An Evaluation of the Effects of a Breastfeeding Support Program on Health Outcomes

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Objective. To estimate the causal effect of a Michigan peer counselor (PC) breastfeeding support program for low-income women on infant health outcomes.

Data Sources. Program referral forms, program forms (enrollment, birth, and exit data), and state administrative data from the Women Infants and Children program, Medicaid, and Vital Records.

Study Design. Quasi-random enrollment due to the excess demand for PC breastfeeding support services allowed us to compare the infants of women who requested services and were enrolled in the program (the treatment group, $N = 274$) to the infants of women who requested services and were not enrolled (the control group, $N = 572$). Data were analyzed using regression.

Principal Findings. The PC program increased the fraction breastfeeding at birth by 19.3 percent and breastfeeding duration by 2.84 weeks. Program participation also reduced the fraction of infants with gastrointestinal disorders by a statistically significant 7.9 percent. The program, if anything, increased the overall health care utilization.

Conclusions. This Michigan PC breastfeeding support program resulted in improvements in breastfeeding and infant health outcomes as measured by the diagnosis of ailments while increasing health care utilization.

Key Words. Breastfeeding, peer counselor support program, program evaluation, health outcome measurement

Breastfeeding imparts numerous health benefits to the infant and mother (Surgeon General, U.S. Department of Health and Human Services 2011).¹ The American Academy of Pediatrics, the American Dietetic Association, and the World Health Organization recommend exclusive breastfeeding through 6 months of life and continued breastfeeding, with complementary foods, to a year or beyond for almost all infants (World Health Organization 2002;

Dobson and Murtaugh 2009; American Academy of Pediatrics, Section on Breastfeeding 2012).

Despite these recommendations, only 77 percent of all US women initiated breastfeeding and 49 percent continued to breastfeed at 6 months in 2010 (Centers of Disease Control and Prevention 2013). These rates are even lower among low socioeconomic status (SES) mothers. For example, among women classified to be at nutritional risk nationally, only 62 percent initiate breastfeeding and 27 percent continue to breastfeed at 6 months (Centers for Disease Control and Prevention 2009). These rates are even lower among at-risk mothers in Michigan, the state on which we focus, with only 54 percent initiating breastfeeding and 19 percent continuing to breastfeed until 6 months (Centers for Disease Control and Prevention 2009).

Public health departments have adopted breastfeeding support programs for low-income mothers that use peer counselors (PC), based on the United States Department of Agriculture's "Loving Support" model (US Department of Agriculture 2013). There are numerous studies of PC support programs, but reviews note that most do not use convincing analytic methods to uncover the causal effects (Renfrew et al. 2007, 2012). There exist two notable exceptions. One used a randomized control trial and found improved breastfeeding rates in an urban, low-income Hispanic population of women (Chapman et al. 2004). Another used a quasi-experimental design based on excess demand for a PC program and found improvements in breastfeeding through 6 months (Olson et al. 2010). No study has used convincing analytic methods to examine how such programs affect infant health or health care utilization.

The primary purpose of this study was to examine the causal effect of a PC breastfeeding program in Michigan on infant health. We used the same quasi-experimental strategy adopted in a previous article to examine whether the same program affected breastfeeding (Olson et al. 2010). The secondary purpose was to examine how our results compared between diagnoses outcomes and health care utilization outcomes.

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METHODS

Program Description

The Breastfeeding Initiative (BFI), a Michigan peer counseling breastfeeding support program, is a collaboration between the Michigan Department of Community Health's Supplemental Nutrition Program for Women, Infants, and Children (WIC) and Michigan State University Extension (MSUE). The BFI was established in 1993 and has served over 33,000 women in the last 10 years. It began in three counties and expanded to 22 counties during the early 2000s, the time period analyzed (Oral communication with Ms. Patricia Benton, MSUE, BFI Program Leader).

The BFI program provides breastfeeding education and support to low SES women through the use of mothers recruited from the community to serve as peer counselors. These PCs were required to have a high school education or equivalent, have access to a means of transportation, and have expressed a positive breastfeeding experience. They are then trained on how to provide breastfeeding support and collect and record program data. These support services include providing breastfeeding advice during home visits and by phone, access to technical advice from lactation consultants, and providing advice regarding nutrition, health, and other local services for which the mothers are eligible. Mothers remain in the program until they discontinue breastfeeding, the baby is 1 year old, or support services are no longer desired.

Participants are referred to the program primarily through WIC clinics but also by hospitals, health care providers, and other participants. About 50 percent of mothers are recruited into the program prenatally and another 35 percent within the first 4 weeks after birth; in our analysis, we only included mothers who were recruited prenatally to avoid crediting the BFI program with assisting mothers to breastfeed who had already started to breastfeed. The BFI program is designed to have the PC provide an in-person contact with a participant just after the birth of her child, with subsequent contacts, at least on a monthly basis, occurring in person or by telephone based on the type of support needed. Program data from our study period indicate that the majority of participants received 1–2 home visits, two personal contacts in locations other than the home, and 5–6 telephone contacts during their participation in the program. A systematic description of the BFI program is provided in Bolton et al. (2009).

Quasi-Experimental Design

The purpose of this study was to determine the causal effect of the BFI program participation on infant health and health care utilization. A naïve method would compare infants of women who participated in the program to those who did not. However, such a method would have ignored the likely confounding factor that mothers who seek out a breastfeeding support program are more likely to breastfeed than those mothers who do not seek out such a program, and may in addition engage in other beneficial behaviors for the infant. Thus, any differences we found with such a naïve method would then reflect the effects of the program on the infant and these other differences between participants and nonparticipants. In short, program participation should be expected to be endogenous, and simple comparisons between participants and nonparticipants would not isolate the causal effect of program participation.

Although a true randomized experiment was not built into the BFI program, a feature of the program existed that can be used to approximate a randomized experiment and address the endogeneity problem. Specifically, there was substantially more demand for the services of the program than could be provided, so PCs contacted only some of the individuals for whom they received a referral form. Any selective contact, at most, could have been based on the limited amount of information available on the referral forms. Under the assumption that PCs contacted individuals independently of underlying breastfeeding propensities once we control for information available on the referral form and the county of residence (which corresponds to the BFI service area), we can obtain the causal effect of the BFI program by comparing those who requested service and were contacted (our treatment group) to those who requested service and were not contacted (our control group).

On the basis of these program features and assumptions, we used the following linear regression to estimate the causal effect of the BFI program,

$$y_{ic} = \beta_1 \text{treat}_{ic} + \beta_2 X_{ic} + \alpha_c + \varepsilon_{ic} \quad (1)$$

where y_{ic} is an outcome, treat_{ic} is an indicator for whether a BFI applicant was contacted by a counselor, X_{ic} is a vector of background characteristics that includes any information that was collected on the referral forms and other characteristics that potentially affect the outcome, and α_c are county-specific intercepts that are estimated by the inclusion of county-specific indicators (i.e., a county “fixed effects” specification; see Wooldridge 2010).² We interpret the coefficient β_1 as the causal effect of the BFI program on y based on our

assumption that, once we control for county and the referral form characteristics that are included in X , the offer of treatment is exogenous.

Using the estimating equation in (1), we first explored the validity of our methods by examining whether treatment status was correlated with several observable, preprogram characteristics that were not on the referral form (Table 1). We then examined whether treatment status affected a variety of breastfeeding (Table 2), health (Tables 3, 4, and A2), and health utilization outcomes (Table 5).

Data

Our sample came from five counties that retained referral forms: Lenawee, Monroe, Newaygo, Sanilac, and Wayne. After restricting our sample to individuals for whom Medicaid claims data were available and who were recruited into the program prenatally, there were 274 women in the treatment group and 572 women in the control group. We used state administrative data obtained from the Michigan Department of Community Health Data Warehouse, which included WIC, Medicaid, and Vital Records information. Using program forms completed at referral (and if enrolled, at enrollment and program completion), we obtained personal information that allowed us to locate individuals' data in the Data Warehouse.

We measured health during the first year of the infant's life by determining whether each infant received any diagnoses for conditions that previous studies have identified as being influenced by breastfeeding, including gastrointestinal issues, lower respiratory tract conditions, ear infections, and allergic reactions (Ip et al. 2009). Gastrointestinal issues included gastroenteritis/diarrhea, constipation, colic, and necrotizing enterocolitis. Lower respiratory tract conditions included reactive airways disease, asthma, wheezing, bronchiolitis, bronchitis, and pneumonia. Allergic reactions included atopic dermatitis and food allergies. As a falsification exercise, we examined the effects on injuries and hernias, which should not be affected by breastfeeding. Diagnoses were identified in the Medicaid claims data using International Classification of Diseases, 9th revision, Clinical Modification (ICD-9-CM) coding schemes. We also quantified total inpatient and outpatient visits, Emergency Department (ED) visits, inpatient days, well-child visits, and sick visits using Current Procedural Terminology (CPT) codes, and number of distinct diagnoses, as well as total medical expenditures that include Medicaid reimbursements and any copays (not accounting for prescription spending), all during the first year of the infant's life.

Table 1: Preprogram Characteristics for the Treatment and Control Groups

	<i>Unadjusted Means</i>		<i>Regression Adjusted</i>	
	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>95% CI</i>
Sample size	274	572		
<i>Background characteristics</i>				
Mother's education (years)	11.7	11.7	0.037 (0.140)	[−0.237, 0.311]
Household annual income (\$)	14,704	13,757	1,031 (788)	[−516, 2,578]
<i>Pregnancy characteristics</i>				
Tobacco use during pregnancy	0.242	0.262	−0.051 (0.033)	[−0.115, 0.014]
Drinks per week during pregnancy	0.004	0.009	−0.005 (0.010)	[−0.024, 0.014]
Early prenatal care [‡]	0.897	0.891	0.005 (0.025)	[−0.044, 0.053]
Adequate prenatal care [§]	0.761	0.771	0.032 (0.034)	[−0.035, 0.099]
<i>Birth characteristics</i>				
Female infant	0.511	0.500	−0.024 (0.040)	[−0.103, 0.054]
Birth weight (g)	3,294	3,254	78.5 (40.8) [†]	[−1.6, 158.6]
Gestational age (weeks)	39.0	38.9	0.268 (0.138) [†]	[−0.003, 0.5401]
Apgar score (0–10)	9.04	8.93	0.076 (0.043) [†]	[−0.008, 0.161]
Admitted to the NICU	0.033	0.025	0.006 (0.013)	[−0.020, 0.031]

Note. The regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother's age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, and indicator variables for whether previous breastfeeding and previous pregnancy information are missing. Standard errors are in parentheses.

Statistical significance is denoted as follows: [†]indicates $p < .1$, ^{*}indicates $p < .05$, and ^{**}indicates $p < .01$.

[‡]Early prenatal care is an indicator variable for whether the woman sought care before the 5th month of pregnancy.

[§]Adequate prenatal care is an indicator variable for whether the amount of prenatal visits is deemed adequate or better as measured by the Kotelchuck index, which compares the actual prenatal visits to the expected prenatal visits from the start of pregnancy care.

More details on our data sources, coding, and sample sizes is available in the Supplemental Appendix.

RESULTS

Validity of the Quasi-Experimental Estimation Strategy

We undertook numerous tests to assess the validity of the quasi-experimental estimation strategy, which were previously described in significant detail (Olson et al. 2010). We performed several additional tests for this analysis because the sample was further restricted from our previous study to

Table 2: Breastfeeding Outcomes for the Treatment and Control Groups

	<i>Unadjusted Means</i>		<i>Regression Adjusted</i>	
	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>95% CI</i>
Total weeks	7.32	5.08	2.84 (.747)**	[1.37, 4.30]
Fraction breastfeeding at				
Birth	0.686	0.493	0.193 (0.038)**	[0.118, 0.268]
3 months	0.234	0.175	0.089 (0.031)**	[0.027, 0.150]
6 months	0.153	0.086	0.087 (0.025)**	[0.038, 0.135]
9 months	0.015	0.010	0.005 (0.009)	[−0.012, 0.022]
12 months	0.000	0.002	−0.002 (0.003)	[−0.007, 0.004]

Note. The regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother's age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, an indicator for whether the infant is female, a quadratic in birth weight, years of education, an indicator for early prenatal care, and indicators for whether information on education, prenatal care, previous breastfeeding experience, and prior pregnancies is missing. Standard errors are in parentheses.

Statistical significance is denoted as follows: † indicates $p < .1$, * indicates $p < .05$, and ** indicates $p < .01$.

Table 3: First Year Health Outcomes for the Treatment and Control Groups, Any Diagnoses

	<i>Unadjusted Means</i>		<i>Regression Adjusted</i>	
	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>95% CI</i>
Any gastrointestinal diagnoses	0.288	0.346	−0.079 (0.038)*	[−0.153, −0.005]
Any lower respiratory diagnoses	0.361	0.379	−0.072 (0.038)†	[−0.146, 0.003]
Any ear infection diagnoses	0.409	0.425	−0.049 (0.039)	[−0.125, 0.027]
Any allergy diagnoses	0.128	0.136	−0.024 (0.027)	[−0.077, 0.030]
Any injury	0.109	0.110	−0.007 (0.025)	[−0.057, 0.042]
Any hernia	0.015	0.017	−0.005 (0.010)	[−0.025, 0.015]

Note. The regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother's age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, an indicator for whether the infant is female, a quadratic in birth weight, years of education, an indicator for early prenatal care, and indicators for whether information on education, prenatal care, previous breastfeeding experience, and prior pregnancies is missing. Standard errors are in parentheses.

Statistical significance is denoted as follows: † indicates $p < .1$, * indicates $p < .05$, and ** indicates $p < .01$.

individuals for whom Medicaid data could be matched. We first examined whether our data were consistent with the assumption that the PCs were unable to target women for treatment based on information that was not pres-

Table 4: First Year Health Outcomes for the Treatment and Control Groups, Number of Diagnoses for Patients with at Least One Diagnosis

	<i>Unadjusted Means</i>		<i>Regression Adjusted</i>	
	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>95% CI</i>
Number of gastrointestinal diagnoses	1.899	1.581	0.258 (0.157)	[−0.050, 0.567]
Number of lower respiratory diagnoses	2.737	3.161	0.005 (0.430)	[−0.842, 0.851]
Number of ear infection diagnoses	1.929	2.152	−0.146 (0.187)	[−0.514, 0.223]
Number of allergy diagnoses	1.486	1.218	0.390 (0.183)*	[−0.026, 0.753]

Note. This table includes those individuals who had at least one diagnosis in each category; for example, the first row contains results for the number of unique visits that an infant had a gastrointestinal diagnosis among those infants with at least one gastrointestinal diagnosis. The regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother’s age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, an indicator for whether the infant is female, a quadratic in birth weight, years of education, an indicator for early prenatal care, and indicators for whether information on education, prenatal care, previous breastfeeding experience, and prior pregnancies is missing. Standard errors are in parentheses.

Statistical significance is denoted as follows: † indicates $p < .1$, * indicates $p < .05$, and ** indicates $p < .01$.

ent on the referral form. We assessed this assumption with equation (1) by specifying our regressor set X to include those characteristics that may have been known at the time of referral: county, maternal race/ethnicity, maternal age, whether there was a previous pregnancy, and whether the mother previously breastfed a child in those counties that collected such information. We then specified the outcome variables to be other preprogram characteristics that were not on the referral from, including background characteristics (mother’s education and household income), pregnancy characteristics (tobacco use during pregnancy, drinks per week during pregnancy, an indicator for early prenatal care, and an indicator for adequate prenatal care), and birth characteristics (whether the infant was female, birth weight, gestational age, the Apgar score, and whether the infant was admitted to the NICU). For each of these preprogram characteristics, we calculated the unadjusted means for both treatment and control groups and the regression adjusted difference (Table 1).

In many cases, the unadjusted means are practically identical, including mother’s education (11.7 years for treatments and controls), the fraction receiving early prenatal care (0.897 for treatments and 0.891 for controls), and gestational age (39.0 weeks for treatments and 38.9 for controls). After adjusting for county and information available on the referral form, the controls and treatments were not statistically different at the .05 level for any of the

Table 5: First Year Health Utilization Outcomes for the Treatment and Control Groups

	<i>Unadjusted Means</i>		<i>Regression Adjusted</i>	
	<i>Treatment</i>	<i>Control</i>	<i>Difference</i>	<i>95% CI</i>
Expenditures	3,017	2,887	770 (865)	[−927, 2467]
Number of encounters	16.0	15.0	1.49 (0.97)	[−0.40, 3.39]
Number of ED visits	1.3	1.4	−0.16 (0.14)	[−0.44, 0.12]
Number of inpatient days	1.7	1.6	0.47 (0.33)	[−0.18, 1.12]
Number of well-baby visits	5.4	4.8	0.50 (0.26)*	[0.00, 1.00]
Number of well-baby visits, capped at a maximum of 7	4.7	4.3	0.42 (0.17)*	[0.09, 0.75]
Number of sick visits	10.7	10.2	1.0 (0.90)	[−0.78, 2.76]
Number of sick visits with capped well-baby visits	11.3	10.7	1.07 (0.93)	[−0.75, 2.89]
Number of distinct diagnoses	10.2	9.9	−0.19 (0.49)	[−1.14, 0.77]

Note. The regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother's age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, an indicator for whether the infant is female, a quadratic in birth weight, years of education, an indicator for early prenatal care, and indicators for whether information on education, prenatal care, previous breastfeeding experience, and prior pregnancies was missing. Encounters refer to any claims on a specific day and can include inpatient and outpatient visits. ED visits refer to number of visits on different days to a hospital emergency department; inpatient stays refer to number of days when there was a hospital stay or any other services received as part of an inpatient stay. Number of sick visits refers to any encounters that were not well-child visits and include outpatient doctor visits, inpatient days, and ED visits. Standard errors are in parentheses. Statistical significance is denoted as follows: † indicates $p < .1$, * indicates $p < .05$, and ** indicates $p < .01$.

preprogram characteristics. Thus, we have no evidence that PCs were able to systematically target mothers based on any information that was not on the referral forms.

On the basis of these encouraging results, we estimated the causal effect of the program using equation (1) as the treatment-control difference after adjusting for county indicators, variables for the information that was available on the referral form, and several additional control variables: maternal education, an indicator for whether the infant is female, a quadratic in birth weight, an indicator for early prenatal care, and indicators for whether maternal education and prenatal care information was missing. Our motivation for including these additional controls variables is to correct for any potential remaining group differences that could affect breastfeeding and/or health outcomes and utilization and to increase the precision of our results.

Breastfeeding Outcomes

Because our sample in this study differs from that in our previous study (we necessarily restricted our data to those mothers for whom Medicaid data were available), we first reanalyzed the effects of the BFI on breastfeeding (Table 2). Our measure of breastfeeding is based on the length of time the mother reported any breastfeeding to the WIC program. The unadjusted mean breastfeeding duration was 7.32 weeks for the treatment group versus 5.08 weeks in the control group in our current sample. This compared to 7.83 and 5.66 weeks found in our previous study. The adjusted difference of 2.84 weeks was statistically significant ($p < .001$), as was the difference in the fraction breastfeeding at birth (0.193, $p < .001$), at 3 months (0.089, $p = 0.005$), and 6 months (0.087, $p < .001$).

Health Outcomes

As our primary health outcome measures, we examined four broad diagnoses categories that the previous literature suggests are affected by breastfeeding (Table 3). See the Appendix for the individual diagnoses that comprise each diagnoses category, as well as results for each of the individual diagnosis codes.

We found that the treatment group had 0.079 fewer individuals diagnosed with any gastrointestinal ailment, a difference that was significantly different from zero at the .05 level. We also found that the treatment group had 0.072 fewer individuals diagnosed with any lower respiratory tract ailment, although the difference was marginally insignificant ($p = .059$). Although there were also fewer ear infection diagnoses (0.049) and allergy diagnoses (0.024) in the treatment group as compared to the control group, the standard errors associated with these differences are large enough that little should be said regarding whether there were any benefits of the program.

We also examined two diagnoses categories that are not believed to be affected by breastfeeding as a falsification exercise: injuries and hernias. The unadjusted means were much closer for these categories between the treatments and controls, as were the adjusted differences. The adjusted differences were not statistically significant at conventional levels. These findings further supported our identification strategy.

For each of these health outcomes, we then examined whether the program affected the number of visits among those patients who had a diagnosis; in other words, we examined the effect of the program on the “intensive

margin” of diagnoses. We defined a visit as the number of unique days that a diagnosis occurred. The results for the number of visits within each diagnosis category are mixed (Table 4). For the two categories for which there was any evidence that the program improved outcomes, gastrointestinal and lower respiratory tract diagnoses, we found no evidence that there were differential visits between the treatment and control groups. For the outcome where there was the least evidence of improvements for the treatment group, the allergy diagnoses category, we found some evidence that the treatment group had *more* visits than the control group (0.390 more visits, $p = .036$). We suspect that this anomalous finding is related to a larger interpretive issue that we return to below: the number of visits measures both the severity/frequency of illness and the propensity of individuals to seek medical care.

Lastly, we examined whether there were treatment-control differences on a variety of health care utilization measures (Table 5). Subjects in the treatment group had *higher* expenditures (an adjusted \$770 more) and *more* doctor visits than did subjects in the control group during the infant’s first year (an adjusted 1.49 more visits), although both differences were not statistically significant. Further probing these results, we found that this difference was driven by the treatment group receiving more well-baby visits (an adjusted 0.50 [$p = .050$] more in total well-baby visits, and an adjusted 0.42 [$p = .012$] more in total well-baby visits when capping the total number of visits at the recommended 7). There were no differences that were even close to being statistically significant for the other outcomes (e.g., the number of diagnoses, the number of sick visits, or the number of emergency department visits).

DISCUSSION

In this study, we examined the causal effects of a breastfeeding support program on the health and health care utilization of infants by comparing those of mothers who participated in the program to those of mothers who applied to the program, but then were not served because of capacity constraints. Confirming our previous analysis, we found that our data were consistent with our key identifying assumption in that treatment was not targeted at preprogram characteristics that should have been unobserved by the PC (Table 1 results) and that the program improved breastfeeding outcomes for participants (Table 2 results). We also found that our data passed an additional test of our key assumption: there were no differences between the treatments and con-

trols for diagnoses that should be unaffected by the program (the falsification exercise in Table 3).

We then examined whether the program affected health and health care utilization by linking program data to high-quality administrative data from the state Medicaid and WIC programs and Vital Records. We found about a 20 percent decline for the treatment group in the prevalence of gastrointestinal diagnoses (7.9 percentage points, $p = .038$). Although the adjusted means suggested declines for lower respiratory tract diagnoses, ear infection diagnoses, and allergy diagnoses for the treatment group, these declines were not statistically significant at the .05 level. We also provided evidence about how the program affected the intensity of health care use among those with a diagnosis. We found no effect on the intensity of use for those outcomes where we found evidence that the program affected health, but we did find that the treatment group sought out services more frequently for allergy-related diagnoses. Importantly, all of these effects were found for a program that prolonged breastfeeding by just under 3 weeks.

Previous research has shown broader effects of breastfeeding on health (Ip et al. 2009). We expected smaller differences between our treatment and control groups for several reasons. Previous studies tend to compare breastfed and nonbreastfed infants, whereas we compared enrolled to nonenrolled referrals. Our data show that among the control group, women almost 50 percent began breastfeeding and 10 percent still breastfed at 9 months. In addition, our study followed children only through their first year of life; it is possible that even greater differences accumulate over a longer time period (Sonnenschein-van der Voort et al. 2012).

Given the richness of our administrative data, we also examined the causal effect of the BFI program on several health utilization outcomes. There were no differences between the treatment and control groups in number of doctor visits, sick visits, distinct diagnoses, or expenditures at usual significance levels. However, infants of mothers in the treatment group had about 10 percent (0.42 visits when the measure is capped, $p = 0.012$) more well-baby visits in the first year of life. Thus, if we were forced to rely just on health care utilization data, as do many other studies, we would have concluded that, if anything, the BFI program *harmed* the infants.

With that said, the finding that the program caused there to be more well-baby visits should not be surprising. Recall that the BFI program trained the PCs to refer women to other services to which they are entitled, including the use of routine medical services provided by Medicaid. This finding has several important implications. First, previous research has found that the use

of well-baby visits among low-income children resulted in better health outcomes (Hakim and Bye 2001). Importantly, the mean number of well-baby visits for both groups (4.7 for the treatments and 4.3 for the controls with the capped measure, or 5.4 and 4.8 respectively with the uncapped) were well below the recommended 7 well-baby visits that are suggested by the American Academy of Pediatrics, so an increase in usage represents an improvement (American Academy of Pediatrics, Committee on Practice and Ambulatory Medicine 2007). The importance of such preventive care was recently emphasized in The Patient Protection and Affordable Care Act, which included a provision for appropriation of up to \$100 million in funding for states to develop incentive-based programs within Medicaid to promote behavior change through the use of preventive services (US Congress, 111th 2010). Second, the utilization findings make clear that the health effects should not be attributed to just increases in breastfeeding. The PCs not only encouraged the mothers to breastfeed, but the PCs also encouraged the mothers to use of many other services, including WIC, community health services, nutrition education, parenting, and any other resources available in the local community. Any improvement in use of these services could have improved the health of the infants.

We suspect that similar factors were behind our results regarding the intensity of diagnoses. Our findings indicated that the treatment group had a similar number of health care visits, and even more for allergy-related diagnoses, when compared to the control group. However, given that the treatment group was being provided medical advice by the peer counselors, including the appropriateness and importance of seeking medical care, the treatment group could have been more likely to seek out medical care for their infants whenever symptoms arose. Because all infants in our sample received at least some medical attention, our outcomes regarding any diagnoses were less influenced by such issues.

A Cost-Benefit Analysis of the BFI Program

Although we did not perform a formal cost-benefit analysis of the BFI program, it is worth considering what such an analysis would look like to put the benefits of the program into perspective. The direct costs of the program are small given its reliance on paraprofessionals: the costs were less than \$434 per woman served in 2001/2002 (Oral Communication, Ms. Patricia Benton, MSUE, BFI Program Leader).

If we took a narrow, short-term view of our results in which we compared these costs to potential savings in health care utilization during the first year, this program would fail a cost-benefit test. Quite simply, our health care utilization results (Table 5) show that, if anything, the health care utilization is *higher* for the treatment group (the only difference that is significantly different from zero is that the treatment group infants receive *more* well-baby visits). Thus, for a cost of \$434 per mother, there is then more health care utilization during the first year.

We believe such a view is incorrect for two reasons. First, our health care utilization results only examine the first year of an infant's life. It is possible that there could be health benefits that extend beyond the first year of life. Second, the narrow view assumed that more well-baby visits were a cost of the program. However, as noted in the previous section, the mothers in our study were obtaining far fewer than the recommended number of well-baby visits. It very well could be that this increase in health care utilization will end up benefiting these infants in future years.

Based on these concerns, our view of the cost effectiveness of the program is much different. Specifically, for the relatively modest cost of \$434 per mother, we found that the BFI program caused an increase in breastfeeding rates and well-baby visits and a decrease in the prevalence of gastrointestinal and respiratory conditions. Importantly, these beneficial effects were found for a low SES group, which is already at a greater risk for early discontinuation of breastfeeding, poor compliance with well care and immunization schedules, and poorer health care outcomes in general (Bauman, Silver, and Stein 2006; Van Berckelaer, Mitra, and Pati 2011). Thus, we find that this breastfeeding support program, for a relatively modest cost, improves health outcomes *and improves health care utilization by increasing well-baby visits*. Consequently, we conclude that this program should be expanded to and evaluated in other settings to assess the extent to which it can become a potentially important part of the public health landscape.

Limitations

There are several limitations to our study. The first is that our results rest on the validity of our assumption that the PCs contacted women randomly upon receipt of an overabundance of referrals. While we did not find any evidence contradicting this assumption, the assumption itself cannot be tested. Second, we were only able to examine the effect of the program on women who requested support services given our identification strategy. It may be true that women who request services make the most of that opportunity and

thus the average effectiveness would be larger for women in our study when compared to the overall population of low-income women. Finally our sample sizes of some of the disease subgroups were small, limiting our ability to detect differences; there very well may be additional health benefits that we lack the statistical power to detect.

CONCLUSIONS

Using a quasi-experimental identification strategy, we examined the causal effects of a breastfeeding support program on the health and health care utilization of infants. We found that the program increased breastfeeding, reduced the prevalence of gastrointestinal disorders among infants, and increased well-baby visits; we found improvements in other health outcomes associated with breastfeeding, but the estimates were not statistically significant at standard levels. On a methodological level, these findings suggested the importance of examining multiple outcomes to determine the effectiveness of a program. On a substantive level, these findings suggested that, at relatively modest costs, the PC program improved multiple health infant outcomes from a vulnerable population that requested breastfeeding counseling services.

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NOTES

1. A vast literature using observational methods has repeatedly documented numerous benefits of breastfeeding. A recent AHRQ review found associations between breastfeeding and lower risk for infants of otitis media, gastroenteritis, lower respiratory infection, atopic dermatitis, asthma, obesity, type 1 and 2 diabetes, childhood

leukemia, and sudden infant death syndrome (Ip et al. 2009). The only intervention of which we are aware in a developed country to improve breastfeeding rates also measuring health outcomes is the PROBIT trial conducted in Belarus. The study found improvements in incidence of gastroenteritis and atopic dermatitis in the first year of life (Kramer et al. 2001).

2. The county referral forms differed with respect to the information they collected. Our regressor set includes characteristics that were found on the referral form of any county (typically just maternal age, previous breastfeeding experience, and name, which might have signaled race/ethnicity), in addition to other observable characteristics that were thought to influence the breastfeeding decision. The complete regressor set includes indicator variables for county, indicator variables for race/ethnicity of mother, a quadratic in mother's age, an indicator for whether there was a previous pregnancy, an indicator variable for whether the mother previously breastfed a child, an indicator for whether the infant is female, a quadratic in birth weight, mother's years of education, an indicator for early prenatal care, and indicators for whether information on education, prenatal care, previous breastfeeding experience, and prior pregnancies is missing.

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of this article:

- Appendix SA1: Author Matrix.
- Data S1. Supplemental Appendix.