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REASSESSING THE WIC EFFECT:
EVIDENCE FROM THE PREGNANCY NUTRITION SURVEILLANCE SYSTEM

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ABSTRACT

Recent analyses differ on how effective the Special Supplemental Nutrition Program for Women, Infants and Children (WIC) is at improving infant health. We use data from nine states that participate in the Pregnancy Nutrition Surveillance System to address limitations in previous work. With information on the mother's timing of WIC enrollment, we test whether greater exposure to WIC is associated with less smoking, improved weight gain during pregnancy, better birth outcomes, and greater likelihood of breastfeeding. Our results suggest that much of the often-reported association between WIC and lower rates of preterm birth is likely spurious, the result of gestational age bias. We find modest effects of WIC on fetal growth, inconsistent associations between WIC and smoking, limited associations with gestational weight gain, and some relationship with breast feeding. A WIC effect exists, but on fewer margins and with less impact than has been claimed by policy analysts and advocates.

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Introduction

The conclusion among policy analysts has been that "WIC works." The 1992 General Accounting Office report concluded that every $1.00 spent on WIC saves the government $3.50 in averted newborn costs (General Accounting Office, 1992). A more recent review of WIC's 31-year history reached similar conclusions,[1] and a new study based on data from 19 states found that participation in WIC was associated with a 29 percent reduction in low birth weight and more than a 50 percent reduction in very low birth weight [Bitler and Currie (BC), 2005a].

However, Besharov and Germanis (2001) and more recently Joyce, Gibson and Colman [(JGC), 2005] have challenged the prevailing wisdom. Besharov and Germanis (2001) reviewed the literature and concluded that evidence linking WIC to improved birth outcomes was dated and based on weak designs that were vulnerable to contamination by selection bias. Joyce, Gibson and Colman (2005) analyzed over 800,000 births to women on Medicaid in New York City between 1988 and 2001. They found little association between WIC and fetal growth. They did find that WIC was strongly associated with preterm birth, but concluded that the association was likely spurious, since there is little support in the clinical literature for such an association.

Bitler and Currie (2005b) challenged such skepticism, arguing that WIC provides more than nutritional supplementation and that the bundle of services associated with WIC creates health-enhancing synergies:

> It is entirely possible that the main benefit of WIC is not the provision of food per se, but the fact that the "carrot" of food packages induces women to initiate prenatal care earlier, follow it more faithfully and receive more continuous care than they otherwise would. It is also possible that women who want to get WIC are less likely to smoke or use illegal

[1] "What is cause for celebration is WIC's extraordinary record of accomplishments for the nutrition and health of the nation's children, and that record has grown as the program has grown." Food Research and Action Center 2005, p. 1. http://www.frac.org/WIC/2004_Report/Summary_Report.pdf.

> drugs in part because they get into care earlier, receive better advice, and are more closely monitored than other similar women. These are important issues that deserve investigation (Bitler and Currie, 2005b).

Ludwig and Miller (2005) reviewed the studies by JGC (2005) and BC (2005a) in an effort to reconcile the conflicting conclusions. Ludwig and Miller (2005) noted that the lack of clinical evidence linking nutritional supplementation and preterm birth did not rule out the possibility that addressing the constellation of risk factors targeted by WIC might be protective of preterm birth. They also stressed that neither JGC nor BC could determine the timing of WIC enrollment but could only compare women who participated in WIC during pregnancy to those who did not. A dichotomous indicator of WIC, argued Ludwig and Miller, will tend to overestimate the protective effect of participation, since women whose pregnancies last longer for reasons unrelated to WIC will have more opportunity to enroll. However, adjustment for the length of gestation tends to "over-fit" the data and underestimates the effect of WIC, if participation prolongs gestation.

In this paper, we address several of the issues raised by Bitler and Currie (2005b) and Ludwig and Miller (2005). With data from the Pregnancy Nutrition Surveillance System (PNSS), we provide new evidence that the association between WIC participation and preterm birth is likely spurious. A major advantage of the PNSS is that we know the woman's exact date of WIC enrollment in both the prenatal and postpartum periods. We show that the rates of low birth weight and preterm birth fall the longer women delay prenatal enrollment in WIC. Moreover, women who enroll into WIC after delivery experience much higher rates of preterm birth than women who enroll during pregnancy. The relatively low rate of adverse birth outcomes among third trimester WIC enrollees suggests a form of fetal selection in which healthier pregnancies endure for reasons unrelated to WIC. The elevated rate of adverse birth outcomes among postpartum enrollees may be driven, in part, by women who would have enrolled in WIC prenatally had they not delivered prematurely. The interpretation that WIC is not protective against premature delivery is consistent with the clinical literature, where trial after trial has failed to produce a reliable intervention to prevent preterm birth (Institute of Medicine, 2007).

In the second half of the paper, we analyze the association between WIC and fetal growth. We argue that nutritional supplementation, health education and timely referrals are more likely to be protective against intrauterine growth retardation (IUGR) than against preterm birth. In addition, we also analyze changes in maternal smoking, breastfeeding and weight gain during pregnancy in the belief that we should be able to support any association between WIC and IUGR with changes in maternal behavior and health.

In the end, we find that prenatal participation in WIC is positively associated with fetal growth, though the association is difficult to support with substantive changes in maternal behavior and health. We conclude that, at least with respect to birth outcomes, WIC works, but on fewer margins and with less impact than has been claimed by policy analysts and advocates. We also argue that linking the success or failure of WIC to its impact on birth outcomes may be myopic. Greater emphasis on affecting life-long habits such as smoking, or behaviors with known benefits to mother and child such as breastfeeding, iron supplementation, and childhood immunizations, may be sufficient to justify additional funding.

Background and Issues

The notion that "WIC works" is based primarily on WIC's protective association against preterm birth. The association between WIC and measures of fetal growth is less robust. Bitler and Currie (2005a), for example, found that prenatal WIC participation was associated with a 29 percent decrease in the odds of a preterm birth (< 37 weeks) and a 53 percent decline in the odds of a very preterm birth (< 32 weeks), but only a 13 percent decline in intrauterine growth retardation. This pattern is common in the literature. Researchers find large and robust associations between WIC and birth outcomes unadjusted for gestational age, but more modest and even non-existent associations with measures of fetal growth.[2] Most researchers have interpreted this pattern as evidence that WIC is strongly protective against preterm birth. Yet there is little in the clinical literature to suggest that nutritional supplementation exerts such an effect. The recently released report on preterm birth by the Institute of Medicine summarizes the literature as follows: "Randomized studies in both developed and developing countries have noted an absence of benefit from dietary supplementation in preventing preterm birth (Berkowitz and Papiernik, 1993). Furthermore, protein supplementation specifically has not been found to reduce the risk of preterm birth and possibly increases the risk (Berkowitz and Papiernik, 1993; Rush et al., 1980) as does multivitamin supplementation (Villar et al., 1998)." (Institute of Medicine 2007, p. 94)

The association between WIC and fetal growth, although less consistent than the association between WIC and preterm birth in the empirical literature, is more plausible. In one recent study, researchers use data from linked administrative and birth certificate files in New York State in 1995 (Lazariu-Bauer et al., 2004) and find that early WIC enrollment is strongly associated with increased fetal growth. The study has a number of strengths. First, the exact dates of WIC enrollment are available. This allows testing of whether fetal growth is directly related to the amount of time a woman participates in WIC. Second, the authors use propensity score matching to minimize observable differences between early and late WIC enrollees. They

[2] See Devaney, Bilheimer and Schore 1992; Gordon and Nelson 1995; Kotelchuck, Schwartz, Anderka and Finison, 1984; Stockbauer, 1987; Buescher and Horton, 2000; Joyce, Gibson and Colman, 2005).

find that early enrollment in WIC is associated with an increase of 70 grams in birth weight among full-term deliveries. Gains among blacks and Hispanics are twice as large as those of whites. Yet despite its methodological sophistication, the study lacks evidence of a plausible mechanism. There is no attempt to associate WIC participation with decreased smoking, improved nutritional intake, greater maternal weight gain or other maternal behaviors consistent with fetal growth.

Those who argue that WIC is protective against preterm birth contend that WIC is more than nutritional supplementation. It is, they maintain, the combination of nutritional supplementation, behavior counseling, and timely referrals that explains WIC's protective effect. However, a recent trial of comprehensive prenatal care failed to show a positive association with improved birth outcomes (Klerman et al., 2001). Components of the augmented care included specific interventions targeted at smoking cessation, weight gain, and vitamin/mineral supplementation, as well as appointment reminders, free transportation, no waiting time for visits, child care, evening office hours, and individualized care with the same practitioner. The focus on access was clearly effective as women in the treatment group averaged almost two more prenatal visits than women in routine care. The study is important because the augmented care was directed at the "constellation of risk factors" that Ludwig and Miller (2005) argue may be lacking from more narrowly targeted clinical trials for preterm birth. Indeed, the effort to enhance access in addition to the augmented prenatal care services went well beyond what could be expected from a typical WIC program.

The WIC program is now over 34 years old. Approximately 35 percent of all pregnant and postpartum women participated in 1998 (Bitler, Currie and Scholz, 2003). The program's often-cited association with improved birth outcomes is predicated on a protective effect against preterm birth, but is unsupported by evidence from the clinical literature. The few studies that report a substantive association between WIC participation and enhanced fetal growth do not provide corroborating evidence on intermediate outcomes such as reduced smoking and improved maternal weight gain. In this study, we update and extend the literature with the largest population-based study of WIC participants ever assembled. Because of the size, recency, and detail of the data, we are able to analyze an array of birth outcomes and behaviors by the timing of WIC enrollment in order to provide a broader assessment of WIC's effectiveness.

Empirical Implementation

Data

Our data are from the Pregnancy Nutritional Surveillance System (PNSS). The PNSS is "…..a program-based surveillance system that monitors the nutritional status of low-income infants, children and women in federally funded maternal and child health programs" (http://www.cdc.gov/pednss/). The PNSS collects information from all WIC program participants at the points of prenatal and postpartum enrollment. At the prenatal interview, WIC intake workers collect demographic information along with indicators of maternal health and behavior. At the postpartum visit, information on infant health and postpartum behaviors is added. The PNSS combines the advantage of administrative data and its detailed information on the timing of WIC enrollment with that of survey data and its information on health outcomes and behaviors. PNSS data on maternal health and behaviors are richer than those available from birth certificates, which have been the primary source of outcomes in previous prenatal WIC evaluations using secondary data (Schramm, 1985, 1986; Stockbauer, 1986, 1987; Devaney, Bilheimer and Schore, 1992; Buescher and Horton, 2000; Ahluwalia, et al. 1998; Lazariu-Bauer, et al. 2004).

The Centers for Disease Control and Prevention (CDC) organize the PNSS data from the twenty-two states and three Indian tribal governments into a central file. We obtained permission to access the PNSS file from the relevant institutional review board or from the supervising WIC agency in each of the 10 states with the largest WIC caseloads in 2004. These include Florida, Georgia, Illinois, Indiana, Michigan, Missouri, New Jersey, North Carolina, Ohio and Virginia. Illinois data were discarded because information on the timing of WIC enrollment was incomplete. Thus, we have PNSS data from nine states over multiple years in all states except Virginia, for which we only have data from 2004 (see Table 1). The nine-state file has 3,311,976 women who enrolled in WIC during pregnancy or postpartum. We included all women who enrolled in WIC during pregnancy and who re-certified for WIC postpartum. Without a postpartum interview, we lacked information on the birth outcome, breastfeeding, and postpartum smoking. Re-certification also suggests that women were enrolled continuously during pregnancy.[3] The comparison group was women who enrolled in WIC after delivery and thus were unexposed to WIC during pregnancy.

We excluded all women who enrolled in WIC during pregnancy but did not return to re-certify after delivery (n=441,945 or 13.3%). This raises two potential issues. First, if these woman had better (worse) birth outcomes, on average, than women who re-certified for WIC postpartum, then our estimates of WIC's effectiveness will be biased downwards (upwards). Second, by excluding these women, our estimates become less comparable to those from studies

[3] A better measure of continuous participation would be the dollar amount of redeemed food vouchers. Relatively few studies have had such data, but he the results from these tend to be consistent with estimates based on month of enrollment (see Kotelchuck et al., 1984).

that compare all prenatal WIC enrollees to eligible non-participants. Fortunately, the PNSS data from North Carolina have also been linked to birth certificates. This linkage provides information on birth outcomes and prenatal smoking for the prenatal WIC enrollees from that state who did not re-certify postpartum. We refer to these women as lost to follow-up. Thus, we compare estimates from models for North Carolina with and without woman who were lost to follow-up. This provides some insight into the potential bias from excluding women who do not re-certify postpartum from the larger sample. Three other studies have also used North Carolina data to analyze WIC (Devaney, Bilheimer and Schore, 1992; Buescher et al., 1993; Buescher and Horton, 2000). In each study, researchers linked WIC administrative data to Medicaid claims for newborn deliveries and then to birth certificates. The comparison group in each was women whose births were paid for by Medicaid, but who were not enrolled in WIC during pregnancy. By contrast, our comparison group is women who did not participate in WIC during pregnancy, but who enrolled in WIC postpartum. There is likely to be substantial overlap in our comparison group and the one frequently used in the literature. We try to replicate the results from those two studies by limiting our sample to women in WIC who were also covered by Medicaid at the time of enrollment.

Econometric Model

The canonical test of WIC is as follows:

$$(1) \qquad H = \alpha_0 + \alpha_1 \mathrm{WIC} + \mathbf{X}\boldsymbol{\beta} + e$$

Let H be a birth outcome such as birth weight and let WIC be a dichotomous indicator of whether the woman enrolled in WIC during pregnancy. The omitted category usually includes eligible non-participants such as women on Medicaid (Devaney, Bilheimer and Schore, 1992;

Bitler and Currie, 2005a). Let **X** be a matrix of other relevant variables, including demographic and socio-economic characteristics of the mother, and let e be the error term. The vast majority of research has found that $\alpha_1>0$ for favorable outcomes such as birth weight and gestational age and $\alpha_1<0$ for adverse outcomes such as low birth weight and preterm birth (Fox, Hamilton and Lin, 2004).

With data on the timing of WIC enrollment, the specification in equation (1) can be expanded as follows:

$$(2) \qquad H = \alpha_0 + \alpha_1 WIC\_1 + \alpha_2 WIC\_2 + \alpha_3 WIC\_3 + \mathbf{X\beta} + e$$

The WIC indicators designate the trimester of pregnancy in which a woman enrolled in WIC. The omitted category is again eligible non-participants (Devaney, Bilheimer and Schore, 1992; Gordon and Nelson, 1995; Lazariu-Bauer et al., 2004). In the PNSS, the omitted category is postpartum enrollees. If greater exposure to WIC improves infant health, then we would expect $\alpha_1>\alpha_2>\alpha_3>0$, or the reverse if H is an adverse outcome. Thus, we follow previous evaluations of WIC and use Equation (2) to test for a dose-response effect.[4] We also present estimates of Equation (1) to link our results with past work.

Our test for a dose-response effect has two caveats. First, if nutritional supplementation can be expected to produce improvements in fetal weight gain only if administered at the time of greatest fetal growth, then third trimester enrollment might well turn out to be equally efficacious compared to first trimester enrollment when little fetal growth is occurring. Second, although we know the timing of WIC enrollment, we have no information about the continuity or intensity of treatment.[5] However, if WIC is more than nutritional supplementation—if women who enroll

[4] See Devaney, Bilheimer and Schore (1992) Gordon and Nelson (1995), Ahluwalia et al. (1998) and Lazariu-Bauer et al. (2004) for tests of a dose-response between the timing of WIC enrollment and birth outcomes.
[5] Earlier studies of prenatal WIC used redeemed vouchers per month to proxy intensity (Kennedy and Kotelchuck 1984; Schramm 1985; Stockbauer 1986) Thus, those who had 7-9 months of redeemed vouchers during pregnancy

early smoke and drink less, eat more nutritiously and receive timely referrals for additional care—then early and continuous participation in WIC should be protective against adverse birth outcomes. Thus, we would expect to find changes in maternal behavior and possibly increases in weight gain during pregnancy.

Outcomes

Most studies have emphasized the association between WIC and the probability of a low birth weight birth. However, low birth weight can be broadly divided between preterm births—those that occur before the 37th week of gestation—and those that are small for gestational age (SGA). The latter is an indication of fetal growth retardation. The causes of preterm birth are largely unknown and few interventions, if any, appear effective (Institute of Medicine, 2007). A stronger case can be made for an association between intrauterine growth retardation (IUGR) and prenatal supplementation, since the latter is more closely linked to nutritional intake, smoking and maternal weight gain (Kramer, 1987; Institute of Medicine, 1990).

We divide our analyses of birth outcomes along these two dimensions. We use birth weight (in grams) and dichotomous indicators of low birth weight (<2500 grams), very low birth weight (<1500 grams) and preterm birth (< 37 weeks gestation). These four outcomes allow for comparisons with previous research and are mostly determined by gestational age.

We also analyze three measures of fetal growth. The first is birth weight adjusted for gestational age.[6] The second measure is a dichotomous indicator of infants below the 10th percentile in weight for gestation, within gender, based on all singleton births to US residents in 1995 (Alexander et al., 1998). These births are referred to as small for gestational age (SGA).

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had to have enrolled in the first trimester. However, a woman could have enrolled in first trimester but only redeemed three months of vouchers. Thus, early enrollment is a necessary condition for more prolonged treatment, but it is not a guarantee.

[6] In other words, we include gestational age as a right-hand side determinant in a birth weight regression.

Our final measure of fetal growth is an indicator of infants that are term, low birth weight ($\geq$ 37 weeks gestation and < 2500 grams).

We study three indicators of maternal behavior. The first is a dichotomous variable of whether women who smoked three months before pregnancy indicate that they are not smoking at the time of the postpartum interview. The second is whether a woman ever breastfed. Lastly, we analyze maternal weight gain during pregnancy (in pounds), adjusted for pre-pregnancy BMI and length of gestation.

Sources of bias

Although the statistical estimation of Equations (1) and (2) is straightforward, identification of treatment effects associated with WIC is quite challenging. In econometric terms, the coefficient on WIC, α_1, estimates the average effect of treatment on the treated under two assumptions: first, that the decision to participate in WIC, conditional on **X,** is uncorrelated with the outcome in the absence of participation (H_0); and second, that the expected gains to participation are constant across individuals (Heckman, 1997; Wooldridge, 2002). These are strong assumptions, and yet we know of no study that has been able to instrument credibly for WIC participation.

One fallback strategy has been to include a rich set of controls to lessen selection bias from omitted variables (Gordon and Nelson, 1995; Bitler and Currie, 2005a). For instance, favorable selection occurs if early WIC enrollment is just one manifestation of healthy behavior. These same women may be less likely to smoke, drink, use illicit drugs, and may be less likely to experience vaginal infections. If we only partially control for these factors, then we will overestimate the treatment effect of WIC. Adverse selection may also contaminate estimates.

In this case, women with a history of reproductive health problems may seek out WIC and prenatal care early in order to lessen the likelihood of an adverse birth outcome. Such pre-pregnancy conditions are poorly measured in population-based data files, which results in down-biased estimates of treatment effects.

Selection bias is only one challenge to identifying credible WIC effects. Others have recognized the importance of gestational age bias (Devaney, Bilheimer and Schore, 1992; Gordon and Nelson, 1995; Lazariu-Bauer et al., 2004). Women whose pregnancies last longer have more opportunity to enroll in WIC. In other words, even if fetal and/or reproductive health is randomly distributed among WIC participants, less healthy fetuses/women are more prone to premature delivery. As a result, women who would have enrolled in the third trimester but deliver prematurely may register for the program during the postpartum period instead. This can generate a mechanical correlation between postpartum enrollment -- or a lack of prenatal exposure to WIC -- and adverse birth outcomes.[7]

Approaches to selection bias

As with previous studies, we lack a convincing instrument for WIC participation. We therefore focus on creating a comparison group that is as similar as possible to women exposed to WIC prenatally. For instance, we will use women who enrolled in WIC postpartum to identify the prenatal effects of WIC on maternal behavior and birth outcomes. One advantage of this comparison group is that everyone is eligible for WIC and everyone participates. Stigma or other barriers to participation in publicly funded nutrition programs are thus unlikely to be factors in our analysis.

[7] Jeffrey Harris (1982) referred to this as fetal selection.

Second, we have very large samples that enable us to stratify the analysis along key dimensions. We will estimate separate models for non-Hispanic whites, non-Hispanic blacks, and Hispanics. This considerably enhances the specification. The standard regression in the literature pools all races and ethnicities and includes dichotomous indicators for each. Our specification is equivalent to a fully interacted model by race and ethnicity. The difference is important because treatment effects can vary substantially by race and ethnicity (Lazariu-Bauer et al., 2004; Joyce, Gibson and Colman, 2005). We also analyze important subgroups whose pre-pregnancy characteristics may put them at elevated risk for anemia, low weight gain and intrauterine growth retardation. Examples include women whose pre-pregnancy body mass index (BMI) is less than 19.8, women who smoked three months before pregnancy, and women with multiple gestations (Rush, Stein and Susser, 1980; Institute of Medicine, 1996). Twinning is a useful risk factor because, unlike smoking, it is exogenously assigned conditional on age (see Joyce, Gibson and Colman, 2005).

Third, we estimate Equation (2) but further limit the sample to first births and early initiators of prenatal care.[8] As we show below, there is substantial variation in the timing of WIC enrollment by trimester of prenatal care. The objective of the stratification by first births and early care is to lessen potential heterogeneity among WIC enrollees. Few of the women in this sub-sample will have enrolled in WIC in a prior pregnancy; this should lessen adverse selection from women whose difficulties in a previous pregnancy motivated them to enroll in WIC early. Fourth, we will use propensity score methods (PSM). Following Imbens (2004), we

[8] Registration for prenatal care almost invariably precedes enrollment in WIC. In our data, 86 percent of women with no previous live births registered for prenatal care in the month prior to enrollment in WIC, 13 percent registered in the same month they enrolled in WIC and in only 1.3 percent of cases did WIC enrollment precede early prenatal care. These data are significant because Ludwig and Miller (2005) argue that WIC can affect birth outcomes by encouraging early enrollment in prenatal care. Thus, stratification by early prenatal care risks "over-controlling." However, our data suggest this is highly unlikely given that registration for prenatal care precedes enrollment in WIC.

weight regressions by the propensity score associated with prenatal versus postpartum enrollment in WIC.[9] Weighting improves the balance of observed characteristics between "treated" and "untreated" women. However, given that selection bias is likely driven by unobservables, we view PSM as primarily a diagnostic and robustness check. Finally, we will use specification and falsification checks as a means of flagging potential contamination from omitted variable bias.[10]

Results

Characteristics of women by the timing of WIC enrollment for our nine-state sample are shown in Table 1. The last column displays the differences between women who enroll during pregnancy and women who do not enroll until postpartum. Given the number of observations, almost all differences are statistically significant. In general, compared with postpartum enrollees, women who enroll in WIC during pregnancy are more likely to be white non-Hispanics, unmarried, teens, high school dropouts, on Medicaid and receiving food stamps Differences by trimester of pregnancy among women who enrolled in WIC during pregnancy are more muted. Table 1 also points out some limitations of the data. Smoking three months prior to pregnancy is poorly reported among postpartum enrollees: in nearly half these cases, pregravid

[9] Let PS_i be the propensity score associated with the probability that a woman enrolls in WIC during pregnancy as compared with postpartum. The weight for all postpartum enrollees is $[PS_i/(1-PS_i)]$ while the weight for prenatal enrollees is simply 1 (see Hirano and Imbens (2001) for an application).

[10] For instance, Bitler and Currie (2005b) have argued that WIC may increase the likelihood that a woman stops or limits her smoking. If true, then WIC enrollment must precede changes in smoking. We can explicitly test this with the PNSS. At enrollment, women are asked if they smoke, and if so, whether they have reduced their smoking or quit altogether. One of the possible responses is, "Stopped smoking before my first prenatal care visit." The clinical literature indicates that most pregnant smokers who quit do so when they realize that they are pregnant and are often referred to as "spontaneous quitters" (Quinn, Mullen and Ershoff, 1991; Sexton and Hebel, 1984; Secker-Walker et al., 1995; Windsor et al., 1993). We regress our measure of spontaneous quitting on the timing of WIC enrollment. Any association between the timing of WIC enrollment and spontaneous quitting is likely spurious, since quitting precedes enrollment. Similarly, if gestational age bias is present, then the likelihood of preterm birth should fall the longer a woman delays enrollment in WIC. This presumes, of course, that early enrollment in WIC is not protective against preterm birth. If WIC is protective, then our test is biased towards the null as the treatment effect of WIC works to offset the gains from fetal selection.

smoking is unknown. In addition, the percentage of postpartum enrollees in Georgia (62.2%) is clearly greater than in other states.[11] In analyzing smoking, we therefore limit the sample to prenatal enrollees and include state and year fixed effects in all regressions. Finally, there are substantially more missing observations for gestational age among postpartum enrollees than there are among prenatal enrollees. This is a potential source of bias that we discuss in the presentation of the results.

Appendix Table 1 indicates that the age and poverty distributions of the women in eight of our states in 2000 are very similar to those reported by the USDA Food and Nutrition Service (2000). However, we have proportionally more non-Hispanic blacks and fewer Hispanics. In addition, women in our sample tend to enroll later in pregnancy. State-specific comparisons, where available, are also reassuring in terms of mean weight gain, parity and birth weight (see Appendix Table 2).

Replication of early work

In Table 2, we show adjusted differences in birth outcomes by the timing of WIC enrollment. The even-numbered columns show estimates for all women on WIC. The odd-numbered columns show estimates from the sub-sample of women who also were enrolled in Medicaid at either the prenatal or postpartum interview. The latter is our attempt to replicate the samples used by Devaney, Bilheimer and Schore (1992), Buescher and Horton (2000), Bitler and Currie (2005a), and Joyce, Gibson and Colman (2005). The first row shows the estimates of α_1 from Equation (1), while the next three rows show estimates of α_1, α_2, and α_3 from Equation (2). We also display differences by trimester of enrollment. We use ordinary least squares for

[11] We have spoken with officials in Georgia who are aware of the high number of enrollees but who feel it reflects the situation in the state.

continuous dependent variables and maximum likelihood probit for dichotomous outcomes.

The estimates in general are remarkably similar to those reported in the literature. Prenatal participation in WIC is associated with an adjusted mean difference of 63 grams in birth weight based on the sub-sample of women on Medicaid and WIC. This is the same estimate obtained by Bitler and Currie (2005, Table 2). The results in Table 2 also indicate that prenatal WIC participation is associated with decreases in the rates of low birth weight (2.7 percentage points), very low birth weight (0.9 percentage point) and preterm birth (2.8 percentage points), These represent declines of between 30 and 72 percent evaluated at the means of the respective birth outcomes, or relative risks of between 0.70 and 0.27. Estimates from the sample of all women on WIC regardless of Medicaid status are slightly smaller in absolute magnitude (even-numbered columns). Given the similarity of the estimates between the two samples, we include all women on WIC in succeeding tables but adjust for participation in Medicaid, TANF and the Food Stamp Program.

Differences by trimester of WIC enrollment suggest a more complex story. Women who enroll in the third trimester of pregnancy have *higher* mean birth weights and *lower* rates of low birth weight, very low birth weight, and preterm birth compared with both postpartum and first-trimester enrollees. Consider preterm birth. Women who delay enrollment into WIC until the third trimester have rates of preterm birth that are 6.1 percentage points lower than postpartum enrollees and 4.8 percentage points less than first-trimester enrollees.

Fetal selection and gestational age bias

As noted previously, the clinical literature has found no consistent reproducible intervention that can prevent preterm birth (Institute of Medicine, 2007). It would therefore seem implausible for WIC to be responsible for the very large differences in preterm birth observed between prenatal and postpartum enrollees. In addition, we find the very large differences in preterm birth between first and third-trimester WIC enrollees to be counterintuitive. Early enrollment in WIC should be associated with more nutritional supplementation, more health education and more timely referrals. An alternative hypothesis, therefore, is that a large portion of the association between WIC and improved birth outcomes is an artifact of fetal selection or what others have termed gestational age bias (Devaney, Bilheimer and Schore, 1992; Gordon and Nelson, 1995). Women who carry into the third trimester have healthier fetuses for reasons largely unrelated to WIC. They also have the opportunity to enroll in WIC late in pregnancy, while their unenrolled counterparts who deliver prematurely do not. To illustrate, Figure 1 shows the rates of low birth weight and preterm birth by the week of pregnancy in which women enrolled in WIC. The unadjusted rate of low birth weight among women who enrolled in WIC in the 11th week of pregnancy is approximately 7 percent. The rate climbs modestly to just under 8 percent by the 23rd week, after which it starts to decline. A similar pattern is observed for preterm birth.[12]

As points of comparison, we have also included the mean rates of low birth weight and preterm birth for all postpartum enrollees. The regression results in Tables 2 and 3 now become clear. Women who enrolled in WIC at 33 weeks experienced, as a group, rates of premature delivery of 8.6 percent, considerably below the 11.9 percent associated with women who enrolled at 23 weeks. WIC participation cannot have been responsible for the prolongation of their gestations to 33 weeks, since these women had not enrolled and so were unexposed to WIC until then. Rather, these women's gestations lasted long enough for them to still be in a "prenatal state" at the time of their enrollment. Some women who would have enrolled in WIC

[12] By definition the rate of preterm birth is zero by the 37th week of pregnancy.

at 33 weeks from the time of conception, but who had already delivered, are likely part of the postpartum cohort. This is a possible explanation for why rates of low birth weight and preterm birth are 10.8 and 14.0 percent, respectively, among postpartum enrollees. These are well in excess of the rates for prenatal enrollees at almost every week of enrollment. The pattern is even more dramatic for very low birth weight and is consistent across race and ethnicity (Figure 2). Very low birth weight is a well-measured proxy of extreme prematurity. Our conclusion is that gestational age bias is impossible to eliminate in the analysis of outcomes that are directly determined by the length of gestation. We turn, therefore, to the analysis of fetal growth and outcomes that condition on gestational age.[13]

WIC and fetal growth

In Table 3 we show regression results for three measures of fetal growth: birth weight adjusted for gestation, SGA, and term low birth weight. We show estimates for all women and then by race and ethnicity. As in the previous tables, the first row in each panel shows adjusted mean differences between prenatal and postpartum enrollees. The next three rows break down the prenatal enrollees by trimester of enrollment. Differences between prenatal and postpartum enrollees are substantive, consistent across race and ethnicity, and precisely estimated. Specifically, mean birth weight, conditional on gestational age, is 40 grams greater among

[13] One concern raised above is the disproportionate number of missing observations for gestational age among postpartum relative to prenatal WIC enrollees. However, we are confident that the missing are not a major factor in our estimates. First, the rate of low birth weight among postpartum enrollees with missing gestational age is 8.4 percent versus 9.0 percent among those with known gestational age. Second, birth weight is well-measured and 98 percent of all very low birth weight births are born preterm. Thus, our story is the same for VLBW as it is for preterm birth. Finally, the PNSS data for North Carolina are linked to birth certificates and less than 0.2 percent of cases are missing for either outcome. As we show below, the results for North Carolina (Table 6) also tell the same story as do the estimates from the larger sample in Tables 2 and 3.

prenatal as compared to postpartum enrollees; rates of SGA and term low birth weight are 1.7 and 0.7 percentage points less, respectively.

Differences by trimester of WIC enrollment are more modest, but unlike results for birth outcomes unadjusted for gestation, earlier exposure to WIC is generally associated with better outcomes. Thus, mean birth weight is 14 grams greater among women who enroll in the first trimester relative to the third; similarly, the rates of SGA are modestly lower among first as opposed to third-trimester enrollees.

If WIC works, then we would expect women with potentially greater need for nutritional supplementation and health education to benefit more from early and persistent participation in WIC than women who receive less exposure during pregnancy or none at all. The next set of results is for three subgroups of women at risk for fetal growth retardation (Table 4). These include women who are underweight prior to pregnancy (BMI < 19.8), women who smoked three months before pregnancy, and women who carry multiple gestations.[14] The estimates are similar to those in Table 3. There are substantial differences in outcomes between women who enroll in WIC during pregnancy compared with women who enroll postpartum, but more moderate gains between first and third-trimester enrollees. Adjusted mean birth weight is 7.7 grams greater among infants of underweight women who enroll in WIC in the first compared with the third trimester.

Figure 3 offers another perspective on the rather modest association between the timing of WIC enrollment and fetal growth. We show the rate of SGA and term low birth weight by the week of prenatal enrollment in WIC. The difference between Figures 2 and 3 is stark. First,

[14] See Joyce, Gibson and Colman (2005) for a discussion of why multiple gestations may be a particularly apt test of WIC's effectiveness.

there is no apparent advantage to women who enrolled in the 33rd week of pregnancy relative to women who enrolled in the 11th week.

The larger point from Figure 3 is that once you condition away fetal selection, the gains to WIC appear modest. We based this assessment on two recent studies which use twin deliveries in the US and Norway to analyze the effect of differences in birth weight—a direct measure of fetal growth since twins are born simultaneously—on short and long-term outcomes. Both studies find within-twin differences in fetal growth have relatively small effects on infant mortality (Almond, Chay and Lee 2005; Black, Devereux and Salvanes, 2007). In Norway, however, researchers are able to follow the twins into their 20s given the country's detailed registries (Black, Devereux and Salvanes, 2007). They find that a 280-gram increase in birth weight is associated with a one percentage point increase in high school graduation, a one percent increase in wages, and a 1.5 percent increase in the birth weight of the first-born child of the twins. Our estimates of WIC's effect on fetal growth are roughly one-tenth as large, which suggests that the longer-term effects of WIC on schooling, labor market outcomes, and the birth weight of offspring are likely to be small. It is possible that WIC has important effects on child nutrition and immunization rates, but whatever these impacts, data from Norway suggest that it takes very large gains in fetal growth to affect long-term outcomes that would be part of a complete cost-benefit analysis.

Maternal health and behavior

The most consistent finding to this point has been the protective effect of prenatal compared to postpartum WIC enrollment on birth outcomes. We believe that fetal selection explains much of the association, but even when we adjust for gestational age, we report gains to prenatal enrollment in WIC. We next test the association between prenatal WIC participation and maternal health and behavior. If WIC improves fetal growth, then we are likely to find associations between WIC and weight gain during pregnancy, smoking and breastfeeding. In Table 5, we show adjusted differences in these three outcomes for all women and then stratify by race and ethnicity. As in previous tables, we display differences between prenatal and postpartum enrollees and then show differences by trimester of WIC enrollment.

Consider the results for breastfeeding among all women (column 1). Prenatal enrollees are 2.6 percentage points more likely to have ever breastfed than postpartum enrollees. The mean for all women is 50 percent. We also find meaningful differences by trimester of care. Women who enroll in WIC in the first trimester are 3.9 percentage points more likely to have ever breastfed than women who enrolled in WIC in the third trimester. We next display differences in ever breastfed within race and ethnicity because the prevalence of breastfeeding varies greatly among our three groups. For each racial and ethnic group, we find statistically significant differences between prenatal and postpartum enrollees, as well as meaningful differences by trimester of care.

The second set of estimates in Table 5 shows differences in smoking quit rates (columns 5-8). A woman is coded as having quit if she smoked three months before pregnancy but reports not smoking when she recertifies for WIC postpartum.[15] Note that we only include women who enrolled in WIC during pregnancy. The omitted category is therefore third trimester enrollees. As with breastfeeding, we show differences in quit rates for all women, and then separately by

[15] We cannot evaluate whether WIC is associated with quitting during pregnancy because smoking is ascertained at the time of enrollment. Women who enroll in the first trimester have less opportunity to quit relative to women who enroll in the third trimester, which creates a potentially spurious relationship between delayed enrollment and higher quit rates. We use the absence of smoking at the postpartum interview, given that a woman smoked prior to pregnancy, as an alternative indicator of whether the smoking cessation message received earlier in pregnancy is associated with less maternal smoking.

race and ethnicity because the prevalence of pre-pregnancy smoking varies dramatically. For instance, 53 percent of white non-Hispanic women who enrolled in WIC during pregnancy smoked three months prior to conception compared with only 13 percent of Hispanics. The association between early WIC enrollment and quitting is statistically significant but clinically inconsequential.[16] Quit rates are about one percentage point greater among early enrollees relative to later enrollees, a relative change of less than 3 percent. Quit rates among whites are even smaller, but those among blacks are greater.

The last set of estimates reflects differences in weight gain during pregnancy by the timing of WIC enrollment (columns 9-12). We show results from two specifications—with and without controls for gestational age—and two samples, all women and the sub-sample of underweight women (pregravid BMI < 19.8). Among all women, late WIC enrollees gain more weight, while first-trimester women gain less weight than postpartum women. After adjustment for length of gestation, however, all groups of prenatal women have lower weight gains than postpartum counterparts. Weight gain for underweight women is more robust. Prenatal enrollees and those who enroll in the first trimester gain one pound more than postpartum enrollees. When we adjust for gestation, the gains associated with first relative to third trimester enrollment are a pound (1.02). Taken together, there is some evidence that early enrollment in WIC is associated with changes in maternal behavior and health. The changes, like those observed in fetal growth, are modest, and impossible to distinguish whether they result from exposure to WIC or from favorable selection.

[16] In the seminal trial of smoking cessation and birth weight, researchers found that quit rates of pregnant women in the treatment were 23 percentage points greater than women in the control group (Sexton and Hebel 1984). This decline was associated with a 92 gram increase in mean birth weight. The small differences in quit rates that we found would not be expected to affect birth outcomes in a meaningful manner. It should also be noted, however, that we are comparing first and second trimester WIC enrollees to those who enrolled in the third trimester and not to women who were unexposed to WIC during pregnancy.

We re-estimated the regressions of fetal growth and maternal behaviors using propensity score weights and the results were largely unchanged. We limited our sample to only first births and to only women who began prenatal care in the first trimesters. Differences in fetal growth between prenatal and postpartum enrollees were more modest, but qualitatively the same (results available upon request). We also found no association between spontaneous quitting of smoking and early enrollment in WIC, a falsification test designed to flag potential selection bias (see footnote 9).

Analysis of North Carolina

Another potential source of bias is that 13.3 percent of our sample was lost to follow up. These are women who enrolled in WIC prenatally, but never re-enrolled postpartum. As noted previously, the PNSS data from North Carolina have been linked to birth certificates, which enable us to assess the likely impact of excluding these women. In Table 6 we re-estimate the models from Tables 3 and 4. For each outcome we show estimates with and without women lost to follow-up. We further limit the sample to women on Medicaid at the time of enrollment in order to make our results more comparable to previous analyses of WIC with North Carolina data.

The first point to note is that the estimated increase in mean birth weight and the decrease in the rates of low birth weight and preterm birth among WIC participants in North Carolina are greater in absolute value than the estimates obtained in all nine states. However, the pattern of the results is very similar to those from the larger sample. The association between WIC and adverse birth outcomes is primarily driven by the association between WIC and preterm birth.

Once we condition on gestational age, the association between WIC and fetal growth becomes more modest.

The second point about Table 6 is that estimates based on samples that include women lost to follow-up are almost identical to the estimates based on samples that exclude these women. Women lost to follow-up have worse birth outcomes (unadjusted) than women who participate in WIC before and after pregnancy.[17] If the results from North Carolina generalize to the larger sample, then the exclusion of women lost to follow up is unlikely to change the estimates in Tables 2 and 3 appreciably. The third point is that our estimates are close to those of Devaney, Bilheimer and Schore (1992), Buescher et al. (1993) and Buescher and Horton (2000). In each study, authors analyzed linkages of WIC administrative and Medicaid claims data in 1987, 1988 and 1997, respectively. Devaney, Bilheimer and Schore report that prenatal WIC participation was associated with a 117-gram increase in mean birth weight and a decline of 5.1 and 5.4 percentage points in low birth weight and preterm birth, respectively.[18] Moreover, the estimated effects of WIC on these outcomes in North Carolina as reported by Devaney, Bilheimer and Schore (1992) were almost double those for Florida, Minnesota and Texas, three other states in their analysis. The similarity of our estimates for North Carolina based on the PNSS to those from studies that used linkages of WIC administrative data with Medicaid claims files suggests that our reference group--postpartum WIC enrollees--is likely comparable to the reference group used in those studies: women on Medicaid who did not participate in WIC during pregnancy.

[17] The rates of low birth weight and very low birth weight are 12.3 and 4.6 percent, respectively, among those lost to follow up versus 7.9 and 1.3 percent among women who enrolled prenatally and re-certified postpartum and 11.6 percent and 2.7 percent among those who enrolled postpartum.

[18] If we estimate the models in Table 6 using only the individual-level data as did Devaney, Bilheimer and Shore (1992), then our estimate for North Carolina are even closer to theirs. Similarly, Buescher and Horton (2000) reported an adjusted odds ratio of 1.36 and 1.90 for low and very low birth weight, respectively, based on 1997 data. We estimated a similar specification based on our PNSS data for 1997 and obtained an adjusted OR of 1.41 and 1.93 for low birth weight and very low birth weight, respectively.

Finally, we show the association between WIC participation and smoking during pregnancy (Table 6, last column). The smoking indicator is based on birth certificates, which tend to underreport smoking prevalence. However, the smoking screen on birth certificates is available for all women and unlike the prenatal smoking indicator in PNSS does not depend on when the women enrolled in WIC. The estimates indicate that the adjusted mean prevalence of smoking during pregnancy among women who enroll in WIC prenatally is greater than those who enroll postpartum, but differs by less than a percentage point.

Conclusions

The causes of preterm birth have baffled clinicians for several decades as few interventions have proven to be protective. Despite pessimism among clinicians, social scientists have reported strong associations between participation in WIC and lower rates of preterm birth. A recent conjecture for the discrepancy between clinical and social research is that WIC represents a multifaceted approach to preterm birth that clinical trials neglect in their focus on a single factor (Bitler and Currie, 2005b; Ludwig and Miller, 2005).

This study uses data on the timing of WIC enrollment to correct limitations in previous observational studies. We are able to replicate the strong protective association between prenatal WIC participation and preterm birth frequently reported by social scientists (GAO, 1992; Bitler and Currie, 2005a). We conclude that the association is largely an artifact. We show that the rates of low and very low birth weight decline dramatically for women who enroll in WIC later in pregnancy relative to women who enroll in the first trimester. At the same time, preterm delivery postpones enrollment for some women from the prenatal to the postpartum period. The result is a correlation between prenatal WIC enrollment, especially third trimester enrollment, and improved birth outcomes that is largely devoid of causal content.

With observational data, it is practically impossible to disentangle the potential effect of WIC at preventing preterm birth from what is termed gestational age bias. Thus, we have emphasized the relationship between prenatal WIC participation and fetal growth, an outcome that conditions away gestational age bias. Moreover, associations between WIC and fetal growth are more plausible clinically and easier to corroborate, given the well-documented relationship between smoking and intrauterine growth retardation. We did find that early WIC participation is associated with lower rates of fetal growth retardation, but the gains are modest. In addition, we were unable to show that WIC was associated with meaningful reductions in smoking. We did find an association between WIC and maternal weight gain during pregnancy, but it was limited to women who were underweight prior to pregnancy. In sum, the association between WIC and fetal growth is modest and hard to substantiate.

The limited association between WIC and measures of fetal growth, however, is a common result in the literature (See Kotelchuck, Schwartz, Anderka and Finison, 1984; Stockbauer, 1987; Devaney, Bilheimer and Schore, 1992; Gordon and Nelson, 1995; Buescher and Horton, 2000; Bitler and Currie, 2005a; Joyce, Gibson and Colman, 2005). Clinical studies identify both fetal and maternal factors that influence fetal growth (Das and Sysyn, 2004). Many of these factors, including chromosomal abnormalities, mother's own history of being small for gestational age, and abnormalities of placental vascular development may not be amenable to environmental adjustment at the time of pregnancy (Svensson, Pawitan, Cnattingius, Reilly and Lichtenstein, 2006).

In the end, we conclude that WIC may work to improve birth outcomes, but on fewer margins and with less impact than has been claimed by policy analysts and advocates. These findings do not constitute an argument for the elimination of WIC; instead, they indicate the need for greater emphasis and resources to be devoted to aspects of the WIC program that target either life-long habits such as smoking, or behaviors with known benefits to mother and child such as breastfeeding, iron supplementation for infants, and childhood immunizations.

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Figure 1: Proportion of LBW and Preterm Births by Timing of WIC Enrollment

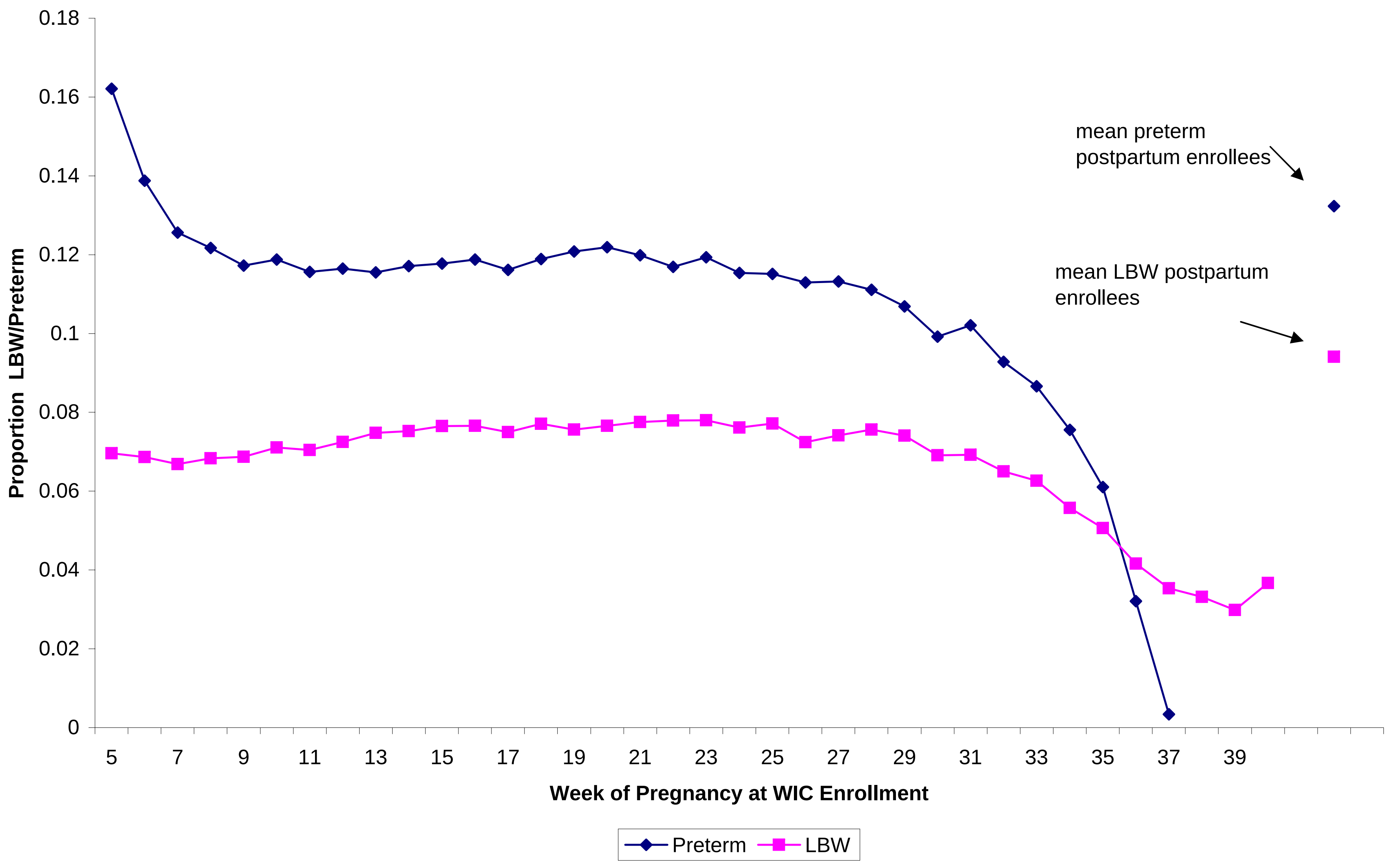

Figure 2: Proportion of VLBW by Race/Ethnicity &Timing of WIC Enrollment

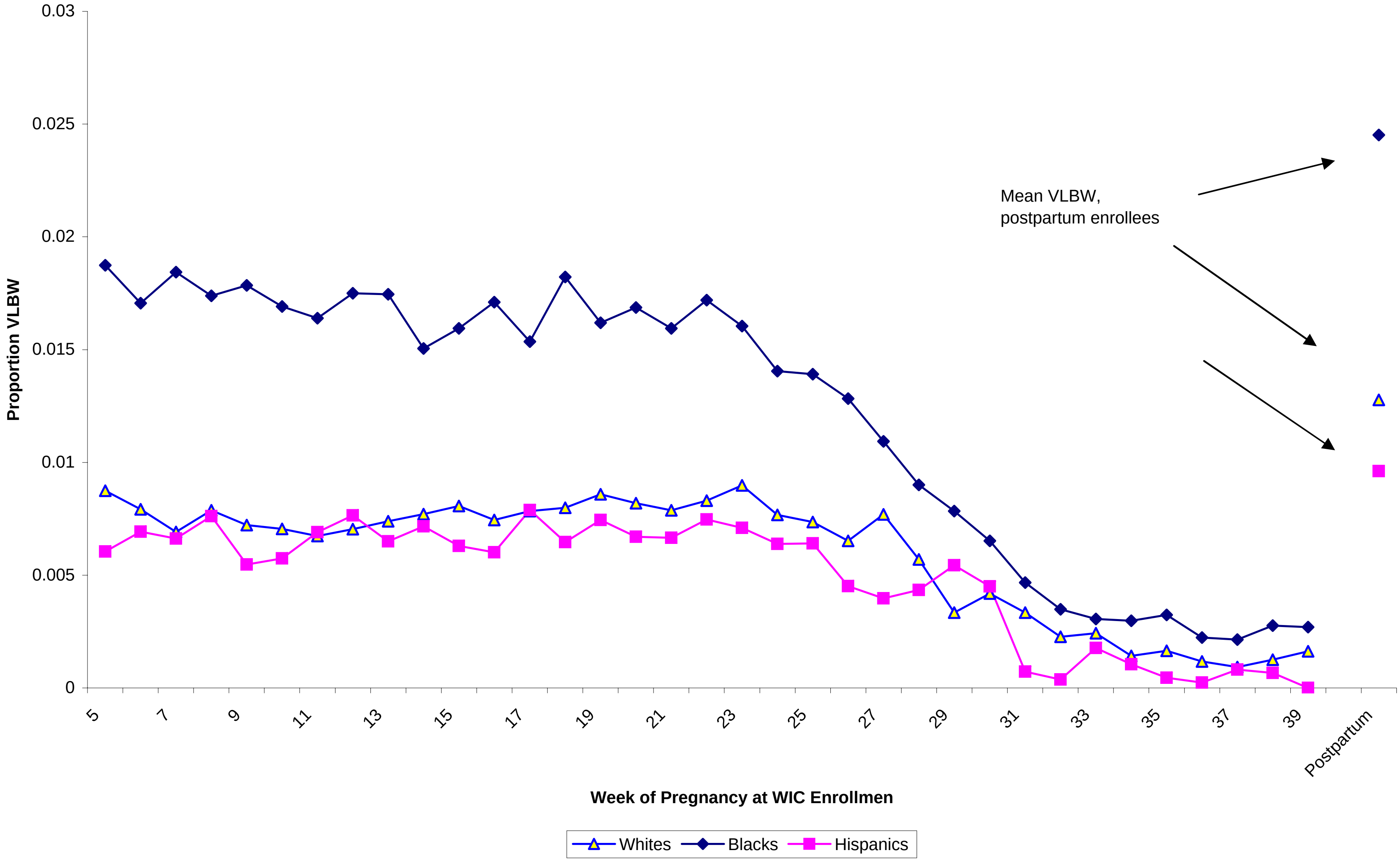

Figure 3: Proportion SGA & Full Term LBW by Timing of WIC Enrollment

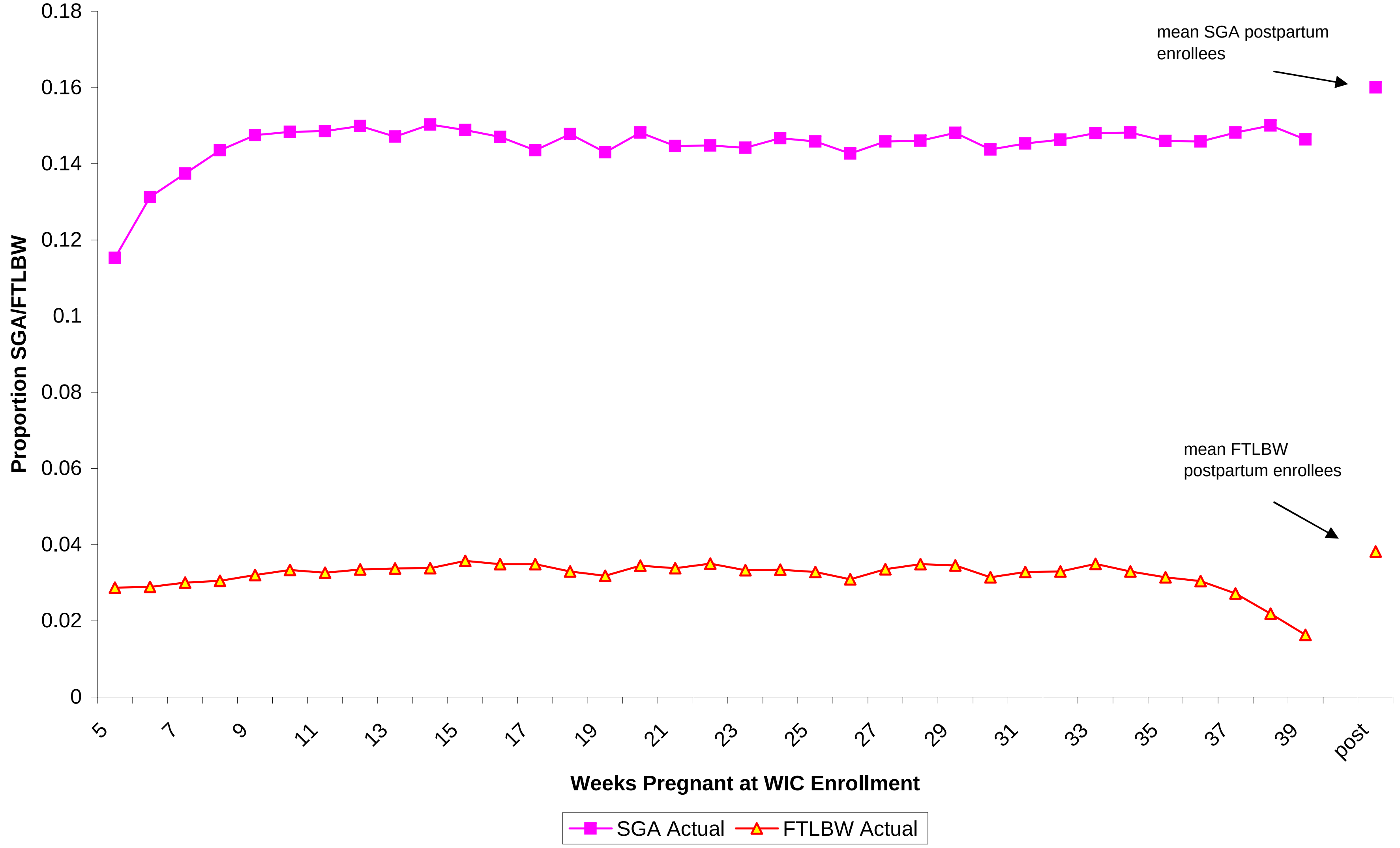

Table 1. Characteristics of Women by Trimester of WIC Enrollment: Nine PNSS States, 1995-2004*

	First Trimester (WIC1)	Second Trimester (WIC2)	Third Trimester (WIC3)	Postpartum (WIC4)	Prenatal-Postpartum
Birth Outcomes					
Birth weight (g)	3289.7	3268.4	3297.7	3231.0	51.9
LBW	6.5	7.2	5.6	8.7	-2.1
VLBW	0.9	1.0	0.3	1.5	-0.7
Unknown	7.2	5.9	5.8	7.2	-0.8
N	782,255	809,996	446,852	742,596	
Gestation (weeks) [1]	38.9	39.0	39.3	38.9	0.1
Preterm [1]	12.1	11.4	7.3	10.0	0.8
Unknown	5.4	4.2	4.1	24.4	-19.8
Term LBW [1]	2.6	2.8	2.7	2.3	0.4
Unknown	18.8	16.8	12.7	38.4	-21.7
N	650,319	656,826	357,169	607,287	
SGA [2]	13.3	13.9	14.0	12.3	1.4
Unknown	6.1	5.0	5.0	22.9	-17.5
N	579,538	607,857	335,287	425,371	
Maternal Behaviors					
Ever Breastfed	46.9	47.9	45.8	50.4	-3.4
Unknown	6.2	5.3	5.4	2.2	3.5
Weight gain (lbs)	30.5	30.8	30.9	30.5	0.2
Unknown	5.8	6.8	6.9	6.6	-0.2
N	782,255	809,996	446,852	742,596	
Smoked Before Pregnancy, Quit Postpartum [3]	31.3	31.4	30.0	15.3	15.7
Unknown	6.1	6.8	6.7	5.8	0.7
N	293,746	251,743	138,229	126,653	

Characteristics					
Race/Ethnicity					
Non-Hispanic White	63.4	49.6	49.8	48.6	6.4
Non-Hispanic Black	23.3	31.3	32.4	32.5	-4.0
Native American	0.6	0.6	0.4	0.4	0.1
Asian	0.9	1.2	1.4	1.7	-0.6
Hispanic	10.9	16.2	14.9	15.6	-1.7
Other/Unknown	0.9	1.1	1.1	1.1	-0.1
Marital Status					
Married	34.2	30.3	31.7	37.4	-5.3
Unknown	4.3	5.6	5.4	5.3	-0.3
Parity					
0	46.0	45.4	44.9	40.0	5.5
1-3	42.1	45.4	46.8	27.5	17.0
4+	3.0	3.7	3.8	2.6	0.9
Unknown	8.9	5.5	4.4	29.9	-23.3
Age					
Under 20	26.1	25.1	22.5	18.5	6.4
20-29	58.7	58.2	60.5	60.8	-1.9
30 and over	15.2	16.7	17.0	20.7	-4.5
Education (Mothers >=20 years old)					
<12 years	27.3	27.9	26.0	24.5	2.9
12 years	48.4	47.5	47.8	46.3	1.6
>12 years	21.3	21.6	23.1	25.7	-4.0
Unknown	3.1	3.1	3.1	3.5	-0.5
Prepregnancy BMI					
Underweight (BMI<19.8)	11.7	12.6	13.0	10.8	1.6
Normal weight	36.7	41.1	43.6	37.3	2.6
Overweight	12.2	12.6	12.6	11.0	1.5
Obese (BMI>29)	27.3	24.4	22.7	19.8	5.4
Unknown	12.1	9.2	8.1	21.1	-11.0
Pregravid Smoking					
Smoked 3 months bef pregnancy	39.6	33.0	32.8	17.1	18.4
Unknown	11.5	10.1	9.5	46.4	-35.9

Medicaid	77.7	71.7	65.1	50.4	22.2
AFDC/TANF	14.2	15.2	14.8	9.1	5.6
Food Stamps	27.1	26.3	24.4	14.1	12.1
Standardized Poverty Distribution					
0 - 50	34.1	36.5	36.6	32.9	2.8
51 - 100	24.8	23.6	21.7	22.0	1.6
101 - 130	12.0	11.1	10.9	11.7	-0.3
131 - 150	6.0	5.5	5.9	6.4	-0.7
151 - 185	5.7	5.5	6.3	7.6	-1.9
186 - 200	1.1	1.1	1.4	1.2	-0.1
Over 200	2.0	2.0	2.4	1.9	0.2
Unknown or adjunctive eligibility	14.5	14.8	14.7	16.2	-1.5
States (Years) Across Trimesters					
Florida (2000-2004)	19.7	30.9	17.5	31.9	
Georgia (1999-2004)	20.7	12.4	5.3	61.5	
Indiana (1995-2004)	30.0	28.6	15.4	26.0	
Michigan (1996-2004)	28.6	33.2	19.4	18.9	
Missouri (1995-2004)	37.9	26.3	13.4	22.5	
North Carolina (1996-2003)	32.2	32.6	16.9	18.3	
New Jersey (2000-2004)	19.5	37.0	19.0	24.5	
Ohio (1999-2004)	29.6	31.9	20.5	17.9	
Virgina (2004)	25.2	32.4	16.4	26.0	
N	782,255	809,996	446,852	742,596	

*Singleton births. Excludes women lost to follow-up.

[1] Excludes MI, postpartum VA, and postpartum NJ. Gestation unknown for these groups.

[2] Excludes MI, GA, VA, and postpartum NJ. Gestation and/or gender unknown for these groups.

[3] Women who smoked pre-pregnancy only. Excludes FL, GA, postpartum VA. Pregravid and/or postpartum smoking unknown for these groups.

Table 2. Adjusted Differences in Birth Outcomes by the Timing of WIC Enrollment

	BW		LBW		VLBW		Preterm	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrolled in WIC…								
During pregnancy	63.3**	59.4**	-0.027**	-0.026**	-0.009**	-0.008**	-0.027**	-0.025**
1st trimester	60.2**	55.5**	-0.023**	-0.021**	-0.005**	-0.005**	-0.013**	-0.009**
2nd trimester	55.6**	51.3**	-0.019**	-0.018**	-0.005**	-0.004**	-0.021**	-0.018**
3rd trimester	85.4**	79.9**	-0.034**	-0.032**	-0.009**	-0.009**	-0.060**	-0.056**
Difference by trimesters								
1st-3rd	-25.2**	-24.4**	0.011**	0.011**	0.004**	0.004**	0.047**	0.047**
1st-2nd	4.7	4.3	-0.004*	-0.003**	0.000	0.000	0.008**	0.009**
Medicaid only	Yes	No	Yes	No	Yes	No	Yes	No
Mean dep var	3,250.4	3,268.9	0.080	0.077	0.011	0.011	0.118	0.115
Observations	1,712,216	2,571,723	1,712,216	2,571,723	1,712,216	2,571,723	1,374,239	2,025,047
R-squared	0.05	0.05						

+ p<0.10, * p<0.05, ** p<.01

Estimates show the difference in birth outcomes based on the timing of WIC enrollment. Estimates for birth weight (BW) are from ordinary least squares. Estimates for low birth weight (LBW), very low birth weight (VLBW) and preterm birth are marginal effects obtained by maximum likelihood probits. Standard errors (not shown) are adjusted for grouping at the state-year level (G=60) with Stata's cluster procedure. Estimates in the top row of each panel compare outcomes among women who enrolled in WIC during pregnancy to women who enolled in WIC postpartum. Estimates in the next three rows compare outcomes among women who enrolled in WIC in either the first, second or third trimester of pregnancy relative to women who enrolled in WIC postpartum; differences between trimesters follow. Estimates are adjusted for age, marital status, mother's schooling, parity, pre-pregnancy BMI, income categories, participation in Medicaid, AFDC and the Food Stamp Program. All models include state and year fixed effects. The samples in odd-numbered columns include all women in WIC whereas the even numbered columns include only women who were also on Medicaid. Data are from 9 states and various years. See Table 1.

Table 3. Adjusted Differences in Measures of Fetal Growth by Race & Ethnicity

	All	White-NH	Black-NH	Hispanic
		BW given gestational age		
During pregnancy	39.5**	30.8**	49.2**	55.8**
1st trimester	47.1**	34.7**	59.8**	77.7**
2nd trimester	35.4**	26.5**	45.2**	51.5**
3rd trimester	33.6**	30.0**	41.5**	35.8**
Difference by trimesters				
1st-3rd	13.5**	4.7	18.2**	41.9**
1st-2nd	11.7**	8.2**	14.6**	26.2**
Mean dep var	3,262.8	3,311.4	3,141.5	3,327.6
Observations	1,971,133	1,050,394	573,738	288,289
		Small for Gestational Age (SGA)		
During pregnancy	-0.017**	-0.012**	-0.023**	-0.025**
1st trimester	-0.018**	-0.012**	-0.026**	-0.028**
2nd trimester	-0.015**	-0.011**	-0.022**	-0.023**
3rd trimester	-0.014**	-0.012**	-0.019**	-0.016**
Difference by trimesters				
1st-3rd	-0.004**	0.000	-0.007**	-0.012**
1st-2nd	-0.003*	-0.001	-0.004**	-0.005**
Mean dep var	0.148	0.138	0.187	0.113
Observations	1,749,900	957,027	482,566	256,201
		Term Low Birth Weight		
During pregnancy	-0.007**	-0.005**	-0.011**	-0.008**
1st trimester	-0.007**	-0.005**	-0.011**	-0.008**
2nd trimester	-0.005**	-0.003**	-0.009**	-0.007**
3rd trimester	-0.008**	-0.006**	-0.011**	-0.007**
Difference by trimesters				
1st-3rd	0.001	0.001*	0.000	-0.001*
1st-2nd	-0.002**	-0.002*	-0.002**	-0.001**
Mean dep var	0.034	0.031	0.046	0.022
Observations	1,744,828	942,762	492,410	257,143

+ p<0.10, * p<0.05, ** p<.01

See notes to Table 2.

Table 4: Adjusted Differences in Measures of Fetal Growth by Risk Factors

	Pre-pregnancy BMI < 19.8			*Pre-pregnancy smokers*			*Multiple gestations*		
	BW|gest	**SGA**	**FTLBW**	**BW|gest**	**SGA**	**FTLBW**	**BW|gest**	**SGA**	**FTLBW**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
During pregnancy	31.9**	-0.013**	-0.008**	41.8**	-0.018**	-0.009**	52.2**	-0.001	-0.024*
1st trimester	36.2**	-0.014**	-0.008**	46.5**	-0.018**	-0.009**	73.7**	-0.035**	-0.030*
2nd trimester	29.9**	-0.014**	-0.006**	37.2**	-0.017**	-0.006**	41.1**	0.001	-0.016
3rd trimester	28.5**	-0.009*	-0.010**	41.0**	-0.015**	-0.011**	41.9**	0.045**	-0.030*
Difference by trimesters									
1st-3rd	7.7+	-0.005*	0.002	5.5+	-0.003	0.002+	31.8**	-0.08**	0.000
1st-2nd	6.3+	0.000	-0.002	9.3**	-0.001	-0.003**	32.6**	-0.036**	-0.014
Mean dep var	3,105.2	0.216	0.057	3,203.1	0.185	0.045	2351.5	0.345	0.303
Observations	246,551	221,153	213,303	626,992	617,995	557,294	32,235	30,757	13,372

+ p<0.10, * p<0.05, ** p<.01

See notes to Table 2.

Table 5: Adjusted Differences in Maternal Behaviors and Health

	Ever Breastfed				*Quit Smoking Postpartum* #				*Pregnancy Weight Gain (lbs)*			
	All Women	**Whites**	**Blacks**	**Hispanics**	**All smokers**	**Whites**	**Blacks**	**Hispanics**	**All women**		**Underweight**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
During pregnancy	0.026**	0.008	0.031**	0.046**					0.075	-0.503**	0.640**	0.045
1st trimester	0.039**	0.022**	0.039**	0.057**	0.009**	0.002	0.035**	0.009	-0.225	-0.516**	0.699**	0.450*
2nd trimester	0.030**	0.010*	0.037**	0.047**	0.012**	0.007**	0.028**	0.010	0.234	-0.452*	0.696**	-0.004
3rd trimester	0.000	-0.022**	0.012	0.024**					0.304	-0.569**	0.460*	-0.524**
Difference by trimesters												
1st-3rd	0.039**	0.044**	0.027**	0.033**					-0.529**	0.053	0.239	0.974**
1st-2nd	0.009**	0.012**	0.002	0.010**	-0.003+	-0.005*	0.007*	-0.001	-0.458**	-0.064	0.003	0.454**
Adj for gestation	na	na	na	na	na	na	na	na	No	Yes	No	Yes
Mean dep var	0.503	0.488	0.405	0.737	0.332	0.293	0.448	0.569	30.7	31.5	34.2	35.3
Observations	2,622,599	1,388,006	779,194	380,646	628,327	489,529	108,039	20,297	2,573,008	1,921,156	314,054	244,546
R-squared									0.08	0.1	0.06	0.08

+ p<0.10, * p<0.05, ** p<.01

Coefficients for ever breastfed and quit smoking show differences in the proportion by timing of WIC enrollment, while weight gain differences are in pounds. The omitted category is postpartum WIC enrollees, except for smoking in which the omitted category is third-trimester enrollees; pre-pregnancy smoking is poorly reported among postpartum enrollees. Underweight women are those with pre-pregnancy BMI < 19.8. Estimates for smoking and breastfeeding are marginal effects obtained by maximum likelihood probit. Also see notes to Table 2.

FL, GA dropped due to missing data.

Table 6. Adjusted Differences in Birth Outcomes by the Timing of WIC Enrollment in North Carolina 1996-2003

	BW		**LBW**		**VLBW**		**Preterm**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrolled in WIC…								
During pregnancy	76.0**	75.6**	-0.033**	-0.033**	-0.011**	-0.011**	-0.047**	-0.047**
1st trimester	59.3**	59.1**	-0.026**	-0.026**	-0.006**	-0.006**	-0.035**	-0.035**
2nd trimester	63.9**	63.4**	-0.027**	-0.026**	-0.008**	-0.008**	-0.041**	-0.041**
3rd trimester	128.3**	128.2**	-0.059**	-0.059**	-0.024**	-0.024**	-0.080**	-0.079**
Difference by trimesters								
1st-3rd	-69.0**	-69.1**	0.033**	0.033**	0.018**	0.018**	0.045**	0.044**
1st-2nd	-4.6	-4.3	0.001	0.000	0.002**	0.002**	0.006**	0.006**
Includes lost to follow-up	Yes	No	Yes	No	Yes	No	Yes	No
Mean dep var	3,215.2	3,225.4	0.095	0.091	0.019	0.016	0.108	0.104
	BW|gest		**SGA**		**Term LBW**		**Smoked during Pregnancy**	
During pregnancy	9.4**	9.5**	-0.001	-0.001	-0.004**	-0.004**	0.004+	0.003*
1st trimester	20.1**	20.3**	-0.003*	-0.003*	-0.005**	-0.005**	0.007**	0.006**
2nd trimester	5.4**	5.5**	0.000	0.000	-0.003**	-0.003**	0.000	0.000
3rd trimester	-2.1	-2.1	0.002	0.001	-0.006**	-0.006**	0.006**	0.005**
Difference by trimesters								
1st-3rd	22.2**	22.4**	-0.005**	-0.004**	0.001	0.001	0.001	0.001
1st-2nd	14.7**	14.8**	-0.003*	-0.003*	-0.002*	0.002*	0.007**	0.006**
Includes lost to follow-up	Yes	No	Yes	No	Yes	No	Yes	No
Mean dep var	3,215.3	3,225.5	0.14	0.139	0.034	0.034	0.324	0.319

+ p<0.10, * p<0.05, ** p<.01

Estimates are from the same specifications as in Tables 2 and 3 but were obtained in a two-step procedure to allow for within-group correlation among categories of WIC within each year. In the first stage, outcomes regressed on all covariates (see notes to Table 2) except for the timing of WIC enrollment. Residuals from this first stage are aggregated by year and WIC participation (N=16) and are regressed on an indicator of WIC and year dummies. The figures and standard errors (not shown) are from these second-stage regressions. The sample is limited to women in North Carolina who were on Medicaid at the time of enrollment. The odd-numbered columns include all prenatal and postpartum WIC enrollees regardless of whether the women who enrolled prenatally re-certified for WIC postpartum. The even-numbered columns include all prenatal and postpartum enrollees; they exclude those who enrolled in WIC prenatally but did not re-certify postpartum. This reflects the PNSS samples in Tables 2 and 3.

Appendix Table 1. Characteristics of Prenatal & Postpartum WIC Participants, 2000

	Selected States, PNSS	U.S.
Trimester of WIC Enrollment (Prenatal Only)		
First Trimester	37.6	48.4
Second Trimester	39.1	39.6
Third Trimester	23.4	11.9
N	311,949	883,559
Race/Ethnicity		
Non-Hispanic White	48.6	39.5
Non-Hispanic Black	32.3	20.8
Native American	0.5	1.3
Asian	1.3	3.2
Hispanic	16.3	34.5
Other/Unknown	0.9	0.9
N	466,907	1,895,353
Age		
Under 15 years	0.6	0.5
15-17	8.3	7.5
18-34	85.1	84.8
35 or older	6.1	7.2
N	466,907	1,893,458
Poverty Distribution		
0-50	41.06	30.2
51-100	28.09	32.9
101-130	13.61	16.3
131-150	6.89	8.6
151-185	7.02	10.6
186-200	1.31	0.5
Over 200	2.03	0.9
N	416,163	1,614,841

Pregnancy Nutrition Surveillance System data from FL,GA,IN,MI,MO,NC,NJ,OH. U.S. data from "WIC Particpant and Program Characteristics 2000", USDA.

Appendix Table 2. Comparison of Selected State Variables, PNSS vs USDA, 2000

	Florida		Georgia		Indiana		Michigan		Missouri		New Jersey		North Carolina		Ohio	
	PNSS	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**	**PNSS**	**USDA**
Trimester of Care (Among Prenatal Participants)																
First Trimester	77.9	71.5	85.2		71.0	89.6	72.1		69.4		72.9	77.9	78.9		81.3	
Second Trimester	18.7	25.6	11.4		24.6	8.5	13.7		10.5		23.2	19.9	18.0		14.0	
Third Trimester	3.4	2.9	1.4		4.4	1.9	1.3		1.0		3.9	1.9	2.7		1.9	
No Care	0.0		2.1		0.0		12.8		19.2				0.3		2.8	
N	55,627	35,618	19,194		10,828	7,452	42,243		30,344		15,542	11,826	39,442		50,191	
Mean Weight Gain	31.1	30.8	30.1		33.7	31.2	27.0	31.6	32.4	32.5	31.2	30.8	29.9	28.1	32.0	32.7
N	68,379	29,746	47,391		18,795	16,512	51,322	22,628	38,291	20,605	29,396	18,101	48,410	21,128	59,784	28,514
Parity (Among Prenatal Participants)																
Zero	43.5	42.9			42.8	33.7	67.4	40.1	43.5	45.0	45.3		43.4		39.2	40.9
N	59,981	41,434			13,828	12,251	45,226	24,581	30,547	15,927	22,317		39,735		46,169	26,177
Mean Birthweight[***]	3,269.1	3,258.3	3,255.1	3,236.6	3,271.2	3,278.7	3,308.0		3,265.6	3,271.7	3,290.3	3,261.5	3,224.1	3,254.7	3,248.6	3,240.0
LBW	8.0	8.3	8.1	8.9	8.1	8.0	7.1		8.2	8.3	7.3	8.6	9.9	9.1	8.8	9.3
N	81,152	104,904	41,759	63,493	17,475	40,022	48,538		38,584	35,846	23,626	33,180	49,993	59,248	60,156	61,840

[***]For USDA, based on Infant WIC Participants, not Women WIC Participants

PNSS figures exclude women lost to follow-up. Percentages for both PNSS & USDA do not account for missing values.

Source for USDA figures: *WIC Participants and Program Characteristics, 2000*

There are substantial discrepancies in sample sizes between the USDA and PNSS. The USDA measures prenatal participation based on redeemed vouchers in April 2000. The PNSS figures are based on births to WIC women in 2000. Thus, the USDA figures would capture about 75% of the prenatal enrollees given a 9-month window for pregnancy. Second, the USDA misses another 8 percent of prenatal enrollees who do not redeem their vouchers in that that month. Finally, the comparisons in Table 2 are based on the 16-item Supplemental Data Set. These data are in addition to the 20-item Minimum Data Set reported by all WIC agencies. Only 80 percent of WIC agencies report SDS and not all items are reported. Finally, the USDA did not have information on birth outcomes for pregnant women. Thus, the sample sizes for birth weight in the USDA are based on the birth weights of infants participants, whereas the PNSS uses births to women who enrolled in WIC. Pregnant women made up 11 percent of participants in 2000 and infants constituted 18 percent.