The Changing Association Between Prenatal Participation in WIC and Birth Outcomes in New York City: What Does It Mean?

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Many previous studies of WIC conclude that "WIC works," but in their article in this issue, Ted Joyce, Diane Gibson, and Silvie Colman come to the opposite conclusion (Joyce, Gibson, & Colman, 2005). They arrive at this position even though their actual estimates are consistent with those of previous studies. For example, like our large-scale study of women on Medicaid in 19 states (Bitler & Currie, 2005), they find that prenatal participation in the WIC program reduces the incidence of low birth weight and preterm birth. We also find that WIC participation is associated with decreases in birth weight adjusted for gestation, the nights an infant or woman was in the hospital at delivery, and positively associated with weight gain during pregnancy, gestation, birthweight, and use of prenatal care during the first trimester. Why then are Joyce, Gibson, and Colman's conclusions so different than those of previous authors?

The crux of the matter is that Joyce et al. reject the idea that WIC could have any effect on preterm birth, while neglecting discussion of other possible positive impacts of WIC. Hence, they reason that any estimated effects of WIC on preterm birth reflect selection effects in the data and ought to be discounted out of hand. Further, they argue that WIC should only affect birth weight through effects on fetal growth, and they do not find any effects on fetal growth (except in their subsample of Black twins). We think that this line of reasoning is flawed on several counts, and perhaps too narrowly focused.

First, Joyce et al. overstate the degree of medical consensus that exists over the question of whether or not prenatal intervention can affect the probability of preterm birth. They cite several studies describing specific clinical interventions that were not effective in preventing preterm births. But the clinical literature also suggests, for example, that smoking causes preterm labor, so that interventions that were effective in reducing smoking might be expected to have an effect on prematurity (compare Werler, 1997).[1] Similarly, gestational diabetes is a common complication of pregnancy that can lead to preterm birth, but it is easily controlled if it is detected early (Xiong, Saunders, Wang, & Demianczuk, 2001). Maternal infections are thought to account for up to a third of preterm births, and some trials have shown that treatment of high-risk women for specific infections can increase the length of pregnancy (Gibbs & Eschenbach, 1997; Goldenberg, Hauth, & Andrews, 2000; Locksmith & Duff, 2001), while others have shown a possible positive impact of treatment for wider groups of women (Lamont, Duncan, Mandal, & Bassett, 2003; Kiss, Petricevic, & Husslein, 2004; Ugwumadu, Manyonda, Reid, & Hay,

[1] To say that a specific intervention is not effective in reducing smoking and has no effect on preterm birth does not rule out the possibility that other interventions might be effective.

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2003).[2] Thus, there are interventions that have been shown to have positive effects on the length of gestation.

While it is important to show that preterm birth can be altered by environmental factors, an alert reader may be wondering what these types of interventions have to do with WIC, which offers nutritional supplementation to pregnant women. This brings us to a second flaw in the Joyce et al. argument, which is that they treat WIC as if its only role was to make nutrition supplements appear in women's pantries, ready to eat. There is no discussion of how WIC actually works. In order to receive benefits, women must go to the local WIC office, which is often housed in a clinic. They must be assessed for nutritional risk factors. They receive nutritional counseling.[3] And they must reappear at frequent intervals in order to continue to receive their benefits. 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 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 further investigation.

A third issue that Joyce et al. do not address has to do with measurement. The clinical literature emphasizes that low birth weight can result either from growth retardation during pregnancy or from the premature delivery of an infant on a normal growth path. This distinction is important, but it is also important to keep in mind that birth weight is much better measured than gestational age or fetal growth. This is easy to see in the data, where birth weight follows the expected normal distribution, while gestational age exhibits large spikes at exactly 39 weeks. Moreover, when in doubt about gestational age, clinicians often use birth weight as a way to validate their best guesses. These measurement issues imply that we should be leery about using the distinction between results based on birth weight and results based on gestational age or fetal growth as a basis for determining whether WIC does or does not work.

A fourth issue is that while Joyce et al. frequently refer to selection bias, they fail to confront it head on. The main critique of much of the previous literature is that women on WIC might have some unobservable characteristics that are correlated both with their propensity to be on WIC, and with better birth outcomes. In this case, positive estimated effects of WIC would reflect better underlying characteristics rather than any program effect. The gist of our previous work is to argue that the WIC women are in fact worse off along virtually every observable dimension than the non-WIC women, so that it is highly unlikely that their unobservable characteristics are associated with positive outcomes. One would have to argue, for example, that even though WIC women have less education, are less likely to be married, are more likely to smoke, are more likely to be obese, and have lower scores on a test of coping skills (Burstein et al., 2000), they are nevertheless more likely to have good birth outcomes than other women for unobservable reasons.

Joyce et al. never tell us what these unobservable reasons might be. Like others, they do show that the WIC women are negatively selected in terms of observable variables in the sense that they are more likely than others to have characteristics that lead to negative pregnancy outcomes. According to Table 1, the WIC women

[2] Recent findings suggest that progesterone too may reduce preterm deliveries among high-risk women (Meis et al., 2003), though this finding was not known during the period under study in Joyce et al.

[3] Besharov and Germanis (2001) argue that the nutritional counseling provided by most clinics is ineffective. However, they present little evidence on this point. It is hard to imagine how one could conduct a nutritional risk assessment without communicating some information about good nutritional practices.

are less educated, more likely to be teenagers, more likely to be single, and more likely to smoke than other women. Moreover, these differences become greater over time, indicating that the WIC women become an increasingly negatively selected sample over the 14 years of the study. For example, Table 1 shows that difference between the fraction of WIC mothers who were single and the fraction of non-WIC mothers who were single almost tripled over the time period, from 4.5% to 13.7%. This is hardly a "modest" change in the relative characteristics of the WIC and non-WIC women (see p. 672 of Joyce et al.).

This observation that the WIC women were increasingly negatively selected could easily explain why the estimated "effects" of WIC become smaller over time—if the WIC women have relatively more and more characteristics that would be associated with bad outcomes in the absence of the program, then any positive program effects will be increasingly obscured by this negative selection. The sole exception to the pattern of more negative selection over time is that the fraction of women thought to have used either heroin or cocaine falls more rapidly among the non-WIC women than among the WIC women. But as Joyce et al. acknowledge, this fraction is always very small.[4]

None of the approaches adopted by Joyce et al. actually deal with the selection issue in a way that is markedly superior to previous studies, so it is perhaps unsurprising that their actual estimation results are similar to those of other studies. The first of the three methods they use to deal with selection is focusing on fetal growth as the sole meaningful outcome. The pitfalls of this approach have been discussed above. Second, they allow the effects of WIC to vary over time and argue that effects that vary over time suggest selection effects. We agree with this, and believe that selection effects are causing Joyce et al. to understate the effects of WIC in the later years of their sample, as explained above.

Joyce et al. also try to estimate the effects of WIC on relatively homogeneous groups of women, an approach that many other authors have pursued (for example, we also focus on only Medicaid women, and we examine effects on single women, adult high school dropouts, and teens separately). They find that when they attempt to control for selection by focusing on a relatively homogeneous group of women (for example, U.S.-born Black mothers on Medicaid who are 20 or over and have less than a high school education), the effects are "roughly double the differences obtained for the larger sample of U.S.-born Blacks" (p. 677). They find these large effects even though, by focusing only on women who began prenatal care in the first trimester, they may be excluding some of the effect of WIC. For example, as discussed above, the availability of WIC might improve outcomes by inducing women to begin prenatal care early, in which case excluding late prenatal care initiators would bias down possible WIC impacts.

Finally, Joyce et al. examine twins in an attempt to focus on the women at highest risk of preterm birth. This is an interesting analysis in its own right, but does not really address the selection issue at all (the selection question in this case would be "Which mothers carrying twins choose to enroll in WIC?"). It is interesting that in this twins analysis they find effects of WIC for Blacks.

The Joyce et al. paper has many strengths, including the large sample and all the analyses by sub-group. The analyses of foreign-born Hispanic women are particularly interesting. The estimates suggest that WIC has positive effects for Blacks but not for foreign-born Hispanics, which might be because these Hispanics are generally at much lower risk of negative pregnancy outcomes than other women. If so, it would be supportive of previous research that finds that WIC has its largest effects on the

[4] It is also unclear how accurately measured drug use is on birth certificates.

women who are at greatest risk. Alternatively, the finding might reflect differences in the programs that serve Hispanics and Black women. This question deserves further investigation.

It would be a tragedy if the results of this study were used to justify the elimination of WIC, given that the study actually finds positive effects for Black mothers, one of the groups at highest risk of negative pregnancy outcomes. Program elimination is a real threat in a time when all social programs are threatened by massive federal budget deficits. The only silver lining is that the controversy invited by this article will doubtless inspire further work asking not only whether WIC works, but *how* WIC works.

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