



Does prenatal WIC participation improve birth outcomes? New evidence from Florida

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ABSTRACT

We study the effects of prenatal receipt of nutritional and educational services provided by the Supplemental Nutrition Program for Women, Infants, and Children (WIC) on birth outcomes. Our identification strategy consists of two elements: (1) identifying families in a very tight income range surrounding the WIC eligibility threshold; and (2) exploiting a policy change that differentially influenced the WIC takeup rates of the families on each side of the eligibility threshold. We conduct this analysis by merging three large statewide administrative datasets from Florida concerning all births during the period 1997–2001. We match the birth records of infants and the school records of their older siblings in order to relatively precisely identify “marginally eligible” and “marginally ineligible” families that are very similar in their observable characteristics. We find that WIC participation has no effect on mean birth weight and gestational age, but substantially reduces the likelihood of adverse birth outcomes, e.g. birth weights below 2500 g.

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1. Introduction

It is well established that consumption behaviors during pregnancy, such as smoking cigarettes and drinking alcohol, can have a substantial influence on infants' health at the time of birth. However, the potential effects of nutritional and educational supplements provided prenatally through the Supplemental Nutrition Program for Women, Infants, and Children (WIC), a program serving half of all pregnant mothers in the United States with federal expenditures exceeding \$5 billion per year, have been notoriously difficult to identify due to non-random selection into the WIC program.

The fundamental difficulty in the WIC literature — and indeed, the literature on the effects of anti-poverty programs in general — is the assembly of treatment and comparison groups that do not suffer from substantial selection bias. An ideal instrument for participation would be “eligibility” if a sample were available in which we could identify potential participants who were close to either side of the eligibility threshold. In this case, a woman's location just above or below the threshold would substantially influence her WIC participation, but could be arguably exogenous to infant birth outcomes if we used a narrow enough slice of the income distribution.

This analysis has not been done previously, however, because existing datasets do not provide the ability to make such comparisons. National datasets such as the Survey of Income and Program Participation provide sufficient information on income to draw fine comparisons between WIC-eligible families and WIC-ineligible families, but are unusable for this purpose both because of the very small sample sizes of pregnant income-eligible and near-eligible mothers and because of the lack of infant birth

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outcomes present in these datasets. Consequently, WIC researchers have relied on large administrative birth certificate datasets, sometimes linked to survey data.

Birth certificate data have the advantage of providing information on birth outcomes, but they do not report income. Indeed, the only measure of income available to birth certificate researchers is Medicaid receipt. But since all Medicaid recipients are also eligible to participate in the WIC program, identifying the effects of WIC using Medicaid receipt explicitly eliminates the possibility of using some measure of eligibility to help predict participation. Moreover, one cannot credibly interpret differences between eligible WIC participants and eligible WIC non-participants as a causal effect of WIC.

Our goal in this study is to document the effects of prenatal WIC receipt on birth outcomes by (1) identifying families in a very tight income range surrounding the WIC eligibility threshold; and (2) exploiting a policy change that differentially influenced the WIC takeup rates of the families on each side of the eligibility threshold. To do so, we merge three large statewide administrative datasets from Florida concerning all births during the period 1997–2001. We use a novel strategy to identify pregnant women who are marginally eligible and marginally ineligible for WIC. The key element of this strategy is a match between the birth records of infants and the school records of their older siblings. While this approach limits the sample to pregnant women who have a school-age child, this older-sibling match is important because school records provide a source of income data by identifying whether a school-age child in the family receives free or reduced-price lunches through the National School Lunch Program (NSLP) simultaneously with the pregnancy in question. The reduced-price lunch income threshold is identical to the WIC income threshold — 185% of the federal poverty line — and reduced-price lunch eligible families are arguably marginally eligible for WIC because their incomes are between 130 and 185% of the poverty line.¹ NSLP takeup is very high in the elementary grades, so this provides a reasonable estimate of the group of families just below the income threshold for WIC eligibility.

While contemporaneous reduced-price lunch participation in the family is a strong proxy for marginal WIC eligibility, it is not as apparent how to construct a reasonable control group of marginal WIC ineligible. We solve this problem by taking advantage of the fact that we can not only match newborns to their older siblings in school simultaneously, but we can also follow the older siblings longitudinally. Therefore, we can identify the set of families that were not participating in NSLP during the pregnancy but were participating during either the year before or the year after (or both). These families — WIC non-participants who were willing participants in another supplemental nutrition program during at least one year immediately adjacent to the pregnancy period — form a natural group to label as marginally ineligible families for the purposes of this analysis. To make the marginally eligible group still more comparable to this marginally ineligible group, we further refine our marginally eligible group to be families that received reduced-price lunches during the pregnancy but that *did not* receive these lunches in at least one adjacent year.

The two groups of women we have defined as “marginally eligible” and “marginally ineligible” are very similar in their observable characteristics, but those who are marginally eligible are more likely to participate in WIC. This pattern becomes more pronounced following a federal policy change that increased income reporting requirements for WIC eligibility effective September 1999. For most states this policy change was non-binding, but in the case of Florida it represented a sharp increase in the documentation requirements for WIC applicants. Studies by Swann (2007) and Tiehen and Jacknowitz (2006) suggest that documentation requirements deter participation in WIC, and thus this policy change may be important in predicting WIC participation in Florida. We argue that this policy change should make it differentially more difficult for ineligible families to obtain WIC benefits than it would for families that are marginally eligible for WIC benefits. Indeed, the interaction of “marginally eligible” with an indicator for the policy change provides a strong first-stage prediction of WIC participation. We then use this prediction to estimate the effects of WIC on birth outcomes for the women in our sample. Because our income proxy is available only for those with a child in school, we must limit our sample to mothers with an older child in school. While this strategy limits the sample size, it actually helps in the sense of creating a more homogeneous sample in which to estimate WIC effects.

Our strategy provides new insight into the effects of WIC using data that are better suited to this task than many previous studies. We find no evidence that WIC participation has an effect on mean birth weight and gestational age, but that this lack of any evident “mean effects” masks substantial effects in the bottom tail of the birth weight distribution. Our results indicate that the likelihood of an infant being born low-birth weight (defined as less than 2500 g) is substantially reduced by maternal WIC participation.² We also find some suggestive evidence that WIC participation reduces the incidence of very high birth weights. These combined results indicate that WIC participation seems to compress the birth weight distribution by bringing the tails closer to the mean, resulting in a healthier range of birth weights. Therefore, our estimates indicate that WIC works primarily through reducing the rate of adverse birth outcomes in participating families.

2. Past analysis of WIC effects

The short-run aim of the WIC program is to increase the consumption of nutritious food in low-income households during pregnancy. Prenatal WIC is available only during a precise time period in the life cycle (pregnancy) and the nature of

¹ Those with income below 130% of the poverty line qualify for free (not reduced-price) lunches.

² We choose a high threshold for low birth weight status rather than a very limited threshold (e.g., less than 1500 g) for power reasons — even with population-level data, our identification strategy has high data requirements and we do not have a sufficiently large number of observations to detect large effects on very rare outcomes.

pregnancy timing is somewhat stochastic. Although the permanent income hypothesis would suggest that upon receiving the benefits the recipients would smooth consumption, resulting in just a minimal increase in consumption during pregnancy, there are at least two reasons to think the consumption may instead stay within the pregnancy time period. First, low-income households are often credit-constrained, making their consumption particularly sensitive to cash-on-hand (Card et al., 2007). Second, since the benefit is in-kind it is not simple to engage in the consumption smoothing that we might otherwise predict.

The intermediate and long-run aim of the prenatal WIC program is to improve the health of at-risk pregnant women and their children by means of increasing the likelihood that birth outcomes will be in a normal, healthy range. In the case of birth weight, WIC is intended to increase birth weights in the bottom tail of the birth weight distribution, i.e. to reduce the occurrence of low-birth weight. At the same time, it is reasonable to expect that in promoting health, WIC may also “regulate” the birth weight of babies that would otherwise be extremely large by reducing excessive pregnancy weight gain through improved maternal nutrition. Bringing down the top tail of the birth weight distribution is also health-improving, since very large babies lead to complications in delivery and introduce potential health problems for both mother and child (Boulet et al, 2004). Most studies of WIC examine its effects on average birth weight; yet if WIC has a “normalizing” effect on the weight of newborns across the birth weight distribution, the program would be health-improving even if the average effect on birth weight were zero. While we do examine the average effects of WIC to compare our study to the past work described below, we also investigate this possibility of heterogeneity of WIC effects across the birth weight distribution.

A plethora of WIC evaluations exists, but the effects of the WIC program remain uncertain. The issue of prenatal WIC effects is particularly contentious, as even the highest quality research has been unable to convincingly solve the problem of selection into WIC.

Bitler and Currie (2005), for instance, birth certificate data from Medicaid participants (all of whom are adjunctively eligible for WIC) from nineteen states merged with survey data that include WIC information from 1992 through 1999. They argue that despite apparent negative selection on observables, WIC participants have healthier birth outcomes than those Medicaid recipients who did not participate in WIC. In particular, they estimate that WIC participation increases birth weight on average by 64 g. Joyce et al. (2005) use data from New York City birth certificates, which include a WIC indicator, for the years 1988 through 2001. The authors argue that large WIC effects on gestational age are clinically implausible, and find little evidence of positive WIC effects on fetal growth as well. Joyce et al. (2008) make use of births from 1995–2004 from nine states that participate in the Pregnancy Nutrition Surveillance System. The authors make use of variation in the timing of WIC enrollment to estimate the effects of WIC spell duration on birth outcomes and again find only modest estimated effects of WIC spell duration. Ludwig and Miller (2005), in their literature review, argue that selection into WIC appears to be negative, suggesting that these papers may be underestimating the effects of the program. The existing studies downplay instrumental variables estimates because of a lack of substantial predictive power.

Because of data limitations, the existing papers restrict their samples to WIC-eligible women, who range from very low-income to only marginally low-income. When examining WIC recipients relative to non-WIC recipients in their samples, they are unable to control for relative income. If participation is at all correlated with income, there may be an omitted variable problem. We are able to improve upon this method by using our indirect source of family income data (participation in the NSLP, for which eligibility requirements did not change during our sample period) to limit our sample to “marginally eligible” and “marginally ineligible” women. This strategy generates a more homogeneous sample in terms of income, and we can also use this eligibility indicator as a control variable in modeling WIC participation. However, since this variable may also belong in the model of birth outcomes (since these outcomes tend to improve with income) it cannot itself be justified as an instrument for participation. Instead, we interact the eligibility indicator with a policy change that differentially affected the two eligibility groups.

3. Identifying marginally eligible and marginally ineligible groups

The data for this project are merged from three administrative sources in Florida. The base data are Florida birth certificates from 1989–2002 (which increased over the period from 165,000 to 212,000 per year). Data on the birth certificates include health information about the newborn and demographic information about the mother such as education, marital status, race, age, country of birth, and Medicaid receipt during pregnancy. We merge these birth certificate data with records from the Florida WIC office. The key information provided in the WIC records is the date that each pregnant WIC recipient became certified for the program. The fraction of children that are born to prenatal WIC participants has grown dramatically over time; over 40% of Florida's new mothers in 2002 had used prenatal WIC.

While somewhat similar birth-record and WIC data have been available in the past, we have the ability to link these data to school records, which provide details on family income changes over time. Newborn infants are matched to their older siblings via their mother's social security number, and older siblings are matched to school records using a 20-step process that successfully matches 85% of Florida older-sibling births to public school records. Given that about 10% of children born in Florida attend private schools, this means that we match more than 90% of the children who were potentially matchable with this algorithm. In just under 14% of Florida births from 1997 through 2002, we can observe the contemporaneous school records of an older sibling. Our sample is necessarily selected: It is the set of multiple-birth families where there exists at least a six-year gap in age between two siblings (though not necessarily successive siblings.)

We use the structure of these data to identify groups of women who appear to be “marginally WIC eligible” and “marginally WIC ineligible”. Because these groups are very narrowly defined, they are small relative to the total sample size, but are still

Table 1

Sample size and WIC participation for marginal eligibility groups

Lunch status of older sibling in year that mother of younger sibling was pregnant	No NSLP participation in adjacent years	Inconsistent NSLP participation in adjacent years	Consistent NSLP participation in adjacent years
No NSLP lunch benefits	11000 WIC: 11.3%	2471 WIC: 38.2%	59
Received reduced-price lunch	3 WIC: 56.6%	1741	3079 WIC: 59.2%
Marginally ineligible N = 2530	Marginally eligible N = 1744		

Notes: Those with “no NSLP lunch benefits” include non-applicants and rejected applicants. Those with “some reduced-price lunch” include those who consistently received reduced-price lunches and those who bounced between reduced-price lunch and free lunch during the pregnancy. Those with consistently free lunch in the current year (or who bounce between free lunch and no lunch) are not included in the table. Those who are recorded with reduced-price lunch during part of the year and no lunch in another part of the same year are also excluded.

sufficiently large for a convincing statistical analysis. The “marginally eligible” are those with an older child who received NSLP reduced-price lunch benefits simultaneous with the pregnancy,³ but who at some point in the year before or the year after (or both) did not participate in NSLP. Older siblings who participated in NSLP during their mother’s pregnancy with their younger sibling but in neither of the surrounding years ($n=3$) are said to have “no NSLP participation in adjacent years,” while those who experience both participation and non-participation in the preceding and/or the following year ($n=1742$) are said to have “inconsistent NSLP participation in adjacent years.” The “marginally ineligible” are those with an older child who did not receive NSLP benefits simultaneous with the pregnancy, but who did participate in NSLP in either the year before or the year after (or both). In 59 cases, older siblings consistently participated in NSLP in both the year before and the year after the pregnancy, but not during the pregnancy itself, while in 2472 cases older siblings participated in NSLP at some point (but not consistently) in the year before or in the year after the pregnancy. Older siblings receiving only free (rather than reduced-price) school lunches simultaneous with the pregnancy are not in either group, since their lower income makes them non-marginal for our purposes. We also do not use consistent school lunch recipients or consistent school lunch non-recipients, since we have no evidence that their income hovers near the eligibility threshold. The breakdown of these groups is presented in Table 1.⁴

Since we are using NSLP participation as a proxy for income, it is important that we establish that changes in NSLP participation are in fact correlated with changes in income. Of course, we cannot check this correlation using our data, since the lack of income information is the impetus for our use of the proxy. We therefore examine this issue using the 1996 panel of the Survey of Income and Program Participation (SIPP) which contains both NSLP and income information for a nationally-representative sample of low-income families. We select a sample of women ages 18–44 whose families have a child that participates in NSLP sometime during the 4-year panel but not the entire time.⁵ We find that income is indeed negatively correlated with NSLP participation among these “off-and-on” families, with a correlation coefficient of about $-.15$. In terms of dollar magnitudes, there is an almost \$100 average gap between the typical monthly income of a family while participating in NSLP and the same family’s typical monthly income when not participating in NSLP. This gap is statistically significant and suggests that families are indeed more likely to participate in NSLP when their incomes are lower.

Since we are working with a special subsample, it is informative to compare this sample to the larger population of births in Florida during our focal time period (1997–2001). We also want to consider the differences between our subsample and a typical sample used in WIC analysis: Medicaid recipients. Table 2 reports descriptive statistics for these three populations, along with disaggregated information on subgroups within our sample of interest. We also estimate average “WIC effects” (though they are

³ Depending on the year, Florida NSLP eligibility is observed either two or (typically) three times per school year. The student meets the “eligible for reduced-price lunch” standard, by our calculations, if he or she is reported to have received reduced-price lunches during any of the observations that took place during the period of the relevant pregnancy. We then calculate NSLP eligibility outside the pregnancy based on NSLP observations during the 12 months preceding the estimated conception date and during the 12 months following birth. Most changes in observed NSLP eligibility occur when a child transitions from one school year to the next. However, it is not uncommon for students to move from ineligible to eligible within a school year due to a change in circumstances that prompts an application or due to a family failing to apply prior to a given round of observation. Also in occurrence are families who transition from eligible to ineligible during a school year, either because of increased reported incomes or due to audits that demonstrate ineligibility. Schools have strong financial incentives to have every eligible family claim NSLP, so we expect most transitions off of NSLP to indicate lost eligibility rather than changes in participation decisions of families.

⁴ We also experiment with an alternative definition of marginal eligibility in which only the previous and current year’s NSLP was taken into consideration (not the following year). However, this reduces our sample size and thus weakens our ability to precisely estimate program effects (the instrument becomes unacceptably weak).

⁵ To focus on changes in participation, we select a subsample for which families were either participating in NSLP at both the beginning and end of the survey but not consistently in between, or who were not participating in NSLP at the beginning and end of the survey but were participating at some point in between. To count as “participating” we require at least 5 months of participation during the 4 years, to avoid coding errors that can affect a single (4-month) wave. Note that we do not restrict the sample to pregnant women since this would make it prohibitively small. Further details about the estimation discussed here are available from the authors.

likely not identified by OLS) with all three samples, to see what effect the sampling choice has on basic correlations. However, our focus remains on exploiting our new instrumental variables strategy to identify causal WIC effects across the distribution of birth outcomes, which can only be done with our distinctive eligibility-related sample.

There are a few distinctive features of the sample we will be using (in the third column) relative to the Medicaid sample and the full set of birth records. Since we choose a sample of women with a child in school, it is not surprising that the average age of women in our sample is a couple of years older than the average in the other samples. In addition, our sample is about 86% native-born, versus about 80% in the other samples. WIC participation is a bit higher in our sample than in the broader set of birth records, as we might expect given that our sample excludes all women whose children have not participated in NSLP (who are mostly high-income and ineligible for WIC). Other than some other small variation, our eligibility-related sample is quite similar to the larger population of birth records. In both samples, about 81% of mothers have a high school degree or more, roughly two-thirds of the mothers are married, and about 45% of mothers participate in Medicaid. The birth outcomes for the large sample and our small sample are very similar, with only a slightly higher rate of premature births in the small sample. Given the complexity of our sample construction, it is encouraging to see that the sample seems quite representative of overall births during this time period.

On the other hand, the Medicaid sample, used in past studies to limit the sample to WIC-eligible women, differs dramatically from both overall births and from our eligibility-related sample. The Medicaid sample is more disadvantaged on every observable margin. Since we have an opportunity to handle endogeneity and to use a sample that is more similar to the larger population, we do not focus on the Medicaid population for our study.

Along with comparing our sample to other larger samples, we need to look within our sample to determine whether we have done an adequate job identifying “marginally eligible” and “marginally ineligible” mothers. Given the narrow definitions we have used, the difference in the two groups’ WIC participation should be one of the only meaningful differences between them. In addition, this difference in WIC participation patterns should be magnified when income documentation requirements are added if those who are marginally ineligible have a harder time obtaining WIC benefits after the policy change. The four columns on the right side of Table 2 show the descriptive characteristics of these two groups before and after the policy was implemented. Because the policy change occurred while some pregnancies we already in progress, there is a cohort of women (who conceived during the 9 months prior to the policy change) who may have been treated under the old or new regimes, depending on the timing of their WIC participation. We eliminate this “partially treated” group to better isolate the effect of the policy change. Since the new policy

Table 2
Descriptive statistics

		Full set of birth records	Births to women on Medicaid during pregnancy	Births to women in our eligibility-related sample (see Table 1)				
				All	Before income documentation	After income documentation		
					Marginally eligible	Marginally ineligible	Marginally eligible	Marginally ineligible
Race	White	73.68	61.43	72.33	72.71	70.53	69.21	75.25***
	Black	23.35	36.61	25.98	26.64	27.87	28.30	23.34***
Mother's birth country	U.S.	79.80	79.97	85.96	84.03	88.96**	83.96	86.65**
	Cuba	3.14	3.35	2.35	2.41	2.02	2.26	2.53
	Mexico	1.46	2.02	1.43	2.84	1.35*	1.79	0.84**
	Rest of world	15.60	14.65	10.26	10.72	7.67*	11.99	9.97*
Mother is married		62.80	35.34	66.39	68.78	63.73*	63.30	69.14***
Mother's education	<High school	18.84	35.19	17.38	17.90	22.67**	16.33	15.77
	High school	34.54	43.26	45.86	44.98	42.13	48.44	45.79
	>High school	46.17	20.95	36.37	36.46	34.93	34.84	38.05*
Father's education	Missing	18.28	33.62	14.83	14.19	17.47	14.31	14.25
	<High school	10.95	18.48	13.91	18.12	14.93	14.54	11.95**
	High school	31.41	33.57	42.38	41.48	39.73	44.95	41.86*
	>High school	39.36	14.33	28.88	26.20	27.87	26.21	31.93***
Mother's age (mean)		27.23	24.30	29.53	29.52	28.71***	29.62	29.81
Adequate prenatal care		88.27	81.36	87.61	89.04	86.35	88.42	87.19
Mother on Medicaid during pregnancy		44.47	100	45.37	46.94	45.39	50.08	41.55***
Mother on WIC during pregnancy		39.28	69.45	45.69	49.78	44.19*	58.94	35.71***
Mother's WIC spell length† (mean)		126.67	132.11	127.75	141.03	128.50**	124.41	126.60
Birth Weight (mean)		3280.23	3213.81	3287.90	3277.76	3302.25	3271.32	3296.44
Fraction low-birth weight		8.24	9.65	8.56	10.70	8.80	7.85	8.42
Gestation in weeks (mean)		38.54	38.51	38.37	38.35	38.39	38.36	38.38
Fraction premature		10.47	11.39	11.67	12.88	13.20	11.66	10.72
Fraction twin births		3.04	2.56	3.23	3.06	2.40	3.42	3.48
Number of observations		704,646	312,066	4276	458	750	1286	1782

†This uses only the sample with a WIC spell. ***s indicate statistically significant differences b/w covariates in columns 4 and 5, or 6 and 7, with *10%, **5%, ***1%. Notes: For consistency, we report records in the same time period for all subgroups: conceptions between Jan. 1997 and Dec. 2001, excluding conceptions in the 9 months prior to September 1999 (since we do not use them for our later estimates). The sample excludes those with missing values for key variables (such as gestational age). A few cells use slightly smaller samples due to missing data on those covariates. Omitted categories: Race = other, Mother's educ. = missing.

went into effect September 1, 1999, this means we drop observations of women who became pregnant between December 1998 and August 1999.

One observes that along a number of dimensions, marginal WIC eligibles and marginal WIC ineligible share similar characteristics. However, there are some meaningful differences across these groups, as indicated by asterisks in the table. Some of these differences persist over time, such as ineligible being consistently more likely to have been born in the United States than the rest of the world. Such differences will be netted out by our estimation strategy. Other differences do suggest some compositional changes in the groups over time, such as in marital status and education levels. Our estimation strategy will condition on these variables to be sure their effects are not confused with the effects of WIC.

Marginal WIC eligibles and marginal WIC ineligible, by our measure, had fairly similar WIC participation rates prior to the policy change — 49.78% for eligibles and 44.19% for ineligible. This similarity reinforces two features of our choice of sample. First, the similar rate of participation between these two groups is reassuring, since we chose the groups to be as comparable as possible. Second, the fact that the participation rate is in fact a bit higher among the eligible suggests that we also have successfully identified a distinction in the eligibility between these groups, even though we rely upon a noisy measure of eligibility. We could observe participation among those we label “marginally ineligible” for a number of reasons, including understating income for the purposes of WIC application or lack of participation in NSLP by NSLP-eligibles, but it is important to note that these two groups face the same reporting requirement in the pre-policy years.

Following the documentation policy change, large differences in WIC participation rates emerged. The post-policy participation rate for marginal eligibles increased from 49.78 to 58.94% but the rate for marginal ineligible fell considerably, from 44.19 to 35.71%. These differences are not simply due to compositional changes in the groups: Using the pre-policy data to predict WIC participation rates, we would expect the marginal eligibles and marginal ineligible in the post-policy period to experience only small changes in their participation rates. The predicted participation rate of marginal eligibles post-policy is 52.51%; the true increase in participation was higher, perhaps reflecting the economic downturn that began in the year 2000 which may have pushed more families into income eligibility. The participation rate for marginal ineligible was also predicted to increase slightly, to 44.76%. We find instead a sharp decline, consistent with documentation policy making it more difficult for marginally ineligible women to obtain WIC benefits.⁶ It is this change in the relative participation rates that provides the first-stage variation in our instrumental variables analysis.

4. Estimating the effects of WIC on birth outcomes

4.1. Ordinary least squares estimates

We begin our empirical work by estimating a simple ordinary least squares (OLS) regression model, including an indicator for WIC participation, for purposes of comparison to past work and comparison to our estimates utilizing instrumental variables (Table 3). Our estimates examine the relationship between WIC and birth weight, an indicator for low-birth weight, gestational age, and an indicator for prematurity. Throughout our analysis, we condition on race, maternal education levels, paternal education levels, mother's place of birth, marital status, maternal age, baby's gender, plurality of birth, the Kessner index of adequate prenatal care, dummy variables for each calendar year and month of conception in the sample,⁷ and the indicator for whether the family is marginally eligible or marginally ineligible for WIC receipt. Some specifications, as noted in the table, also include gestational age as a covariate.

The OLS estimates with our eligibility-related sample suggest no association between WIC participation and average birth weight. The same result holds for the larger samples, as even the statistically significant estimates are so small as to be unremarkable. WIC participation also does not predict meaningful reductions in the occurrence of low-birth weight. (Note that both of these estimates condition on gestational age as a covariate). All three samples suggest that WIC is associated with increased gestational age, but only about one to two days. The estimated relationship between prematurity and WIC is small and insignificant in our eligibility-based sample, but the relationship in the larger samples appears to be negative and fairly large (consistent with Bitler and Currie, 2005). Except for gestational age in these large samples, there is essentially no evidence of a meaningful association between WIC and birth outcomes. However, because these analyses require the implausible assumption of random selection into WIC, it is difficult to interpret these results as effects of WIC.

4.2. Graphical analysis

We instead approach the WIC selection problem with an instrumental variables strategy. The differential effect of the WIC documentation policy by eligibility group allows us to construct an interaction variable (policy indicator and eligibility indicator) that can be examined as a possible instrument for WIC participation. Note that we do not require the policy indicator and eligibility

⁶ Another possible way to check whether this divergence is related to the policy is to investigate Medicaid participation during the same period. If Medicaid participation had the same wide divergence in participation during this period, it is possible that these changes in participation were not related to WIC policy (although they could still be, depending on how often WIC receipt influences Medicaid receipt). In our data, there is only a small divergence in Medicaid participation over this time period: for “marginal eligibles” participation goes from 46.9% to 50.1%, and for “marginal ineligible” participation goes from 45.4% to 41.5%. Incorporating changes in observables explains part of this divergence (estimates are available upon request). This suggests there was a distinct effect of the WIC documentation policy.

⁷ We have also run the estimation with year-by-month indicators, with similar results.

Table 3

OLS estimates of the effects of WIC on birth weight and gestational age

Sample	Birth outcome			
	Birth weight (grams)	Low-birth weight indicator (<2500 g)	Gestational age (weeks)	Premature birth indicator (<37 weeks)
Eligibility-related sample (N=4190)	5.88 (14.51)	.005 (.007)	.167** (.072)	-.009 (.010)
Prenatal Medicaid recipients (N=303,006)	10.14*** (1.76)	-.003*** (.0009)	.259*** (.009)	-.028*** (.001)
Full set of birth records (N=684,117)	1.57 (1.25)	-.001 (.0006)	.175*** (.006)	-.017*** (.0008)

Note: All control variables are described in the text. In addition, the first two columns include gestational age as a covariate. The first row also contains the eligibility dummy variable (which cannot be constructed for the other rows).

significant at 5%; *significant at 1%.

indicator to themselves be instruments; both eligibility and time trends may have an overall effect on birth outcomes and thus should be included in the outcome equation. The pattern of WIC receipt between the two eligibility groups over time — by month of conception — is provided in Fig. 1. It is clear from the figure that there has been a significant widening of the gap in WIC participation rates among the marginally eligible and ineligible groups since the implementation of income documentation, and that this widening occurs at the appropriate time. The partial *F*-statistic of the first stage of our instrumental variables regression invariably exceeds the general threshold of 10 suggested by Staiger and Stock (1997).

Our second stage of estimation examines the possible effects of WIC on four birth outcomes related to weight and length of gestation. The time trends in each of these outcomes for the treatment and comparison groups, pre- and post-policy change, are shown in Fig. 2.

A few key features of the data are apparent. First, there is no indication that there were differential trends in any of the outcomes between the two groups prior to the policy change. Second, there is no apparent indication of strong differences in outcomes following the policy that were not present pre-policy, with the exception of the “fraction low-birth weight” seen in the third panel of the figure. Before the policy change, the marginally ineligible had quite consistently lower fractions of low-birth-weight infants, consistent with the typical pattern of better health outcomes for those with higher incomes. But after WIC benefits became more difficult for them to obtain (after the policy change) there is some indication that they ceased to maintain this advantage. We suspect that the increase in WIC participation of the marginally eligible after the policy change, and the steep drop in participation among the marginally ineligible, is responsible for this convergence in outcomes. The finding of an apparent effect of reduced incidence of low-birth weight but no change in mean birth weight is consistent with the notion that WIC primarily works through the improvement of adverse birth outcomes, rather than an across-the-board change in infant health. We next study this phenomenon more formally using instrumental variables regression.

4.3. Instrumental variables estimates

The instrumental variables estimates use the same set of covariates as our earlier OLS estimates. The difference here is the inclusion of our instrumental variable: the interaction between the eligibility dummy and a post-policy-change dummy. Because the endogenous regressor (WIC receipt) is an indicator variable, we implement the estimation of birth weight and gestational age effects using a generated instrument (“predicted probability of WIC participation”) derived from a probit model that utilizes the policy/eligibility interaction. For those regressions with a binary outcome (low-birth weight, premature birth) we use a bivariate

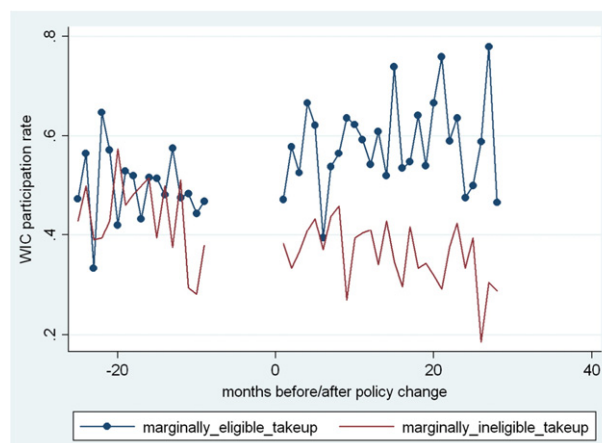


Fig. 1. Differential WIC participation by month of conception for marginally eligible and marginally ineligible families. Note: Conceptions during the 9 months prior to the policy change are dropped since their treatment status is ambiguous. WIC participation is defined as “any WIC” during the pregnancy.

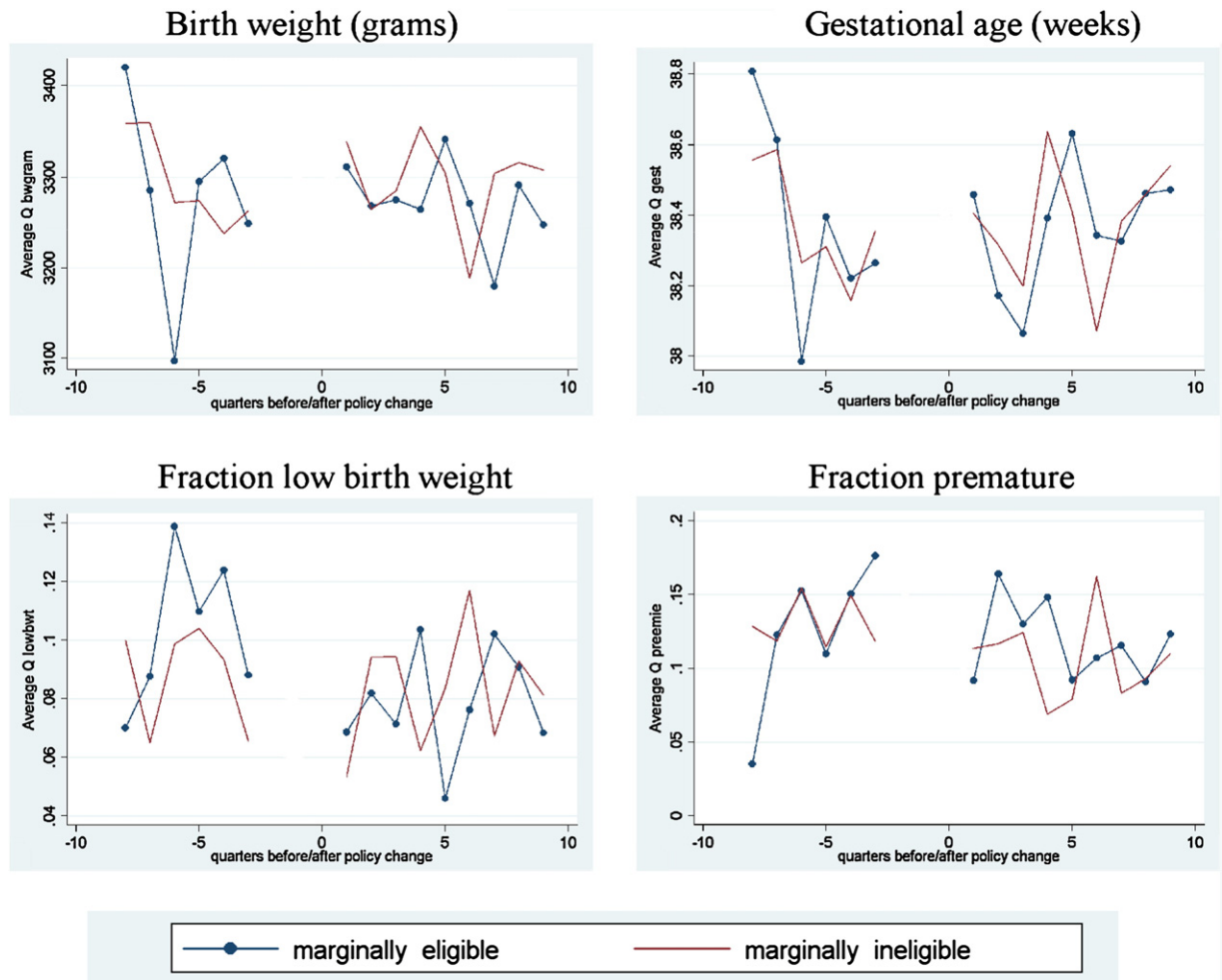


Fig. 2. Birth outcomes by quarter of conception – marginally eligible/ineligible families. Notes: Quarterly averages (using January–March, April–June, etc.) are reported rather than monthly averages in order to reduce the noise in the graphs. Conceptions during the 3 quarters 1999:1, 1999:2, and 1999:3 are dropped since their treatment status is ambiguous (note that the policy passed September 1, 1999, but dropping the whole third quarter effectively treats the month of September as a month of ambiguous treatment status).

probit model that allows us to account for both the binary outcome and binary endogenous regressor. (Results using some alternative methods are mentioned in the table notes). Table 4 presents the estimated marginal effects of WIC on the outcomes of interest.⁸

The estimated effect of WIC participation during pregnancy on birth weight is similar in magnitude (94 g) to those reported in the existing literature, but is imprecisely estimated. At the same time, WIC participation appears to statistically significantly reduce the likelihood that a newborn will be born with low-birth weight. In fact, the estimated effects are large, suggesting a 12.9 percentage point reduction in the probability of low-birth weight. While we do not place great stock in the magnitude of this estimate (since the confidence interval is still fairly wide) the results suggest strongly that WIC participation during pregnancy substantially reduces the risk of low-birth weight. However, we do not find evidence that WIC participation meaningfully affects gestational age or the likelihood of prematurity. (The instrument verification for likelihood of prematurity suggests that the IV strategy is not significantly different from the OLS strategy.)

4.4. Distributional impacts

The fact that WIC apparently reduces the likelihood of low-birth weight while not substantially changing the likelihood of a pre-term birth indicates that WIC must have differential effects on different parts of the prospective birth weight distribution. In fact, this would be consistent with the goals of the WIC program to bring birth weights into a healthier range. Table 5 estimates

⁸ If we cluster the standard errors at the month-group level, the standard errors in Table 4 change, from left to right, to 181.97, 0.051, 1.24, 0.089, so inference is unchanged with clustering.

Table 4

Instrumental variables estimates of the effects of WIC participation on birth weight and gestational age

Birth outcome	Birth weight (g)	Low-birth weight indicator (<2500 g)	Gestational age (weeks)	Premature birth indicator (<37 weeks)
Estimated marginal effect of WIC participation	94.00 (203.13)	-.129*** (.044)	-.540 (1.03)	.032 (.107)
Instrument verification	First-stage partial <i>F</i> -stat: 21.3	Wald test rejects exogeneity	First-stage partial <i>F</i> -stat: 20.69	Wald test does not reject exogeneity

Note: $N=4190$. Standard errors are in parentheses. All control variables, as described in the text, are included in the first and second stages of the instrumental variables regression. In addition, the first two columns include gestational age as a covariate. An alternative specification for the first two columns that does not include gestational age as a covariate has second stage estimates of: 4.30 (271.56) and -0.099 (0.086). Using two-stage least squares (rather than the methods described in the text, which handle the binary endogenous regressor) produces estimates of the same signs and similar significance, although magnitudes vary somewhat and the coefficient in the low-birth-weight estimates is significant at the 10% but not 5% level. Including additional information about prematurity of previous births does not meaningfully affect the estimated IV coefficient. Estimates marked.

***significant at 1%.

separate WIC effects on birth weight for different subgroups within the sample to investigate this pattern more closely. As with the estimates in the previous table, these estimates use the generated instrument strategy to handle the binary endogenous regressor.

First, we stratify the sample based on whether or not the newborn was born prematurely. One observes that the estimated effects of WIC are concentrated entirely in the 12% of births that occur prior to 37 weeks of gestation, though the small sample generates imprecise estimates. While WIC does not appear to influence the likelihood that a newborn will be premature (based on Table 4), it appears in Table 5 that newborns born prematurely to WIC participants are likely to have much higher birth weights than those born prematurely to non-participants, although the confidence interval is wide.⁹ No apparent difference occurs for full-term births. For the relatively small premature birth subsample, the first-stage *F*-statistic falls below the conventional threshold of 10, but does remain well above 4 (which corresponds to 95% statistical significance of the instrument in the first stage).

Second, we stratify the sample based on the quartile of the newborn's expected birth weight. We estimate expected birth weight as a simple average of the birth weights of previous children born to that mother.¹⁰ One sees the same pattern here as with prematurity: The WIC program appears to have large positive effects (just over one pound) on birth weights for those infants who might have been expected to be small. Although not precisely estimated, there is also some more suggestive evidence that WIC may decrease some birth weights at the top of the distribution. It would appear that WIC may have a "normalizing" or "regulating" effect on birth weights in both dangerous extremes of the birthweight distribution.¹¹ The *F*-statistic is very close to the conventional threshold of 10.¹² This again seems to suggest that the strongest, perhaps only, positive effects of WIC on birth weight occur for infants at risk for low-birth weight.

5. Robustness checks and falsification tests

Our results provide strong support for the hypothesis that there are heterogeneous effects of WIC, and that the bulk of the WIC health benefits accrue to higher-risk pregnancies. The remainder of our paper investigates the robustness and credibility of these findings.

5.1. Robustness to WIC spell length

Throughout this paper, we have followed the bulk of the literature by treating prenatal WIC receipt as a binary variable. However, our data also provide the date that WIC takeover began for all of the women who participated, which Joyce et al. (2008) utilize for identification in their recent study. By documenting the days between that date and the infant's birthdate, we can distinguish intensity of participation. If our method measures the nutritional effects of WIC, we would expect a smaller WIC effect on the probability of low-birth weight for those with short WIC spells and a larger effect for those with longer WIC spells.

We find that the estimated effects of WIC participation remain statistically significant when we break down the sample this way, and the magnitudes differ in the expected way – effects appear to be largest for participants who were in the program longest. Our estimated WIC effect on the probability of low-birth weight is for women with less than three months of WIC participation is $-.09$ percentage points while the estimated effect for those with more than three months is $-.14$ percentage points. This is consistent with scientific evidence that birth weight is most strongly affected by nutrition early in pregnancy, since early nutrition establishes placental function (Moore et al., 2004).¹³ WIC participation early in pregnancy appears to positively influence

⁹ Since prematurity is more common among black mothers than others, we also thought this pattern may hold if we stratified by race; results are indeed similar and are available upon request.

¹⁰ The most recent 3 births are available in our data set. Note that every mother has at least one previous birth, since our sample of mothers all have at least one child in school.

¹¹ About 10% of our sample are clinically "macrosomic" (over 4000 g), and macrosomia has been shown to be associated with adverse birth outcomes such as fetal injury, perinatal asphyxia, and fetal death along with maternal complications such as increased probability of caesarean delivery (Boulet et al., 2004). A more detailed quantile-regression analysis by decile (available upon request) also provides suggestive evidence that WIC may compress the birth weight distribution.

¹² The same analysis can be done in principle for any choice of stratification. For example, we also tried stratifying by whether an infant was predicted to have clinically defined low birth weight (<2500 g), but the sample of these infants is too small to get precision in the first or second stage estimates. (First-stage partial *F*-statistic=1.82).

¹³ Inadequate maternoplacental supply is considered the chief determinant of subsequent intrauterine growth retardation (Harding, 2001).

Table 5

Differential estimated effects of WIC participation on birth weight, by prematurity, predicted birth weight, and race

	Stratification by prematurity		Stratification by predicted birth weight		
	Premature newborns	Full-term newborns	Predicted <25 percentile	Predicted 25–50 percentile	Predicted >50 percentile
Estimated marginal effect of WIC participation	491.50 (401.69)	–33.56 (223.82)	592.81* (309.54)	267.39 (340.48)	–399.95 (379.78)
First-stage partial <i>F</i> -statistic	6.16	16.99	9.90	6.57	7.11
Sample size	<i>N</i> =489	<i>N</i> =3701	<i>N</i> =958	<i>N</i> =1193	<i>N</i> =2037

Note: All control variables, as described in the text, are included in the first and second stages of the instrumental variables regression. We also include gestational age as a covariate. Predicted birth weight is the average of the woman's birth weights for up to three previous births. Alternative estimates using 2SLS (that do not account for the binary endogenous regressor) produce results of similar significance, although the magnitudes are much larger for premature and black newborns. An alternative specification for these, without gestational age as a covariate, has similar first-stage results, with second stage estimates of (from left to right): 746.22 (645.00), –63.14 (238.51), 145.46 (441.32), 192.77 (415.17), –400.90 (456.48). However, without the gestational age variable, the 2nd stage overall *F*-statistics are even smaller than those reported in the table, suggesting (unsurprisingly) that gestational age is an important covariate for predicting birth weight. The last column is not divided into two quartiles due to very low first-stage *F*-statistics when the sample is split.

*significant at 10%.

these crucial early developments. However, even for those with short durations on WIC (starting later in pregnancy) the effects are substantial. This finding makes sense if birth weight can be affected by nutrition late in pregnancy. There is historical evidence of this phenomenon: During the Dutch famine (1944–1945), women who experienced nutritional deficiency late in pregnancy delivered children with lower birth weights, even conditional on delivering at full term (Painter, Roseboom, and Bleker, 2005). Our results suggest that WIC is able to do the opposite by providing a supplement that increases birth weight, even late in pregnancy. Of course, an alternative interpretation of these results is that other features of the WIC program – such as nutritional education – may also contribute to healthy births even if received later in the pregnancy. Regardless of the interpretation, our results simply continue to indicate that participation in the WIC program reduces the probability of low-birth weight.

5.2. Falsification test using prenatal care

Although our choice of treatment and comparison groups, as well as the policy change, seem to properly isolate the effect of WIC on birth outcomes, it is still possible that some other unobserved difference between WIC recipients and non-recipients could cause the estimated treatment effect to be biased. If there are remaining differences between the health behaviors of WIC recipients and non-recipients, they may be partly observable through prenatal care. We expect that if WIC mothers are more likely to engage in healthy behaviors (and have healthier birth outcomes unrelated to WIC), this might also show up in decisions about prenatal care. If we find that there is no significant difference in prenatal care take-up between WIC and non-WIC mothers (conditional on observables), this provides some reassurance that our estimates of WIC effects can be interpreted causally.

We estimate the relationship between WIC and prenatal care using two different specifications and the same generated instrument procedure as before. First, we define prenatal care as the number of total prenatal doctor visits. Second, we use the Kessner index in which we consider both “inadequate” and “marginally adequate” care (12% of the total) to be “insufficient.” We find no evidence that WIC mothers are more likely to receive sufficient prenatal care, or to engage in more prenatal visits. The estimated relationship between WIC participation and number of prenatal visits was –.30 (with a standard error of 1.357) and the estimated relationship between WIC participation and an indicator for sufficient prenatal care was .09 (with a standard error of .17). Since the estimates are not precise around zero, some differences (positive or negative) cannot be ruled out. However, while not definitive, we find this supportive of our argument that our main findings are not simply driven by selection.

5.3. Falsification test using twin births

There may be selection into WIC based on health conditions – perhaps unobserved to us – that make a pregnancy higher risk. If so, our estimates of WIC effects will be biased downward due to the worse outcomes that would be expected from a higher-risk pool of mothers. One health condition that we can observe in our data is whether the pregnancy is a singleton or twins. We therefore conduct a falsification test to see whether we find an “effect” of WIC on whether or not a child is a twin.¹⁴ Since WIC participation could not feasibly affect whether the pregnancy is a singleton or twins, any “effect” we find would suggest that there is some selection into WIC based on the risk status of a pregnancy.

We find no evidence that WIC participation increases the predicted probability of multiple births. Depending on the method used, the indicator for WIC participation has an estimated marginal effect between –.009 and –.053 and is always very imprecisely estimated.¹⁵ While this finding is again only a falsification test, our failure to find a relationship between multiple births and the

¹⁴ Twinning is known to be caused by either genetic factors or external fertility hormone treatments, and not health behaviors. The low-income women in our sample are very unlikely to be able to afford fertility treatments.

¹⁵ Complete tables of results are available upon request. Estimates include 2SLS, 2SLS with generated instrument, instrumental variables with 2nd stage probit, and bivariate probit.

decision to participate in WIC gives us further confidence that our estimation seems to have adequately accounted for the determinants of WIC participation.

6. Conclusion

This investigation provides evidence on the causal links between prenatal WIC participation and two key birth outcomes: birth weight and gestational age. Using a unique identification strategy that allows us to create tightly-defined comparison groups and utilize an exogenous policy change that affected one marginal group but not another, we find that WIC participation during pregnancy may modestly increase mean birth weight, but that it apparently strongly reduces the likelihood of low-birth weight newborns. On the other hand, we do not find that WIC participation substantially increases gestational age or reduces the likelihood of premature birth. Instead, we find that WIC participation apparently substantially increases birth weights for premature infants and infants who would be predicted to have low-birth weight (based on birth weight of mother's previously delivered children). We also find some evidence that WIC reduces birth weights at the top end of the birth weight distribution, perhaps by providing nutritional counseling and monitoring of women at risk for high birth weight (such as women with gestational diabetes). These two findings both indicate improved birth outcomes through compression of the birth weight distribution. By looking across the distribution — particularly in the tails — rather than at means, this study allows us to see this variation even when the mean is not strongly affected. These findings especially demonstrate that WIC participation may have strong effects on birth outcomes for at-risk mothers, a result that may have important implications for the deployment of WIC recruitment efforts.

It should also be noted that it is likely that the largest effects of WIC participation may not be for the marginal WIC participants, but rather the more worse off participants who are not included in our sample. As such, we may be presenting underestimates of the potential benefits of WIC participation. The hypothetical policy experiment that we can conduct with our identification strategy involves extending WIC eligibility up the income distribution; our results indicate that doing so may continue to bear fruit in terms of improved birth outcomes.

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