

Interpreting the WIC Debate

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The editors of *JPAM* have asked us to help readers make sense of the competing claims about the efficacy of the Special Supplemental Nutrition Program for Women, Infants and Children (WIC) on pregnancy and birth outcomes that have appeared in the journal over the past three issues: The paper by Marianne P. Bitler and Janet Currie in *JPAM*, 24(1), (hereafter BC) argues that “WIC works”; the paper in this issue by Ted Joyce, Diane Gibson, and Silvie Colman (hereafter JGC), which—at the risk of oversimplifying a bit—argues that “WIC doesn’t work”¹; and the comment by BC. We were chosen for this task in part because we are outsiders to the WIC debates, although the neutrality that comes as a benefit of being outsiders also entails some cost in terms of our immersion in the full history of the WIC literature. As a result we focus our attention here on understanding the sources of disagreement between the recent *JPAM* papers, both of which improve in a variety of ways upon previous research. Our goal is to help readers reconcile apparently conflicting papers operating at the frontier of the WIC evaluation literature.

The second section of our essay focuses on what is arguably the main source of disagreement between BC and JGC: the plausibility that WIC could theoretically influence preterm birth rates. Joyce and colleagues argue that randomized clinical trials (RCTs) and other evidence from the medical literature rule out the possibility that nutritional programs could have an effect on preterm births, and by extension, that the only outcomes plausibly subject to a causal effect from WIC are birth outcomes conditional on gestational age (for example, infants born small for their gestational age, or SGA). Evidence that WIC is associated with outcomes that should not be affected by the program under JGC’s working hypothesis—such as preterm births—raises fundamental questions in their view about whether *any* of these estimates are successful in identifying the causal impacts of WIC itself rather than selection effects.

In our view, there is currently more uncertainty within the medical literature than JGC suggest concerning the potential effects of nutritional and other health interventions on preterm births. Although the existing clinical evidence of a beneficial effect of isolated prenatal interventions on preterm births isn’t very good, weak evidence about the efficacy of prenatal care is fundamentally different from good evidence that prenatal care is ineffective. On the basis of existing clinical research, there seems to be no compelling reason to exclude some birth outcomes from consideration in WIC evaluation studies.

¹ More accurately, in JGC’s own words, “prenatal participation in WIC has had a minimal effect on adverse birth outcomes in New York City.”

In the third section of the essay, we offer our interpretation of the BC and JGC estimates across the full range of birth outcomes. For most outcomes, the two papers yield qualitatively similar results: Compared to other low-income women (specifically, Medicaid recipients), WIC participants' birth outcomes are characterized by higher mean birth weights, fewer low or very low weight births, and fewer preterm births. As a proportion of the comparison group's mean the estimates presented by BC are typically larger than those presented by JGC. On the other hand, evidence for WIC effects on SGA is more mixed, with estimated effects on this outcome in BC but not in JGC.

One challenge for non-experimental evaluations of WIC impacts is the tradeoff between "under-controlling" for maternal characteristics that may affect both birth outcomes and WIC enrollment, and "over-controlling" for factors that influence birth outcomes and could themselves be causally affected by the WIC program. For example, controlling for early initiation of prenatal care may help account for differences in health motivation between mothers, but early prenatal care may be one mechanism through which WIC affects birth outcomes. Regression-adjusting for early prenatal care is thus "over-controlling" in the sense that in the idealized RCT evaluation of WIC the treatment-control comparison would capture the program's effects on birth outcomes through effects on prenatal care; we would not need to condition on prenatal care because the propensity to initiate such care on one's own will be balanced across treatment and control groups by virtue of randomization. JGC seem to err a bit more on the side of "over-controlling" than do BC. In addition, JGC's sample may also lead them to understate the national effects of WIC because the difference in social services available to WIC participants and non-participants may be smaller in the relatively generous state of New York compared to other parts of the country.

Of course both studies, by their reliance on non-experimental evaluation methods, are also susceptible to concerns about under-controlling, that is, selection bias. In our view, a simple model of the WIC enrollment decision together with available empirical evidence points—albeit tentatively—in the direction of negative selection into WIC, in which case both studies might understate WIC's effects on birth outcomes to some degree. In any case, we agree with BC that more evidence on what drives WIC participation would be extremely valuable for both research and policy.

CAN WIC AFFECT PRETERM BIRTHS? THE CLINICAL EVIDENCE

The question of whether the bundle of services provided by WIC could affect preterm birth rates is important in part because JGC argue that most of the estimated effects of WIC on birth weight—a standard birth outcome measure that is correlated with child health and long-term development, as well as requirements for more intensive neonatal medical services—is driven by WIC impacts on preterm birth rates, rather than on birth weight conditional on gestational age.² If, as JGC argue, it is "implausible clinically" (p. 677) for WIC to have any impact on preterm births, then much of the evidence for WIC's impact is arguably suspect.

Our reading of the medical literature suggests that there is no conceptual or theoretical reason why the bundle of services provided by WIC could not affect

² Low birth weight (LBW) is defined as a birth weight < 2,500 grams, and is associated with a range of short- and long-term adverse consequences. Approximately half of all LBW infants in industrial countries are born preterm (that is, < 37 weeks gestation), the other half are term infants who are small for gestational age (Ramakrishnan & Neufeld, 2001; Villar & Belizan, 1982).

preterm birth rates. The possibility of WIC impacts on preterm births is instead an empirical question, and on this point the previous empirical research within the medical literature in our view cannot rule out the possibility of WIC impacts on preterm births.

What are the risk factors for preterm birth? Previous research points to low pregnancy weight (Kramer, 1987) and more recently to low maternal weight gain during pregnancy as well, particularly during the last half or trimester of pregnancy (Abrams & Newman, 1991; Abrams, Newman, Key, & Parker, 1989; Lang, 1996; Siega-Riz, 1996). The etiology of preterm birth also involves several non-nutritional factors, including maternal infections, maternal hypertension, gestational diabetes, smoking, indoor air pollution, maternal stress, poor housing quality, poverty, teen pregnancy, and sexually transmitted disease (Gibbs & Eschenbach, 1997; Goldenberg, Hauth, & Andrews, 2000; Grijbovski, Bygren, Svartbo, & Magnus, 2004; Iams, 1998; Locksmith & Duff, 2001; Ramakrishnan & Neufeld, 2001). Little is known about the possible interaction of these other risk factors with nutrition, despite awareness within the medical literature about the interactive effect between nutrition and infection for children's health outcomes more generally (Schroeder, 2001).

Putative "risk factors" identified in these studies may, in and of themselves, be correlated with but not necessarily causally related to birth outcomes—or they may be, *per se*, necessary (that is, permissive) but not sufficient causal factors. But in any case, the fact that the medical literature has identified several environmental risk factors for preterm birth means that there is no theoretical reason that the probability of preterm birth should be beyond the reach of policy interventions. The clinical argument against a WIC effect on preterm births is empirical rather than theoretical, and must stand or fall or remain precariously balanced on empirical grounds.

In evaluating the existing clinical evidence on the efficacy of prenatal programs, which interventions are relevant for considering the potential effects of WIC on preterm birth? WIC is designed to provide more than nutritional support and education, as BC note in their comment. As the U.S. Department of Agriculture's Fact Sheet for the program notes, "WIC provides nutritious foods, nutrition counseling, and referrals to health and other social services to participants at no charge," and the program's eligibility determination itself requires an assessment of nutritional risk by a "health professional" (U.S. Department of Agriculture, 2005). Consistent with the idea that WIC's services extend beyond the provision of food, previous research finds that children in WIC are more likely to receive preventive medical care than other low-income children (Buescher et al., 2003).³

Our reading of the clinical literature suggests that the efficacy of interventions focused primarily on nutritional supplementation remains uncertain, rather than being certainly ineffective.⁴ The evidence for interventions targeted at other risk factors for preterm births is also mixed, perhaps because, as some medical commen-

³ Of course, this finding is for health utilization by children rather than by pregnant women, and is also subject to a selection interpretation; we return to both points in the next section.

⁴ Consider, for example, the characterization of this literature by Goldenberg and Rouse (1998), one of the studies cited by JGC in support of their clinical argument that WIC should not impact preterm births: "Overall, because the enhancements to prenatal care have varied from study to study and because the associated reductions in preterm birth have been inconsistent, it is not clear which specific additions to prenatal care, if any, are likely to result in a reduction in preterm births . . . Despite the large number of studies that have been performed and the variations in institutional practices, it remains unclear whether any nutritional intervention is associated with a reduction in the rate of preterm birth" (pp. 314–315).

tators argue, some such interventions target only individual risk factors in isolation,⁵ whereas other interventions may affect a constellation of risk factors that may have mutually reinforcing or mutually dependent effects in combination. If risk factors interact in their influence on birth outcomes, then variation across RCTs in study samples could also provide an explanation above and beyond variation in intervention programs for the mixed results in the clinical literature. That is, inter-trial differences in the subjects analyzed may lead to different estimates of treatment effects on different populations, even though intra-trial randomized allocation of the intervention proceeds without bias.^{6,7}

Both the uncertainties and the potential promise of interventions to reduce preterm birth are highlighted by a recent prospective randomized controlled trial of an infection-screening program by Kiss, Petricevic, and Husslein (2004). Maternal infections are thought to account for up to a third of pre-term births, and while some trials have shown that treatment of high-risk women for specific infections can increase the length of pregnancy, others have not.⁸ As the *British Medical Journal* editorial commenting on the Kiss RCT notes (Alanen, 2004), although the incidence of preterm birth was lower in the intervention group, the antibiotic treatment itself (of bacterial vaginosis) did not significantly reduce the rate of preterm birth. The difference occurred mostly in women with a normal vaginal flora, who received no treatment, but who were randomized to the group that received screening—and among women colonized with *Candida*, which surprised both the authors and the editorial commentator because *Candida* is not considered to be a risk factor for preterm birth. The implication, as argued by the *BMJ* editorial, is “that factors connected to the screening programme . . . deserve further studies” (Alanen, 2004).

In sum, there is no conceptual reason offered by the medical literature for why WIC could not affect preterm births and, in fact, some reason to believe that WIC might have such an effect—namely, the program targets simultaneously a number of the risk factors that have been identified for preterm births, and includes to some degree a health screening component. Given the current state of knowledge within the medical field, the impact of WIC on preterm births remains in our mind an open empirical question.

WIC EVIDENCE

If we allow for the logical possibility that WIC could in principle have causal effects on each of the birth outcomes considered by BC and JGC, then the results of the

⁵ Consider, for example, the 1998 editorial in the *New England Journal of Medicine* cited by JGC as part of their argument against the possibility of WIC impacts on preterm births: “When these [risk] factors [for preterm birth] have been studied in isolation, not one has resulted in a decline in preterm birth. The failure of multiple trials of single risk factors provides substantial evidence that no single factor is responsible . . . Supporting the idea that preterm birth is multifactorial is the observation that although specific interventions have no effect, ‘paying attention’ can reduce prematurity” (Iams, 1998, pp. 54–55).

⁶ This bias may be a result, for example, of eligibility criteria that differ across studies, because the populations from which the samples are drawn differ in ways related to the likelihood (or even direction) of response to an intervention, or for more obvious reasons such as protocol violations or selective reporting bias where results presented are a biased representation of the full results (Tierney & Stewart, 2005).

⁷ A recent and dramatic example of this comes from the findings from five RCTs of beta-carotene—three of which found no beneficial or even adverse effect of supplementation and two of which found large negative (that is, beneficial) effects on mortality rates (see Duffield-Lillico & Begg, 2004).

⁸ See, for example, Brocklehurst, Hannah, and McDonald (2000); Carey et al. (2000); Gibbs and Eschenbach (1997); Goldenberg, Hauth, and Andrews (2000); Lamont, Dunchan, Mandal, and Basset (2003); Leitich et al. (2003); Locksmith and Duff (2001); McDonald, Brocklehurst, Parsons, and Vignewswaran (2003); and Ugwumadu, Manyonda, Ried, and Hay (2003).

two studies are fairly consistent for most outcome measures. Both papers find that for mean birth weight, the probability of a low- or very low-weight birth, and the probability of a premature birth, women on WIC have better birth outcomes than do other low-income women on Medicaid. The magnitudes of these estimates are typically larger in BC than JGC when expressed as a proportion of the comparison group's mean, and are quite large in BC for some outcomes (for example, nearly a 54% reduction in very low-weight births from WIC participation). The one birth outcome where the two studies conflict is birth weight in the bottom decile of the national distribution for their gestational age, or small for gestational age (SGA). JGC find no statistically significant effect, while BC find a statistically significant 13% reduction in SGA for WIC participants compared to non-participants.⁹

The conflicting results for SGA highlight the tension between potentially under-controlling and over-controlling for confounding factors in non-experimental studies of WIC. While we have argued above that the clinical evidence does not support a narrow focus on this birth outcome, a different reason to prefer this outcome to others (as BC note) is that examining SGA is one way to account for gestational age bias (as would, for example, a restricted focus on birth outcomes among the set of mothers who have full-term births). There could be a spurious positive association between WIC participation and gestational age arising from the fact that women with longer pregnancies have more opportunity to enroll in WIC. Unfortunately the data sets used by BC and JGC simply identify whether women participate in WIC, not when during the pregnancy WIC participation began. On the other hand, focusing on SGA will exclude WIC effects on birth outcomes that may arise from program impacts on preterm births. So focusing on SGA alone will provide a conservative picture of WIC's effects on birth outcomes.

Why do the results of BC and JGC differ for SGA? One potential explanation is that while both studies err in a number of ways in the direction of over-controlling for potential confounders, JGC perhaps leans even more so in this direction than does BC. Both studies regression-adjust their estimates for endogenous health behaviors such as maternal smoking and (in the case of JGC) drug use. Controlling for these behaviors helps account for unmeasured differences in maternal health motivation between WIC participants and non-participants, although in principle WIC could have a causal effect on these risky behaviors. Similarly, JGC condition most of their estimates on initiation of prenatal care during the first trimester of pregnancy, although as BC note this is another outcome that could plausibly be affected by WIC participation.

A second candidate explanation for the difference in results for SGA comes from the different samples used in the two studies: JGC focus on data from New York City, while BC draw on data from 19 states. The difference in samples could be important because New York State spends more on public welfare and health pro-

⁹ JGC argue that "studies that show an association between WIC and low birth weight or mean birth weight report statistically insignificant associations or small effects of WIC on proxies for fetal growth. . . . Bitler and Currie (2005) find that WIC is associated with more than a 50 percent decline in preterm birth but only a 13 percent decline in the odds that a newborn is SGA [small for gestational age]; moreover, the coefficient is statistically insignificant in two of the three high-risk subgroups that they analyze separately" (p. 665). Our reading of BC is more in the direction of concluding that there is an association between WIC and SGA: In the main sample (N = 60,731) the odds ratio and standard error equal 0.870 (0.038). When the sample is restricted to women receiving aid last year the odds ratio is fairly similar, 0.907, but no longer statistically significant (se = 0.060) in large part because the standard error increases due to the reduction in sample size (N = 27,733). The same is true for the other sub-sample analyses for single over-18 high school dropouts (OR = 0.880, se = .110, N = 7,363) and teen mothers (OR = 0.831, se = 0.072, N = 17,102).

grams per capita and per poor person compared to the U.S. as a whole.¹⁰ In this case the “treatment dose” that results from WIC—that is, the difference in social services provided to WIC participants and non-participants—might be less pronounced in the New York sample of JGC compared to the multi-state sample analyzed by BC.¹¹

In principle, a third explanation for the difference in SGA results across the two studies could be differences in how they handle selection bias. However, there is no clear-cut advantage to one study or the other in terms of their accounting for this problem. As both BC and JGC note, neither paper has a clearly exogenous source of identifying variation (that is, a randomized or natural experiment that drives variation across low-income women in WIC enrollment). Compared to most previous studies of WIC, both BC and JGC condition on an unusually rich set of observable covariates.¹² In addition, as noted above, JGC condition their estimates on early initiation of prenatal care.

Perhaps the key concern is whether *either* study is informative about the effects of WIC in the face of concern about under-controlling for maternal characteristics associated with both birth outcomes and WIC participation—that is, selection bias. There necessarily remains some uncertainty on this point. But our simple theorizing about the WIC enrollment decision, together with the available empirical evidence, leads us to the tentative conclusion that net negative selection into WIC may dominate, in which case both studies might understate WIC impacts to some degree.

Enrollment into WIC is, in principle, a function of both demand- and supply-side decisions. In practice, the WIC program does not seem to have had much excess demand in recent years (BC, 2005, p. 76; see also JGC, 2005, and Besharov & Germanis, 2001), so decisions to volunteer for WIC by low-income women rather than administrative selection might explain most of the variation across the eligible population in WIC enrollment.

For low-income women, the decision to enroll in WIC presumably involves some comparison of the benefits and costs. All else equal, we might expect the costs of enrolling in WIC to be lower for more motivated and well-informed women (Devaney, Bilheimer, & Shore, 1992), although working in the other direction is the fact that the opportunity costs of visiting WIC offices should be lower for women with weaker earnings prospects. Women who derive the greatest benefits from WIC involvement should be more likely to willingly bear the costs of program participation. Variation across women in the benefits from WIC may arise because some women derive from the program larger gains in maternal or child health than do other women, or because some women value a given improvement in health more highly.

¹⁰ For example, the U.S. Statistical Abstracts for 1999 suggests that the poverty rate in New York State in 1997 was about one-quarter higher than the national average (p. 485), while spending on education per capita was 96 percent higher in New York and spending on health and hospitals was 68 percent higher (U.S. Census Bureau, 1999, p. 319).

¹¹ The same point is made more generally by Brien and Swann (2001).

¹² For example, one recent review of the literature notes that “no WIC study seems to have known the mother’s marital status or age at first birth, possibly crucial determinants of her motivation and functioning” (Besharov & Germanis, 2001, p. 37). In contrast, both BC and JGC draw on large and quite rich data on WIC participants to control for a variety of maternal characteristics including marital status, education, age, smoking, and a variety of other socio-demographic characteristics and health behaviors.

The available empirical evidence does not provide much support for the idea that WIC participants necessarily value health more highly than do other low-income women, since women in WIC may smoke more, or at least don't seem to smoke less, compared to other women (BC, 2005; Burstein et al., 2000).¹³ Both BC and JGC present at least suggestive evidence that the effects of WIC on birth outcomes may be more pronounced for relatively more disadvantaged women. Given this finding, consistent with the prediction of our simple model both papers and other WIC research as well suggest that WIC participants are on average more disadvantaged than other women on Medicaid with respect to observable dimensions such as socio-economic status or scores on coping or AFQT cognitive tests (see also Bitler, 1998; Burstein et al., 2000).

It is possible that WIC participants are negatively selected on unobserved maternal characteristics as well, a possibility that receives some highly qualified support from a few different pieces of indirect evidence:

- First is the notion that selection on observable and unobservable characteristics may work in the same direction if the variables measured in social science datasets represent a sample of all relevant individual characteristics (Altonji, Elder, & Taber, 2000).
- Second is evidence from JGC that with respect to most maternal characteristics, increasingly more disadvantaged women are selecting into WIC over time in New York City during the course of their study period. JGC also find that for at least some birth outcomes, estimated WIC effects decline over time even after conditioning on observable maternal characteristics. This pattern is consistent with the idea that increasingly negative selection on observable characteristics is associated with more negative selection on unobservables as well, although these findings could in principle also be due to the waning effects of crack cocaine over the course of their study period.
- Third is evidence by Kowaleski-Jones and Duncan (2002) that controlling for unmeasured maternal fixed effects by comparing outcomes across siblings who differ with respect to prenatal experience with WIC yields slightly larger estimated effects of WIC on birth weights than do standard ordinary least squares estimates. The weight that this branch of the evidence can bear is limited by the fact that their fixed-effects estimates are identified off of only 71 discordant-sibling pairs, and so given the estimated standard errors we cannot formally reject the possibility that the OLS and fixed-effects results are the same.

The argument for negative selection on unobservable maternal characteristics strikes us as plausible, although not as strong as one might wish. We agree with BC's observation that more evidence on the process through which women decide to enroll in WIC would be extremely helpful for understanding program impacts on health outcomes more generally.

Given lingering uncertainty about the role of selection bias for these non-experimental estimates of WIC impacts on birth outcomes, one check on the credibility

¹³ JGC's Table 1 presents more mixed evidence on risky health behavior for WIC women compared to other women on Medicaid in New York City. WIC participants are more likely than other women to smoke in 1988–1992 and 1998–2001, but somewhat less likely to smoke in 1993–1997. Recorded heroin/cocaine use is lower among WIC participants in 1988–1992 than among other women (there are no differences in other years), although as JGC and BC both note, the reliability of this drug-use measure is open to question.

of the results is to determine whether they are consistent with other known facts. For example, as Ted Joyce notes, WIC participation seems to have increased substantially over the past 20 years while the rate of preterm births among African-American women in the U.S. has remained essentially flat.¹⁴ Is it plausible that WIC can have large effects on preterm birth rates that are not reflected in national trends? Drawing any causal inferences about WIC's impacts from the national time series is complicated by the fact that other factors that influence birth outcomes also changed over this period, including maternal age, the distribution of multiple versus singleton births, increasing use of sonogram-guided gestational age estimation, and the share of mothers who are unmarried.¹⁵ Given these concurrent changes in other factors and the fact that the preterm birth rate increased by around one-quarter for whites over the past several decades, it is not obvious that the flat trend in preterm births for Blacks necessarily rules out the possibility of potentially large effects of WIC on this outcome.

CONCLUSION

While we disagree with JGC's argument that birth weight for gestational age is the only birth outcome to consider based on the clinical evidence, we agree with BC's observation that this outcome is particularly important because, in the absence of good information about person-time opportunity to enroll in WIC, it helps account for gestational age bias. Yet focusing on SGA to account for gestational age bias comes at the cost of omitting WIC impacts on birth outcomes that operate through effects on gestational age.

For most other birth outcomes, including birth weight and preterm births, BC and JGC obtain qualitatively similar results, although BC's estimates are typically larger in proportional terms and in some cases very large in some absolute sense as well. For SGA, BC estimate an impact of WIC while JGC do not. It is possible that the two studies bound the true effect of WIC on SGA. JGC's estimates might be understated because of their decision to control for early initiation of prenatal care, which is a potential outcome that could be affected by WIC. On the other hand, early initiation of prenatal care may also be an indication of maternal health motivation, so that the failure to hold this factor constant could lead a non-experimental evaluation to overstate WIC's impacts on birth outcomes. JGC's study might also provide a lower-bound estimate for WIC's impacts nationwide by focusing on New York, which in general seems to have unusually generous social services.

In the end, our reading of the BC and JGC papers taken together leaves us with a less-pessimistic conclusion about WIC's impacts on birth outcomes compared to the interpretation offered by JGC. In part, this reading is also due to the difficulty associated with confidently concluding that WIC "doesn't work," given the unavoidable sampling uncertainty (reflected by standard errors) and conceptual uncertainty (more difficult to quantify) surrounding estimates by BC, JGC, and others in

¹⁴In private correspondence, Ted Joyce notes that from 1981 to 1999 the number of women and of infants in WIC nearly tripled (Committee on Ways and Means, 2000, Tables 15–36) while the rate of preterm birth rates among Black women is about the same in 1981 as in 2002, with a slight increase and then decline coinciding with the crack epidemic (National Vital Statistics Reports, 2003, Table 44).

¹⁵For example, from 1989 to 1996 the share of all singleton births born to mothers age 35 or older increased by 43 percent, while the share born to unmarried women increased by 20 percent; these increases are observed for all race and ethnic groups (Morbidity and Mortality Weekly Report, 1999). From 1980 to 1997, the fraction of births that were twin births increased by 25 percent for Blacks and 59 percent for Whites (National Vital Statistics Reports, 1999, p. 3).

the literature. Small WIC impacts on birth outcomes may be sufficient for program benefits to exceed costs, given the relatively modest program costs per pregnant mother and the substantial medical and other social savings associated with averting even a small number of poor birth outcomes. Even studies that fail to reject the null hypothesis of no WIC impact on birth outcomes will have a difficult time ruling out modest impacts that are difficult to detect with available measurement techniques, but still large enough to generate positive net benefits. In any case we agree with both JGC and BC that there is great public benefit from learning more about WIC's effectiveness.

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