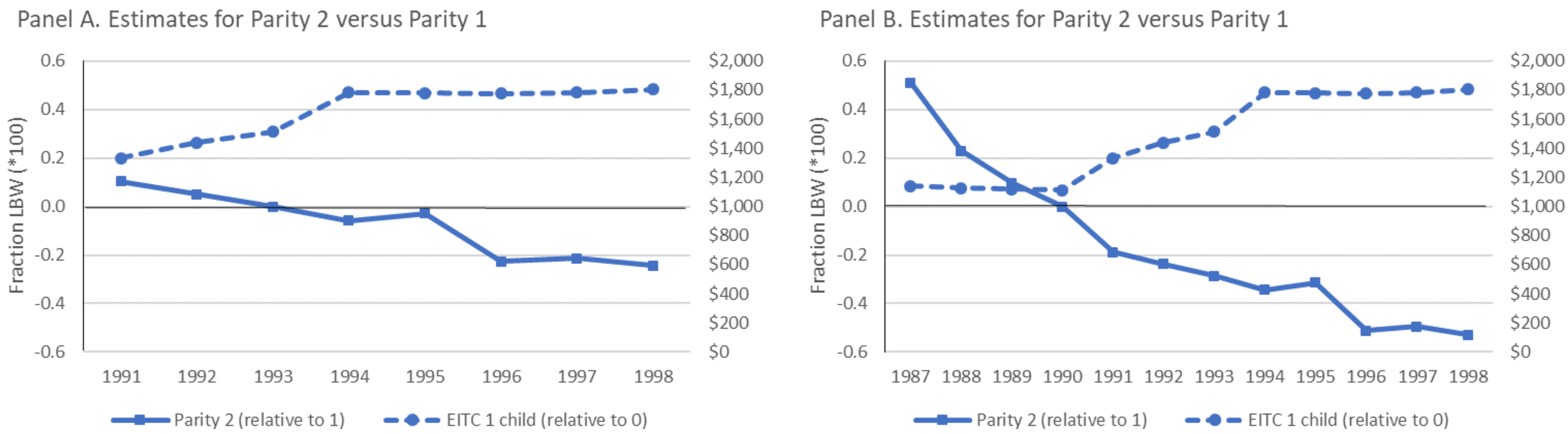
Figure 1: Maximum Available Credit from the EITC by Parity in 1995 $



Source: HMS (2015) via Tax Policy Center (2011)

Notes: The relative change in available tax credits from the EITC is $401 from 1990 to 1993 ($222+$179=$401) among women of parity 2 relative to parity 1; the relative change is $179 from 1991-1993 and $264 from 1994-1998 ($578-$314=$264) all in 1995 dollars.

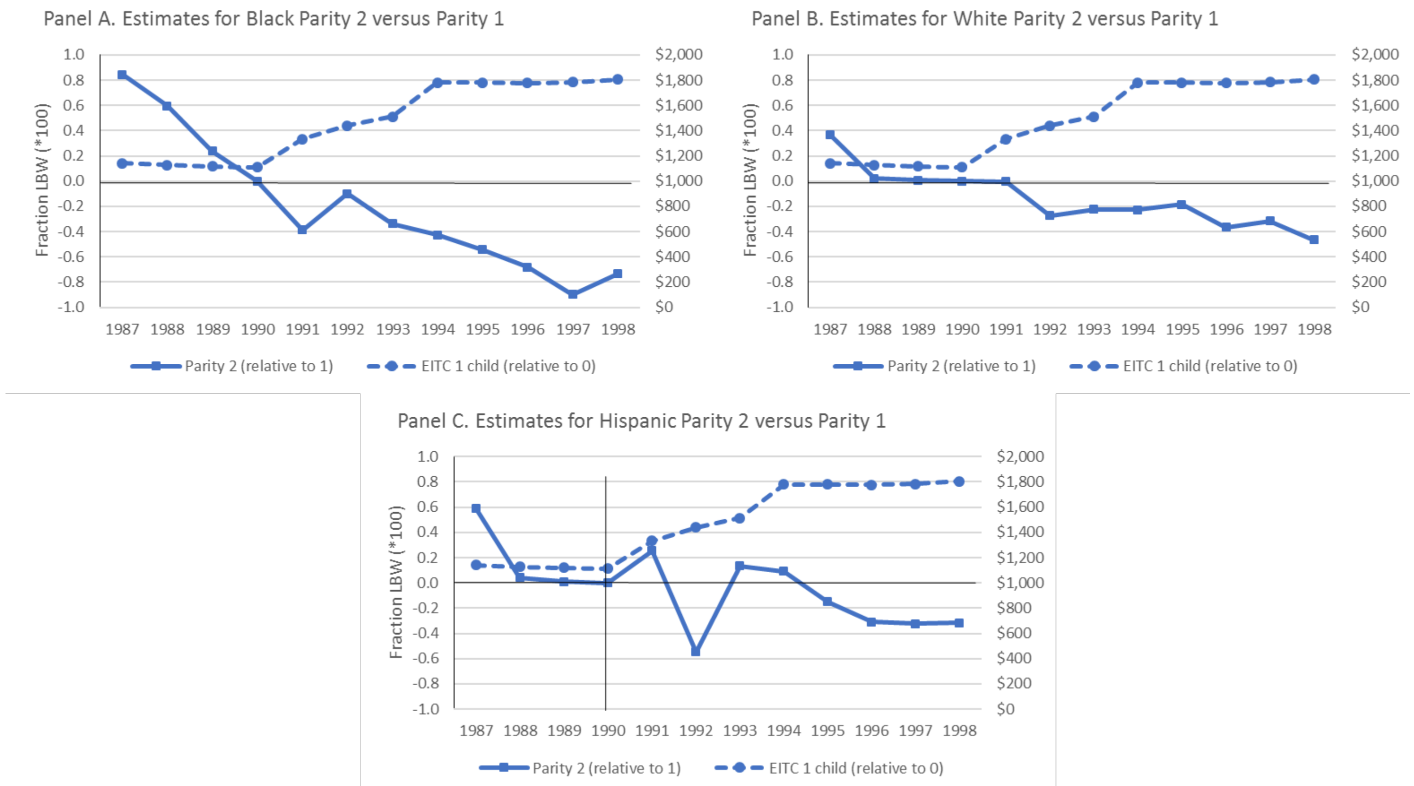
Figure 2: Event Time Estimates on Low Birth Weight, Single Women with at most a High School Diploma



Source: HMS (2015) via NCHS Birth Certificates (1989-2000)

Notes: Similar to HMS Figure 3, each figure plots coefficients from an event-study analysis where each point represents coefficients of effective year and parity 2 interactions. Similar to HMS, the specifications include controls for year, state, parity, demographic group and state-year covariates for Medicaid/SCHIP, welfare reform, and unemployment rates. These figures differ from HMS figure 3 in that they exclude California, New York, Texas and Washington because those states are missing education in some years proceeding effective year 1991. The figure shows that from 1991-1993 the trend is relatively flat and we do not reject the null that the coefficients in these years are different from zero [$F_{(2,46)}$=2.12; α=0.1]. Panel B shows a steep downward trend 1987 to 1990) in LBW among women of parity 2 relative to parity 1. An F-test rejects the null that the coefficients for these years are zero [$F_{(3,46)}$=5.42; α=0.01.]. Appendix Figure 1 shows the same but including New York, California and Texas for panel A.

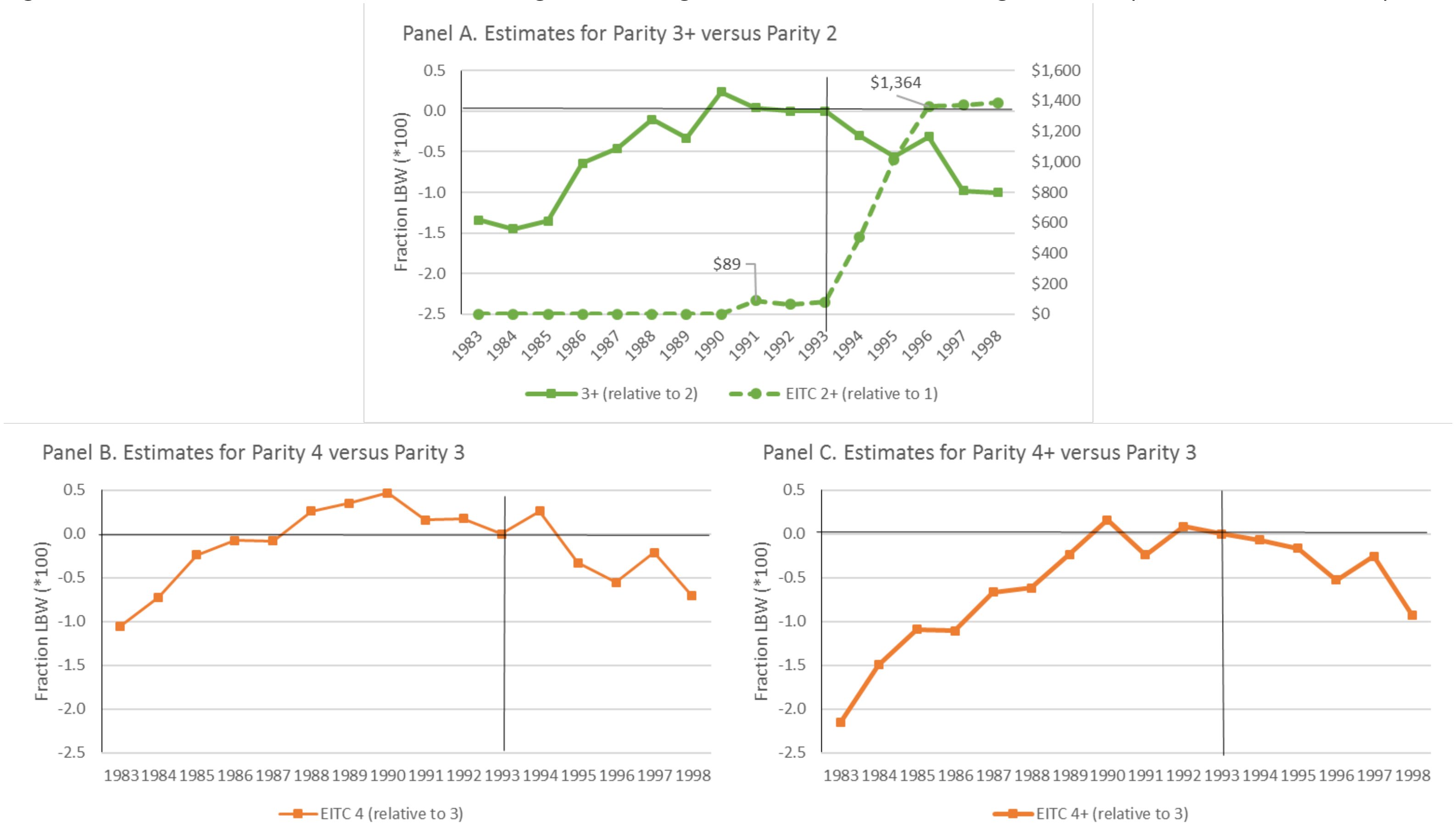
Figure 3: Event Time Estimates on Low Birth Weight, Single Women with at Most a High School Diploma by Race



Source: HMS (2015) using NCHS Birth Certificates (1989-2000)

Notes: Similar to HMS Figure 3, each figure plots coefficients from an event-study analysis where each point represents coefficients of effective year and parity 2 interactions. Similar to HMS, the specifications include controls for year, state, parity, demographic group and state-year covariates for Medicaid/SCHIP, welfare reform, and unemployment rates. These figures differ from HMS figure 3 in that they exclude California, New York, Texas and Washington because those states are missing education in some years proceeding effective year 1991.

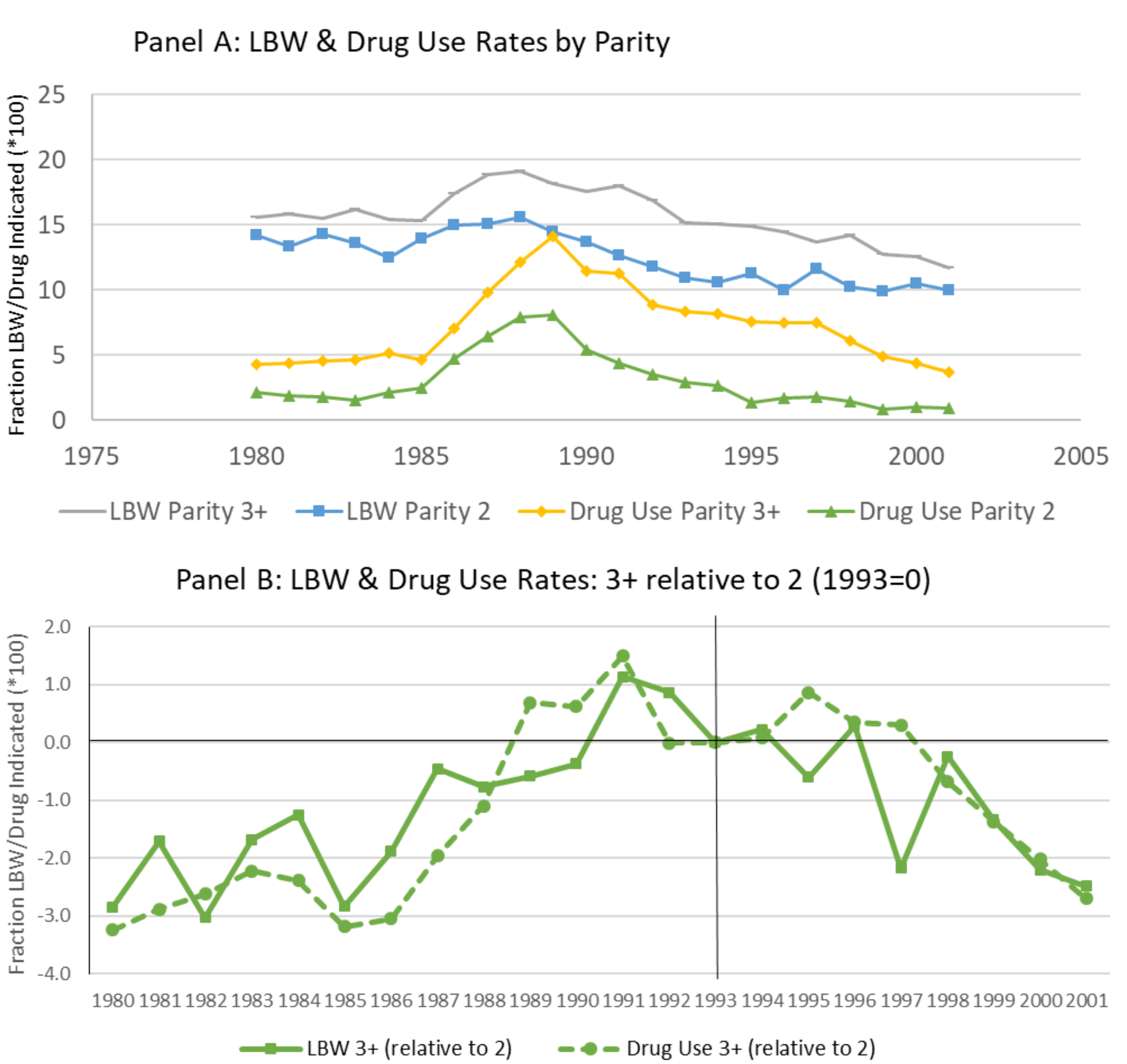
Figure 4: Event Time Estimates on Low Birth Weight, Black Single Women with at most a High School Diploma for Various Comparisons by Parity



Source: HMS (2015) using NCHS Birth Certificates (1985-2000)

Notes: Each figure plots coefficients from an event-study analysis in which each point represents coefficients of effective year and parity interactions. Similar to HMS, the specifications include controls for year, state, parity, demographic group and state-year covariates for Medicaid/SCHIP, welfare reform, and unemployment rates. Panel A contrast women of parity 3+ versus parity 2 and is most similar to HMS's Figure 3 except that it excludes California, New York, Texas and Washington because those states are missing education in some years proceeding effective year 1991. The Figures in Panel B and C provide two placebo tests as there was no differential expansion of the EITC for these parities.

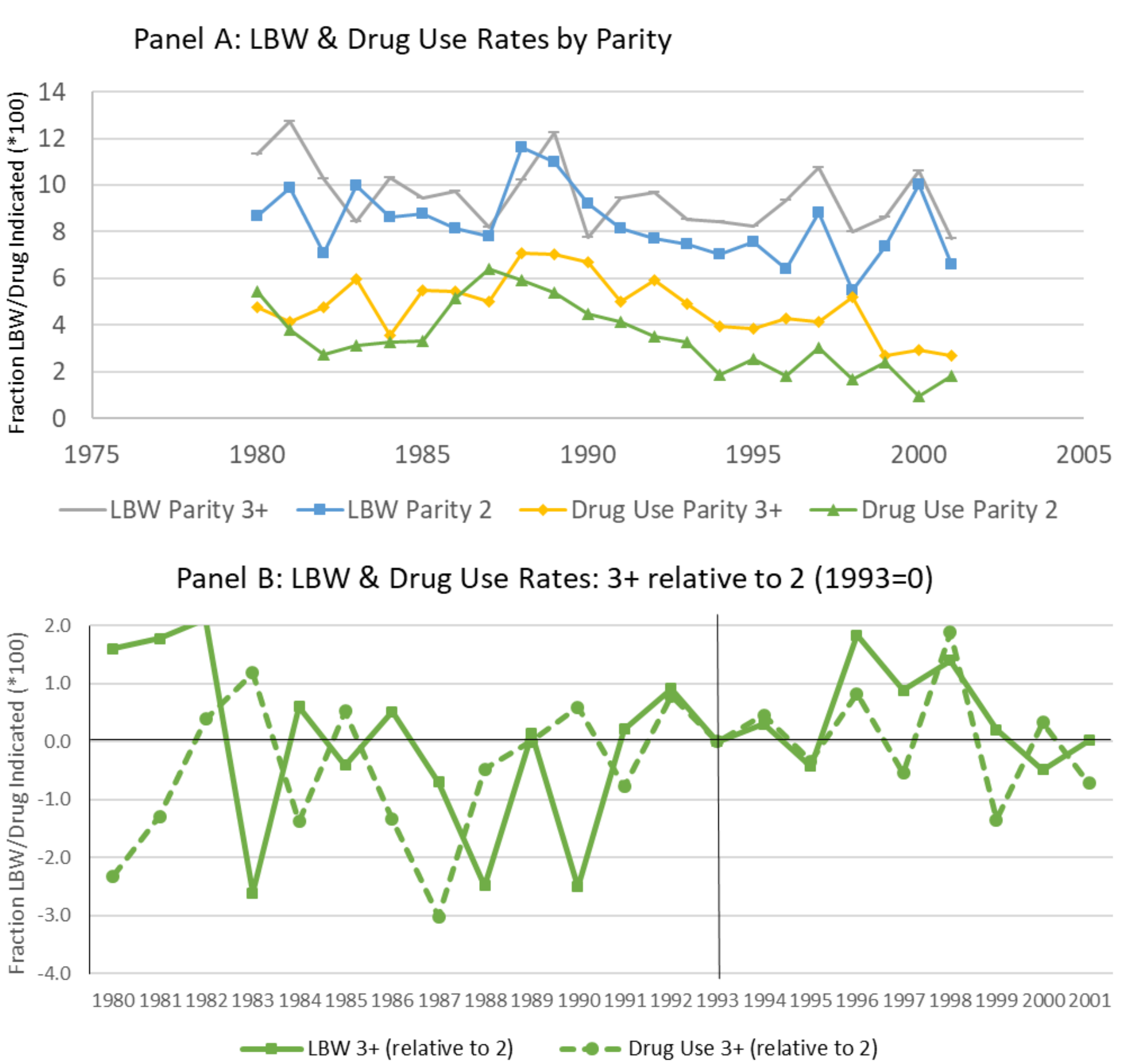
Figure 5: Percent of Births Exposed Prenatally to Narcotics and Cocaine (Drug Use) and Percent Low Birth Weight (LBW) Among Single Black Women with at most a High School Diploma in New York City by Year and Parity



Source: Authors tabulations of NYC Birth Certificates (1980-2001)

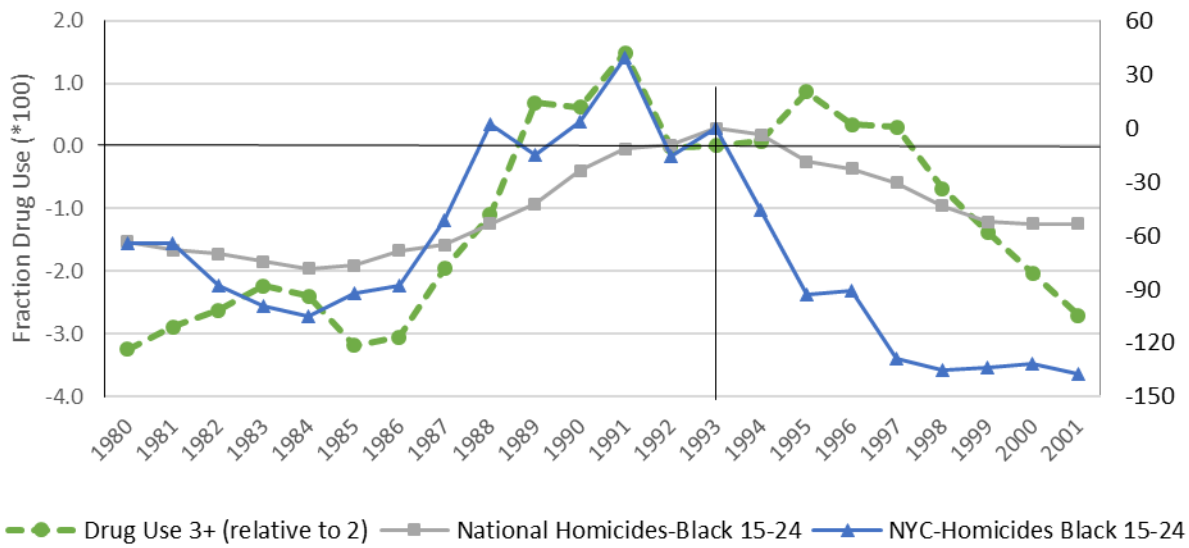
Notes: Panel A shows the absolute rate of low birth weight and prenatal use of narcotics from 1980-1987 and heroin and cocaine from 1988-1998 based on indications on the birth certificate. We refer to this as prenatal drug use. Panel B shows the same for parity 3+ relative to 2 using 1993 as the reference year.

Figure 6: Percent of Births Exposed Prenatally to Narcotics and Cocaine (Drug Use) and Percent Low Birth Weight (LBW) Among Single White Women with at most a High School Diploma in New York City by Year and Parity



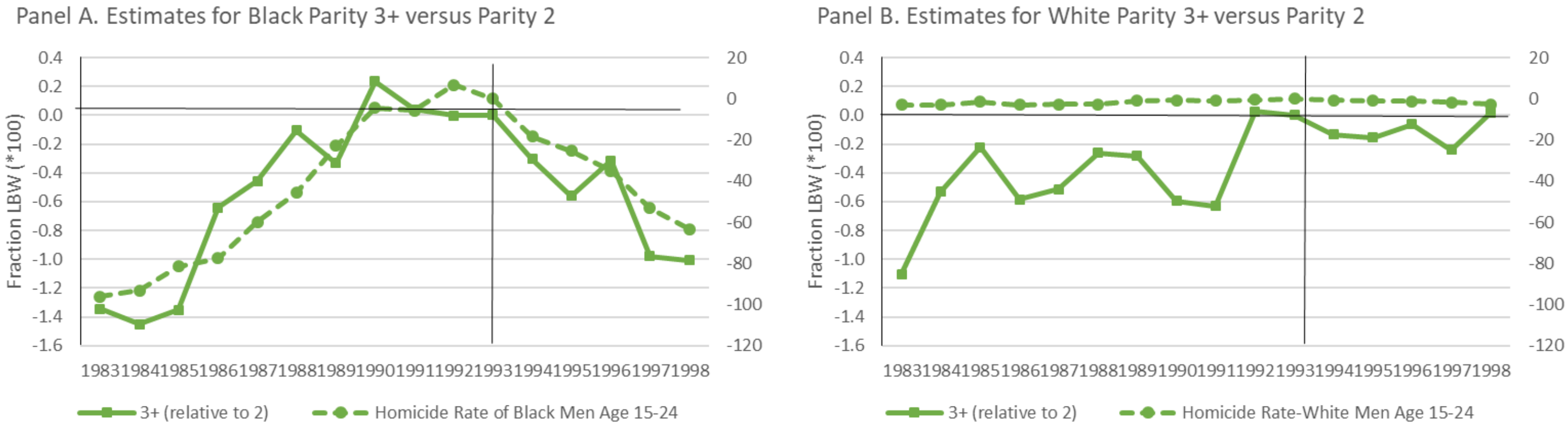
Source: Authors tabulations of NYC Birth Certificates (1980-2001). See Note to Figure 5.

Figure 7: Homicide Rates of Black Males 15-24 Years of Age Separately in New York City and the U.S (Right Access) with the Percent of Births Exposed Prenatally to Narcotics and Cocaine (Drug Use) Among Single Black Women of Parity 3+ Relative to Parity 2 with at most a High School Diploma in New York City



Source: Authors tabulation of NYC birth certificates and Multiple Cause of Death Files (1980-1988) and Compressed Mortality Files (1989-2001). We thank Tim Moore for data on homicides (see Evans, Garthwaite and Moore 2018).

Figure 8: Event-Time Estimates of Low Birth Weight of Women of Parity 3+ Relative to Parity 2 Among Single Women with at most a High School Diploma Overlaid with National and homicide rates for Males Ages 15-24 for Black women (Panel A) and White women (Panel B)



Source: HMS (2015) and Multiple Cause of Death Files (1980-1988) and Compressed Mortality Files (1989-2001). We thank Tim Moore for data on homicides (see Evans, Garthwaite and Moore 2018).

Notes: Both panels contrast women of parity 3+ versus parity 2. They exclude California, New York, Texas and Washington because those states are missing education in some years proceeding effective year 1991.

Table 1- Difference-in-differences estimates of OBRA93 on Low Birth Weight Single Women with a High School Education or Less of Parity 1 or 2

Model	All (1)	Black (2)	White (3)	Hispanic (4)
Panel A: Pre-Period is 1991-1993 (HMS)				
	-0.164**	-0.310**	-0.114*	-0.130*
Parity2 *After	(0.072)	(0.144)	(0.072)	(0.07)
Mean LBW	10.2	14.4	8.1	7.0
Observations	47,687	13,780	21,775	14,823
Panel B: Pre-Period is 1991-1993^				
	-0.209***	-0.390***	-0.146*	-0.161
Parity2 * After	(0.072)	(0.138)	(0.075)	(0.195)
Mean LBW	9.9	13.3	8.3	7.5
Observations	21,677	6,155	9,997	6,771
Panel C: Pre-Period is 1987-1990				
	-0.564***	-0.920***	-0.352***	-0.288*
Parity2 * After	(0.076)	(0.147)	(0.070)	(0.167)
Mean LBW	10.1	13.5	8.3	7.6
Observations	32,399	9,237	14,794	9,744

Notes: Estimate in Panel A are slightly different than those in HMS because the regressions only include women of parity 1 and 2. HMS pool these groups with women of parity 3+ and estimate the effect of parity 2 versus parity 1 separately but the pooled model restricts the coefficients on the other covariates to be the same across all parities. In Panel B we use 1991-1993 as the pre-period, but we drop California, New York, Texas, and Washington because they did not report mothers schooling in some years proceeding effective year '91. The differences in the estimates between their specification and ours shown here are small. For example, Panel A (Column 1) is -0.164 in HMS and -0.209 in our Panel B, column (1). In Panel C we use the same states as in Panel B but we use 1987-1990 as the pre-period.

Table 2- Difference-in-differences estimates of OBRA93 on Low Birth Weight of Single Women with a High School Education or Less (1991-1998)-Placebo Comparisons

Model	All (1 HMS)	Black (2)	White (3)	Hispanic (4)
Pane A: Parity 3+ v. 2 (HMS)				
Parity3+ * After	-0.347	-0.715	-0.028	-0.121
	(0.067)***	(0.121)***	(0.073)	(0.092)
Mean of the dependent variable	10.7	14.9	8.1	6.8
Observation	35,467	10,273	16,247	10,951
Pane B: Parity 3 v. 2 (HMS)				
Parity3 * After	-0.225***	-0.476***	-0.046	-0.05
	(0.062)	(0.135)	(0.085)	(0.073)
Mean of the dependent variable	9.7	13.4	7.8	6.3
Observation	23,916	6,865	10,967	7,422
Pane C: Parity 4 v. 3				
Parity4 * After	-0.240***	-0.421***	-0.020	-0.124
	(0.089)	(0.135)	(0.108)	(0.216)
Mean of the dependent variable	11.1	15.2	8.6	6.8
Observation	22,021	6,326	10,381	6,625
Pane D: Parity 4+ v. 3				
Parity4+ * After	-0.261**	-0.483***	0.062	-0.107
	(0.105)	(0.136)	(0.134)	(0.142)
Mean of the dependent variable	12.0	16.5	9.0	7.3
Observation	38,675	11,352	18,593	11,041

Notes: Estimates in Panel A replicate HMS results from HMS (Tables 2 and 3) correcting for the minor coding errors in the HMS specification. Estimates in column (1) of Panels B and C replicate the results from HMS's Appendix Table 6. Estimates in Panel D compare women of parity 3 to women of parity 4 and higher.

Table 3- Maximum Credit Estimates of EITC on Low Birth Weight, Single Women with a High School Education of less by Race

Model	**All**			**White**			**Black**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: 1983-1998									
Maximum Credit	-0.304***	-0.772***	-0.075	-0.117**	-0.075	0.005	-0.518***	-1.357***	-0.0214
($1,000 of 95$)	(0.066)	(0.128)	(0.150)	(0.053)	(0.107)	(0.159)	(0.114)	(0.179)	(0.220)
Parity * linear time		X	X		X	X		X	X
Parity * quadratic time			X			X			X
Mean LBW	11.21	11.21	11.21	8.81	8.81	8.81	14.76	14.76	14.76
Observation	81,782	81,782	81,782	37,335	37,335	37,335	23,746	23,746	23,746
Panel B: 1983-1993									
Maximum Credit	-0.087	-0.330	0.527*	-0.467***	0.004	0.072	0.147	-0.359	1.14
($1,000 of 95$)	(0.147)	(0.360)	(0.291)	(0.119)	(0.362)	(0.361)	(0.284)	(0.552)	(0.478)**
Parity * linear time		X	X		X	X		X	X
Parity * quadratic time			X			X			X
Mean LBW	11.43	11.43	11.43	8.83	8.83	8.83	14.88	14.88	14.88
Observation	55,003	55,003	81,782	24,805	24,805	24,805	16,220	16,220	16,220

Notes: Estimates in Panel A, columns (1,2,4,5,7 and 8) are from Equation (2) in the text and they replicate those from HMS's Table 5 correcting HMS's minor specification error. Estimates in columns (3, 6, and 9) add a quadratic trend term interacted with parity. Standard errors are clustered at the state level. Estimates in Panel B use the same specifications as in Panel A but restrict the sample to the years 1983-1993. We reject the null hypothesis that the trend terms are zero in each specification (p<.01).. * p<.10; ** p<.05; *** p<.01

Table 4- Difference-in-differences Estimates of OBRA93 on Low Birth Weight Single Women with a High School Education or Less-Controlling for the Black Male Homicide Rate Ages 15-24

Model	All (1^)	Black (2)	White (3)	Hispanic (4)
Panel A: Parity 3+ vs. 2				
Parity3+ * After	-0.080 (0.109)	0.013 (0.218)	-0.032 (0.146)	-0.145 (0.226)
Mean LBW	11.3	15.4	9.9	8.7
Observation	35,467	10,273	16,247	10,951
Panel B: Parity 4+ vs. 3				
Parity4+ * After	0.250 (0.118)	0.295 (0.259)	0.370 (0.291)	0.160 (0.426)
Mean LBW	11.8	16.1	10.3	8.1
Observations	23,237	6,759	10,689	7,422

Notes: All estimates are based on the specification in Equation (1) of the text with the addition of the national homicide rate of black males 15-24 years of age interacted with the treatment dummy (see notes to Table 1).